Treatise on cholera asphyxia, or epidemic cholera, as it appeared in Asia, and more recently in Europe. With practical remarks on the disease in Europe ; an appendix of cases ; and the reports and regulations of the Boards of Health of London and Edinburgh.

TREATISE

ON

CHOLERA ASPHYXIA,

OR

EPIDEMIC CHOLERA,

AS IT APPEARED IN ASIA, AND MORE RECENTLY IN EUROPE.

WITH

PRACTICAL REMARKS ON THE DISEASE IN EUROPE; AN APPENDIX OF CASES; AND THE REPORTS AND REGULATIONS OF THE BOARDS OF HEALTH OF LONDON AND EDINBURGH.

BY

GEORGE HAMILTON BELL,

FELLOW OF THE ROYAL COLLEGE OF SURGEONS, EDINBURGH; LATE RESIDENCY SURGEON, TANJORE; AND JOINT MEDICAL SECRETARY TO THE EDINBURGH BOARD OF HEALTH.

SECOND EDITION,

VERY GREATLY ENLARGED.

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TO

SIR CHARLES BELL, F. R. S.

CIVIL KNIGHT OF THE GUELPHIC ORDER, ETC.

My Dear Sir,

The favourable reception which the former Edition of this work has met with from the profession and the public, affords me the pleasure of bringing the Second Edition in this manner under your notice. During the fourteen years which have elapsed since I ceased to be your pupil, it has been my constant aim to profit by the lessons which I was taught by you, and by your late brother, Mr John Bell; and as there is no one to whom, both professionally and individually, I owe so many obligations, I have the sincerest gratification in inscribing this Volume to you, as a trifling, but most willing, tribute of respect and gratitude.

I am,

With great regard,

Your obliged nephew and faithful servant,

G. HAMILTON BELL.

ADVERTISEMENT.

When the former edition of this work was published in June last, the disease of which it treats was comparatively remote from us. But those acquainted with its habits had at that time the greatest reason to fear, from the direction in which it seemed to be moving, that it would overcome every barrier, and within a short time make its appearance in Great Britain. This anticipation has been unfortunately verified; and now that Cholera may be said to be actually amongst us, it is still more to be regretted that both medical men and the public press, should have lent themselves so readily to the propagation of exaggerated and alarming accounts of the mortality, as well as of the supposed contagious nature, of the disease.

Entertaining the opinions which I have formed on the subject of contagion, I have found it necessary in the present edition to treat the subject

pretty fully; but I hope that, in stating my views, I have shewn no want of the respect and deference which I owe to those distinguished medical men from whom I differ. Neither has it been my wish to undervalue the numerous unprofessional writers who, in various ways, have favoured the public with their sentiments on this question; although it is impossible not to regret that, on a point of such momentous importance, some of those writers had not been a little more calm and dispassionate in their examination of the evidence; and rather better informed as to the facts on which they profess to found their conclusions.

Now, indeed, that the disease has gained a footing in this country, and that the chief effect of inculcating the doctrine of contagion must be, to excite panic and uneasiness at home, and to impose additional restrictions on our foreign commerce, it seems to me any thing but judicious to press this controversy in the manner in which the contagionists have done. If the disease be contagious, the contagion, to say the least of it, must be of a very peculiar and capricious character, and far from dangerous to every one who comes in contact with the sick. Hence we may hope, that in those towns in which Cholera may make its appearance, the

medical men and others whose assistance is indispensable, will not allow their exertions to be paralyzed, by fear for their personal safety. On this subject I take the liberty of referring to the more extended discussion, which occupies a portion of the following work.

In preparing this edition, I have availed myself of all the information concerning the disease, its symptoms and treatment, which its nearer approach has brought within my reach. I have also adverted to those peculiarities which have marked its progress through Europe, and particularly in England; and, in the Appendix, I have arranged the Reports and the practical directions of the Boards of Health of London and Edinburgh, in a manner which I hope will be found useful. It is a fortunate circumstance, that, however much medical men may differ as to the mode in which Cholera is propagated, they are now pretty much agreed as to the course of treatment which has been found, on the whole, most successful; and best deserving of their sanction, in the general directions given to the public.

By the kindness of my friends Drs MEIKLE and STEVENSON, who saw much of the disease in India, I have been enabled to give, in the Appen-

dix, several of the cases which they treated or saw at Haddington; and Mr STEELE has been so obliging as to allow me to add to those cases an exceedingly interesting case of the disease which occurred in the parish of Newton, and was successfully treated by him.

EDINBURGH,
No. 13. CASTLE STREET,
17*th January* 1832.

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ERRATA.

Page 2, line 21, *for* his *read* my
—— 69, —— 14, *for* delicate *read* deliberate
—— 100, —— 4, *for* all *read* most

CHOLERA ASPHYXIA.

INTRODUCTORY OBSERVATIONS.

From the year 1818 to 1827 the Author served as Assistant-Surgeon in the Honourable East India Company's Service within the Presidency of Madras; and during that period the Epidemic Cholera prevailed, more or less generally, in the Peninsula. On his arrival in India, he had the advantage of being placed in the General Hospital at Madras under Mr Surgeon Annesley, who was then in charge of that establishment; and as Cholera at that time prevailed to an alarming extent, and in a very destructive form, in that Presidency, every opportunity was afforded of observing and studying the disease; and both duty and interest concurred in recommending the study of it to the Author's fullest attention.

At Madras, and subsequently in various districts in the interior of India, I saw the disease under every possible aspect; in all its stages; in every variety of the patient's condition; in the hospital, in the camp, and in private dwellings; among the natives and

among Europeans; among those newly arrived in India, and among those who had been long inured to the climate. Patients of every class, native and European, are received into the Madras Hospital; and, at the time when I was appointed to do duty under Mr Annesley, the King's 13th Regiment of Light Dragoons had just arrived from England, while the 84th Regiment of Infantry had, after a long residence in India, reached the coast for re-embarkation. There was, consequently, presented the most complete exhibition of the disease, on almost every imaginable condition of the human constitution.

At that time Mr Annesley was prosecuting, with great care, his inquiries into the nature and treatment of the Epidemic Cholera, which was then very generally believed to be a new disease; and I had not only an opportunity of partaking in those inquiries, and of seeing Mr Annesley's practice, but of assisting him in *post-mortem* examinations. In the course of my subsequent duties in various districts of India, I had much reason to congratulate myself, that my attention had been so early and so particularly called to this disease, and that I had become acquainted with it under such peculiar advantages. It is not therefore as a speculative inquirer that I now enter on the discussion of this subject; and in what I have now to say as to the nature and mode of treatment of Cholera, I hope it will be found, that I support my statements and views by direct appeals to facts, the result of my own observation and experience, acquired at the bedside of the sick, where alone accurate data are to be found on which rules of practice or theoretical opinions in medicine can safely rest.

SECTION I.

NATURE OF THE DISEASE.

It is of the utmost importance in the outset to counteract, if possible, the false inferences resulting from the inappropriate names which this disease has received.

The epidemic of India has been called Cholera Morbus, Cholera Spasmodica, Cholera Indica, Cholera Epidemica, and, more lately and more appropriately, Cholera Asphyxia *.

Of all these appellations, that of Cholera Morbus is the most unfortunate, though perhaps more likely to mislead the student than the practitioner. To the latter, the first case of the disease points out the inapplicability of the name; whereas the student can never get rid of the idea that he is studying a disease which is seated in the abdominal viscera. It will be seen in the sequel, that it is of the utmost consequence to forget that the Indian disease has ever received a name, so likely to interfere with our attaining a correct acquaintance with its nature.

* Mr Kennedy calls the disease *Contagious Cholera*. To say nothing of the reasoning on which he has arrived at the conclusion that Cholera is contagious, it is enough to observe, that while so many medical men deny contagion, in the sense in which Mr Kennedy uses it, there is neither courtesy nor propriety in giving the disease a name, which necessarily assumes the question of contagion to be determined.

Cholera Spasmodica is not much more appropriate, as spasms not only occur in the common Cholera Morbus, but are far from being invariably an accompaniment of the Indian epidemic; indeed, it often happens that in the most unmanageable cases there are no spasms

Mr Scott, in the Madras Report on Cholera, has proposed the term Cholera Asphyxia. And though it would be an advantage to get rid of the word Cholera altogether, still it has become too commonly received a name for this disease to admit of our disusing it *. Asphyxia, in its original signification, is particularly appropriate and distinctive; the disease never continuing for any length of time without producing a suspension of the pulse. In this treatise, therefore, the name which Mr Scott has proposed for the disease shall be used.

Cholera Asphyxia, as it affects different individuals, and from variety, perhaps as to the state of concentration of its cause, varies so much, as to have induced some practitioners to propose that it should be divided into two species. There can be no doubt that the disease is sometimes much more unmanageable than at others: at such times, every patient seems to be immediately struck with death, and all the efforts of his medical attendants prove unavailing to arrest its fatal progress. This is often found to be the case on

* Whether we derive Cholera from χολη, bile, or from χολας, intestine, it is equally inappropriate to this disease. Bile does not even appear to be secreted during its progress, and the alimentary discharges are merely symptoms; but, as has been said in the text, it does not now appear advisable to alter a term which has been so long considered applicable to the disease.

the first breaking out of the disease; and it is remarkable, that as the disease is disappearing, it seems to become milder. The insidious nature of Cholera, to a certain extent, accounts for this; for persons not aware of the disease, and unprepared for it, are often beyond recovery before they are known to be attacked; whereas, after the alarm has spread, the first symptoms are watched, and the patient is immediately put under medical treatment. But whether mild or uncontrollable, the symptoms and course of the disease are always so well marked, that there appears no reason to encumber our nomenclature by subdividing it into species.

Mr Kennedy, a late writer on Cholera, in order to meet some distinctions which have occurred to Doctors Barry and Russel in the progress of their inquiries in Russia, proposes to add *Chronic Cholera* to the classification. I see no good reason for this. During the prevalence of Cholera, affections of the chylopoetic organs are common, and these, whether the effect of alarm or of miasma, no doubt predispose to Cholera, just as a debauch or any other debilitating cause predisposes. But a looseness surely has no claim to be considered a chronic type of Cholera. The subsequent fever, which occurs more frequently in Europe than in Asia, is likewise regarded by some as a stage of the disease. This fever, however, appears to me to be rather the natural effect of re-action when the blood is in the unhealthy state to which Cholera reduces it; and I should as soon think of calling an attack of fever, which sometimes follows suspended animation from immersion in water, choke-damp or the like, a stage in the accident, as of class-

ing this fever as a stage in the peculiar disease under consideration.

The prominent symptoms of Cholera Asphyxia are very frequently the first intimation to the patient or his friends of the accession of the disease; so that the pulse may be gone, and many of the peculiar serous evacuations have taken place, before the victim is aware of his having more than a slight indisposition. This insidious disease, like the marsh fevers of Rome, commences so frequently during sleep, that some have been inclined to consider this as one of its peculiarities.

There has been naturally much anxiety to discover premonitory symptoms of a disease, which is thus often far advanced to its fatal termination before it is known to exist. But there is no reason to believe that there is any such stage in Cholera Asphyxia; although no doubt, while Cholera prevails in a city or camp, peculiar feelings of indigestion, loss of appetite, and disordered bowels, are very general. So much indeed is this the case, that "*false alarms*" are, at such times, of frequent occurrence. And it would even appear, that there is a tendency to chylopoetic derangements during the prevalence of Cholera, beyond what may naturally be supposed to result from the dread and horror excited by exposure to the hidden causes of so rapidly fatal a disease, having apparently its seat in the abdomen *.

* I am informed, however, that in Sunderland and Newcastle a diarrhœa of one or more days' continuance generally precedes an attack of collapse. Such a premonitory symptom cannot but be regarded as fortunate, and ought never to be neglected wherever the disease is prevalent.

In describing this disease, it may be useful to attend to what may be called four stages :—

1. The invasion of Cholera is so insidious, that the individual attacked may be quite unconscious of the presence of the first stage of it. Very often, indeed, it is only to be detected by those well acquainted with the disease, and by careful observation. Not only in this stage, but throughout the whole course of the disease, the appearance of the countenance is one of the most highly characteristic symptoms. An intimate friend perhaps, observes, that the person attacked has an expression of anxiety, that his complexion is unnaturally earthy, and that his eyes seem sunk in his head. In reply to inquiries, the patient will perhaps deny that he is unwell : he may say, however, that he is a little deaf; and if minutely questioned, he may admit that he has indescribable sensations of being out of order ; that he has oppression at the chest, and is unaccountably depressed and listless : he may have no nausea, but he has tormina of the bowels, and perhaps an uncomfortable sensation of heat at the pit of the stomach : his pulse is quick and weak *(I have seen no case in which it had the febrile throb)* ; the hands and feet feel cold ; the nails are blue, and he has had one or more unnatural alvine discharges. The first of these is generally characteristic ; there is a sudden call, and the whole intestines seem to be at once emptied, followed by a feeling of weakness.

Thus, " while the servant (a Hindoo) of Captain H. was bringing in breakfast, his master was struck with his appearance, and asked him what ailed him ? he replied he had nothing to complain of but deafness, which he ascribed to sleeping in the cold night

wind. His master, alarmed at his looks, sent him to the hospital tent (the epidemic was prevailing in the camp to which this gentleman belonged): on minute examination the man was found to have had some suspicious stools, his pulse had sunk, his skin was cold: He had Cholera, and became rapidly worse; and though he was put immediately under treatment, and was a man of good habits, and strong constitution, he had a hard struggle for his life *."

It is unfortunate that the first stage of Cholera is to be discovered only by the most experienced eye; and that even when the patient has a suspicion of his condition, the risk of exposing himself to ridicule, from its being a "false alarm," may keep him silent, while the acknowledgment of his fears might have been the means of saving his life. It is to this insidious approach of a malady at all times so mortal, that, in part at least, must be ascribed its being so alarmingly destructive on its first breaking out at an Indian station; patients being extremely apt to allow it to go beyond remedial means before they seek assistance.

2. In the second stage Cholera is more easily recognised: The pathognomic character of the countenance, to those who have ever seen the disease, is no longer doubtful: the eyes are sunk in the head, the lips are blue, there is a ghastly look about the mouth, and the whole features are shrunk. The patient does not now deny being unwell; he acknowledges having passed some peculiar stools, and probably has had vomiting; these have been followed by great prostration of strength: there is tinnitus aurium, often slight

* Author's Case-Book, and many similar instances might be mentioned.

deafness and vertigo; the pulse is more decidedly affected, it is weak and thready; the skin is cold; there is much thirst, and a burning pain at the pit of the stomach; but the tongue is not dry—it is moist, white, and cold. When the ratio symptomatum of the disease is under consideration, it will be necessary to take particular notice of the fluid which is passed from the stomach and bowels; at present it is sufficient to say, that it is not alimentary, or excrementitious; it is either a homogeneous and almost limpid, or a turbid fluid; or it is like water in which grain has been boiled, with pieces, more or less numerous, of opaque white or yellowish coagulated matter floating in it. The manner in which the discharges take place is characteristic—the desire is sudden, unaccompanied by griping or nausea, and the stomach or bowels are emptied at once, and with some violence: the gastric and intestinal evacuations are nearly similar in their appearance and nature.

3. In the third stage, the sinking of the countenance is still the most prominently characteristic symptom. The eyes, surrounded by a dark circle, are completely sunk in the sockets; the whole countenance is collapsed; the skin is livid; and the expression is so altered, that the patient's most intimate friend can hardly recognise him. The surface is now generally covered with a cold sweat, the nails are a pearly blue, and the skin of the hands and feet is corrugated, as if they had been long steeped in water; the sensibility of the whole surface is deficient, but it is by no means uncommon for patients to complain of a burning heat in their *cold* skin. Vesicatories do not act; even boiling water does not raise a blister. If the case be attended with spasms, the suffering of the patient is

much aggravated, and is sometimes excruciating. The spasms commence in the hands and feet like cramp; they stretch up the limbs to the trunk, which however they do not always reach. In some cases, again, the muscles of the abdomen are principally affected, and they are drawn towards the spine during the whole course of the disease. These spasms may have commenced early in the disease, being on some occasions the very first symptom. The discharges from the stomach and bowels are at this stage very irregular; in some cases they are still very frequent, in others, after one or two evacuations, they wholly cease. The pulse at the wrist, if it have not ceased, is scarcely perceptible, and the heart beats feebly. When the spasms are severe, they frequently, even though the pulse be still quite distinct, stop it during the paroxysm,—the same effect is produced by excessive vomiting. The breathing is slow and oppressed: this also sometimes occurs early in the disease; the voice is low, and but few words can be spoken with one expiration; the exhaled breath is cold; and though the heat of the body is many degrees below the standard, the patient throws off the bed-clothes, and entreats the bystanders to allow him cool air and cold water. Hiccup is not uncommon, and is rather a favourable than an unfavourable symptom.

4. In the last stage the intestinal evacuations have most likely ceased; the eyes, completely sunk in their orbits, are glazed and flaccid, turned upwards, and half-covered with the eyelids; the spasms are now commonly at an end; the extremities, indeed the whole body, is that of a corpse, and the impression communicated by the skin has been well likened to that of "a damp hide." Every artery has ceased to

pulsate, and the action of the heart, if perceptible, is a mere flutter; the whole body is bathed in a cold and clammy sweat: it is painful to witness the oppression of respiration and jactitation of the sufferer. The dying man can still be roused, but when undisturbed, he generally appears in a state approaching to stupor; and though perhaps in a humour which might almost be termed sulky, is in most cases coherent to the last. These symptoms are the immediate forerunners of death.

If blood be drawn during the progress of the disease, it is found in the outset dark-coloured; as the case advances, the blood becomes thick, there is a deficiency of serum, it coagulates quickly, and does not assume the buffy coat. In the latter stages of the disease the current in the veins has stopped, and the blood is so grumous that it can scarcely be forced out, in the smallest quantity, through a large orifice.

The whole course of these stages, generally speaking, does not exceed sixteen hours, and, unluckily, the practitioner is seldom called in until the first, and part of the second, stage have passed. The period consumed by each of the above artificial divisions varies in every case. I have seen instances in which death ensued in less than four hours after the commencement of the disease; and others, in which its latter stages, with the pulse quite imperceptible, had already lasted a whole day.

The condition of the mind is remarkably collected during the whole progress of this terrible illness; for, though patients, as the malady advances, are unwilling to be disturbed with questions, this appears rather to result from the want of physical energy than from any intellectual failure. Indeed, as the fatal event

approaches, the only wish a patient seems to have is to be allowed cold water, and to be left to die in peace. It has been well said, that a patient, in the last stage of cholera, may be called "a living corpse."

A curious phenomenon has been several times observed:—After a patient has been some time to all appearance dead, and when the attendants were dressing the corpse, spasmodic twitches have taken place in the limbs, having, in some instances, even extended to the muscles of the body, and general spasmodic contractions have supervened *.

Cases often occur in which one or more of the above detailed symptoms do not appear. There may be no spasms; the vomiting and purging may early cease, or there may have been only one large alvine discharge, followed by a mortal collapse, the patient seeming to be at once struck with death; and though, on the very first appearance of the disease, he has *walked* to the Surgeon, his pulse is found to be gone, his heart has ceased to beat, blood can be got only in drops from the veins, he lays down his head, and dies without a complaint.

These anomalies are not confined to individual instances, but are found to occur, as Mr Scott well expresses it, in "local epidemic visitations." Thus, he says, "when the disease appears epidemically in a town or district, or in the lines of a corps or the camp of a marching regiment, it may on one occasion be distin-

* This peculiarity has led to some very improper attempts to excite popular clamour, on the risk of being buried alive. After death, from failure in the circulation of the blood, as is the case in cholera, resuscitation is impossible,—insensibility, from sensorial injury, leaves the system in a different condition, as I shall have occasion to show in a subsequent chapter.

guished throughout, by the absence of vomiting, and the prevalence of purging; and on another, by the excess of vomiting, and, though more rarely, by the absence of purging. Spasm may be generally present in one instance of invasion; in another, it may not be distinguishable*."

I was on one occasion called upon to send assistance to a district in which the disease was thus described—"It commences by attacking the sufferer with an agonizing heat in the stomach, vomiting and purging, and lock-jaw; death ensues in two hours, and often more rapidly: it bids defiance to every remedy." Extract from letter of the Honourable Mr Harris, Principal Collector, Soonda.

But in all cases in which the disease has established itself, there are *the collapsed countenance, blue lips and nails, shrunken fingers, the total failure of the usual secretions, deficient animal heat, suspension of the pulse, and remora in the venous circulation.*

Post-Mortem Appearances.—The appearance of the body after death from Cholera Asphyxia, is always peculiar; the whole surface is livid, the countenance collapsed, and the body shrunken, resembling rather the body of a patient who had suffered from a prolonged illness, than that of a victim to a disease of a few hours' duration. It must be borne in mind, however, that the present description is confined to dissections made on bodies in which death has proceeded *immediately* from an active attack of the disease, and that organic lesions are not included, which,

* Madras Report, p. 21.

though evidently unconnected with the cause of death, have frequently been particularly noted in descriptions of dissections of the bodies of persons who have died from Cholera.

On opening the abdomen, the first peculiarity which presents itself is the gorged appearance of the veins, and semi-fluid state of the venous blood. The peritoneum and omentum have nothing unnatural in their appearance; the characteristic colour of the viscera is a light pinky hue, shading to purple; and the veins, wherever they are to be seen, appear loaded with black blood. The floating viscera generally feel more pulpy than usual, as if their coats were thickened; the solid viscera look purple, or are mottled: the stomach may appear full; in general, however, it is partially contracted: the small intestines, though full, are not always distended. And I have almost invariably found alternate portions of the great intestines contracted and dilated. The whole canal is sometimes distended with flatus. The mesenteric veins are in a state of great congestion.

On a more minute examination, the contents of the alimentary canal are found to be a muddy fluid, resembling that which has been passed in such quantities, from the stomach and bowels, during the disease: there are no fæces, but always more or less flatus. The enteric mucous membrane, particularly at the upper portions of the canal, is often coated with a glary and tenacious matter, which would appear to be an accumulation of the coagulated opaque substance which is passed with the serous fluid. This is not unfrequently in such quantities, at particular parts of the canal, as almost to fill up the tube: its colour is not always opaque white; it has been

described as a dark grey; and I have seen it of a greenish tinge. It is said to be sometimes bloody. These slight variations in the colour of this matter, do not appear to affect the nature of the disease; and from Mr Annesley's experiments they would appear often to be attributable to admixture with the medicines prescribed; nor is this matter invariably present. The internal coat of the stomach, and sometimes of a portion of the duodenum, has very generally a peculiar appearance; there are vascular patches, not of inflammation, but as if vessels had burst, and blood had been extravasated between the coats. These spots, though no doubt commonly a congeries of congested veins, do certainly sometimes arise from extravasation. Patches also are described as having the character of inflammation, generally near the pylorus; which are by some considered the result of local irritation, produced by portions of the remedies prescribed having adhered to these parts of the stomach; and they must be taken as a proof that reaction had commenced before death. Worms are very frequently found in the intestinal canal.

Having removed the alimentary canal, venous congestion is still found to be the most remarkable feature of the dissection. The large veins are gorged, and there is nowhere a trace of arterialized blood perceptible: indeed this has been found to be the case during life; for one surgeon attempted in vain to get blood from the brachial artery *. The liver is in most cases dark-coloured, and mottled, being at its dependent parts usually gorged with black blood;

* Dr Davy on one occasion, in bleeding from the temporal artery, in Cholera, found very little difference between the appearance of arterial and venous blood.

but it is said to have been found, in some dissections, perfectly natural in its appearance. The gall-bladder is generally, but not invariably, full of bile. I have always found marks of congestion in the spleen; but the appearance of this organ seems to be very variable. Dr Christie mentions a case in which he found it empty, and, in the dissections made by the Ceylon practitioners, it seems to have been often found smaller than usual.

The urinary bladder is always contracted and empty; the kidneys are healthy, but partake of the character of venous congestion; the substance of the liver, when cut into, is found gorged with black blood, not coagulated, but thicker than usual; the colour of the bile in the gall-bladder is generally green, but I have seen it healthy in appearance; the great veins of the trunk and liver are full of thick impure blood, as are the right auricle and ventricle of the heart. The lungs are black, and in appearance almost of a fleshy structure, and, when cut into, freely give out the same grumous black blood. In some instances this impure blood is even found in the left side of the heart, and to have reached the aorta; and cases are mentioned in which black blood was found in the carotids.

In the brain there is the same character of venous congestion; but here there is generally some slight appearance of arterial blood:—there is often found a small quantity of serum in the ventricles, or at the base of the brain. The descriptions which have been given, as if portions of the brain had been in a state of high inflammation, are wholly at variance with the character of the disease in its course, and seem rather to belong to a disordered state of the circulation than

to this disease; except indeed in cases in which death had not proceeded immediately from Cholera, and when reaction had taken place.

Such is generally the condition of the body after death, arising from a *pure* case of Cholera. It may be remarked, however, that in particular dissections, some of the organs here described as being in a state of congestion, are found not perceptibly different from their healthy condition; and instances are said to have been met with, in which the whole abdominal viscera presented nothing very peculiar in appearance. In such cases, (none of which have fallen within my experience), the lungs, or encephalon, will no doubt be found highly congested, and the external veins much loaded.

The above description of the disease, and of the appearances on dissection, which are the result of observations made when I was unbiassed by theory or hypothesis, will be found, I am confident, to correspond with the descriptions given by all those who have treated of the disease under similar circumstances. But before attempting to give the *ratio symptomatum* of Cholera, a sketch of the progress of an actual case of the disease will greatly assist this elucidation. Every one practically acquainted with the disease, must have met with such a case as the following:—

A patient is brought into the hospital, in what I have called the third stage of the disease: his countenance is sunk; he has vomiting and purging; his skin is cold; his nails are blue; his pulse is scarcely perceptible; his breathing is oppressed, and he has spasms in the extremities. He is immediately placed

in warm blankets; stimulants, including a dose of calomel, are administered; and a vein is opened in each arm, with the largest orifice. At first the blood flows very sluggishly, perhaps it is only procured by kneading the arms, but by-and-by the stream is more free, and as the blood flows it is improved in its colour; the patient feels the greatest relief, *the pulse rises*, and the colour of the blood testifies that the lungs are restored to their function. Little else is necessary; the patient has a second dose of calomel administered to him, is left in a warm bed, and falls asleep. In the course of a few hours a cathartic is prescribed, to remove the colluvies which the restored secretions are pouring into the intestinal canal. It only remains to guard against local congestion and reaction; but it was generally found in India, that, in a case treated as above, there was no such interference with recovery. The suspended functions seemed to be at once restored: the blood was arterialized; the animal heat returned; the excrementitious secretions took place, and the kidneys resumed their function. And it is particularly worthy of notice, that even persons the most ignorant of the doctrine of diseases, who are at all accustomed to see cases of Cholera, are aware that the passing of urine by the patient, is an unerring test that the disease has been overcome.

This summary of a successfully treated case of Cholera is given, not as an anticipation of the observations on the practice (which, of course, belong to a subsequent division of this Work), but because the rationale of treatment is often an important help in our endeavours to understand the nature of a disease.

And what has been said seems fully to warrant the conclusion, that Cholera is, early in its progress, a disease in which the due proportion between venous and arterial blood is destroyed. No stage of the disease has been hitherto discovered, in which venous blood does not preponderate in the system: let it be observed, too, that this is found to be the case so immediately after what must be considered the accession of the disease, that had the venous congestion been the consequence of high action, that stage must have been discovered. For instance, a man in high health and spirits accompanies his companions to bathe; he is seized, while in the water, with vomiting and purging, or with spasm, is brought immediately to a surgeon, and is found in what I have called the second stage of the disease. This is not by any means an extraordinary example of the sudden and unlooked-for manner in which this remarkable disease makes its attacks. Or a man goes to bed perfectly well, and is roused at three o'clock in the morning by a call to stool; animal heat has already failed, and in three hours he is dying.

In what to all appearance, therefore, is the very outset of the disease, the animal heat sinks, the action of the heart is lowered, arterial secretion has ceased; and if death ensue, every vein is found congested, while the arteries are empty. But what is far more valuable in a practical point of view, this position may be illustrated by the effect of the medical treatment: We have a patient suffering under extreme prostration of strength, and the pulse is scarcely perceptible, perhaps is not to be felt; we remove blood, and as it flows the pulse rises, the powers of life return, and if we can sufficiently relieve

the oppressed system, nothing more is necessary to save the man's life.

It may be said, that this point is unnecessarily pressed, as it is generally admitted that the disease is one of venous congestion; but, though it is acknowledged by most practitioners that ultimately there is venous congestion, still they are so much trammelled by the opinions of the schools, that they find it difficult to admit this state sufficiently early in the disease. Bleeding is found a successful remedy in their own hands or by others; but then bleeding is indicated only when there is increased action, and they are searching in vain, in the earlier stages of Cholera Asphyxia, for a period of high excitement. Nay, we find ingenious pathologists doing their utmost to prove, in their dissections, that inflammation has actually existed; and, in the face of evidence which, to unprejudiced minds, would be demonstration, they persist in regarding the disease as one of excited vascular action throughout its whole course.

Before proceeding farther, it is very important to say a few words on the general question of bloodletting, not only to satisfy such reasoners that the withdrawal of blood may remove prostration of strength, but to clear the way for the Author's explanation of the nature of Cholera Asphyxia.

There are two opposite conditions of the system, in which it may be advisable to withdraw a portion of the circulating blood. The first, when high general arterial action exists, or when local inflammation has taken place: in such circumstances bleeding is called for, because it is necessary to lower the heart's action, or, it may be said, because arterial blood unduly preponderates in the system. Indeed this may be re-

garded as the fact, for, in cases of highly excited action, the blood circulating in the veins will be found to retain the arterial hue.

The second state of the system in which bloodletting is expedient, is when the balance of the circulation is depressed on the other side, and the venous blood is in excess.

Fever, Peritonitis, and Acute Rheumatism, are examples of the first, and Cholera Asphyxia is the most remarkable example of the second which practice presents. The condition of the system in Cholera has been already spoken of; but a case of more common occurrence, which affords an example of bleeding being indicated to remove an excess of venous blood, will supply a farther illustration. When there is a broken rib, the action of the chest being impeded, respiration is checked, the blood thrown into the lungs is not purified, the circulation is obstructed, and venous blood soon preponderates in the system. In such a case, therefore, we bleed not merely to ward off inflammation, but in order to reduce the blood requiring the function of the lungs; so that the quantity thrown into those organs being proportioned to their restricted capacity, they may be enabled to prepare it for arterial circulation.

But, in the latter class of cases, it is not indifferent whether we draw blood from an artery or a vein—a practitioner who would open the temporal artery of a patient whose lungs were clogged with unarterialized blood, would increase the risk of suffocation. Yet we actually find Indian writers so completely mistaken in the rationale of bloodletting in Cholera, that they narrate their attempts to remove blood by the

temporal artery, when the surface is livid, and after the arteries have ceased to beat.

There is another preliminary consideration, which has been lost sight of by many inquirers into the nature of Cholera, viz. That diseases of sudden collapse are not necessarily preceded by a state of high arterial action.—Where, it may be asked, is such a stage in the instantaneous debility produced by the poison of a viper? But, in order to illustrate this fact, we need not leave the doctrine of diseases. Intermittent fever supplies an apposite illustration: On the approach of a fit of ague, the symptoms are those of great depression; there is lassitude, and weariness of the limbs, followed by chilliness; the nails and lips become blue, the countenance sinks, and shivering ensues; and there are, in some cases, spasms of the extremities. In short, the condition of the system approaches to that which prevails in Cholera Asphyxia; and there can hardly be a doubt, that in the cold stage of fever the balance of the circulation is interrupted, and that venous blood preponderates in the system.

In 1825, when in India, and while maturing these opinions on the nature of Cholera, I was called to the capital of a native prince (Tondeman, Bahadur), to treat a very violent fever, which was committing great ravages; and I had then an opportunity of seeing the occurrence of death, in what there was every reason to consider the cold stage of an intermittent. The first instance in which this happened was in the case of the principal minister of the Rajah. This patient had suffered from remittent fever for some days. When first visited, though much weakened, he was

free from feverish symptoms; and on the following morning he continued convalescent: about mid-day, however, he was attacked with what had all the character of the commencement of an intermittent fever; he had a severe fit of ague, but instead of the hot fit ensuing, the symptoms of collapse continued, the coldness of the extremities became death-like, the pulse failed, the countenance sank, and the man died in the evening.

Two similar cases occurred at the same time. There was no epidemic Cholera in the district,—the prevailing disease at the place mentioned was a violent remittent fever, followed, as is very common in India, by an intermittent; and such was the termination of many of the cases at that time under treatment, in all of which the primary fever was similar to that in the cases which proved fatal. Unfortunately, from the *caste* of the patients, it was impossible to obtain post-mortem examinations of those interesting cases; but there can be little doubt that venous congestion would have been found the principal character of the dissection.

Collapse, then, may occur without previous high action; it is produced by many poisons; we see it in Ague, and it occurs in Cholera.

SECTION II.

PROXIMATE CAUSE OF THE DISEASE.

MOST practitioners in India seem to have been impressed with the belief, that the disease which has obtained the name of Cholera is, if not a disease *sui generis*, a malady very peculiar in its character; and many theories have consequently been proposed in explanation of its nature. Not one of these, however, will be found to account for all the phenomena of this very remarkable disease; and some of the most intelligent medical men have not been able to satisfy themselves of the rationale of the line of practice, which, even in their own hands, has proved most successful; for, while pouring in with one hand the most powerful *stimulants*, they were *depleting* their patients to the utmost with the other. Hence, the opinions on the nature and treatment of Cholera have become so conflicting, that the disease is very generally believed not to be understood; and its treatment is considered to be mere guess-work.

Early in the Author's practice in Cholera, he, like others, was impressed with the paramount importance of becoming acquainted with the true nature of the disease; not merely to direct his practice, but also in the hope of being enabled to explain the rationale of what, by general consent, was admitted to be the most successful mode of treatment.

The first case he saw satisfied him, that the term *Cholera Morbus* was calculated to mislead all who had never seen the disease. No doubt, discharges upwards and downwards are common to this and to the disease which had already obtained the name of *Cholera Morbus;* but this fact may be said to be the full extent of the analogy—for even the nature of the evacuations in the two diseases is totally different; and in no other symptom do they bear any resemblance to each other. But this misnomer has not only proved a stumblingblock to those practically unacquainted with the disease, it has been in the way at every turn—treatment is indicated and controlled by it, and the most elaborate hypotheses in explanation of the disease have been thereby shackled.

The vomiting and purging of the Indian Cholera are in truth of very secondary importance in indicating the treatment of the disease; every practitioner, not misled by a name, or blinded by a theory, being aware, that as soon as the balance of the circulation is restored, the symptoms which have thus led away the pathologist at once disappear.

In the course of the Author's inquiry into the nature of Cholera, it became evident to him, that the symptoms, course, and effects of the disease, could not be explained on the received doctrines of Pathology. He found, that even had dissection afforded, as some imagined, evidence of high inflammatory action having existed, no hypothesis on the nature of inflammation would support the idea of this state being present, when every artery had ceased to beat, and when the action of the heart was no longer perceptible. He found, that pure nervous debility, in its usual acceptation, would not explain the disease; for when the

body was like a corpse, asphyxiated and cold, the patient could walk, retaining his sensorial and voluntary powers. Neither was depraved secretion sufficient; for the breath was cold, and the skin blue, so that arterialization must have been deficient; and to all appearance every *secretion* was at an end.

Nor did the respiratory system supply any means of removing the difficulty; for though, no doubt, when the lungs are as it were clogged up, the breathing becomes laborious; yet this, to any extent, was not an unfailing symptom, and seemed always, when present, to be the result of the morbid condition, and not its cause.

Here, then, was a disease in which, although there were great discharges of serous fluid from the alimentary canal, all the natural secretions were at an end, the animal heat had disappeared, the heart and arteries had almost ceased to act, and the blood in the veins was impeded or stopped; yet the sensorial and respiratory powers were little, if at all impaired.

The questions thence suggested are,—

I. Is this a local or a general affection?—and,

II. Is it vascular or nervous?

1. Though, no doubt, a severe and extensive affection of the alimentary canal will produce serious interference with the circulation, still, under such circumstances, we should have no reason to expect so complete a state of Asphyxia as occurs in the disease under consideration. In an inflammatory condition of the gastro-enteric canal, the action of the heart may be affected, and the pulse become thready; but the skin does not become livid, nor the body cold;—and every one who has considered the post-mortem

examinations of victims of Cholera is aware, that it is even questionable whether partial appearances of inflammatory action can be discovered, far less those of extensive inflammation. Besides the immediate and complete recovery from a state nearly of death, which so frequently follows successful treatment in Cholera, is wholly at variance with the doctrine that the disease is an extensive inflammation.

If the symptoms were entirely restricted to depraved secretion into the stomach and intestines, we might be justified in considering the disease a local affection of the gastro-enteric mucous membrane. But, truly, the vomiting and purging are of very secondary consideration. The skin is blue, the surface cold, the arteries empty, the veins loaded, and the current of their blood stopped throughout the whole periphery; and, in such a condition of the system, we cannot be justified in supposing that there is an increased, even though depraved, secretion going on within the alimentary canal. And if we are to consider the disease as the result of a local affection, the evil might with more propriety be ascribed to the pulmonary function. But that it is not the result of a failure in this important function, is proved by there being frequently no marked symptom referable to the chest, and by the disease being generally advanced in its course before the respiration becomes oppressed. This supposition is also irreconcilable with the known immediate effect which the suspended function of the lungs produces on the sensorium. Were the lungs to fail in their function, so far as to render the blood unfit for its purposes, it would follow, that wherever arterial blood was required, the defect would be apparent, and hence the brain would immediately feel the

injury. Accordingly we find this to be the case in circumstances in which the system suffers from the sudden decarbonization of the blood. For example, when a bed-room gradually fills with the fumes of charcoal, insensibility is the first symptom of the danger of the sufferer*.

Cholera, therefore, cannot be referred to a topical affection of any of the great organs—the sensorial, respiratory, or alimentary.

The difficulty of accounting for all the symptoms, on the supposition that the cause of Cholera is a local affection of some of the great viscera, has led to the hypothesis, that it is an affection of every mucous surface, and that it consists of a depraved condition of the secerning function of those surfaces. But this implies, that secretion is excited on the most extensive surface of the body, when that surface is nearly or entirely deprived of arterial blood; for in Cholera the discharges continue after the circulation has to all appearance ceased.

2. Every dissection, after death from Cholera, proves, that this disease produces great venous congestion; and there can be no doubt that such a state of the system invariably exists in Cholera. But this

* While engaged in the preparation of this edition, I have had an opportunity of hearing a paper read by my friend Dr Adam Hunter of Edinburgh, in which he endeavours with much ingenuity to prove, that the immediate cause of Cholera is a failure in the function of the lungs. This he proposes to remedy by oxygenated gases, and for that purpose he has prepared a simple and ingenious apparatus. This invention ought certainly to be tried; for although I believe the failure of the pulmonary function to be one of the effects, and not the cause of the disease, still when re-action commences, such steps as Dr Hunter recommends may prove very beneficial.

must be regarded as the effect, and not the cause, of the disease. Blood drawn from a patient labouring under Cholera, is not only much blacker than usual, but is generally grumous; and, late in the disease, attains the consistence of tar. This must still be regarded as the effect, and not the cause, of the disease; for, were the immediate cause of a disease such a morbid condition of the circulating blood, as to destroy those organic functions which are found to have failed in the very commencement of Cholera, the sensorium would likewise suffer, and insensibility would accompany the other symptoms.

It is not therefore likely, that Cholera Asphyxia results from a morbid state of the circulating blood.

When in the animal body we find a *circle* of actions performed, distant parts united in function, and the flow of fluids to particular organs determined, controlled, or suspended, as may be necessary for certain purposes; whatever may be our notions with regard to *contractility*, we must attribute such combined actions to the power of the nervous system. And whatever may be our opinions as to the nature of those processes from which the various secretions immediately result—whether we consider them chemical, or mechanical, or ascribe them to an unknown vital principle,—still the determination of the pabulum to the secreting surfaces, must be attributed to a general, and not to a local agent. The nervous system stretches itself out to every point of the body, and we find it distributed most largely where we may expect the energy of such an agent to be most required; demonstrating, as it were, that those actions on which the performance of the various organic functions depend, are traceable directly to this system. Hence

we seem to be justified in concluding, that if we find, by disease or accident, that an entire class of the functions of the living body is at once suspended, such suspension must be ascribed to an injury to that portion of the nervous system by which these functions are carried on.

In Cholera, in the very outset of the disease, all secretions, properly so called, are found to have failed; the alimentary ejecta are not gastric juice, pancreatic fluid, bile, mucus, or excrementitious matter. The kidneys cease to secrete urine; saliva no longer flows into the mouth, nor are the eyes moistened with tears; carbonic acid gas is not thrown off in its usual quantities from the lungs, and animal heat is not evolved in the body. Here, then, is a disease which suspends secretion in every part of the body. The functions of the parts which are thus rendered powerless, are unquestionably dependent on nervous energy, the involuntary nerves being the class which have failed; and hence it seems to follow, that the immediate cause of this disease is to be sought in the nervous system.

But here it may be asked, How can a disease be considered a nervous failure, in which we find the sensorium, the respiratory, and the voluntary powers unaffected? The answer to this difficulty may be comprehended in the following propositions, which embody the conclusions on this subject at which the Author has arrived.

1. The great ganglionic or sympathetic system of nerves, is possessed of a power wholly unconnected with cerebral influence, which it may retain after the brain and spinal marrow are removed, and which may cease to exist while these retain the full exercise of their functions.

2. To this system belongs the circulation and distribution of the blood; and it consequently has a most important share in regulating secretion, and in carrying on the involuntary functions. And,

3. To the suspension of this power of the system, as I conceive, is to be ascribed the disease which has obtained the name of Cholera Asphyxia.

These conclusions are the result of an intimate practical acquaintance with this disease, and an anxious study of all its phenomena; and they are supported by a careful and minute inquiry into the nervous system in all its bearings. A disquisition on the subject would be out of place here; but still it is necessary to state some physiological distinctions, not generally received, but which appear to me to be well founded, and which, I think, may be rendered of much value in pathological inquiries.

Physiologists have proposed various classifications of the powers of the system in the perfect animal. We have the animal and organic life of Bichat; the sensorial, nervous, and muscular powers of Phillips; the contractility and sensibility of Bostock, &c.— The classing of muscularity with the brain and nerves seems plainly objectionable; for though Bostock considers contractility as a power independent of the brain, or of nervous agency, it is difficult to detect its independent operation; and such a *separation* seems worse than useless in a living organized body, in which we are in danger of losing sight of the true source of action, in restricting our attention to contractility as a power independent of nervous agency. No doubt, a muscle, removed from the body, can, for a longer or shorter time, be excited to action, according to its nature; but this muscle is

still supplied by innumerable ramifications of nervous filaments, and we must destroy their vitality before it can be considered as proved, that the contractility of the muscle is independent of nervous agency. On the other hand, it is unquestionable, as is exemplified in death by electricity, and by the operation of certain poisons, that whatever destroys the excitability of the nervous system puts an end to muscular contractility*.

It appears to me, that *disease* has not been sufficiently considered in the classifications of the powers of the system; and that, by keeping the effects of various diseases, accidents, or poisons, in view, the agents carrying on life in the more perfect animal system, might be brought within a simple intelligible classification, regulated by the following proposition: —That we are to consider as belonging to a distinct *power* of the system, any set of complicated actions and functions which can exist independently of the other powers of the system, and which, when destroyed, leave the other powers entire. For example, a man gets a blow on the head, and is immediately deprived of his mental faculties, his senses, and all voluntary power of motion; but though he has stertorous breathing, from the loss of control over the

* The contraction which boiling produces, can scarcely be considered similar to living contractility: besides, a muscle has also expansibility; so here is another power. In fact, even in admitting that contractility has an independent existence, we advance little in the explanation of its excitement, control, continuance, regulation, and suspension, which the various motions and actions of the system are constantly calling forth; while the altered power which cerebral or nervous affections produce, proves how great the influence of the brain and nerves must be on muscular action.

muscles connected with the air-passages, his respiration is little affected, and the circulation of the blood goes on regularly. In such circumstances, there is clearly a suspension, or destruction, of *a source* of faculties and actions, which, though necessary to the existence of a complicated system, is independent of other powers which are in operation for the preservation of life.

Keeping this general proposition in view, then, the powers carrying on life in the more perfect animal may be divided into three: viz. the Sensorial; Respiratory*; and the Sympathetic, Ganglionic, or Organic.

I. The limits of this Essay do not admit of such a disquisition as would be necessary fully to illustrate the correctness of this classification. It is sufficient to observe, that the nature and extent of the sensorial power is exhibited in apoplexy—in injuries of the head—in acephalous children—and in experiments on animals after the brain has been removed—or with poisons which suspend the sensorial influence, without affecting the other powers of the system.

II. Respiration is so important to life, that neither disease nor accident can suspend it for more than a very short time, without the other powers being annihilated. But still its independence of the senso-

* I limit this term to the mere mechanical operation by which inhalation and expiration are performed for pulmonary purposes. The functions of the lungs belong, in part at least, to the circulating power.

rial and sympathetic systems can be demonstrated. In apoplexy, and other causes of stupor, respiration continues after the sensorium has failed; and the same thing can be proved, by removing the cerebrum and cerebellum of living animals. The following experiment of Mr Brodie proves, that respiration is also independent of the power which circulates the blood.

Mr Brodie inoculated the thigh of a dog with the Upas Antiar. "In twelve minutes the dog was languid. In fifteen minutes the heart was beating very irregularly, with frequent intermissions, and there was a slight rigor. In twenty minutes, the heart was beating feebly and irregularly; he was sick, and vomited; but the respirations were as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes he suddenly fell on one side, and was apparently dead. The thorax was immediately opened: the heart had ceased contracting, and was distended with blood; there was one distinct full inspiration after the dissection commenced *."

This was not a solitary experiment; and, in a footnote, Mr Brodie mentions that M. de Lile had found that the Upas Tieutte entirely suspends the power of respiration, without immediately destroying the sensorium.

It has been shown that Cholera Asphyxia affords a remarkable illustration of the fact, that respiration may continue free after the circulation to all appearance has ceased.

Having thus shortly pointed out grounds for ascribing independent action to the powers which peculiarly

* Philosophical Transactions, 1811.

belong to the more perfectly organized classes of animals, the purpose of introducing these speculations in this Essay renders it necessary that a fuller consideration should be given to what is considered a third power of the system.

III. The Circulating Power.

The most important function of this power of the system being the circulation and distribution of the blood, the term Circulating Power, although not the most scientific which might be chosen, seems sufficiently expressive of the sense in which it is here employed.

The seat of this power is, I conclude, in the ganglionic system of the great sympathetic nerve. But it would be out of place here to enter into a disquisition on the nature of this system, from its first appearance in organized life, to the condition in which it presents itself in the more perfect animal.

The sympathetic nerve is said to arise from the sixth nerve of the skull. But really this derivation of the great system of abdominal ganglions and nerves is worse than useless, since it leads the inquirer astray from the true nature of this portion of the animal system. The branch of the sympathetic which joins the sixth, is no more to be regarded as the origin of the sympathetic system of nerves, than is the branch which joins the eighth, or fifth, or any one of the branches which join the spinal nerves. And if the Author's idea of the nature of the abdominal nervous system be correct, we must view these merely as nervous communications between the different powers of the system.

My opinions as to the nature of the sympathetic nerve are founded on pathology, and were originally formed in my endeavours to discover the true nature of Cholera Asphyxia. They are further supported, and in truth established, by the experiments of others.

I regard the semi-lunar ganglion and the solar plexus as the principal source of the energy of the sympathetic system. And it appears to me, that the integrity of this system is necessary to the complete circulation of the blood, and to the progress of inert fluids in non-muscular vessels. It has been proved by Sir Charles Bell, that, towards the perfection of voluntary muscular motion, not only is a nerve of motion requisite, but one of sensation. And I conceive, that, towards secretion, not only is a medullary nerve required, but a branch of the sympathetic system, which, without intending it as a perfect analogy, may be illustrated by the necessity of the presence of both the positive and negative wires towards galvanic operations.

According to this notion, the sympathetic system must possess an important share in the function of secretion.

It is difficult to say to what extent the sympathetic system has subservient to it contractility *. It

* I think it is evident, that the current of venous blood can neither be maintained by the power of the left ventricle of the heart, nor by the "*suction*" of the right side of the heart, while the current in the lymphatics, lacteals, and all non-pulsating vessels, cannot be accounted for on the doctrine of contractility.

Dr Sharpey has discovered, that certain animals have even a power of giving motion to fluids which are external to their own bodies, and which cannot be referred to muscular action. Seeing that the tadpole and mussel possess such a control over foreign fluids, we surely shall not arrogate too much to the vessels of the

does not appear to have any power over voluntary muscles. To it may belong the expansile and contractile properties of the arteries, and the purely vermicular motions of the intestines *. But there is no occasion to pursue this farther, as it is not indispensably necessary for my present purpose.

As already said, the classification of the powers of the system which I consider best adapted for pathological inquiries, is founded on the proposition,—that any circle of actions which can exist independent of the other powers of the system, and which can be destroyed without their immediately failing, is *a power*. The fact is best ascertained by disease, because experiments with the knife may destroy more than one class of actions. But there are some experiments which assume very much the character of disease, viz. those with poisons.

One of Mr Brodie's very interesting series of experiments with poisons, has been cited above (*vide* p. 34), and is of importance, as proving that the circulation may be destroyed, leaving entire the other powers of the system. Another of those experiments is no less illustrative of my present view. Mr Brodie found, that certain poisons, while they destroyed the sensorium and respiration, left the circulation unaffected; and that the heart continued to act, and to circulate black blood, some minutes after voluntary motions were at an end; and he concluded, that if he

more perfect animal, if we conclude that they are enabled to propel their contents independently of contractility.

* When Dr W. Phillip divided the par vagum, he found reason to believe, that, though the animal lost the power of emptying its stomach by vomiting, still it possessed the ability of passing slowly on the digested food.

could support the circulation sufficiently long, to overcome the injury done by the poison to the brain and respiratory system, he would resuscitate the animal. He describes the experiment as follows: "Some woorara was inserted into a wound in a young cat; she became affected by it in a few minutes, and lay in a drowsy and half-sensible state, in which she continued at the end of an hour and fifteen minutes, when the application of the poison was repeated. In four minutes after the second application respiration entirely ceased, and the animal appeared to be dead; but the heart was still felt acting about 140 times in the minute. She was placed in a temperature of 85° of Fahrenheit's thermometer, and the lungs were artificially inflated, about forty times in a minute.

"The heart continued acting regularly.

"When the artificial respiration had been kept up for forty minutes, the pupils of the eyes were observed to contract and dilate on the increase or diminution of light, saliva had flowed from the mouth, and a small quantity of tears was collected between the eye and the eye-lids. But the animal continued perfectly motionless and insensible.

"At the end of one hour and forty minutes from the same period, there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. The involuntary motions continued, and the efforts to breathe became more frequent. At the end of another hour the animal, for the first time, gave some signs of sensibility when roused, and made spontaneous efforts to breathe 22 times in a minute. The artificial respiration was discontinued. She lay as if in a state of profound sleep for forty minutes, when she suddenly awoke and

went away. On the following day she appeared slightly indisposed; but she gradually recovered, and is at this time still alive, and in health *."

In this experiment we have a proof that the power which circulates the blood may retain its energy for forty minutes after the perfect death of the sensorial and respiratory powers. But, at the same time, the fact that the various powers of the system are not wholly independent of each other, is proved by other experiments similar to this. Thus Mr Brodie found, "that the circulation of the blood may be maintained in an animal from whom the brain has been removed, for a considerable, but not for an unlimited time †." The experiment cited at page 34. proves, that the power which supports the circulation may be destroyed, yet the sensorium and respiration remain, for a time, unaffected.

The peculiar and remarkable symptom of Cholera Asphyxia is, that after the heart has ceased to beat, and when the blood appears no longer to be circulated, the respiratory and sensorial powers continue wonderfully entire, so that the condition of the patient is not very different from that described as produced by *Upas Antiar*.

The following case, which occurred early in my practice in India, will serve to illustrate this peculiarity of the disease:—

Arcot, 26th July 1819.—Ensign W. æt. 18, had only been a few weeks in India—on his march to join his regiment. This young gentleman had a feverish attack, some stages back, but has been able to accompany the detachment in his palanquin, and had

* Philosophical Transactions, 1812, p. 207.

† Ibidem.

yesterday quite recovered. He, this morning, after breakfast, sent his servant for a dose of salts, without reporting himself sick. I saw him about ten o'clock A. M. He said he had not felt well after the morning's march, and as he required medicine, he had no doubt the salts, which he had just taken, would set all to rights.

At three o'clock P. M., while in attendance on a sepoy who was dying of Cholera, it was reported to me that Ensign W. was attacked with the disease. On reaching his tent, I found him in the third stage of Cholera Asphyxia. The vomiting and purging had nearly ceased. His skin was cold—his pulse not perceptible—much thirst—spasms of the extremities —countenance sunk. He was perfectly collected. He said, that soon after I had seen him he had been purged, and that he had vomited up the salts. He had been then attacked with cramps, but he did not suspect Cholera. I immediately attempted to bleed him, but could not, even by kneading the arm, procure above a few drops of grumous blood. Calomel and opium, laudanum and ether draughts, and hot brandy and water, were administered. Bottles of hot water were applied to the extremities, frictions and shampooing, &c. resorted to. All was vain; clammy and profuse perspiration broke out, jactitation came on, and he was a corpse by eleven o'clock P. M., retaining his mental faculties till the last; having been able to give information relative to his friends in England, immediately before his death.

But as it is important that this point should be established, the Author will here adopt the language of other practitioners in describing the condition of patients labouring under Cholera. Mr Marshall,

when staff-surgeon at Ceylon, (1819), says, " In a few instances the vital energies appeared to be rapidly depressed, and indeed almost extinguished; and every endeavour to arrest the progress of the disease became unavailing. One man, a Caffree sergeant, whom I saw at half-past six P. M. in apparent good health, was suddenly seized at half-past seven P. M. with a slight uneasiness in his bowels, which was followed by one watery evacuation, and once or twice he belched up a little watery fluid from his stomach. Warm water being in constant readiness at the hospital, he was in a warm bath by about ten or fifteen minutes after the griping pain came on. A scruple of calomel, with a stimulating draught, were instantly exhibited; a vein was opened while he was in the warm bath, but very little blood flowed. All the means employed were of no avail; the mortal symptoms became rapidly aggravated, and, by a quarter past nine, he had expired."

Dr Davy, Physician to the Forces, Ceylon, (1819), in the report of a case, five hours after the accession of the disease, observes, " I could perceive no pulse of the heart or arteries; his extremities were cold, the whole surface of the body cool, and bathed in sweat; even the axilla felt cool, the thermometer in it did not rise above 96°; under the tongue, it was stationary at 97°. I asked if he felt any uneasiness about the chest, he replied, not the least; his breathing was quick, about 34 in a minute, not laborious; he said he had no fixed pain anywhere, nor was pain produced by pressure on the abdomen. Cramps occurred frequently in different muscles, particularly of the inferior and superior extremities, and in the abdomen; and once, while I was present, the muscles of the face

were extremely affected, as in Trismus. The spasms were of short duration; they were so painful, that under the agony they occasioned, the patient cried aloud. The tongue was clean; he was rather thirsty, and often called for water. Whilst I was with him (about an hour) he had no vomiting, or retching, or evacuation from the bowels; an enema that was prescribed was not retained. His expression of countenance was wild; his eyes sunk a little; his features not collapsed; his expression was agitated; his intellect clear." This man continued conscious to the last, and quietly expired after ten hours' illness.

Mr Farrel, Deputy-inspector of Hospitals, Ceylon, (1819), in his observations on the reports on Cholera, made by the medical men in that island, says, "When the heart ceases to act; when the countenance collapses, and the eyes sink deep in the orbits; when the body becomes cold, and has a death-like appearance, the more frightful from the faculties of the mind and the power of speech remaining in the living corpse;—when all this takes place in less than one hour from a state of perfect health, what can be said of it, but that it is the work of the hand of death *?"

According to Dr Christie, "Sometimes, when the pulse is scarcely perceptible at the wrist, the patient is still able to walk; and long after the pulse has ceased at the wrist, (although perhaps not able to move about), he is often sufficiently strong to sit up, to use his arms and hands, to speak †," &c.

* I have taken the liberty of making these quotations from the manuscript reports of the Ceylon Medical Officers, the use of which has been kindly afforded me by Sir George Ballingall, in whose possession copies of them are.

† Observations on Cholera, by Dr A. T. Christie.

These quotations (and similar passages occur in every work on Cholera) are sufficient to satisfy any unprejudiced mind, that after Cholera has put almost an entire stop to the circulation of the blood, the sensorial and respiratory powers retain their functions. My own observations of the phenomena of the disease, coincide in every particular with the reports just cited. Indeed, it was from finding this fact so prominent in this disease, that I was first led to draw the above physiological references.

The indispensable necessity of arterial blood for the due performance of the functions of every part of the body, makes it a very remarkable feature of Cholera, that the mental faculties should be retained, apparently in such perfect preservation, after the heart must have ceased to send, in due quantity and quality, arterialized blood to the brain ; and that the sensorial power should frequently continue to exist after the circulation of the blood has, to all appearance, ceased.

The circulation within the head has several peculiarities. The cerebral arteries have not a cellular coat ; they are reduced to extreme minuteness ; and they do not appear to be accompanied by nerves *. The communication between the veins and the sinuses of the dura mater is peculiar, and the sinuses themselves are widely different from veins. The direct course of the blood, however, from the heart to

* Could it be demonstrated that the sympathetic system does not extend its branches to the circulating vessels of the brain, it would be a justifiable conclusion that the cerebral matter is capable of communicating energy to the vessels imbedded in it ; and it might in some degree support the idea which I have ventured to propose, that Cholera Asphyxia is to be attributed to a failure in the sympathetic system.

the head, will have considerable effect in preserving entire the cerebral circulation, after that of the trunk and extremities has ceased.

But it is not necessary to speculate further on this point; it is sufficient for the present purpose to state, that notwithstanding the cessation of circulation in the trunk, the sensorial and respiratory powers are nearly perfect. And the fact that, in this disease, the mental functions remain comparatively unimpaired till the last moment of life, is very valuable in one point of view. The symptoms of Cholera Asphyxia throughout its course, must be referred to the condition of the circulation. At whatever period of the disease the state of this important function is examined, it is found to be morbidly affected. Even before patients themselves are aware that they have been attacked, the arteries are beating feebly, and the venous circulation is perceptibly obstructed; and at however early a period in the course of the disease venesection is performed, the current of the blood is found to be retarded, and the colour of the fluid is unnaturally dark. If, in such circumstances, we should find every function of the system suspended, we might conclude that the disease arose from a morbid change in the blood itself. But the continuance of the sensorial and respiratory powers, after the failure of the circulation in the trunk and extremities, and the suspension of all the organic functions, is a direct proof that the disease is not to be ascribed to "death of the blood."

Every character of Cholera authorizes us in concluding, that it arises from failure of a portion of the *nervous system*. When, in consequence of a blow on the head, a man loses all sensorial and voluntary power, while the respiration and circulation remain

little affected, no one doubts that it is a systematic portion of his frame which has been suspended, and that the loss of perception and of motion has arisen from the source of nervous energy to which these belong having lost its controlling power. In Cholera, the animal heat sinks, secretion fails, and the circulation ceases. Whatever may be the ultimate means by which these important properties of the system are produced, whether chemical, mechanical, or vital, every one will admit, that the combinations which effect them must depend on the nervous system. It has been demonstrated, that, in this disease, the sensorial voluntary, and respiratory involuntary nerves, retain their functions. A man on the point of death from Cholera, breathes, and can give expression to his thoughts. We must, therefore, in endeavouring to ascertain the source of the disease, look to the sympathetic system of nerves, which we find perfect in those tribes of animals which have no brain, and which is proved by experiments to retain its independence in the higher classes of organized animals, where the brain is superadded. If we refer to the symptoms of Cholera we shall find, that the functions over which I have endeavoured to prove this system to have control, are those which the disease suspends; and it is particularly worthy of notice, that wherever the branches of the sympathetic system are largely distributed, there the symptoms of the disease are most prominent. It would even appear that the sinking of the eye may be ascribed to the failure of the energy of the sympathetic system, for Dupuy found that the removal of the superior cervical ganglion of the sympathetic nerve in a horse, was immediately followed by sinking of the eye in its socket.

"Le 24 Juin, on lui extirpa le ganglion guttural gauche. Aussitôt après l'operation, l'œil de ce côté parut plus enfoncé dans l'orbite."—*Journal de Medecine,* t. xxxvii. p. 343.*

It now becomes necessary to endeavour to account for the discharges which so frequently become a principal feature in Cholera Asphyxia; and with that view, it is proper to premise a few words on the general subject of secretion.

Secretion, according to Bostock, is to be referred to chemical and mechanical agencies. He says, "If the substances produced are identical with any of the constituents of the primary fluid, or even very similar to them, it may appear probable that the operation is principally mechanical; whereas, if the secondary substance differs considerably from any of the constituents of the primary fluid, we should naturally suppose that it has been produced by a chemical affinity, or by the combined effect of chemical and mechanical

* Since the publication of the former edition of this Work, a writer in the Foreign Quarterly Review, without noticing my work, adopts and proposes my hypothesis, but in a reversed order. He supposes the nervous system, to a suspension of which I have ascribed the disease, to be affected by paralysis of the functional apparatus of the lungs, occasioned by a human *miasm.* This, he says, injures the blood, and the diseased blood injures "the vital energy of the nerves distributed to the respiratory, circulatory, and secreting organs."—*Foreign Quarterly Review for October* 1831. In a previous page I have stated the reasons which induce me to think that the disease is not to be ascribed to the lungs. The able writer of the article in question has more recently adopted the name which I have selected as most appropriate for this disease.—See *Medical Gazette for November* 1831, p. 223.

action *." And he is anxious to throw entirely out of view *nervous agency*, and to avoid using the term *vital principle*. In my mind, little is gained by referring this function, in the more perfect animal, to unknown chemical affinities, or incomprehensible mechanical actions; and until we have some better authority than mere conjecture for ascribing these processes to such agents, I think it better to retain the use of an unknown (except by its effects) vital principle. And it is unquestionable, that whatever may be the nature of the ultimate actions from which secretion results, the circles of actions, which are called into operation by each secretion, must be ascribed to the nervous system. Neither mechanical action, nor chemical affinities, will account for the determination of blood to organs at the moment when it is required, or for the suspension of functions under particular circumstances. In truth, it would appear that the efforts of the vital energy are often rather to overcome mechanical actions and chemical affinities, than to appropriate them to its own purposes; and so far at least as secretion is concerned, I should be inclined to lay it down as an axiom, that whenever these agents come prominently forward, it is to be ascribed to a failure of the principle of life in the system.

There is another objection to Bostock's account of secretion. He has not, as it appears to me, sufficiently restricted the meaning of the term, but confounded processes, which are not only different when carried on in their proper organs, but which are different when they take place in the same viscus. He makes secretion and exhalation the same operation. But does it

* Bostock, vol. ii. p. 495.

not happen, that, from disease or accident, a secretion may be changed to a mere serous fluid? It is most inconvenient to the pathologist, in such circumstances, to apply the term secretion to both processes. No doubt the word depraved is added, but this only adds to the objection; for "depraved secretion" does not imply that there is a new action, which is the fact, but that the secerning apparatus is still acting, though disordered. The consequence is, that the pathologist is misled; whereas, were it understood that secretion has ceased, and that a new action has taken place, inquiry would be made into the new condition of the system which had led to this change.

This point cannot be better illustrated, than by the two diseases which have received the same name, Cholera Morbus and Cholera Asphyxia. In the common Cholera Morbus, *secretion* is not at an end: the matters discharged from the stomach and bowels are the gastro-enteric secretions in a vitiated state, and bilious and excrementitious colluvies; the pulse rises, the skin becomes hot and dry, the tongue is furred and parched; every symptom of the disease is referrible to the secerning functions of the abdominal viscera, and to irritation, consequent on depraved secretions being thrown into the intestinal canal. How opposite is the condition in Cholera Asphyxia! We have, in this, a disease in which there is a complete suspension of every organic function; and although, as one of its symptoms, it is attended by great intestinal discharges, yet in these there is not a trace of any of the abdominal secretions.

In the former disease there are *depraved secretions*, producing great constitutional derangement. In the latter there is *suspension of secretion*, result-

ing from a general disorder. In the first case, the object of the physician is to evacuate crudities, and *correct* disordered actions. In the second, the curative indications are to relieve the system, and to *restore* the function.

Secretion, according to the proper acceptation of the term, should be restricted to that change on the blood which is productive of a substance wholly different in its nature and character from the primary fluid, and which cannot previously be detected in it: Whereas I should denominate the disengagement of any of the component parts of the blood, in an unchanged state, as exudation or exhalation. When a disease fills the cellular membrane with the serous part of the blood, it is not a secretion: When a sac is filled with pus, it is by secretion. And on the same principle, a discharge of serum into the stomach or bowels is to be regarded, not as a secretion from the mucous membrane, but as an exudation from the capillary vessels. Besides, it will be found, that such exudations are not always to be ascribed to arterial action, but may on some occasions result from a remora in the venous circulation; which, by forcing back the venous blood into the capillaries, the more attenuated parts of the blood are by these thrown off on the secreting surfaces. It is at least evident, that when the capillary vessels continue to throw off large quantities of fluid, after the left side of the heart has ceased to supply them with blood, the process can only be accounted for in this way.

The above observations on secretion generally, appeared necessary to render the following explanation of the Cholera discharges intelligible.

The fluids evacuated by the stomach and bowels

during Cholera Asphyxia, are found to be portions of the component parts of the blood. In health we never see such fluids discharged from the abdominal viscera. They are not gastric or duodenal juice*,—they are not bile,—they are not excrementitious matter,—nor are they the mucous secretion of the canal. The Cholera discharge consists of the serum and fibrin of the blood. But then evacuations go on, and the bowels are filled, after the heart has almost ceased to act, when the arteries are empty, and when the capillary vessels are no longer supplied with blood by the usual course. The great veins, however, the liver, and the right side of the heart, are gorged with blood; the abdominal veins having no valves, regurgitation takes place, the capillaries are filled by a retrograde course of the blood, and are thus enabled to discharge the more attenuated parts of it. And although I believe that, in these circumstances, there is a deficiency of that nervous energy which is necessary towards *secretion*, still, as the medullary nerves retain their functions, the action by which this fluid is excreted will retain the character of life, and will consequently differ from the purely mechanical and chemical changes which take place after death.

The source of nervous energy by which secretion is excited, gives rise to one of the most interesting questions in physiology. When a man has his spine dis-

* I am aware that some recent chemical experiments seem to warrant the conclusion, that the Cholera discharge from the stomach partakes of the nature of gastric juice. But it would require more faith than I have in chemical experiments on the vital fluids, to satisfy me that the secretion of one of the most important fluids in the system, is inordinately excited in the state of the arterial circulation which exists during Cholera.

located, and loses power over the portion of his body below the injured part, if he continue to breathe, the secretions go on. When in apoplexy or concussion of the brain, the sensorial power only is suspended, secretion is not destroyed. In acephalous monsters, a very small portion apparently of the medulla oblongata suffices towards the performance of secretion; and we find that some monstrous productions, in which there was not only no appearance of brain, but a very imperfect formation of the medulla oblongata*, have lived, even for some days, and secreted urine. In all Mr Brodie's experiments, in which the sources of the sensorial and respiratory powers were destroyed, although, by artificially inflating the lungs, he could maintain the circulation of the blood, and produce apparently all the changes on that fluid which usually take place within its own vessels, *secretion* was at an end †.

* Much stress is laid by certain physiologists, who are anxious to lower the importance of the nervous system, on the fact of uterine monsters being formed, in which not only no brain or spinal marrow was found, but little appearance of a nervous system. Without stopping to inquire, whether due examination was made in these cases into the existence of the sympathetic system, I may observe, that, until the powers of the placenta be ascertained, little weight can be given to the evidence afforded by imperfect fœtal productions. The reasoning in the text has reference to animals which can live by breathing.

† As venous blood, after removal from the body, by exposure to atmospheric air, acquires the arterial hue, the changes on the blood, exhibited in Mr Brodie's experiments, may have been purely mechanical.

I may observe, that while Mr Brodie found that the loss of the sensorial and respiratory powers put an end to all the usual secretions, *exudation* went on, and the process of inflating the lungs was generally put a stop to, by the air-cells becoming obstructed by the exuded fluid.

The conclusion to which these facts lead is, that the medullary source of secretion lies between the pons Varolii and the beginning of the dorsal vertebræ.

In Cholera, where the failure appears to be in the sympathetic system, secretion is suspended; as also happens in experiments with poisons, by which the power which circulates the blood is destroyed *.

It is therefore evident, that the integrity of more than one of the powers of the system is necessary to the function of secretion; and we are justified in concluding, that this process not only requires the preservation of the upper portion of the spinal column, but the vital energy of the sympathetic system.

It is important, before concluding this branch of the subject, to attend to the effect produced by Cholera on the cuticular discharges. We have seen, that perspiration almost invariably makes its appearance in the latter stages of the disease. I have never seen a fatal case in which, sooner or later, this symptom has not been present. According to Dr Christie, "profuse perspiration is a very common symptom of Cholera. It is a curious fact, that there is sometimes profuse perspiration when the pulse at the wrist is extremely small, or altogether imperceptible. This has generally been considered as indicative of extreme debility of the cuticular perspiratory vessels. On the other hand, I am inclined to think, that these vessels have their action very much increased; for were the

* The destruction of the fifth nerve produces ulceration and sloughing of the eye, membrane of the nose, and in the gums of the side of the face supplied by the destroyed nerve. And Mr Dupuy found that the removal of the upper cervical ganglion of the sympathetic in the neck produces nearly similar results.—*Leroux's Journal de Medecine*, t. xxxvii.

reverse the case, how does it happen that, when only a small quantity of blood flows sluggishly, or in drops, from a large orifice made in a vein, or even when no blood can be procured, the cuticular secretion forces its way through the minute pores of the skin? It is easily conceivable how debility of the perspiratory vessels can occasion increased perspiration, when the superficial arteries which are immediately behind them act with vigour; but when these arteries do not contain a sufficient quantity of blood to enable them to continue their own action, the only way in which perspiration can be thrown out, is evidently by an increased action of the perspiratory vessels *."

This explanation seems to me to refute itself. The pulse is altogether imperceptible, the heart is beating languidly, " and the arteries, immediately behind the secreting vessels, have not a sufficient quantity of blood to enable them to continue their action." When, in fact, all appearance of arterial blood, colour of the skin, animal heat, &c. are absent, this author supposes that the exhalant arteries have the power of throwing off profuse perspiration. Now to this I cannot assent. On what, in the first place, it may be asked, are the exhalant arteries to act? Even admitting that the capillary arteries have an action independent of dilatation, their action surely must be useless when the arterial blood is no longer sent to them†. It has been found, that the fluid which ex-

* Observations, pp. 80, 81.

† The action in arteries would, I think, be much better expressed by calling it simply *contractility*; the term *muscularity* having led to much bad theory, and consequently, injurious practice. A muscular power is not only uncalled for, but, with all its attendant liability to spasm, irregular action, &c. would prove

udes from the inner surface of the intestinal canal, must be supplied by the veins; and if it be admitted, that the capillary vessels in the periphery of the *arterial* system are filled at all, this also must be ascribed to regurgitation. And let it be kept in mind, that, late in the disease, when the alarming symptom of profuse cold perspiration makes its appearance, the external veins are invariably gorged, so that their valves will be no hinderance to a retrograde course of any *elected* fluid portion of the blood.

I may again notice the fact, that though the skin is thus throwing off profuse perspiration, vesication cannot be produced. This has been a subject of much wonder. But vesication is the result of inflammation, and inflammation belongs to the *arterial* system; so that, under the view here given of Cholera, vesication is not to be looked for while the disease lasts.

Before proceeding to a review of my inferences, I refer to the following case, extracted from the MS. Ceylon Reports, as illustrative, not only of the difficulties which are encountered in the treatment of this disease, but also as amounting, almost to a demonstration, of the correctness of my views; while, at the same time, it shews how they may be rendered practically available.

highly injurious, by interfering with the circulation in its course; and a spasmodic local affection of an artery might irretrievably suspend a function on which the life, not merely of the body as a whole, but of each individual part, depends. It appears to me as absurd to attribute action to arteries independent of a supply of blood, as it would be to expect a mill-wheel to continue its gyrations after we had turned off the supply of water.

Extract.—" Private Thomas Layton, 19th Regiment. Æt. 22; 3 years in Ceylon.

" Had not previously suffered from disease, when admitted into hospital labouring under epidemic Cholera. Says he has been affected with slight looseness for the last two days: at present, 10 o'clock A.M. 14th August (1819), complains of headach, vertigo and faintness, with violent vomiting and purging of watery fluid: the features are shrunk, and expressive of great anxiety; pulse small and intermitting, profuse perspiration, skin moderately hot, tongue clean; has had some spasmodic twitches in the fingers and toes. Although the state of collapse was fast coming on, I still considered that the circulation might be relieved by venesection; lb. iss. of blood was abstracted from two orifices before it ceased to flow. He was instantly placed on a spirituous vapour bath, took hydrargyri submuriatis gr. xxx. in powder, and several stimulant antispasmodic draughts, the second of which was combined with tinct. opii ℥ i.

" At 12 o'clock the vomiting and purging still continued; took another dose of hydr. submur. gr. xxx. had his abdomen fomented, and rubbed with ol. terebinthinæ. His head was also shaved, which, together with his legs and arms, was rubbed with a liniment composed of antimonii tartaris and ol. commune. At this time, the state of collapse was completely formed; he tossed his body and limbs to and fro on the couch: the vomiting and purging was now relieved, but cold clammy sweats continued, with spasms in the extremities, for the space of ten hours, during which time blisters were applied to the

head and thorax; he was kept warm by the vapour bath, had repeated stimulant glysters, and his limbs rubbed with various strong embrocations.

" Ten o'clock P. M.—Was still in a cold perspiration; *continued perfectly collected;* pulse began to flutter, and both blisters had vesicated. From this period the pulse gradually rose, and by eight o'clock next morning the skin was moderately hot, pulse slow and steady, had not had a natural evacuation. *15th August,* R. pulv. rhei ℈ ij. aquæ cinnam. ℥ ii. Vespere,—Had not had a stool, but had *made water several times,* and slept two hours; is extremely exhausted and torpid; was allowed some wine, and such food as he fancied. R. enema purgan.

" *August* 16.—Is still in a state of torpor and listlessness; pulse steady; no appetite. The enema brought away some fœtid stools. R. hydrarg. submur. gr. x. ter in die.—Repet. enema cathart.

" *August* 17.—The stupor and insanity, with obstinate costiveness, continue; he also complained of pain and fulness of the left side, with oppression of the breathing. Thirty leeches were applied to the thorax, and four full dozes of cathartic pills were administered, which produced some bilious stools; pulse and heat natural.

" *August* 18.—The stupor and *catching in breathing* having increased, he was put into a warm bath; the former blister being healed, another was applied to the thorax, and the catharsis kept up by a cordial rhubarb mixture."

The case goes on till the 22d August, symptoms and treatment continuing much the same, on which day this is the report:—" Breathing more oppressed; *pulse still regular;* has not had a stool from

the medicine. R. enema purgan. c. ol. terebinthinæ ℥ ij. Vespere,—Low muttering delirium came on during this day. He gradually sunk, and died about three o'clock on the 23d instant.

"*Sectio Cadaveris Five Hours after Death.*

"On taking off the skull-cap nothing unnatural appeared on the surface of the dura mater, excepting a number of veins running over it being much distended with blood. A great proportion of the veins likewise on the surface of the brain were highly turgescent, and some of the smaller branches contained portions of air intermixed with blood. The cortical and medullary parts of the brain were natural in appearance and consistence, except the latter exhibiting, when cut across, numerous bloody points. The right lateral ventricles contained about two drachms of serous fluid, the left about one and a half drachms. The pineal gland was very firm in consistence, but natural in colour; there was nothing remarkable about the other parts of the encephalon. There was about half an ounce of serum slightly tinged in the base of the brain. The medulla spinalis was so firm in texture as to approach to the consistence of ligament: the vertebral canal could not be examined in a manner sufficiently satisfactory, for want of proper instruments; and the only circumstance which I was capable of noting, was part of the serous fluid oozing out, which no doubt ran along the sheath lining the canal from the base of the brain*." The appearances of the chest and abdomen are given. In the former there was evidence of the existence of inflammatory

* Ceylon Manuscript Reports.

action; in the latter, the viscera had the usual character of disordered circulation consequent on a partial recovery from Cholera.

Now, the progress of this case is not only valuable, as illustrative of the nature of Cholera, but it assumes something of the shape of a prolonged experiment, confirmatory of the speculations which, with a view to pathology, I have offered on what appears to me the most useful subdivisions of the powers of the system. This case farther proves the practical value of the opinions which I have ventured to propound regarding the treatment of the disease. So long as the symptoms of the disease had their source in a suspension of the energy of the sympathetic system, the intellect was clear, and, as nothing is said of the respiration, we may conclude that the breathing was not more affected than it sometimes is in Cholera. When that disease was overcome, there was a period of exhaustion, followed next day by symptoms of cerebral congestion. Reaction then apparently took place within the skull, still complicated, however, with congestion; but, although the pulse was natural, the breathing was now becoming affected. Next day the symptoms are more decidedly referrible to the origin of the respiratory nerves, and there is "catching in breathing." It may be added, that a correct notion of what was going on in such a case would have directed attention, in the first instance, to the sympathetic system, in the second to the brain, and, in the third, to the upper portion of the medulla spinalis.

The conclusions and inferences at which I have arrived, therefore, are,—

1. That there are three powers in the system, viz.

the sensorial, respiratory, and sympathetic; and that each of these, on all common occasions, is enabled to perform its functions without the active interference of the others.

2. That the destruction of any one of these powers does not immediately annihilate the other two. (1.) The sensorium may be suspended, and the circulation and respiration continue unimpaired. (2.) The circulation may be supported after the source of respiratory power and the brain have been removed. (3.) The circulation of the body may be stopped, and the sensorium and respiration remain unaffected.

3. That the source of the sensorial and voluntary power is in the cerebrum and cerebellum; that of the respiratory power, in the upper portion of the medulla spinalis; and that of the circulating power, in the ganglionic system of the great sympathetic nerves.

4. That secretion requires the integrity of the upper portion of the spinal column and of the sympathetic system; and that when the power of either of these sources of nervous energy is suspended, any appearance of secretion is fallacious.

5. That secreting surfaces may throw off component parts of the blood, after their power of secretion is at an end.

6. That Cholera Asphyxia is a suspension of the power by which the circulation of the blood is carried on.

7. That the fluid which, during a fit of Cholera, is poured into the alimentary canal, and which exudes from the skin, mouth, eyes, and mucous lining of the bronchiæ, is excreted after *secretion* has ceased, when animal heat is no longer evolved, and when arterial action is at an end; and therefore must be regarded

as an exudation from the venous capillaries, or if from the arterial capillaries, is occasioned by their being supplied with blood by regurgitation.

I have thus brought to a conclusion my hypothesis as to the immediate cause of Cholera; and have to return my professional brethren thanks for the indulgence with which they have received these physiological views in explanation of the Pathology of that disease. I could hardly have expected that a theory, promulgated for the first time, and differing so widely from any which has been heretofore suggested, should have been at once adopted; but as many of those experienced in Cholera have expressed their assent, I have reason to hope, that, as European practitioners become acquainted with the disease, the correctness of my conclusions will be more generally admitted.

SECTION III.

REMOTE CAUSE OF THE DISEASE.

THE remote cause of any particular disease is frequently the most obscure question connected with its nature; and I much fear that practically we should gain little by becoming acquainted with the remote cause of Cholera. The disease, to whatever origin it is to be traced, seems to be now gradually diffusing itself over the globe,—uninfluenced apparently by climate,—and hitherto setting at defiance all human means of retarding its progress, or of lessening to any considerable extent its average mortality. Were it of any importance to hazard conjectures as to its remote cause, I should be disposed to say that the theory most consonant with the phenomena of the disease, is that which ascribes it to certain galvanic changes on the earth's surface; but really such speculations are of little value, nor is there any theory of the disease which has led to important practical results, except that which traces its origin and spread to contagion alone.

Some writers go so far as to say, that even the question of contagion resolves into a mere splitting of straws; but no one who looks around him can view this question in that light, and therefore I propose in the next Section, to discuss the subject very fully; merely observing here, that the difficulty of account-

ing for the rise and extraordinary progress of this disease has very naturally led speculative writers to seek its remote cause in contagion. In my apprehension these writers have failed in establishing their position; but whether they have done so or not, I am sure that all my professional brethren will readily concede to me that, independently of contagion altogether, the extension of a disease, having its source in terrestrial or atmospheric causes, is no new phenomenon. Influenzas, it is well known, have on various occasions become universal; so also a town or district has been frequently visited by a fever without apparent cause; while other places have as frequently and as unaccountably become exempt from diseases, to which they had been formerly subject.

I have seen intermittent fever become epidemic in an extensive district of India (the Southern Mahratta provinces), and prevail for a time over the whole face of that country. We are ignorant of the cause of ague. It is said to be marsh miasm; but it prevails constantly in some of the highest and driest stations in India; whereas other districts, which, during the hot season, are flooded over their whole surface, are peculiarly free from fever. The most interesting example of this is afforded by the kingdom of Tanjore, which is annually flooded *during the dry season* by rivers, having their flood-heads in the western Ghauts, which are filled during the rains on the opposite side of the peninsula; and yet, in that country, which, during six months of the year, is kept in a constant state of irrigation and evaporation, intermittent fever is comparatively exceedingly rare. But if fever and ague can become epidemic, as I have seen it in the Southern Mahratta provinces, where the

country is varied and far from marshy, there is no assignable or obvious reason why that disease may not, as Cholera is now doing, extend itself over the world. If, therefore, the hidden cause of a disease, which is admitted on all hands not to be contagious, can pervade many thousand square miles, as intermittent fever did, during the year 1819–20, in the district of India to which I have referred, we may wonder at, but have no right to deny the possibility of, such a disease as Cholera extending itself by means equally inscrutable.

If we could point out the source of nervous energy, we might (on the explanatory hypothesis which I have attempted) hope to become acquainted with the remote cause of Cholera. But, in the present state of our information, it is much easier to prove what does *not* give rise to the disease, than to form any plausible conjecture as to its true source. There can be no serious doubt, however, that a disease similar to Cholera has been known in Asia from time immemorial. Bontius, upwards of two centuries ago, in treating of "Cholera Morbus," describes very correctly the prevailing disorder. "This disease," he says, "is attended with a weak pulse, difficult respiration, and coldness of the extreme parts; to which are joined great internal heat, insatiable thirst, perpetual retching, and restless and incessant tossing of the body. If, together with these symptoms, a cold and fœtid sweat should break forth, it is certain that death is at hand." There is also frequent mention of what was undoubtedly this disease, by the medical officers of the East India Company's Service, in the latter portion of the last century; and, in 1775, it seems to have assumed the character of an epidemic in India,

and to have extended itself to the Island of Mauritius. Hence we may almost conclude, that the tremendous pestilences which are so frequently mentioned by native historians, as having desolated Indian armies, were frequently no other than the modern epidemic or malignant Cholera.

Nor does the disease appear to have been confined to India. Dr J. Johnson, in his Diseases of Tropical Climates, publishes a letter from Mr J. B. Shepherd, who met with a disease in the Brazils, which he calls *Mort de chien* (fancifully corrupted from the Hindoo name of the disease, *Mor dexim*), which, from the success of bloodletting in the treatment of it, I have little doubt must have been Cholera Asphyxia; and I think it by no means improbable that the English "sweating sickness" partook of the character of this epidemic. I have also very high authority for believing that a disease very like the Indian Cholera prevailed at the Havanah in the year 1810 *: and still more recently, in 1829, several cases resembling Cholera Asphyxia, in its most malignant form, made their appearance in a public school at Clapham.

On this subject I may refer to Mr Secretary Scott's able digest of what the Medical Board at Madras knew concerning Cholera prior to 1817 †; from all which it is evident that this disease was not unknown

* The authority to which I refer here is that of Admiral Fleming, who informs me, that in 1810, on his arrival at the Havannah, some of the crew of his ship suffered from a disease strongly resembling the disease now travelling through Europe. The disease was at that time prevalent in the Island of Cuba; and was called *Pasma*. The remedies were, warm-bath, antispasmodics, and bleeding.

† Report on Epidemic Cholera, as it appeared at the Presidency of Fort St George.

before its appearance as an epidemic in 1817 ; although its appalling mortality at that time, and rapidly extended ravages, forced it on public attention with a character of novelty which led to the general belief that it was a new and unheard of disease ; and this opinion has been fostered and circulated by European writers who have not sufficiently attended to the evidence on the other side.

Whether, in the more recent and wider spread of the disease over countries differing from each other so completely in climate, temperature, habits, and all the other predisposing or counteracting causes of an ordinary epidemic, it may not be guided by some element with the nature of which we are as yet unacquainted, is a speculation into which I have already said I will not enter. All that I would venture to suggest is, that possibly galvanism may present phenomena which might aid us in our inquiries into the remote cause of the disease. Ritter found that while positive electricity seemed to augment the vital powers, negative electricity diminished them; the former increased the pulse, the latter lowered it ; the former caused the sensation of heat, the latter of cold. We again have seen that the distinctive character of Cholera Asphyxia is the sudden deprivation of the nervous energy of the sympathetic system ; and all the length to which I go is to suppose it not impossible that the source of the disease may be found in some sudden change of the electric or galvanic condition of the surface of the earth. I say this, however, with no intention of supporting the hypothesis that galvanism is identical with nervous energy ; but unquestionably that agent is so intimately connected with every portion of the creation, that any sudden withdrawal of it

from the animal system might not improbably produce such a disease as Cholera;—a conjecture which receives some countenance from the very extraordinary effects which thunder-storms are reported to have had on many occasions on the progress of this disease in India*.

* A remarkable instance of the sudden cessation of Cholera after a thunder-storm is said to have occurred at Jassy. One of the most accurate observers of the peculiarities of the disease, also says, "several instances have been known of its (the Cholera) having suddenly ceased on the occurrence of a thunder-storm." And, in a foot-note, he adds, "I witnessed a remarkable instance of this at Kulladghee in 1824. After the disease had prevailed for several weeks in the troop of horse artillery, there was a violent thunder-storm; after which not a single case occurred."—*Dr T. Christie's Observations on Cholera*, pp. 97, 98.

It is a circumstance calculated so far to give support to the theory which ascribes Cholera to some terrestrial influence, that, in India, the lower animals, such as cattle, monkeys, and poultry, suffered from the disease; and, if newspaper reports are to be credited, a similar phenomenon has been observed in some of the European continental towns; and in Hamburgh in particular, it is said that the *fish* also in the rivers were affected. The latter circumstance is very remarkable, and, if well-founded, may lead to some interesting speculations.

Some speculators on Cholera have also observed that, in India, *mounted troops*, as being farther removed from the earth's surface, suffered less from the disease than infantry; while a distinguished physician in this country has ascribed this immunity to the circumstance of the cavalry being generally *picked men*. *See a review of several works on Cholera, in the Edinburgh Medical and Surgical Journal, for December* 1831. My own opinion is, that there are not facts sufficient to support either hypothesis; for, 1*st*, The mounted troops in India, in proportion to their numbers, suffered as severely, or nearly so, as the infantry, as will be seen from the reports of the different presidencies, and also from Dr Turnbull Christie's work on Cholera; many of his cases being taken from the horse artillery corps which was under his medical charge. 2*dly*, The native cavalry in the Company's

After all, however, the result at which we arrive is, that we know absolutely nothing of the remote cause of Cholera. This is the conclusion to which all practical writers have come; and although the phenomena to which I have just alluded, and others in the history of the disease, may no doubt lead to much curious and interesting speculation, it is of infinitely greater consequence, in the present state of our knowledge, to confine ourselves to the practical questions which the actual presence of the disease suggests; one of the most important of which is that which forms the subject of the following section.

service are not, properly speaking, picked men; and, besides, whatever the troopers may be, it is well known that, in India, a cavalry regiment is always attended by a greater number of dismounted camp followers and native servants than an infantry regiment; all of whom are included in the returns made by the medical man in charge of the corps. 3*dly*, Even were it true that mounted troops were not so liable to the disease as pedestrians, I should be inclined to attribute their exemption to the circumstance of their being less exposed than infantry are, to fatigue and the other discomfits of an Indian march.

In connexion with the remote cause of Cholera, I may mention that my friend Dr Meikle of Edinburgh, who saw much of the disease in India, and who himself suffered no fewer than five attacks of it, is subject to a very peculiar sensation when exposed to the hidden source of the disease, which enabled him repeatedly in India to announce its approach *before any actual case had occurred*. The sensation, as he describes it, is that of numbness in the fingers; and it is singular that, the other day, in Haddington, he had again the same feeling.

SECTION IV.

ON THE COMMUNICABILITY OF CHOLERA ASPHYXIA, BY MEANS OF CONTAGION OR INFECTION.

This disease is generally supposed to have commenced in 1817, at Jessore, a town within the Delta of the Ganges: but this is not the fact; and it is important that it should be known, because much of the European speculation, on the subject of contagion, rests on that assumption. The Report of the Medical Board of Bengal, on the Epidemic Cholera, puts this matter beyond dispute, and proves that the disease, instead of spreading from Jessore as a centre, broke out in Bengal Proper, in several places, many miles distant from each other, nearly about the same time; and, consequently, that whether the disease be contagious or not, the conclusions which have been so confidently deduced from the localities of Jessore, and the habits of the natives of that place, must be laid out of view in determining that controversy.

The question, as already remarked, is one of very high importance, whether we confine our view to the disease itself, and the means of arresting its progress, or look to the subject as involving the commercial relations of the country to an extent almost incalculable. It is, therefore, much to be regretted, that most of the writers who have taken part in the argument have yielded much too readily to their prejudices or pre-

conceived opinions. They have not approached the inquiry with that caution which becomes persons, many of whom are very imperfectly informed as to the facts on which their reasoning is founded; and almost all of whom are speculating on a disease, with the habits and peculiarities of which they have had little or no practical acquaintance. At the same time they reject at once, and without scruple, the testimony and the conclusions of a large and intelligent class of professional men, who arrogate no superior knowledge as to the hidden sources of this extraordinary disease; but who, upon the particular question of contagion, have had opportunities of forming a deliberate opinion, which has been somewhat rashly set at nought by a crowd of authors, both professional and unprofessional, who have committed themselves eagerly and conclusively on that question. It is not improbable that some of those gentlemen may, ere long, see reason to modify their opinions; and, at any rate, the weight of their authority would not have been lessened, had they expressed themselves with less confidence on a point which, to say the least of it, is not yet determined.

I am far from supposing myself exempt from bias or prejudice. But although my acquaintance with the disease in India has led me to the conclusion, that it is not contagious in the sense in which that term is used by European writers; and although I will not presume to say, that in Europe changes may not have taken place in the nature of the disease, sufficient to render it, to a certain extent, communicable by one person to another; I must on the other hand declare, that while I have seen no evidence that the disease was contagious in India, nothing has hitherto

occurred in its progress through Europe, which may not, in my humble apprehension, be explained, without admitting it to be communicable in that manner.

Under this impression I have spared no pains in my attempts to trace and to illustrate the progress of the disease on the Maps, which form part of this Work. Every thing likely to contribute to the dissemination of correct information is of vast importance; and the following details, founded chiefly on the public records of the disease, or on my own personal experience, will not, I hope, be deemed uninteresting or trivial, at a time when so much injury has been done by the publication of theories and speculations, in which these historical facts are either entirely overlooked, or much misrepresented. Many of the writers on this subject, indeed, seem to me more eager to advocate and support particular opinions, than to engage in a dispassionate and impartial inquiry concerning a matter in which the community has so deep an interest*.

* One of the most influential examples of hasty and partial generalization which I have met with on this subject, is to be found in an article in No. XCI. of the Quarterly Review. Nothing can be more specious than the argument in that article; but, on reference to the details which I have given in the text, I think it will appear that the reviewer, like many other European writers who have taken part in the same controversy, has been misled by erroneous information, particularly as regards the progress and spread of the disease in Asia. The facts, as he has arranged them, undoubtedly harmonize exceedingly well with the theory of contagion; and a similar classification of facts may be made, with at least equal plausibility, in support of the opposite theory. Instead, however, of following that course, and, like other controversialists, passing over the facts of an opposite tendency, I have preferred a simple historical detail of the progress of the disease, leaving it to the contagionists to account for certain well authenticated anomalies, which appear to me irreconcileable with their theory.

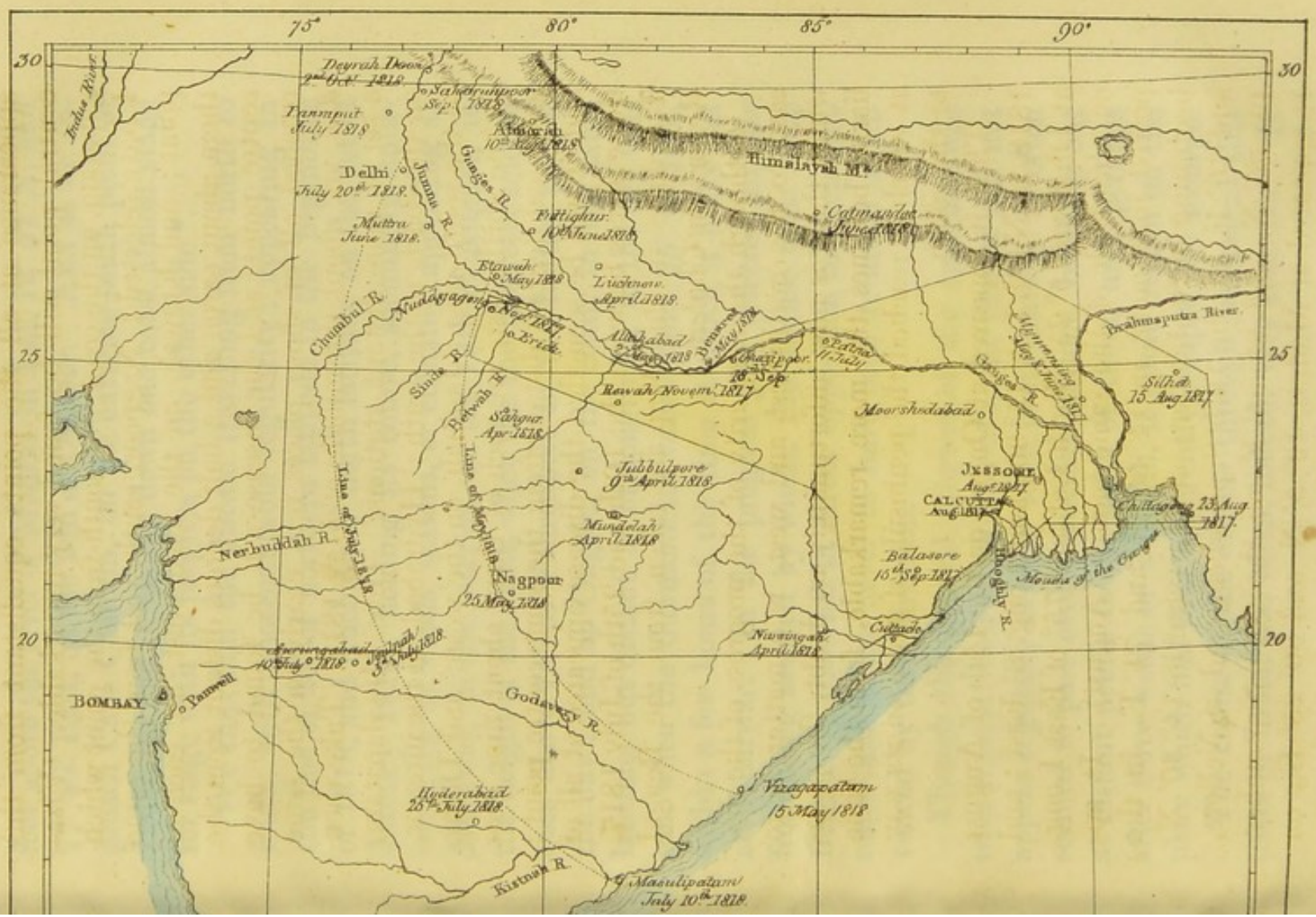
According to the most authentic information which we have on this subject, Epidemic Cholera first appeared on the banks of the river Barumpooter, at Mymensing, in Lat. 24° 40′, and Long. 90° 15′ E., in the end of May 1817, and during June and July of that year it prevailed over the whole of that district. On the 11th of July 1817, it broke out at Patna, in Lat. 25° 37″, and Long. 85° 15″, distant upwards of 300 miles from Mymensing. In the course of August 1817 it appeared at Calcutta, and on the 17th of that month we find it at Silbet, 325 miles north-east of Calcutta. On the 19th of August 1817 it broke out at *Jessore*, and by the middle of September it had extended over six degrees of latitude and eight degrees of longitude. In six weeks more it was found to have stretched westward six additional degrees of longitude; for by the beginning of November 1817 it had reached the river Sinde, in 25° Lat. and 78° Long. The yellow portion of the accompanying Map shows very nearly the extent to which the disease had spread during these seven months.

During the next four months the disease did not extend itself; but on the 27th March 1818, it began to move to the north, west, and south, by regular stages at the rate of about two miles a-day,—stepping on from station to station, and apparently arriving at no place prematurely, or out of the regular progress. The dates on the accompanying Map exhibit this striking feature of the disease. Thus, take the centre of the space over which the disease so rapidly extended itself in the autumn of 1817, and let a circle be described from that centre, embracing any of the stations at which the disease broke out; and its simultaneous

appearance at all the points equidistant from this central point will be found very remarkable. Vizagapatam, on the coast, due south from the point which I have selected; Nagpoor, south-west from the same point; and Etawah, on the north-west, are each of them about seven and a half degrees distant from the centre point, and the disease will be found to have reached all of those places very nearly on the same day, viz. about the middle of May 1818. Take then a set of more distant stations. Thus Musalapatam, on the coast, to the south; Hydrabad, in the interior, to the south-west; Jaulnah, on the west; and Delhi, on the north-west, are each of them about nine geographical degrees distant from the same centre point; and it will be seen that the disease appeared at all of those places between the 10th and 25th of July 1818. The dates in this Map are taken from the Bengal Report on Cholera.

Although the regularity of the progress of the disease to the north-west and west, is a matter as to which there is now unfortunately no doubt, the peculiarity becomes the more remarkable, from the time at which it reached the peninsula of India on its way to the south.

Thus, it appeared at Ongole on the 14th of August 1818, in North Lat. 15° 30′, and Long. 8° East; and at Dharwar 15° 25′ North Lat. and 75° of East Long. on the 13th of the same month (August). It appeared in Soonda, in 14° 50′ N. and 74° E., in the beginning of September; at Hurryhur in 14° 30′ N. and 76° E., on the 12th September; and at Nellore, 14° 20′ N. and 80° 25′ E. on the 20th September. It appeared at Madras, 13° 5′ N. and 80° 25′ E., on the 8th October; and at Bangalore, which is 3000


75°
80°
85°
90°
30
25
20
Indus River
Deyrah Doon
2nd Oct. 1818
Saharunpoor
Sep. 1818
Tanmpit
July 1818
Ahmed
10th Aug. 1818
Himalayah Mts
Delhi
July 20th 1818
Jumna R.
Ganges R.
Futtighur
10th June 1818
Catmandoo
Muttra
June 1818
Etawah
May 1818
Lucknow
April 1818
Chumbul R.
Nudeegaom
Sept. 1817
Brinda
Allahabad
3rd May 1818
Benares
May 1818
Chazipoor
18th Sep
Patna
11 July
Brahmaputra River
Sinde R.
Betwah R.
Rewah
Novem. 1817
Saugor
Apr. 1818
Moorshedabad
Silhet
15 Aug 1817
Jubbulpore
9th April 1818
Jessore
Aug. 1817
CALCUTTA
Aug. 1817
Chittagong
23 Aug
1817
Line of July 1818
Line of Mahratta
Mundlah
April 1818
Nerbuddah R.
Balasore
15th Sep 1817
Mouths of the Ganges
Hooghly R.
Nagpoor
25 May 1818
Cuttack
Nuwagaut
April 1818
Aurungabad
10th July 1818
Jaulnah
3rd July 1818
BOMBAY
Panwell
Godavery R.
Hyderabad
25th July 1818
Vizagapatam
15 May 1818
Kistnah R.
Masulipatam
July 10th 1818

feet above the level of Madras, in Lat. 12° 57′ N. and 77° 46′ East, on the 22d of October. And as we approach Cape Comorin, on the extreme south point of India, we find it coming down one coast almost stage for stage with its progress on the other side *.

We thus find the disease travelling from north to south, with unaccountable regularity; appearing in the line of 20° of northern latitude in the beginning of 1818, and reaching 8° north latitude on the 1st of January 1819; holding its course, seemingly uninterrupted by winds, seasons, or climate; appearing at Dharwar, in the interior, on the 13th of August, in the height of the rains, when the thermometer seldom rises above 75° of Fahrenheit in the hottest part of the day; and at Ongole, on the coast, in the same latitude, but distant 5° of longitude, in the dry season, when the thermometer varies from 95° to 105°, within a few hours of the time at which it broke out at Dharwar. In the progress just referred to, far from being assisted by winds, the disease must have frequently travelled in direct opposition to their currents, and seems never to have been retarded or advanced by their direction.

Though the general progress of the epidemic was thus regular from north to south, the disease did not appear regularly at every town in its course. It often seemed to pass over particular stations, breaking out at distant points to the south of them, and, perhaps after three weeks or a month, it would return on the place which had thus apparently escaped the scourge.

The course of the disease through individual districts in India, was often, indeed, very eccentric. It

* Vide Map affixed to Madras Report, and the Report.

frequently happened, that, instead of advancing directly to populous places by the principal and most frequented roads, it would "take a complete circuit round the village, and, leaving it untouched, pass on as if it were about wholly to depart from the district, then, after a lapse of weeks, or even months, it would suddenly return, and, scarcely reappearing in the parts which had already undergone its ravages, would nearly depopulate the spot that had so lately congratulated itself on its escape*." Or, it would sometimes nearly depopulate small villages in the neighbourhood of a principal station, before it made its appearance there. Thus, during a period of upwards of five years, from 1819 to 1824, while I had medical charge of the civil and political establishment in the southern Mahratta provinces, Cholera, in an epidemic form, appeared annually in that district in the months of April, May, and June. The town of Dharwar is the principal civil station in these provinces, and, during a great part of the time referred to, I was also in medical charge of the garrison of Dharwar, yet the disease appeared in that town in the years 1820 and 1821 only. In the latter of these years, as stated in my official report, the disease had "prevailed for the last two weeks in several surrounding villages of from six to twelve miles distant, and has in some of them swept off many of the inhabitants," being thus, for many days, within sight of the capital of the district before it reached it. In 1824, although the disease again prevailed in the immediate neighbourhood, it did not appear at all either in the town, or jail of Dharwar, although no attempt to impose any restraint on the intercourse

* Bombay Report, p. 6.

with the villages in which the disease prevailed, was ever thought of. This peculiarity in the habits of the disease was still more strikingly illustrated on its first breaking out in the neighbourhood of Benares, the Brahminical metropolis, the facts connected with which, I shall immediately have occasion to refer to.

A still more striking peculiarity in the history of the disease is, that when it made its appearance in a camp or city, instead of extending to every habitation, it was almost invariably confined to particular portions of even the most populous places. Sometimes, in an army for instance, one or two regiments encamped together, or separated by other corps, were the only sufferers in an attack of the epidemic. One division, or even one street only of a town, was sometimes the seat of the disease,—nay, its prevalence has been known to be limited to one side of a market-place, and, notwithstanding the most uninterrupted communication, the ravages of the disease would be for a time confined to these particular spots.

What is still more remarkable, troops, carrying their sick along with them, could leave the source of the disease behind them; an entire and immediate stop having been frequently put to its ravages by removing a camp a few miles*. So also it frequently happened,

* This did not occur in single regiments only. The Marquis of Hastings, in 1817, moved ground with an army of nearly 100,000 men, including camp-followers, when the Cholera was at its height,—when "all business had given way to solicitude for the suffering. Not a smile could be discerned, nor a sound heard, except the groans of the dying, and the wailing over the dead."—"It was clear that such a frightful state of things could not last long. It was therefore wisely determined by the Commander-in-chief, to move in search of a healthier soil and purer air."—

that, when Cholera prevailed destructively in a village, the inhabitants got rid of it by deserting their houses for a time, although, in doing so, they necessarily exposed themselves to many discomforts, which we should be inclined to consider exciting causes of an infectious or contagious malady.

On the other hand, however, the disease has been said to accompany troops in marching into a district where it had not previously prevailed, and into which it has been thus introduced. This is a fact so much at variance with the well ascertained habits of the disease, that, without contradicting it, I should not be inclined to admit it without a minute inquiry and unquestionable evidence. We have just seen that an army, by shifting its ground, has put an entire stop to the ravages of the disease; and yet it is said that a detachment of troops have not only carried it with them for ten or twenty marches, but have communicated it to healthy persons at that distance. But this is not all; for this very question has been largely

Bengal Report, pp. 14, 15. The army moved upon the 13th November 1817, and after marching between forty and fifty miles, halted on the 19th. The description of the march is most appalling: "The road was strewed with the dead and the dying; the ground of encampment and line of march presented the appearance of a field of battle, and of the track of an army retreating under every circumstance of discomfiture and distress."—*Ibid.* p. 17. "The army on the high and dry banks of the Betwah at Erich, soon got rid of the pestilence and met with returning health." The disease ceased to be epidemic on the 22d or 23d, so that this crowded camp, carrying thousands of sick along with it, by moving forty miles, and in less than ten days, shook off this disease, which some of the recent writers on contagion represent as having been carried from Nagpoor to Jaulnah, nearly 300 miles, by a small force; and from Ceylon to the Isle of France, about 2000 miles, by a British frigate.

discussed in the Bengal Report, and, amongst many others, the following remarkable instance to the contrary is given. " There is yet a still stronger instance " (the reporter observes) of a diseased body (of troops) " joining a healthy one, without thereby communicat- " ing the infection to it. On the morning of the 11th " of May 1818, a detachment of ninety men, of the " 1st battalion 26th native infantry, marched from an " inferior post to join the main body of troops then " encamped at Sangor. After an ordinary march, it " halted in perfect health half way, under shelter of " a few trees on the banks of a small lake, situated in " the midst of an open space about three miles in cir- " cuit, and surrounded by low woody hills. The whole " remained well until the fall of night, when Cholera " broke out amongst them. The first man was taken " ill at midnight, and died in half an hour. Several " others fell sick within the next few hours, and before " sunrise twenty out of the ninety were overtaken by " the disease. Although the Sangor camp was distant " only five or six miles, the detachment was too weak " to move without assistance. The sick of the Sepoys " and followers were, therefore, carried on in carts and " doolies sent from the main body; but before 11 A. M. " when they got to their ground, five were already dead, " and two others moribund. Next morning a man of " the same party was seized in the act of scouring his " accoutrements, immediately became insensible, and " expired in a few minutes. During the three succeed- " ing days, several others were taken ill, and before the " end of the week, of the whole detachment there was " not a single man but was sent to the hospital labour- " ing under Cholera, or other modifications of bowel " complaints. *The men of this party mixed promis-*

"*cuously with those of the Sangor troop, and yet,*
"*of the latter, not one individual got the disease.*"
—*Bengal Report*, p. 132–3–4.

Similar instances are innumerable; and, in my apprehension, the apparent exceptions, to what may be called the general rule, may be otherwise accounted for. When travelling on circuit, I have found the disease prevailing in a district before any report had been made of the fact, notwithstanding the most positive orders on the subject; and I am persuaded, that, were the instances adduced in support of the statement now under consideration strictly inquired into, it would be found that, instead of the troops carrying the disease along with them, the usual apathy of the natives of India had prevented its existence from being noticed, until the fact was brought prominently forward by the presence of Europeans.

My own experience, if I may be permitted to mention it, presents a very extraordinary example of the general habits of the disease, so adverse to the notion of its being carried forward by troops, that I shall here abridge a portion of the journal of a march which I made in India. In July 1819 I marched from Madras in medical charge of a large party of young officers who had just arrived in India, and who were on their way to join regiments in the interior. There was also a detachment of Sepoys, and the usual numerous and miscellaneous attendants and camp-followers of such a party in India. Cholera prevailed at Madras when we left it. Until the fifth day's march (50 miles from Madras), no cases of the disease occurred. On that day several of the party were attacked on the line of march, and, during the next three stages, we continued to have additional cases. Cholera prevailed in

the countries through which we were passing. In consultation with the commanding officer of the detachment, it was determined that we should endeavour to leave the disease behind us; and, as we were informed that the country beyond the Ghauts was free of it, we marched without a halt until we reached the high table-land of Mysore. The consequence was, that we left the disease at Vellore, eighty-seven miles from Madras, and had not a case of it until we had marched seventy miles farther (seven stages), when we were informed that it prevailed at one of our appointed places of encampment. Our camp was, in consequence, pushed on a few miles, and only one case, a fatal one, occurred in the detachment. The man was attacked on the line of march. We again left the disease, and were free of it during the next 115 miles of travelling. We then entered a tainted district, and found many of the villages deserted by the inhabitants, who had fled in search of healthier ground. Here we had cases of the disease occurring for three stages. We once more got beyond its limits, and reached our journey's end, 260 miles farther on, without having a case. Thus, in a journey of 560 miles, this small detachment was exposed to, and left the disease behind it, four different times, and on none of those occasions did a single case occur after we had left the tainted districts.

I have known the disease to prevail for several weeks at a village in the southern Mahratta country, within a few miles of the principal station in the district, and then leave that division of the country entirely; or, perhaps, cases would occur at some distant point. In travelling in circuit with the Judge of that district, I have found the disease prevailing destruc-

tively in a small and secluded village, while no cases were reported from any other part of the district.

It happened oftener than once, when the disease attacked the battalion under my charge at Dharwar, in the southern Mahratta country, that it was confined to one particular subdivision of the lines of the regiment, from which only cases were brought into hospital during that particular round of the disease; and a similar phenomenon took place in the town of Dharwar, certain streets only suffering from the disease at one time.

Other instances of still a more limited prevalence of the disease occurred at particular places; for example, in a camp, only one tent would appear to have been within the sphere of the remote cause of the disease, and almost every person who had slept in that tent would be attacked in the course of one night. An instance occurred to a friend of mine, in which the disease seemed to be caught in a very remarkable manner. The division of the army to which this gentleman was attached, had suffered severely from Cholera. One evening, when the disease appeared to have subsided, he, in a stroll with a friend, met a lady of their acquaintance on horseback, who stopped and conversed with them, and in the course of that night each of these individuals was attacked with Cholera.

The great practical question suggested by these peculiarities in the history and progress of Cholera is that of contagion; and in order that there may be no misunderstanding about terms, I think it proper to premise, that, by contagion, I mean that unknown quality in a disease whereby it is capable of being communicated from man to man *without the presence of its original source.* And if it be ascertained that,

in this sense, Cholera is contagious, and still more if there be any foundation for the notion that contagion is the *sole* cause of the dissemination of this pestilence, neither individual risk, nor the sacrifice of a whole town or district, ought to prevent the shutting up of the disease in any place into which, by misfortune or negligence, it has been imported. If, on the other hand, the disease be non-contagious, or if something more than mere contact or communication be necessary in order to bring the *virus* into operation, no one can seriously doubt that rigorous quarantine regulations and sanitary cordons are worse than useless.

The supporters of contagion in Cholera have invariably taken it for granted, that the disease commenced at Jessore, about seventy miles to the north-east of Calcutta; and they trace it to the local unhealthiness of Jessore, and to the poverty and fish diet of the inhabitants. But we have seen that the disease truly did *not* commence at Jessore. No doubt when it broke out at that place, the poverty of the inhabitants, and the unhealthiness of the town, rendered it so fatal as to bring its existence prominently before the public, and thus led to the belief that it had been generated in this town, and carried by contagion from it as a centre. There is, however, no foundation for this hypothesis. On the contrary, the disease had commenced six weeks before, at the places already mentioned; and it is not a little remarkable, that, on its first appearance in the summer of 1817, it broke out simultaneously at several places in the Bengal territory, at the distance of many hundred miles from each other, whereas, ever since, its march has been progressive; and, since 1817, I know of no instance on land in which the disease has made its

appearance at two points, at the distance of 300 miles from each other, without occurring at any of the intervening stations.

I am quite aware that this is a fact which may be adduced as proving the disease to be contagious, since it may be said that it is by contagion that it is carried forward in the particular direction in which it seems to be moving. But, on the other hand, it must be remembered, that it is not by great lines of road or other thoroughfares only, that the disease proceeds. Of course, where there are no inhabitants there can be no victims to the disease; but again and again, in India, it has arrived in the course of its extraordinary progress at a town or village, without a trace of contact or communication with any place or person affected with the disease. Besides, a contagious disease never moves with the steady and undeviating progress so remarkable in Cholera. Its course is more erratic; and hence, if Cholera had been communicable by contagion alone, it must have been carried, times without number, by regiments on their march from infected to healthy quarters, whereas, it is matter of notoriety, that the very reverse has been the fact; and that regiments, instead of propelling the disease in the line of their march, have at once suspended its ravages by changing their ground; the most remarkable instance of which is the well-known movement, just alluded to, of the grand army under the Marquis of Hastings, in the year 1817, whereby the most appalling mortality was arrested as if by a miracle*. These are

* As an example of the rashness with which statements concerning the contagious character of the disease in India are made, I may refer to the article in the Quarterly Review just cited. In that article, it is said that the British army carried Cholera into

facts which can neither be explained away nor disputed. They are matter of public record, and the subject of reports by the most distinguished medical men in India; and so far at least as regards Indian experience, nothing surely can be more unreasonable than the conduct of those medical writers and journalists in England, who, without the slightest practical acquaintance with the disease, set their own impressions and speculations against the authority of almost the whole medical profession in India, or at least of very nearly all the members of that profession who have had sufficient opportunities of making themselves acquainted with the habits of this disease *.

The progress of this disease from town to town, and from country to country, has been very extraordinary. It has extended itself over countries differing entirely from each other in climate and in geographical position; and it appears to have been little if at all affected by climate or temperature. Hence, it has been very naturally said, that a disease which has thus spread, must have been propagated by contagion; and the conclusion would be a legitimate one,

the Burman empire in 1823; whereas, it is an historical fact, that the army *found* the disease prevailing among the Burmese.

* It is said, in a recent Number of the Foreign Quarterly Review (Oct. 1831), that the majority of Indian practitioners are contagionists. This is so notoriously contrary to the fact, that the talented writer of the article to which I allude must have proceeded on erroneous information. The reports of the Medical Boards of Bengal and Madras are conclusive upon this subject; and with the exception of one or two young men who had seen little or nothing of the disease, I have never met with an Indian practitioner who believed Cholera to be contagious in that country.

had Cholera advanced more rapidly on great thoroughfares, or along the sea-coast, where unrestricted intercourse prevailed, than in the interior of the country, where little or no communication existed; but instead of having been there propelled in one direction, and retarded in another, the disease was found in India to travel with an equal pace over the whole country, extending in its course south over eight or ten degrees of longitude, and reaching the least frequented spots and most secluded villages no less than the most populous districts *.

Much stress has been laid by writers on this subject, on the fact that Cholera has travelled along the banks of rivers. Undoubtedly it has done so; but in so far as regards India, the writers who rely so confidently on this fact, have not adverted to the peculiarity, that the progress of the disease along the banks of rivers, has not been confined, as they seem to suppose, to *navigable* rivers. It has extended itself

* I am aware it has been supposed, that the disease adhered to thoroughfares, and was only found in great cities. In answer to this, I may refer to the Reports of the Indian Boards, which conclusively negative that supposition. Besides, it must be borne in mind, that even these reports are prepared by *Europeans*, who are seldom, and only accidentally, in unfrequented towns and villages in India. But I ask, under correction of every man who has been in India, whether it has not again and again happened that parties of officers and their servants have been fatally attacked with Cholera, on hunting excursions in the interior of the country? How common was it also, during the war, for detachments of troops to meet the disease in the most unfrequented places. On great thoroughfares, there will always be fatigue, exposure to climate, unwholesome food, and other predisposing causes, sufficient to account for the occurrence of the disease, when it is prevalent in the country, without having recourse to the agency of contagion.

along the banks of rivers, whether navigable or not, for a reason which will at once be satisfactory to every one acquainted with India, viz. that in India, as in other hot climates, the inhabitants are almost always to be found on the banks of rivers; and in absence of inhabitants, the disease of course cannot manifest itself. Besides, it is a well-known peculiarity of the disease, that it is apt to attach itself to low lying places, such as the banks of rivers usually are. But on the banks of rivers in India, as elsewhere, it has not happened that the disease has outstripped its regular march *. Even on navigable rivers, where the most uninterrupted intercourse has prevailed, it may be safely said, that there has been no instance in which the progress of the disease has been accelerated by the small trading vessels which go from port to port, and which, if the disease could have been so communicated, must, in innumerable instances, have carried with them the supposed pestilential *virus*, which nevertheless remained innocuous in every place where it had not been preceded by the unknown and hitherto inscrutable primary cause of the disease.

It may be said, that we are not to expect that a disease, which, like Cholera, at once arrests the wayfaring traveller, is to pursue its course rapidly on land. But this remark does not apply either to navigable rivers or to the sea-coast. Hence, had Cholera been transmissible merely by contagion, instead of reaching

* In the article in the Quarterly Review to which I have alluded, the progress of the disease is said to have been accelerated along the banks of navigable rivers, and on the sea-coasts; but this statement is made in the face of the facts established by the Indian Reports, and shown on the maps accompanying this work, and other works on Cholera.

the sea-port of Madras simultaneously with its appearance in parallel latitudes in the interior, it must have been carried from the tainted districts in the north to the seat of the Presidency, by some of the many trading vessels, which, free of all quarantine regulations, maintained a constant intercourse between the districts where the disease prevailed and the ports along the coast which it had not yet reached, and to which those coasting-vessels in no instance expedited its advances.

The progress of Cholera down the peninsula of India, when it visited that country in 1818, is so instructive on this point, that I have obtained an abridgment of the map published by Mr Scott in his report already referred to. The authenticity of this piece of evidence is indisputable; and, notwithstanding all that has been written on the subject of contagion, I have met with no successful attempt to account for this phenomenon. This map shews that Cholera, as an epidemic, was in 19° N. on the 10th March 1818; that it travelled regularly at the rate of about one degree of latitude a-month; and that it reached Madras on the 8th October 1818, in 13° N. This was its progress during the dry season, and when there was no interference with the constant commercial intercourse which prevails on the Coromandel coast. On the 15th October annually the port of Madras is closed; and in consequence of the prevailing winds, and of the surf which, during the next two months, breaks upon the whole of that open coast, every vessel is forced to leave it, and the small trading vessels are drawn high and dry on land. Yet, on looking to the annexed map, it will be seen, that this interruption of intercourse had no effect in retarding the pro-

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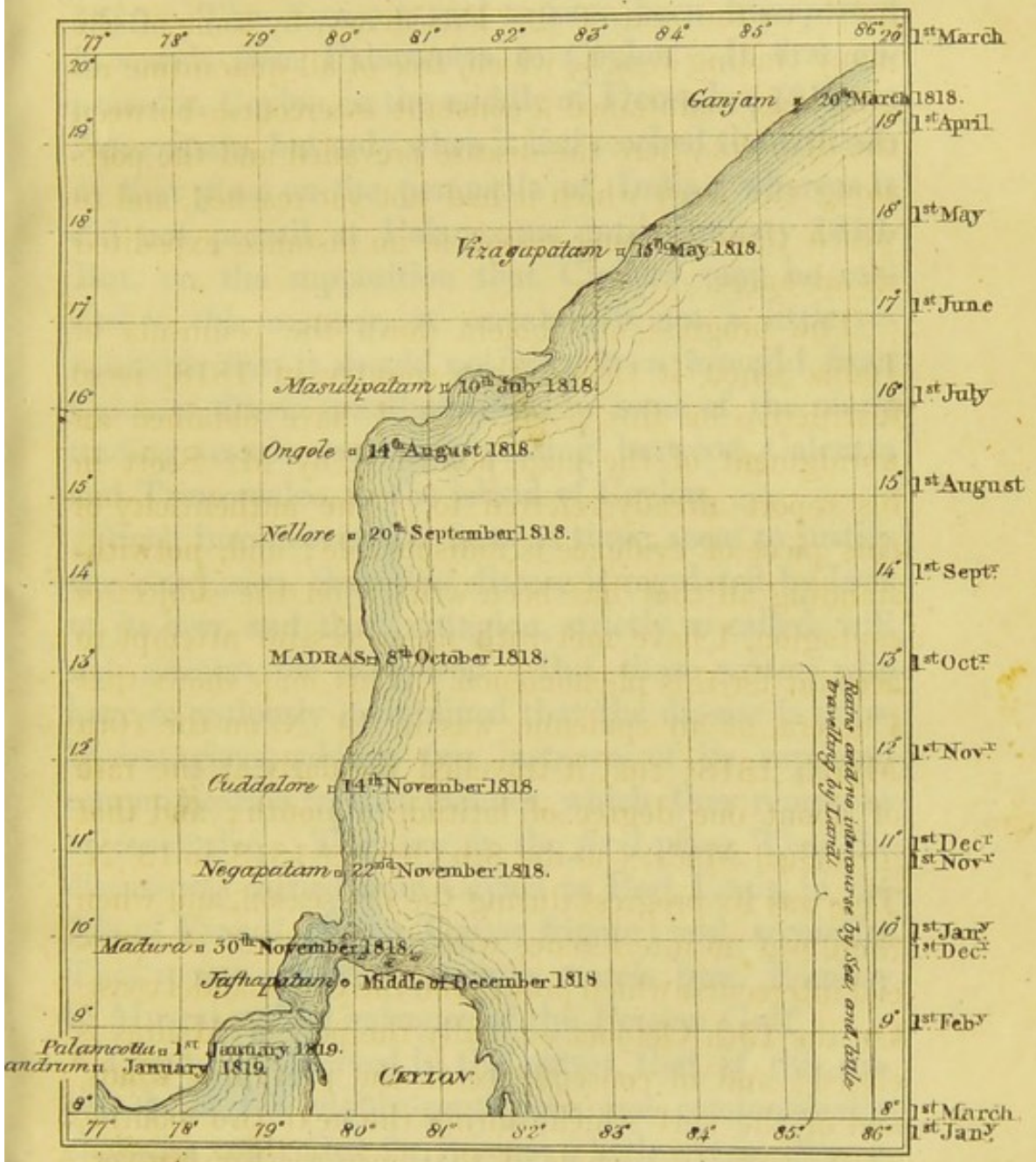

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gress of the disease. It reached Cuddalore on the 14th November, Madura on the 30th of the same month, and Cape Comorin by the 1st of January 1819. The disease is said to have been transported in vessels from Palamcotta to Ceylon. It first appeared in Ceylon, in the middle of December 1818, at Jafnapatam, but only when it had reached the latitude of that place on the peninsula of India; whereas it did not prevail at Palamcotta until January 1819. But, on the supposition that Cholera may be carried in this manner, it certainly is not a little remarkable that it should not have been brought from Calcutta fifteen months earlier, by some of the many trading vessels constantly passing between Calcutta and Trincomalee, in the island of Ceylon.

Such incontrovertible facts as these, seem to justify the conclusion, that this disease is regulated by laws of its own, and that contagion, strictly so called, will not account for its spread. But those writers who have so zealously maintained that the disease is purely contagious, adduce two instances of its supposed communication in that manner, which they represent as conclusive. They say, in the *first* place, That the disease was carried from Ceylon to Port Louis, in the Isle of France, by the Topaze frigate; and, *secondly*, That it was carried by country vessels from Bombay to Muscat, at the entrance of the Persian Gulf.

It may be observed in the outset, that, if this disease be of the highly contagious and communicable character contended for, it is not a little extraordinary, that, at the end of fourteen years, no more than two instances are to be found of the supposed conveyance of the disease by sea; while it is notorious, that, during all that time, there have been no quarantine re-

gulations in India, and while it must be obvious, that, if the disease could have been so transported, it must long ere this time have been carried by regular stages, from the Isle of France, for example, to the Cape of Good Hope *, thence to St Helena, and so onward to Europe, by some of the thousands of vessels which must have brought with them the seeds of the supposed contagion; more particularly if it be of that subtle and extraordinary nature described by Drs Russell and Barry, who, in one of their communications to Government, talk of the *virus* of Cholera having remained latent for thirty days in an individual, and in another represent it as inherent in the cordage or seams of a vessel, although none of the passengers have suffered from it—a refinement in the doctrine of contagion, and a sublimation of pestilential virus, which I believe to be wholly unexampled.

But this is not the only answer to the two instances alluded to. The evidence, so far as regards the Topaze frigate, is of this nature. That vessel left Trincomalee in Ceylon on the 9th of October 1819, while Cholera prevailed there, for the Mauritius, and reached Port Louis on the 29th of October 1819. When the Topaze arrived at Port Louis, the crew appear to have been very unhealthy, for on the day after the arrival of the frigate, several patients were sent on

* In the Quarterly Review (above cited) it is said, that the Cape has been protected by the old Dutch quarantine laws! Does the Reviewer really suppose that ships from India are subjected to any quarantine at the Cape? The health officer comes on board, and requires the usual assurance of the health of the crew; but this is a mere ceremony, and would be wholly unavailing to arrest Cholera, if that disease be transmissible, as the contagionists maintain, by *goods*, and by passengers who are not themselves labouring under the disease, and *who never had it*.

shore, suffering under dysentery, inflammation of the liver, and other complaints. *There was no case of Cholera;* but, *on the forenoon of the day of the vessel's arrival at Port Louis,* one man on board was attacked with severe spasms and vomiting. Three weeks after the arrival of the vessel, Cholera began to spread in the town of Port Louis.

Such is the substance of Mr Kennedy's statement in his work on Contagious Cholera. At the same time, however, he admits, that cases of Cholera had occurred at Port Louis early in September, *nearly two months before the arrival of the Topaze.* This admission appears to me perfectly fatal to the whole of his theory, so far as it is founded on the Topaze frigate; but this is not all, for it is well known that a Medical Board was appointed to inquire whether there was sufficient evidence that the disease had been introduced by this vessel, and that Board reported on the 4th December 1819, that the disease had prevailed before the arrival of the Topaze:—

"*That the first case occurred so early as the* 6*th*
"*September* 1819*; and that they feel the strongest*
"*persuasion that it is not of a contagious nature,*
"*and that it is not of foreign introduction. In*
"*these conclusions the French medical gentlemen*
"*unanimously concurred, and both considered the*
"*disorder promoted, if not produced, by the great*
"*and sudden vicissitudes in the temperature**."

It is not therefore altogether fair, either in Mr Kennedy, or in Sir Gilbert Blane and the other contagionists, who rest so much upon this instance, to represent it as an incontestible proof of the trans-

* Asiatic Journal, December 1831, p. 328.

mission of the disease in this manner. Nor will it do for two home-bred physicians, one of whom never saw a case of the disease, and the other of whom had no very extended Indian experience, to set their conjectures and hypotheses, at the distance of fourteen years, against a report drawn up at the time, and on the spot, and unanimously concurred in both by the English and French medical officers. In truth, the case of the Topaze frigate, so far from supporting Mr Kennedy's conclusion, manifestly *disproves* it, as far as negative evidence can disprove any thing.

But Mr Kennedy says that seventeen cases of Cholera occurred on the passage of the Topaze from Ceylon to the Mauritius. The precise place where, and the time when, these cases occurred, are not mentioned by Dr Kennedy; but it appears from the journal of Mr James Foy, the surgeon of the ship, that these seventeen were attacked *immediately after* the ship sailed from Trincomalee, and while of course they were within the tainted limits. It is not stated that any other cases of Cholera occurred on the passage, and it is almost certain that none would occur, or if so, they must have occurred within a few hours of each other, while the vessel was passing through, as it were, a tainted current, beyond the limits of which there could be no new case. On this point, I can confidently appeal to every medical man who has witnessed the disease; for, while it has been proved repeatedly that a change of ground will relieve a detachment of troops on land from the ravages of the disease, it has been equally well ascertained, that a vessel, after leaving a tainted port, or passing through a tainted current, enjoys a similar exemption.

It appears to me, therefore, that the evidence af-

forded by the Topaze frigate refutes itself; and, for that reason, I have been at no pains to find evidence on the other side. But having accidentally met my friend Captain Deare, I have great satisfaction in quoting the following letter from him, in which it will be found, that his conclusion is precisely that to which his profession generally have come on this subject. His letter is as follows:—"*Edinburgh, 28th November* 1831.—My Dear Sir, In compliance with your request, I have much pleasure in stating to you such particulars as came under my personal observation in his Majesty's ship Liffey, and now suggest themselves to my recollection. In the month of October 1823, that ship was lying in the Madras Roads. The wind for several days previous to the disease making its appearance had been light, and generally off shore; the evening on which the malady did break out being unusually close and oppressive. In the course of twenty-four hours nearly fifty men were in their beds, twelve of whom I think died. At the expiration of this time the ship was ordered to sea; the disease up to the hour of our sailing evidently gaining ground. We accordingly stood out from the Roads for forty or fifty leagues, and returned to the anchorage in forty-eight hours with the malady completely checked, and all of our sick convalescent. The inference to be drawn from this, in my opinion, is, that the Cholera Morbus is not contagious, and that it is communicated entirely by unknown local causes. The crew of the Liffey consisted of between 300 and 400 men, and had the disease been contagious, instead of merely local, it must have spread through the whole or greater part of the ship's crew, in place of yielding as it did imme-

diately to change of air. It is perhaps as well to mention, that the officers and men generally were in constant communication and *contact* with the sick, nor am I induced to suppose that the slightest danger was incurred thereby. I remain, &c. (Signed) CHARLES DEARE."

The truth is, that the case of the Topaze, as stated by Mr Kennedy, would be perfectly unique in the history of the disease. Cholera has now been spreading epidemically for upwards of fourteen years, during the whole of which time it has constantly prevailed in numerous sea-ports. Had it, therefore, been capable of taking a sea voyage of 2000 miles, and of being propagated by a vessel six weeks after it left the diseased port, the solitary instance of the Topaze would not have been the only one in the British navy to which the contagionists could have appealed; and we should not have remained up to the present day without examples of new cases of the disease at our quarantine stations.

The second instance of the supposed transmission of the disease by sea, referred to by Mr Kennedy and others, is that of Muscat, to which, he says, that the disease was carried from Bombay. Muscat is about 1000 miles of clear sea-way from Bombay, and between these two ports there is an uninterrupted and extensive commercial intercourse. The Cholera reached Bombay in its regular course, and at what, according to the rate of its progress, may be looked upon as its *appointed time*, on the 10th August 1818; and Bombay continued to suffer severely in the years 1819, 1820, and 1821. During all this time, the intercourse with Muscat remained uninterrupted; and the disease did not break out there until

June 1821. On reference to the map, it will be seen that the disease appeared at Muscat, just as it had previously done at Bombay, in the regular course of its progress over that part of the world. Mr Kennedy's argument, in short, proves too much; for if nothing more be requisite in order to propagate the disease than intercourse between an infected and a healthy port, and if it be also true (as it undoubtedly is), that the intercourse between Bombay and Muscat was as constant and uninterrupted in the years 1818, 1819, and in 1820, as in 1821, Muscat ought to have suffered nearly three years earlier than it did. If, indeed, a well-aired and well-cleaned British frigate carried the disease, and in three weeks, from Ceylon to the Mauritius, a distance of 2000 miles, it is incredible that three years should have been required to transport the same disease from Bombay to Muscat, between which two ports the intercourse is carried on by filthy Arab trading vessels, crowded with passengers of every description.

In short, it must be obvious to every dispassionate inquirer, that the attempt to prove contagion by instances such as these is desperate. Had the disease been truly contagious in the East, the last fourteen years would have afforded thousands of instances; and, as happens in other contagious diseases, the populace would have been familiar with the fact, and would have rejected any attempt on the part of medical speculators to satisfy them that the disease was not so communicable. But the contrary has happened; and, in India, the natives, learned and unlearned, concurred with the medical profession, generally speaking, in regarding the disease as non-contagious. This is a fact of more value in the present argument, than all that

the European contagionists have written or can write; and, therefore, leaving the Indian question on the basis on which experience has placed it, let us next inquire whether there be any evidence that the character of the disease, as regards contagion, has been altered in the Temperate Zone. The points for inquiry here are similar to those which have been already considered, viz. *first*, the mode of progression of the disease on the great scale, and its habits where it has broken out; and, *secondly*, the question as to whether or not there has been sufficient evidence advanced to support the theory of contagion.

The most remarkable feature connected with the entry of the disease into Europe, is the length of time during which it seemed to hover on the frontiers. Thus the disease, approaching Europe apparently in a direct course, passed on from India to Persia, thence to Asiatic Turkey, and from Asiatic Turkey across the Caucasus to Astracan, which it reached in September 1823,—creeping on, as it had done in India, from country to country, at a pace so regulated, that, had no cause of detention occurred, it ought to have reached the western parts of Europe by the year 1825. Instead, however, of forcing its way into Russia in the winter of 1823, it did not reach the Don or the Wolga until the autumn of 1830.

This is unquestionably a very remarkable circumstance, but such pauses are not new in the history of the disease. In Bengal it occasionally made halts, although not for years, yet for several months, which were equally unaccountable. For example, it is a well known fact, that it stopped in the neighbourhood of Benares, the Mecca of the Hindoos, soon after its first breaking out in India in 1817. At that time

the alarm occasioned by its appearance and appalling mortality, excited a panic among the natives of India, who looked upon it as an immediate judgment from Heaven. It prevailed within a few miles of Benares, which, as being the holy city of the Hindoos, was inundated by crowds from all quarters, hastening to deprecate the wrath of Heaven by vows and sacrifices. Benares, as is well known, contains a population exceeding half a million. The site of the city is reckoned holy, but the holy precincts are of a very limited extent; and in order to have the benefit of a residence within the sacred territory, the houses are piled upon each other in successive storeys, something in the style of the old town of Edinburgh. "The streets are so "extremely narrow, that it is difficult to penetrate them "even on horseback. The houses are built of stone, "some six storeys high, close to each other, with ex-"tremely small windows to keep them cool," (to keep out the glare, and hot winds), "and prevent inspection. "The opposite sides of the streets, in some places, ap-"proach so near each other as to be united by galleries. "The number of stone and brick houses from one to "six storeys high is upwards of 12,000, the mud houses "above 16,000; and, in 1823, the permanent inhabi-"tants by enumeration exceeded 582,000. During the "festivals, the concourse is beyond all calculation *."

Here, then, it may be said, was a city marked out almost for extermination by a contagious disease. In August and September 1817, it prevailed, as will be seen from the map, within a short distance of this city. The inhabitants of the tainted neighbourhood hastened in thousands into Benares; and strange,

* Vide Hamilton's Gazetteer, *voce* Benares.

and, on the theory of contagion, wholly unaccountable, as it may appear, *this city was not visited by the pestilence until eight months after it had come almost up to its gates.* The Cholera was at Gazypore, Buxar, &c. in September 1817, yet it did not prevail at Benares until May 1818. It made one of its extraordinary and capricious halts in the immediate vicinity, and for the time, the inhabitants, who had never thought either of contagion or of sanitary cordons, entirely escaped.

By some cause equally unaccountable, the epidemic was stayed for seven years on the confines of Russia and Turkey. The contagionists say that it was thus retarded by the Russian quarantine regulations. It is not pretended, however, that, during this interval, cases of the disease occurred at the quarantine stations, or that it came close up to the sanitary cordons which arrested its progress. Besides, even were it admitted that the Russian regulations stopped the progress of the disease, how came it not to advance into European Turkey? We hear of no Turkish quarantine restrictions. The truth is, that no means, either restrictive or permissive, with which we are acquainted, will explain these anomalies. The disease did not pass into Turkey in 1824, although the road was open; and since the autumn of 1830 it has pursued an undeviating course westward, in utter defiance of quarantine regulations and sanitary cordons, and every other means which the most vigilant and despotic sanitary police could devise.

The disease entered Russia in a northerly course in the beginning of autumn 1830, and it reached Moscow in the course of two months, or little more; prevailing over a large extent of territory during the


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20
40
60
White Sea
NORWAY
SWEDEN
FINLAND
RUSSIA
Dwina R.
Petersburg June 26th 1831
Perm Dec.r 1830.
SCOTLAND
Edinburgh
Riga May 1831.
Moscow Sep.r 1830.
Ilinsk Oct.r 1830.
ENGLAND
Sunderland 8th Oct.r 1831
Hamburgh
IRELAND
DENMARK
BALTIC SEA
Dantzic May 1831
POLAND
Mohilew Feb.y 1831
Warsaw April 1831.
Penza Aug.t 1830
NETHER.ds
Berlin Aug.t 31st 1831
PRUSSIA
Lemberg May 1831.
Brody May 1831
Vienna Sep.r 1831.
Pesth July 14th 1831.
FRANCE
GERMANY
Danube R.
Dniester R.
Don R.
Volga R.
Ural R.
Astracan Sep.r 1825 July 20th 1830
SEA
Bucharest July 1831
BLACK SEA
Caucasus
CASPIAN
TURKEY
ITALY
SPAIN
MEDITERRANEAN SEA
ARABIA
PERSIA
AFRICA
60
50
40
30

whole of the latter part of autumn 1830. The Russian winter seemed to have some effect on it; for it did not proceed actively until the spring of 1831 *. It then began to advance on an extensive line, reaching Riga in May, St Petersburgh in June, in its north-westerly course,—and in its westerly course arriving at Warsaw in April. It appeared in Dantzic, Lemberg, and Brody, in May—Jassy in June—Pesth and Bucharest in July—Berlin and Vienna in September—Hamburgh in the end of September—and Sunderland in the end of October.

Compare this progress with what has been exhibited of the habits of the disease in India, and observe the result. The want of quarantine regulations, and the disbelief in contagion by the natives, and by the medical men in India, occasioned, as we are told, the spread of the disease in that quarter of the world. Admitting for a moment the justice of this implied censure on the British Government in India, How has the fact turned out in Europe? There, surely, it cannot be said that there has been any want of quarantine regulations and of sanitary cordons. On the contrary, these regulations have been most rigorously enforced, both by land and sea; and yet a disease which, in absence of quarantine regulations, took thirteen years to reach Europe, has, within little more than twelve months, traversed the north of Europe,

* A recent writer, of high professional attainments, ascribes this pause to *non-intercourse;* assuming that, in Russia, the internal communication is interrupted. This, however, is a mistake. During the winter, the intercourse in the chief Russian thoroughfares is not only uninterrupted, but expedited by the Russian mode of winter-travelling.—*See the Edinburgh Medical and Surgical Journal for December* 1831.

and arrived in Britain, in defiance of such regulations, and with a progress similar in every respect to that by which it was characterized in the East. If this be not demonstration, at all events, it ought to be sufficient to beget some little distrust in the minds of those controversialists, who persist in applying rules founded on their acquaintance with the habits and peculiarities of known diseases, to a disease which, both in its nature and progress, is anomalous, and as to which there are as yet no grounds for coming to any certain conclusion.

The next branch of this inquiry resolves properly into a question of evidence. The British Government naturally, and most properly, anxious to have full and satisfactory information concerning the phenomena of this disease, despatched Doctors Barry and Russell to St Petersburgh, in summer 1831, to watch its progress. These gentlemen were necessarily placed in somewhat of a difficult and trying situation; and it is perhaps to be regretted, that, in drawing their conclusions from the facts which came under their notice, or which were communicated to them, they did not proceed with a little more caution and circumspection. Most undoubtedly, the result of their mission has been, generally speaking, unsatisfactory to the profession to which these gentlemen belong, and to the public; since both contagionists and anticontagionists, who have occasion to look to the materials collected by Doctors Barry and Russell, must at once perceive that there are many and most striking defects, both in their facts and in their conclusions; and that, truly, that part of their inquiry which embraced the question of contagion, has been accidentally left incomplete, in consequence of the death of a gentleman, on

whose information, in answer to certain of their queries, they seem to have placed their chief reliance.

We must take the papers of these gentlemen, however, as they have been laid before the public. It appears from that publication, that they arrived at St Petersburgh on the 29th of June 1831; and their first communication, at the distance of two days afterwards, settled the question as to the identity of the Asiatic and European diseases. Then, seven days after their arrival at St Petersburgh, they write to the following effect:—"Although we feel that the time since our arrival here, *has been much too short to allow of a full investigation of the facts tending to illustrate the origin of the disease*, yet, under all the circumstances of our position, we have resolved not to defer the expression of those *conclusions on this subject to which we have already arrived with the most perfect unanimity*."—Papers, p. 26. And they then go on to state their conclusion, viz. "that, from the manner in which the disease has now broken out in this city, *we see no other mode of accounting for its sudden appearance here, than by concluding that the barks from places on the Wolga, where the disease prevails, have brought* SOMETHING *with them, which, disseminated in this atmosphere, has been the immediate cause of the eruption of Cholera which has just occurred*."—Papers, *ibidem*.

When Doctors Russell and Barry wrote the preceding passage, they certainly must have forgotten that they were charged with an important public mission; and that the question on which they had so speedily come to an unanimous conclusion, without "full investigation," was, in its practical consequences, one of the most important and difficult which they had to

determine. It is understood that, when Dr Russell left this country for St Petersburgh, he, as having been an Indian practitioner, was an anticontagionist. Dr Barry, on the other hand, like all European medical men who had not seen the disease, entertained a strong prejudice in favour of contagion. However that may be, the question which they had to decide, was one involving commercial property to the amount of millions, and intended to lead to regulations in which the whole community of Great Britain had a vital interest. These two gentlemen knew, moreover, that the disease, the identity of which with the Indian disease they had ascertained within a day or two after their arrival, was not reckoned contagious by their professional brethren in India. On this subject they had reports from the Medical Boards of at least two Presidencies, stating that to be the result at which they had arrived, after patient and anxious inquiry, founded on their own experience, and on the reports regularly furnished by their medical establishments. It was further matter of notoriety, that some of the most distinguished European physicians, including, in particular, those who had treated the disease in Russia, concurred with the Indian practitioners. And yet, in this state of matters, we find Doctors Russell and Barry, within *six days* after the commencement of their inquiry, and, as they say themselves, before they had fully investigated, and on mere hearsay and conjecture, came to be unanimously and decidedly of opinion that the disease was contagious, and that its sudden appearance at St Petersburgh was to be accounted for by "*something*" brought by barks from infected places on the Wolga.

Had these gentlemen said, that the barks to which they allude had brought individuals with them suffering under the disease, by whom it was communicated to the inhabitants of St Petersburgh, their conclusion would have been intelligible; or had they even explained the nature of the pestilent substance brought by the barks, there might have been less cause to complain; but as their communication to Government stands at present, it is impossible to imagine any thing more vague and unsatisfactory.

But it is not merely for drawing a hasty conclusion on such premises, that Drs Russell and Barry are responsible. On the contrary, one would be apt to suppose, that they had engaged in their inquiry without being historically acquainted with the habits and peculiarities of the disease, as it had exhibited itself elsewhere: since nothing is more certain than that the disease in its first appearance in a town or district, is not traceable to any visible or known origin. No doubt if it be taken for granted that the first victim of the disease in any particular town, must, as a matter of absolute necessity, have received it from having been placed in contact or juxtaposition with an infected person or substance, then, indeed, Drs Russell and Barry, finding no actual sufferer to whom they could trace the contagion, were driven *per force* to look for an inanimate substitute, among the cordage, or in the cargoes of the barks on the Wolga. But if, on the other hand, the history of the disease has demonstrated, as far as a negative can be demonstrated, that it has broken out almost invariably as if it were indigenous, where is the necessity for finding some visible external cause? This in truth is the capital mistake into which all inquirers after the ori-

gin of the disease have fallen. They must find "something," and yet what that something is, no man has been hitherto able to say. One thing, however, is quite certain, and that is, that a person actually suffering under the disease is not indispensable. There is no occasion to go to India in order to prove that fact; for I believe it may be safely said, that (except in places where there was reason to look for the disease) there is no town in Europe to which Cholera has been conveyed by an actual patient, or even by a convalescent; and what is more remarkable still, there is I believe but one authenticated case of a death from the disease at any of our numerous quarantine stations. Certainly there is no instance of a *new* case having occurred at any of those stations;—the disease, in the only cases which have occurred, having been clearly brought on board by the sufferer, and not having spread in the ship after it left the diseased port. On the other hand, in Europe as in India, the first appearance of the disease in a town is generally speaking traceable to no external cause; nor is it necessary to go out of this country in order to establish that fact, the first victims of the disease at Sunderland having been, as is well known, persons who were unable to trace the attack to any thing like contact or communication with either persons or vessels coming from any infected port.

Drs Russell and Barry, therefore, would have more correctly represented the fact, had they stated that the disease had broken out at St Petersburgh, according to its usual habit in India, in a manner wholly unaccountable. A glance indeed at the accompanying map with reference to the established habits of the disease in its progress in any particular direction,

will demonstrate that there was no occasion to look for its remote cause in the sails, cordage, or timber of barks coming from infected towns. It will be seen from this map, that Cholera reached Moscow and its neighbourhood in the autumn of 1830, that its progress was stayed during the winter, and that during the following spring we find it in February 1831 in Mohilew travelling westward; in April at Warsaw, about 250 miles due west; in May at Dantzic, about 400 miles west from Mohilew; in the same month at Riga about 400 miles to the north, and Lemberg about 400 miles south-west of Mohilew. Then, on the 26th of June, it arrived at St Petersburgh, 7° due north from Mohilew, and Bucharest, 8° due south of Mohilew, in the beginning of July 1831. In short, let its progress to those places, or to any other at which it appeared, be looked to, and the same uniform system of dissemination will be found to prevail. Its arrival therefore at St Petersburgh, just like its arrival at every other place which it has visited, must be ascribed to some general cause, not to any local peculiarities in the trade of St Petersburgh; more particularly when it is found that the date of its arrival at that city coincided with the period at which, without reference to any local peculiarities, it might have been expected.

In Europe, recovery from the collapsed state of Cholera appears to have been very generally followed by a fever of typhoid character, of which I shall have occasion to treat more largely in the sequel. Doctors Russell and Barry regard this fever as a stage of European Cholera, and seem to think it likely that contagion from the fever will communicate not only typhus but the collapse stage of Cholera. They say,

because the attendants of patients labouring under this fever are liable to take, not the fever, but Cholera, therefore the fever propagates Cholera. Here, also, these gentlemen overlook a very remarkable feature in Cholera, which is, that, during its prevalence, it seems to absorb or neutralize all other morbid affections. This is a well known peculiarity, to which I am myself able to bear testimony, for I have repeatedly seen the disease break out during the aguish season, in a district of India, where intermittent fever is endemic; and while Cholera prevailed, that is, for fifteen or twenty days, not a single case of intermittent fever was reported or brought to the hospital, although both before the arrival and after the departure of Cholera, five or six fever cases a-day were the average. If such be the fact, and it is matter of notoriety, it is not at all remarkable that typhus fever should not be in activity at such a time, or that persons susceptible of disease within the tainted limits should suffer from the prevailing epidemic.

In treating of the subject of contagion, Doctors Russell and Barry report it as a matter of fact, that the *virus* of the disease may be carried about in the human constitution without manifesting itself for upwards of thirty days. They announce this singular circumstance in the following passage, from which, it will be perceived, that here, as in other instances, they speak from mere hearsay. "About the month of November last year (1830), when the Epidemic Cholera was on the decline at Casan, and when the prisoners were assembling from different parts of the empire to be transported to Siberia, a party of them were dispatched from Casan to Perm, the principal town of the district or government of that name, which

they reached in about twenty-five days. They were all healthy at the time of their setting out—no casualties occurred on the road—the Cholera was not prevalent on any part of the road through which they passed, and when they arrived at Perm, the disease was unknown there, never having reached it. They were conveyed to a jail out of the town by a detour, so that they might not pass through it at all. A very few days after their arrival the Cholera broke out among them, and spread to the other prisoners in the jail, and about fifteen died in all."— *Papers*, p. 42.

In order to prove contagion by such an example as the preceding one, it is necessary to suppose, that, on this particular occasion, the disease had laid aside all its ordinary habits. In several instances mentioned in these papers, Doctors Russell and Barry represent the disease as seizing the patient within an hour or two after contact with the infected person. At p. 62. they use an argument to prove that contagion may be left behind by patients removed from their own houses to hospitals, and that after *nine days all risk of contagion is over*; and, in addition to all this, the fact is indisputable, that Cholera has generally begun and ended its ravages at any one place within less time than that during which it must have remained latent in the persons of these prisoners. But this is not all, for, on the hypothesis on which Doctors Russell and Barry proceed, one or more of these prisoners who left Casan, to all appearance, in perfect health, must have marched for twenty-five days with the *virus* of this mortal and extremely rapid disease in his system. Nothing can be more violent than such a supposition, while nothing is more in accordance with the ordinary habits of the disease than that it

should have broken out spontaneously at Perm (which, as appears from the map, lay within its range) as it did at other places, and that some of its first victims should have been these poor prisoners, worn out with the fatigue of a long winter march, and dispirited by the dreary prospect of the exile to which they were proceeding.

The next instance given by Drs Russell and Barry is intended to prove that Cholera may be conveyed to a village by an individual, and that in its progress it can be barricadoed out, or limited to a particular quarter. The evidence, however, on this point, is extremely loose. In the first place, the reporter of the fact is not only a contagionist, but one who is very eager to prove his own success in suspending the progress of the disease. He says that Cholera was brought by a coachman fifty versts, and propagated by him to a particular portion of the village,—that the diseased limits were ascertained, and a barricade formed, by means of which the rest of the village escaped attack. The only explanation which can be given of such an occurrence is, that, with a disease so capricious as Cholera, it is impossible to say whether the exemption enjoyed by the barricadoed quarter was owing to the precautions taken, or to the peculiarity frequently exemplified in the history of the disease, that it chooses its own limits, and does not transgress them. I am far from wishing to evade anomalies of this description, of which there are several in the course of the papers now under examination, the most remarkable of which certainly is that of Zarcozelo and Peterhoff, to which the Russian Court retired, while the epidemic prevailed at St Petersburgh. A similar instance of immunity, much relied on by contagionists

as illustrative of the same fact, is that of the French Consul's garden in the neighbourhood of Aleppo; and, I believe, there are similar instances to be found in most of the towns in which the disease has appeared.

Now, in reference to the whole of these, all that can be said is, that they are in no way more unaccountable than many of the other anomalies by which this epidemic is distinguished. They prove nothing except that, in certain circumstances, the disease did not transgress certain limits, while, on the other hand, there are innumerable instances in which the most rigid sanitary precautions have proved unavailing, and in which the disease has broken out in persons who are admitted to have had no contact with any infected person or substance. In addition to this, there are facts connected with the disease as it appeared in India, still more extraordinary even than those in question. On what hypothesis, for example, can the following facts, which fell under my personal knowledge, be explained? The military hospital at Dharwar, an oblong apartment of about 90 feet by 20, was within the fort, while the lines of the garrison were about a mile distant, beyond the walls. On two different occasions, in 1820 and 1821, when the disease prevailed epidemically among the troops of that station, but while no case had occurred in the fort, within which the hospital was situated, I brought the patients at once from their quarters, into the hospital, which was crowded with sick labouring under other disorders. No attempt was made to separate the Cholera patients from the others. On one of those occasions no case of Cholera occurred within the hospital among the numerous patients or the hospital

attendants; on the other occasion, one of the sick was attacked, but he was a convalescent Sepoy, who was at liberty to leave the fort during the day. The disease on each of those occasions was confined to a particular subdivision of the lines, and none of those residing within the fort were attacked.

Here, then, there were from twenty to thirty cases of Cholera admitted in the course of a few days into the same apartment with from forty to fifty patients, suffering from other ailments, and yet not a single instance to countenance the notion of contagion occurred. It is hardly fair in European writers to disregard facts such as these, the authenticity of which is put beyond all question by the reports made at the time, corroborated by the testimony of almost every medical man in India. According to Mr Scott, "The most striking instances of immunity from the "disease, under the most intimate personal inter- "course, will be found recorded in the original re- "ports. In the hospital of the Royal Regiment on- "ly one individual out of one hundred and one at- "tendants was attacked with the disease. In that "of the 11th Native Regiment, at Vizianagram, as "recited by Mr M'Andrew, p. 33, not one was "seized, although their numbers would seem to have "been great. In the hospitals at Trichinopoly no "attendants were taken ill. Many medical officers "appear to have slept in their hospitals, without suf- "fering any bad consequences. At St Thomas' "Mount, where a general receiving hospital for pa- "tients with the Cholera was established, and where "the numerous attendants were people not at all ac- "customed to hospitals, not one of them was taken "ill; yet it was not uncommon to see them using

" the bed-clothes of patients who had just recovered " or died. The same observation applies to the nu- " merous receiving hospitals at Madras. Mr Acting- " Surgeon Gibson, on reporting on a late attack " (April 1823), experienced by the 69th Regiment " at Wallajabad, observes, I had ninety-two admis- " sions, and increased the establishment of servants " to double : I lived in the hospital, amidst the sick, " day and night, and yet neither I myself, nor any " of the servants, got the disease ; but the hospital- " serjeant's wife, living in a retired room, not near " any disease, had a severe attack *."

In Bengal, likewise, the same phenomena were observed. " At Hutta, again, a healthy town on the " banks of the Sonar, in Bundlekund, the epidemic " committed such ravages that the inhabitants fled, " and took refuge in the neighbouring villages ; and " so virulent was the poison, that three Sepoys and " seven camp-followers of the 2d Battalion 1st Re- " giment, were seized, merely on that corps march- " ing through the place. And yet, the disease ne- " ver appeared amongst a company of Sepoys, or their " followers, then in the fort, which was divided from " the town only by a broad street. What here served " to skreen these men from infection ? Certainly no " suspension of intercourse between the town and " fort ; for this always remained free ; much less su- " perior salubrity of situation, for the fort was small, " and crowded with buildings, and the town high and " open. Thus, too, whilst the disease raged in Sau- " gor, and in the lines of the 1st Battalion 26th Na- " tive Infantry, about a mile and a half distant, not

* Madras Report, p. 1.

" a case occurred in the fort in the centre of the " town, which was then garrisoned by 200 men of " the 2d Battalion 1st Regiment. In like manner, " in Kotah, three companies stationed in the fort es- " caped entirely, whilst 100 persons were daily pe- " rishing in the town. And at Muhadpore, where " the epidemic prevailed in the vicinity, and was " daily attacking a detachment of Bengal troops, con- " sisting of the 1st Battalion 6th Regiment Native " Infantry, two Rissalus of Skinner's Horse, and " 1500 camp-followers, it entirely spared a body of " 500 of Holkar's Reformed Horse, although the two " camps closely adjoined, and a man who had been " sent in from the Bengal Division, after getting the " disease, went through every period of it amongst " the healthy Mahrattas *."

In dealing with a disease, whose ascertained habits are so extraordinary, and where the facts arrayed on the one side can always be neutralized by facts no less conclusive on the other, it is in vain to talk of Zarcozelo and Peterhoff, or of Aleppo, or of the Foundling Hospital at St Petersburgh, or, in short, of any instance of the kind, as *proving* any thing either one way or other†. The test which I would

* Bengal Report, p. 136–7.

† I observe that a very able medical writer on this subject refers to the jails in India as illustrative of the same fact; but the immunity from attack enjoyed by certain of these prisons, proves nothing in favour of contagion. The prisoners in those jails are always employed during the day in outdoor work; there has never been any attempt at seclusion; and their guards, like our policemen, reside, when relieved, in their own houses in the villages, so that the immunity of prisoners, when it occurred, must be referred either to the caprice of the disease, or the locality of the jail.—*See the Edinburgh Medical and Surgical Journal for December* 1831.

apply is this. Is there any well authenticated case, in which, by means of contagion *alone*, the disease has been communicated to a town or district, in which there was not reason to expect its appearance, and therefore, when no unknown remote cause of the disease could have been present? Do the contagionists go the length of saying, that if a patient, labouring under Cholera, were brought from Newcastle, where the disease now is, to Edinburgh, where it has not yet appeared, that individual would cause the disease to break out in Edinburgh? Or, if this be said, is there any fact to support it? Assuredly I have met with none; for whether the disease can be barred out or not, nothing appears more certain than that, without some predisposing cause, atmospheric or terrestrial, the *virus*, whether apparently brought by persons or goods, never has manifested itself.

But if that be the fact, and if, as seems now to be admitted by the most distinguished professional writers on the subject, *something more* than mere contagion be necessary towards the propagation of the disease, where is the occasion for resorting to contagion in order to explain its progress? It is admitted that the disease does not require contagion for its propagation; that a particular state of the atmosphere or of the earth will cause it to break out spontaneously. This seems now, indeed, to be the approved doctrine of some of the contagionists themselves. Nay, they go farther, and admit that the remote cause must cooperate with the contagion, in order to give it efficacy. Such at least seems to be the result at which some late writers, of great European experience, have arrived, and, if so, I am really at a loss to perceive on what sound principle the element of contagion has

been superadded to a remote cause of the disease, which is admitted to act independently of contagion.

It may be imagined, indeed, that when the disease has established itself in a town, personal communication with the sick may excite it in a person who might otherwise have resisted its remote cause; but why should this hypothesis be resorted to, except on very sure grounds? The theory of contagion is practically a very inconvenient one, and it seems really unnecessary to *assume* that it co-operates with the hidden cause of the disease. Mere contagion, according to this notion, is insufficient; while it is admitted that the unknown cause will produce the disease without contagion; but if contagion, in absence of this malignant influence, be harmless, and if the pestilence can advance and spread independently of contagion, I can see no use in embarrassing ourselves with two exciting causes of the same disease; one of which is conceded not to be essential to its dissemination, and the other confessedly requires no such aid. But, on the contrary, I should rather say, that when the disease has once gained a footing it will run its course,—attacking all those who are susceptible of it within the limits to which it is confined; nor do I believe that a susceptible person would be in more danger from contact with a person labouring under the disease, than he is from exposure to the supposed unknown influence. Besides, on the hypothesis now under consideration, it is taken for granted, that, without the aid of a remote cause, a susceptible person will receive no injury from being placed in juxtaposition with a Cholera patient,—so that at least it is quite clear that a sick person himself is innocuous after the pes-

tilential miasm, or whatever else it may be, has exhausted itself in the particular town or district.

We come now to a series of facts collected by Drs Russell and Barry, for the purpose of shewing that the disease is directly communicable by contagion. One general observation applicable to the whole of this class of facts is, that the persons who are supposed to have been thus infected, were one and all of them within the limits, where at the time the remote cause of the disease seemed to be in activity. But, in an inquiry of this kind, it is proper to look in detail to the particular instances which Drs Russell and Barry have adduced. The first is that of the city prison at St Petersburgh, where it is said that a woman was attacked with the disease a few hours after her arrival, and that several others in the same apartment suffered. Her husband (whom she is supposed to have infected *before* she was herself attacked) after a short time was likewise seized, and several in the same apartment with him "were also taken ill. In short, of twenty-seven attacked (fifteen dead) there is but one to whom communication cannot be traced. He was confined for a capital offence, and had less liberty than the others. There were about 400 prisoners and attendants."—*Papers*, p. 45. Had these cases been insulated, and had the disease never broken out without communication between a patient and the person attacked, they might have served to support the theory of contagion; but, with reference to the numberless instances in which the disease has broken out, without any traceable contact, the passage just quoted proves no more than that the remote cause of the disease was in operation within this particular prison, and that those of the prisoners who

were susceptible were attacked; and the fact that the prisoner confined for a capital offence, and separated from the others, did not escape, is, so far as it goes, a very important confirmation of that supposition.

The next instance is that of Mr Baird's works. These are on the banks of the Neva, and are very extensive. "Three hundred and fifty men, and their families, the serfs or property of Mr B., live and work with him in the inclosure." They are well fed. Seventeen were attacked with the disease, seven died. "First case, on 19th June O. S., a man of 80; second case, on 20th, a man of 19; not the slightest connexion could be traced between them." Mr Baird's proceedings are creditable and i structive. He improved the dormitories, attended to the feeding of his people, and erected a temporary cholera hospital. "There were, besides the above, about 100 men employed in a sugar refinery on the premises, not one of whom was attacked, though exposed to great heat and unrestricted communication with the world as before the Cholera: these were free men. Three hundred and fifty other freemen live outside, and work within Mr B.'s establishment, in the various manufactories carried on by him. Of these seventeen were attacked with the Cholera, ten died."—*Papers*, p. 45.

If this instance prove any thing, I should rather say that it proves the disease *not* to be contagious, since otherwise it is difficult to account for its not having spread more extensively in Mr Baird's works; more particularly, as he does not appear to have been himself a contagionist, or to have taken any steps for preventing communication between the sick and the

healthy. The immediately following case in the papers is of the same description, and is an interesting example of the disease not crossing a river. It is that of the village of La Nouvelle Saratova, which is chiefly inhabited by Germans, with a population of about 1000.—" A servant belonging to one of the families who arrived on the 26th of June, O. S., complained of being cold, and being slightly indisposed that very day, but attended to her ordinary occupations until the 30th, when she was suddenly attacked with vomiting and purging towards evening. During the night violent cramps came on; she had but three motions, and vomited but twice in all. Hands and feet very cold and blue, lips blue; died in twenty hours from the first attack of purging. The above is the evidence of the woman of the house. The medical man who saw the sick maid about an hour before her death, stated that she had all the symptoms of Cholera. *The woman of the house, the mother of the maid, and others, were about her in her illness; none of them attacked.*"—*Papers*, p. 46. The bedding on which she lay and died was given to a beggar, and was afterwards carefully traced, *and found to have been used with impunity.* This case, therefore, so far from proving the disease to be contagious, strikingly illustrates the peculiarity to which I have already adverted, viz. that when a Cholera patient is carried beyond the tainted spot, the disease is not communicable to those about him.

We are next told that Cholera was carried to the village of Colpina by a party of healthy marines, who left St Petersburgh on the 21st of June and arrived at Colpina on the 23d. " They had been all examined before they marched, and brought a certificate of good

health. On the day of their arrival they were also examined by Dr Bowman, and found in good health." The first case occurred among one of these men on the 27th. "He had been drunk, and in company with the second case the day previous to his being taken ill; the woman, No. 3. had nursed No. 2. while at home, so that there appears a strong case of communication which cannot be traced farther. From a plan which I made out, marking the houses in which the disease occurred, it appears that some rows remained free of Cholera, but there is a sprinkling in most of the streets."—*General Wilson's Letter, Papers*, p. 35.

The simple account, then, of this outbreak of Cholera is, that it reached the village of Colpina in its progress over the face of the country; that it made its appearance suddenly in one spot, among a set of dissipated, ill fed, and ill clad people, three of whom appear to have been attacked within a few hours of each other; and that it then appeared simultaneously in other parts of the village, without any trace of communication between the sufferers and those originally attacked. Contagionists, such as Drs Russell and Barry, certainly involve themselves in strange contradictions and inconsistencies; for while, on some occasions, they carry the *virus* in the cordage and sails of a ship, and in others suppose it to remain dormant in the human constitution for periods varying from seven to thirty days; in others, such as that just mentioned, they make the contact and the development of the disease all but simultaneous. And my firm persuasion is, that all those reasoners who set out with a predetermination to account for every case of the disease on the theory of contagion, must unavoidably land themselves in similar difficulties.

Accordingly, the papers now under consideration are full of these inconsistencies. Thus the hypothesis of Drs Russell and Barry is, that the disease was carried from St Petersburgh to the villages around by the fugitive work people, who fled on the first appearance of the disease. But see how their facts desert them. They refer to the case of "a German colony about one verst and a half from Colpina, on the Moscow road, that is the Moscow road used since the establishment of quarantine at Peterhoff and Zarcozelo," containing about 200 inhabitants. No precautions against contagion were taken; "*and yet* "*not a case of Cholera occurred in this village, al-*" *though the disease prevailed all around.*"

Take then their next proof of the same fact, which is that of Alexandryski, containing nearly 4500 inhabitants, besides those in the villages and country houses near it. The first appearance of the disease here "was on the 23d of June, when a flax-dealer, who had arrived from Yaroslav *on the 8th of June*, and continued to enjoy good health till the above date (23d), was violently seized with Cholera, was sent to the military hospital in town, and died next day."—*General Wilson's Letter, Papers*, p. 56. Immediately after this the disease became general, but there is no attempt to shew that it spread from the flax-dealer; so that, in this instance, the only evidence of contagion is, that the first case of the disease occurred in a man "*who had reached the town* "*nearly three weeks before, and who had continued* "*until the day of his attack in perfect health;*" while the general prevalence of the disease immediately afterwards shews that it had a local, not a personal, origin.

Drs Barry and Russell are avowed contagionists, and they appeal to their communication to the British government, as containing the evidence on which they have formed their opinion. I believe it is now generally admitted, even by contagionists, that the facts brought forward by these physicians are hardly sufficient to justify the conclusions at which they have arrived; and most undoubtedly every second example in these papers is not only explainable on an opposite theory, but is actually the very fact which an anti-contagionist would most readily select in support of his views. Thus, having disposed of the report from St Petersburgh, look at what they say of Cronstadt. Here the disease made its appearance at a place called the Merchant's Mole, the first case having occurred on board a boat which had just arrived from St Petersburgh. The boat was ordered off to the distance of three or four versts from the fortress, where the crew, and a barber who had bled the man first attacked, were placed in quarantine. The patient died within twenty hours; "the other ten, six attendants at the hospital, the medical man in charge, and the barber, continued in close quarantine for five or six days, *i. e. until the town was generally infected, yet not one of them was taken ill.*"—*Papers*, p. 50. The next case occurred in the centre of the town, more than a quarter of a mile from the mole-head, "*not a trace of communication between him and the first case.*" This man also had arrived two days before from St Petersburgh, and the house in which he lay "was closed, put in quarantine, and continued strictly guarded for four days. *The surviving inmates were nine in number, four of whom had assisted the dying man, not one of whom was attacked.*"—*Papers*,

p. 51. In short, by a careful examination of what took place at Cronstadt, it will be found that here, as elsewhere, the disease commenced at a particular spot —that it was not carried thence to other points by the persons attacked and removed to a healthy quarter, but that, as usual, it gradually passed on, directed by its hidden source, until it had become general in the other parts of the fortress.

The School of Cadets at Cronstadt is also referred to, as illustrative of the advantages of quarantine regulations. It may be thought that the advance of the disease, in the face of all quarantine regulations, is the best practical answer to any single instance of supposed benefit from this species of restraint. But it is unnecessary to meet this particular instance by a mere general remark. The statement of Drs Russell and Barry is, that " the gates of the establishment were shut on the 19th of June, and as strict a quarantine as possible maintained till the 6th of August, O. S.—No case amongst the pupils, who were from 9 to 20 years of age. The general health of this establishment was rather better than usual during the epidemic.—Great praise is due to the officers of this establishment, and particularly to Dr Arden, for the very prudent sanitary and dietetic regulations recommended by him.—The discipline of this school was perfect."—*Papers*, p. 51.

Now, had no case of the disease occurred within this establishment, it might have been a proof, if not of the preventive effects of quarantine, certainly of the benefit to be derived from dietetic and sanitary regulations. The truth, however, is, that cases of Cholera *did occur here.* " *There were one or two cases among the servants, some of whom were una-*

voidably obliged to go out of bounds." But Drs Russell and Barry elsewhere inform us, that Cholera is not only communicable by contact with a sick person, but so subtile is the *virus*, and so diffusible, " that neither the near approach, nor the immediate contact, of an affected individual were indispensable to the infection of a healthy individual susceptible of the disease at the moment." So that even had communication been prevented between the sick servants and the pupils of the School of Cadets at Cronstadt, this highly diffusible contagion, on the principles of Drs Barry and Russell themselves, must have obtained a footing in the establishment, and necessarily must have been communicated to the pupils, unless it is meant to be said that not one of these young gentlemen was susceptible of the disease; and if that be the fact, their immunity was not due to quarantine.

With regard to the evidence afforded by the shipping in the roads at Cronstadt, it merely corroborates what had been again and again found to be the case in India in similar circumstances, viz. that the crews of vessels do not suffer until they come *within the tainted limits.* Thus we are told that the frigates Anna and Olga arrived at Cronstadt from Dantzic about the end of June, and were ordered into ten days' quarantine, during which time they had no sick; but that immediately on being admitted to Pratique, they had some cases of Cholera.—*Papers*, p. 53. Of the Venus frigate it is said, " This ship had been here twenty-three days before the epidemic. In consequence of her having sick, she had two short quarantines of five and three days, during which time the captain states that his ship was more healthy than when in free communication with the

shore and other ships."—*Ibidem*, p. 53. And the last instance is still more striking. "The Phœnix brig, Captain Baron Soenenthac, had been stationed for two months midway between Cronstadt and Peterhoff, about six versts or four miles from either, to the SE. of the former, for the purpose of telegraphic communication with his Majesty. She continued in this situation till the 15th of July in perfect health, *though she had sent her boat twice a-week of the fortress for provisions.* On the 15th, *she took up her station one verst to the SW. of the town, and had two men attacked with Cholera on the 17th. Neither of these men had been on shore.* The surgeon only had gone to Dr Lang's hospital on the 16th, for the purpose of observing the Cholera, which he had not seen before. The Doctor said to him, You had better not remain here long, else you may carry the disease to your ship, and I shall be responsible. Seven cases in all, and one death,—*not a man of the seven had been out of the ship*, and not one of those who had been on shore was attacked then or since."

To what, I would ask, do Drs Russell and Barry ascribe the attack of the two men mentioned in the last instance, as having been attacked on the 17th July? It is stated, that they were not on shore, while the only person who was on shore appears to have been the Doctor, who was not attacked. Is it meant, then, to be inferred, that the Doctor carried the contagion along with him on board the vessel, and communicated it to those men, and if so, in what shape did he carry it? In short, on the hypothesis of contagion, the attack on the crew of this vessel is wholly unaccountable; while on the other hand, it appears to me equally clear, that the movement on

the 15th of July, when the vessel left her former station, *and took a position one verst to the south-west of the town*, brought her within the limits of the disease, and fully explains the attack by which the vessel afterwards suffered.

Before leaving the subject of contagion, it is proper to say something of the supposed mortality among medical men, and hospital attendants, who may be exposed to Cholera. It is said that medical men have suffered more than others of their rank, and certainly, *a priori*, we should expect this to be the fact, for whatever may be the remote cause of the disease, it is admitted on all hands, that mental anxiety and bodily fatigue are powerfully predisposing causes; while at the same time both the medical men and the other attendants, as well as the hospital assistants, are much, if not constantly, exposed to the hidden cause of the disease, whether it be atmospherical or terrestrial. Instead, therefore, of looking upon the preponderance of deaths (if such be the fact) among medical men, as a proof of the contagious nature of the disease, it appears to me that the general immunity enjoyed by medical men affords very strong evidence the other way. I am persuaded, indeed, that the almost universal belief of Indian practitioners, that the disease is not contagious, rests in no inconsiderable degree on the conviction, that had the disease been communicable in this manner from the sick to the healthy, no medical man who did his duty could by possibility have escaped.

Looking to the Madras Presidency, the limited number of medical officers who have fallen victims to the disease is very remarkable *, and in Bengal the

* During the years from 1818 to 1822 inclusive, the numerical

result is even more surprising. In the latter Presidency during the first three years after the breaking out of the disease, only *three* medical officers out of an establishment of 300 were attacked, of whom only *one* died *. Much of this immunity I think may be ascribed to the circumstance of Cholera patients being almost invariably brought into hospitals at some distance from their own habitations, and, as I conceive, beyond the immediate and limited local influence of the disease. I cannot help thinking, indeed, that this point has not been sufficiently attended to, or rather that it has been entirely lost sight of by the speculators on contagion. Were a medical man or a nurse called upon to attend a patient labouring under ague in the Fens of Lincolnshire or among the Pontine marches, if either the nurse or the physician were attacked, the attack would be at once ascribed to marsh miasm. But if a nurse or a doctor

strength of the European troops in the Madras Presidency was kept up at 10,000 men. In the course of these five years 3664 cases of Cholera occurred in that force, of which 695 were fatal, that is at the rate per annum of 1 casualty from Cholera to every 72 men. During the same period, the Madras Medical Establishment was kept up at 200 commissioned officers, 33 of whom are known to have had attacks of Cholera, 13 of which were fatal, that is 1 in 77 per annum. See Madras Report. Under ordinary circumstances, the risk run by officers bears no proportion to the dangers arising from the habits of the European private soldiers in India, as predisposing causes to this disease. But medical officers, in addition to exposure to the remote cause, have the predisposing causes of fatigue and anxiety, and are moreover liable to be detached whenever the disease breaks out within reach of them.

* "From a medical list, consisting of between 250 and 300 "individuals, most of whom saw the disease largely, only three "persons were attacked, and one death only occurred."—*Bengal Report*, p. 129.

in attendance on a Cholera patient be attacked with the disease, no farther inquiry is made, nor does it ever occur to a contagionist to doubt that such an attack is proof positive of the existence of contagion. This appears to me to be not only illogical, but one of the strongest possible illustrations of the precipitate haste with which many of the writers on this subject have leapt to their conclusion *.

In Russia, according to the communications of Drs Russell and Barry, the mortality among medical attendants appears to have been considerably greater. It is said that 15 hospital physicians out of 264 were attacked during the first eighteen days of the prevalence of the disease, and that six hospital attendants (invalids) out of 40 had been attacked in one hos-

* Nothing can prove the disadvantages under which home-bred authors on Indian Cholera labour, more thoroughly than certain passages in the article in the Foreign Quarterly Review for October 1831 already alluded to. The writer of that article extracts the opinions of a few of the least experienced medical officers, as set forth in the Madras Report; and even these extracts prove nothing except that the attendants on the sick sometimes suffer. This reviewer also affects to regard the Bengal report as the individual work of Mr Jameson, the Secretary to the Medical Board, although he ought to have known that that report was drawn up by Mr Jameson, from the communications and reports on Cholera made to him by the whole medical officers of the Presidency who had seen the disease. And it is with all that information before him, that Mr Jameson, as the organ of the most distinguished Medical Board in India, reports that "the whole body of the medical officers in Bengal who have had an opportunity of seeing and remarking on the disease, without a a dissenting voice, concur in declaring that it is not contagious;" and in a foot-note, "*to this unanimity of conviction, there was "originally one exception, but from more extended experience, "that individual has since modified his opinion.*"—*Bengal Report*, p. 124.

pital. But at the same time, Drs Russell and Barry report, that the number of sick daily brought to the hospital was overwhelming, that there was a paucity of medical men, and that they were under a general alarm for their personal safety; so that, in addition to being within the influence of the remote and unknown cause of the disease, these medical men were subject to two of the most powerful predisposing causes, viz. fatigue and fear, the latter of which in that sense was almost unknown in India, although even in that country, I have seen one or two instances of its predisposing effects.

The hospitals also in which these deaths happened, were most probably within the limits in which the hidden source of the disease was most active. At p. 39 of the papers, Drs Russell and Barry observe, "It is certain, however, that in some Cholera hospitals, favourably circumstanced as to site, ventilation, and space, very few attendants have suffered." These hospitals again, according to my theory, will probably be found to have been placed beyond the tainted limits; for wherever hospitals were so placed in India, it was invariably found that the attendants on the sick were just as safe as their neighbours, who were not exposed to any communication with the sufferers. I have elsewhere referred to some remarkable illustrations of this fact in the hospitals under the charge of myself and others, and similar testimony will be borne by almost every medical man, who has had occasion to treat the disease in India on an extensive scale.

But it is not in India only that such immunity from attack was enjoyed by the hospital attendants; for we see that this occurred even at St Petersburgh; and the habits of the Indian and Russian

disease were in this respect identical in other quarters. Thus Dr Smirnov, a Staff-Physician, mentions that "during two months, while the disease prevailed at "Orenburg, and 299 patients were admitted with "Cholera into the military hospital, the personal at- "tendants on the sick remained entirely exempt from "the disease. They consisted of one hospital assistant, "six pupils, as many Baschkir lads, and fourteen hos- "pital servants,—in all 27; and their duties were to "perform bloodlettings, apply leeches, poultices, and "frictions, and administer baths and the like; so that "they were compelled to be constantly breathing the "exhalations from the bodies and clothes of the sick, "as well as to touch and handle them. Besides, the "same immunity was enjoyed by certain officers and "subalterns who lived in this hospital, and were in the "sick-wards once a-day, besides being at other times "in constant communication with the hospital-assist- "ant and the pupils. Farther, the washerwomen of "the hospital likewise escaped,—a class of individuals, "who, it is well known, are extremely apt to suffer "from contagious diseases. One only was attacked "with Cholera; but she washed only the clothes of "the officers, none of whom had been affected before "she took ill. Lastly, Dr Smirnov, and his colleague "Dr Sokolov, were in constant attendance on the pa- "tients affected with Cholera, but did not suffer *."

The results in England have been, generally speaking, similar. The disease has now prevailed in Sunderland and in Newcastle, and in that neighbourhood, for upwards of two months, during which time the medical men in these districts have fearlessly performed their duty, and have been aided by many volun-

* Edinburgh Medical and Surgical Journal, vol. xxxvi. p. 132.

teers from other parts of the country; yet, hitherto, no medical man has been attacked, and, so far as I can learn, two only of the nurses have suffered.

If such, then, have been the practical results, and if, even at the best, the supposed instances of injury to medical men and hospital attendants from personal communication with the sick, be otherwise explainable, I can imagine nothing more pernicious than the attempts which are made, even by medical writers, in this country to prove that the disease has been peculiarly severe among hospital attendants. Every hospital physician is aware of the serious importance, both to himself and to his patients, of having the subordinate hospital duties performed promptly, efficiently, and fearlessly. A high-minded gentleman, notwithstanding his belief in contagion, will boldly perform his professional duty; but it requires much more nerve than is usually possessed by persons in the rank of nurses and hospital servants, to enable them to brave death in such a shape, from a mere sense of duty. I have seen the bad effects on such persons of even mooting this point, and, for all practical purposes, I am sure that, even were the question of contagion less debatable than it is, it would be much better to drop the subject entirely, than thus to inflict injury on the patients, and to excite a degree of alarm among their attendants, much more likely to bring on the disease than the contagion which seems to be so much dreaded.

On the general question of contagion, the following, then, are the conclusions to which I have come, after a careful examination of all the facts falling under my own notice, or derived from the information of others.

I. With regard to the disease as it exhibited itself in India, I remain decidedly of the opinion, which I formed after much experience in that country, viz. that Cholera there was not contagious, or infectious, or in any way communicable by a sick to a healthy person; and although the evidence on which I found this opinion is necessarily subject to the objection to which all mere negative evidence is liable, the facts are so numerous and so irreconcileable with the opposite theory, that I conceive myself warranted in stating my conviction in terms as positive as any man can be justified in using on such a subject.

II. In Europe I have hitherto had little individual experience of the disease, and therefore hold myself bound to speak with more diffidence on the question of contagion, as applicable to this quarter of the world. But from the descriptions which I have read of the disease as it has manifested itself in Europe, and from personal communication with many medical men who have seen it, I am fully satisfied of the identity of the two diseases both in their characteristics and in their peculiar habits; and while I entertain the most sincere deference and respect for the high medical names ranked on the side of contagion, I nevertheless must say that I have met with no instance of the communication or spread of the disease which may not, in my apprehension, be satisfactorily accounted for on the supposition that it is not contagious. Indeed, while on the theory of non-contagion, the progress of the disease, and, in particular, its admitted outbreaks, without traceable contact between the sufferers and previously infected persons, may be explained or accounted for; it is, I think, impossible to reconcile the

phenomena with the notion that contagion is the only source of the disease *.

* The recent outbreak of the disease at Haddington is a remarkable instance of its arising spontaneously. The first case in that town occurred on the 17th December 1831. The man attacked resided in a filthy close, by the river side, and no contact or communication with any person coming from Newcastle or Sunderland, or from any other suspected quarter, could be traced: he was a man of dissipated habits, and he died of the disease on the 20th December. His case will be found in the Appendix. This case was immediately followed by several others, from the same locality, all on the immediate margin of the river. In connexion with this subject it is proper to notice a coincidence which, had it not been investigated at the time, might have given some countenance to the theory of contagion. It happened that three shoemakers, natives of Haddington, who had left Newcastle on the 14th December to walk to Haddington, arrived there on the 19th, *two days after the occurrence of the first case.* These shoemakers were in perfect health when they left Newcastle, and, since their arrival in Haddington, they have continued to enjoy good health. They had no communication with any of the sufferers from the disease; yet, as soon as it was discovered that these three individuals had arrived from Newcastle, it was generally reported that they had brought the contagion with them, and, until the matter was investigated, no doubt was entertained upon the subject. This is a notable example of the manner in which such reports arise; and I have little doubt that many similar reports, some of which have been admitted as authentic into works on this subject, have no better foundation. The truth is, that, if Cholera could have been carried by healthy persons coming from Newcastle and Sunderland, it must, by this time, have spread over half the towns both of England and Scotland; since the intercourse has never been interrupted, and many medical men in particular, have come direct from the bedsides of Cholera patients, to Edinburgh, and to other places, not on foot, and by a five days' journey, like the Haddington shoemakers, but as quickly as the public conveyances could carry them; and hitherto without any prejudicial effect either to themselves or their friends, or others who may have come in contact with them.

III. The idea entertained by some distinguished medical men, and promulgated in the report of the Edinburgh Board of Health, that "the disease may arise spontaneously from hidden causes, and that it may also become contagious in circumstances not yet ascertained," seems to me to imply an unnecessary combination of two exciting causes for the dissemination of a disease, the spread of which can be sufficiently accounted for by the former cause alone. That the disease may arise, and has arisen, spontaneously, I understand to be now the prevailing belief amongst the influential medical men of this country; and if so, and if at the same time it be conceded that there are no ascertained or intelligible *data* for holding the disease to be more contagious in the temperate zone than it was within the tropics, I am at a loss to discover what practical benefit we are to derive from this supposed new feature in its character. Hitherto, undoubtedly, the doctrine of contagion has done nothing but injury both to the trade of the country and to the sufferers from the disease; while, either as a means of arresting its progress through Europe, or of lessening its mortality, the practical application of the doctrine has been much worse than useless.

I have just had an opportunity, while these sheets are going through the press, of perusing Dr Becker's first letter to Dr Thomson of Edinburgh, on the Cholera in Prussia*. Dr Becker's high professional attainments are not unknown to many of the medical

* Letters on the Cholera in Prussia, by F. W. Becker, M.D., &c. &c., to John Thomson, M.D., F.R.S., &c. &c. 1832.

men in this country. But it is hardly possible to read his first letter, (which is the only one yet published) without perceiving that he feels himself greatly embarrassed, in his attempts to account for the spread of Cholera, by means of contagion alone. Many of the facts mentioned by him are indeed utterly irreconcileable with that hypothesis; and those which he has selected as best illustrative of his argument, belong exclusively to that class of cases which every contagionist can adduce, and which, as I have elsewhere remarked, may be explained very nearly as well on the theory of non-contagion, as on that of contagion. Without disparagement to Dr Becker, I may safely say, that every alternate fact mentioned by him, if not sufficient to prove that the disease is *non*-contagious, certainly proves that, under circumstances of the most intimate personal contact and juxtaposition, it was not so communicated.

Besides, it must be borne in mind, that long before Cholera had appeared in Berlin, and I rather believe before Dr Becker had seen a single case of it, he had publicly committed himself on the question of contagion; and, hence, that he approached the investigation with the insurmountable bias of a partisan. In his letter, moreover, as I understand him, he admits that he has seen but few cases of Cholera within the reach of his own practice; and that he found a large and influential class of his European professional brethren, who had been familiar with the disease, directly opposed to him on the question of contagion. In estimating the weight of Dr Becker's testimony, therefore, what we have chiefly to consider is, whether the speculations of an ingenious and talented physician, strongly prejudiced in favour of contagion, and who at best has had but two

months' actual experience of the disease, are sufficient to counterbalance the evidence, and the high medical authority, on the other side. Let it be farther observed, that Dr Becker confines himself almost entirely to Berlin, in which the disease never appears to have been very formidable, or very generally prevalent; while he overlooks, or at least makes no attempt to meet or explain, the phenomena of the disease, either in Asia or in any part of Europe except Prussia.

In other respects, Dr Becker's paper affords a valuable corroboration of many of those *traits* in the history of the Cholera, to which I have had occasion to advert in the preceding work. I refer in particular to what he says of the disease having almost invariably made its first appearance on low ground, and on the margins of rivers or canals. The extraordinary manner in which it broke through the Prussian sanitary cordons, as explained by Dr Becker, and the still more remarkable fact (which he has made any thing but a successful attempt to explain on the theory of contagion), that, during the prevalence of the disease in Berlin and elsewhere, chylopoetic derangements, and what he calls an unsatisfactory condition of the stomach and bowels, prevailed to a very great extent, are also important illustrations of the identity of the habits of the Asiatic and European diseases. Generally speaking, indeed, I consider Dr Becker's letter as a very useful addition to the information which we already possess concerning Cholera; and I look forward with great satisfaction to his second letter, in which we are led to expect an account of his mode of treatment, which, coming from such a quarter, will, I am sure, be duly appreciated by his professional brethren in this country.

SECTION V.

PREDISPOSING CAUSES OF THE DISEASE.

HAVING as yet found no reason to alter the opinions as to the non-contagious nature of Cholera, which I formed after much experience of the disease, and the most serious consideration; and having in the preceding section fully discussed the subject, it now becomes necessary to point out some of those collateral circumstances which have been found to influence the dissemination of the disease.

I. Whatever has a tendency to depress the general powers of life, may be regarded as an exciting cause of a disease which has its source in a failure of one of the principal powers of the system; such as fatigue, cold, hunger, a debauch, mental anxiety, depressing passions, &c.

II. Every thing likely to produce chylopoetic derangements—unwholesome food, unripe fruit, or indulgence to excess in fruit, or acescent vegetables—are to be dreaded during the prevalence of a disease which so immediately destroys the source of all the abdominal functions.

III. Whatever is likely to determine the fluids of the body to the alimentary canal, seems to act as an exciting cause of Cholera.

(1.) When Cholera prevails in a district, or on a line of country, through which it is necessary to march, it is of great consequence to avoid as much as possible all exposure to fatigue, long marches, harassing night watching, &c. Cold is also to be avoided, particularly while asleep. In a hot climate, it is extremely difficult to adhere to this rule; and the well known manner in which the disease frequently makes its attack in India, illustrates the fact. It often happens, that the heat of the early part of the night renders sleeping within doors impossible. The couch or cot, therefore, is placed where there is the best chance of obtaining a breath of air, viz. between open doors, in the verandah, or even in the open air; at last a delightful cool breeze springs up, and the wearied Indian drops asleep. Towards morning he awakes chilled, or perhaps has an alvine call; by and by he becomes sick, and by seven or eight o'clock he is found to be in the third stage of Cholera. No doubt many sleep, thus exposed, without catching the disease; but the frequency with which such exposure is followed by an attack renders it evident that it is dangerous, when superadded to the remote cause. Exposure to cool night winds, therefore, during the prevalence of Cholera, however agreeable, should be carefully avoided.

Moderately warm clothing, even within the tropics, is also of importance, with a view to the prevention of disease. I was in the habit, in the hottest weather, both by precept and example, of recommending the use of flannel next the skin *. It is the best

* I am informed that the commanding officer of one of the European regiments in India made each man sleep in a flannel

means of regulating the warmth of the body, and of preventing sudden and injurious chills. In our climate, attention to clothing becomes even more important than in India, particularly during our variable winter. Flannel next the skin, woollen clothes, and strong or double shoes, should be worn as generally as possible. Chamois leather waiscoat and drawers may also be used with great advantage.

It is unnecessary to enlarge on the depressing effects consequent on a debauch. All who have seen the disease, are aware how frequently an attack of it has succeeded intoxication. Indeed, the common excuse among the soldiery, for not applying for medical aid in time, is, that they thought they were only suffering from the effects of the excess of the previous evening.

The minds of all exposed to the remote cause of Cholera, should be as much supported as possible. In a particular manner it is necessary, that those in attendance on sick relations should be saved, if possible, from fatigue, that bodily lassitude may not be added to mental distress; and every attempt should be made to remove any alarm as to the contagious nature of the disease. It is a good rule, to avoid making the disease, when prevalent, a subject of conversation; and in camps, on such occasions, it is better to conduct funerals without the usual noisy ceremonial.

Nothing need be said of the injurious effects of hunger; but it may be observed, that it is dangerous, during the prevalence of Cholera, to march or take

bag! and this regiment, it is said, escaped the disease, while others near them suffered.

severe exercise before breakfast, with an empty stomach *.

Residents in tropical climates, when unavoidably exposed to rain, ought to take the first opportunity of changing their dress.

(2.) It is usual with the natives of India, to ascribe an attack of Cholera to their having taken an early meal of cold rice; and it has been already observed, that, during an epidemic visitation of Cholera, there is a general feeling of insecurity, it may be said, in the abdominal viscera. This is very likely, even if the result of miasma, to be increased by the helpless condition in which human beings feel themselves, when exposed to a danger which is beyond their comprehension and control †. The impression thus

* I call the attention of my professional brethren to this point. They are apt to be called out at all hours; and perhaps the best arrangement would be to have some coffee always ready.

† I may here insert the observations, on this subject, of a highly valued friend of mine, now no more, who had seen much of Cholera, having himself suffered from two attacks of it, and whose treatment of himself under the latter of these attacks, I shall have occasion to refer to in a subsequent part of this treatise. He says, "Indigestion, I think, is a prevailing complaint with every one whenever Cholera is common; and the greater the degree of the disordered state of the stomach that exists, the more likely is the person to be affected by the epidemic, and the less likely to recover. I think medical men, from the particular fatigue of mind or body that they undergo during the prevalence of an attack, are more liable to the disease than other persons of a similar rank of life."—*Private Letter from J. Cox, Esq. Assistant Surgeon, H. E. I. C. S.* I have already had occasion to refer to Dr Becker's letter on the disease as it appeared in Berlin, in which the same peculiarity is noted; and I may add, on the information of my friend Major Youngson, that on one occasion at Salem, in the Madras territory, not only were chylopoetic de-

made on those exposed to the disease, therefore, added to the character of the symptoms, renders it advisable that much attention should be paid to the digestive organs, when Cholera is prevalent. The aliment should be of easy digestion, generous and nutritive; those accustomed to live well, should not be stinted of any of their usual luxuries; long fasts and subsequent heavy meals should be avoided; and fruit, and all vegetable matter likely to produce acidity, to disagree with the stomach, or to derange the bowels, must be prohibited.

(3.) It is so much the fashion in these times, for individuals to dose themselves with medicine, that many are apt to resort, for the relief of the uncomfortable feelings which arise during the prevalence of Cholera, to some favourite purgative; and innumerable instances have occurred, in which the invasion of the disease seemed the result of an over-dose of medicine. I agree, therefore, with those who disapprove of powerful doses of medicine, while Cholera is prevalent; in particular, such cathartics as produce watery evacuations are to be avoided. At the same time, the bowels should be most carefully regulated, and if medicine be necessary, warm purgatives, in moderate doses, should be prescribed. It is probable that an opportune course of quinine, would assist the system in warding off the disease.

There is reason to believe, that those residing in well-built houses, and sleeping one or two storeys above the ground floor, are less liable to the disease than those who sleep on the ground, or on mud floors

rangements common, but during the time that Cholera prevailed, the whole community, native and European, suffered from ringing in the ears, or slight deafness.

little raised from the surface. As a preventive, therefore, let as much attention as possible be paid to comfortable condition of the dwellings of those exposed to Cholera *.

It appears, as before observed, that an attack of Cholera, is in Europe, very frequently ushered in by a diarrhœa of one or more days continuance. This should be made as generally known as possible, so that whether the looseness be incipient Cholera, or only a predisposing cause of the disease, it may be treated in time. Medical men, in particular, ought, in their own persons, to attend to such an indisposition during the prevalence of Cholera; for, whether the cause of that disease be terrestrial or atmospheric, or a human effluvium, they are necessarily exposed to it. With them, therefore, it should be a rule to abstain from duty, when they find their bowels disordered.

* I use the freedom to refer on this subject to a Letter which I some time ago published, addressed to Sir Henry Halford, Bart., which will be found in the Appendix.

SECTION VI.

TREATMENT OF THE DISEASE.

CHOLERA ASPHYXIA is a disease which leaves no time for hesitation or delay in its treatment; but, unfortunately, its prominent symptoms are far from indicating the means best adapted for its cure. In such circumstances, a correct notion of the nature of the disease, or experience, is the only foundation on which we can hope to establish a successful mode of treatment. The opinion which has been offered, in the previous pages, in explanation of the nature of Cholera, has this to recommend it, that it explains, to a certain extent, the line of practice which has been found most successful in overcoming the disease.

The following may be considered the indications of cure:—1. To relieve the oppressed circulation; 2. To stimulate the system; 3. To restore the heat of the body; 4. To guard against local congestion; 5. To keep down reaction; and, 6. To produce a healthy condition of the alimentary secretions.

1. Indian practitioners and medical writers have found it so difficult to account for the almost magical effects of venesection in Cholera, that they have puzzled themselves for explanations of the *modus medendi* of this remedy. One class are anxious to discover a stage of high action, which, as they assume, will be the proper time for bleeding. Others, anxious-

ly look for proofs of local inflammation, as an apology for removing blood from a patient in the last stage of general collapse. And a third set bleed, because, as they say, there is determination of blood to the abdominal viscera. It will be admitted, that not one of these reasons is satisfactory. Cholera, as I have endeavoured to shew, is not collapse consequent on high action. Inflammation is not conceived to be possible after the heart has ceased to circulate arterial blood, and when its action, if it have not ceased, is sunk into a mere flutter. And determination of blood to internal parts, is explained, I think, by the fact, that venous congestion becomes greater, where the circulation is freest, and where there are no valves to prevent the veins from being gorged by regurgitation. If indeed the discharge of serous fluid by capillary vessels is to be regarded as a proof of determination of blood to internal parts, the blood must also be determined to the surface, from which a similar fluid is simultaneously discharged. But it will hardly be received as a legitimate reason for removing blood from the surface, that it had been already withdrawn from it.

The effect of bloodletting in Cholera is obviously in a great degree mechanical. The power of circulation has been injured; it is no longer capable of duly distributing the mass of blood in the system; and the cessation of the usual secretions, the deficient pulmonary function, and the effect of the discharges, have rendered the blood wholly unfit for vital purposes. By bleeding in such circumstances, we relieve the gorged vessels, and thus enable the weakened energies of the circulating power to act on the disburdened organs of circulation, and to restore the current of the

blood. The lungs recover their function, pure blood is thrown into the left heart, the arteries are again filled with fluid fit to support life: this, it may be supposed, reacts on the sympathetic system, and by and by its energies are completely restored. In this way, only, can the effect of bloodletting in Cholera be explained. No other method will account for the almost instantaneous recovery which so often follows venesection in such a condition of the system as has been described—a recovery more immediate than that which follows the removal of mechanical pressure from the brain. And I should say very confidently, that, in no case in which it is possible to persevere in bloodletting, *until blood of a healthy colour and consistence flows freely from the vein*, will the patient die from collapse.

The value of this remedy is well stated by Mr Scott in his report already alluded to*. "Few remedies," he says, "on a fair trial, have been more generally and unequivocally advocated than free bloodletting; and the most that has been urged against it is, that it is not always successful. The advocates for bleeding proceed, however, on the principle, that a certain quantity of blood is to be obtained in order to ensure success, which few of them estimate at less than about thirty ounces. Those who are disposed either less favourably towards bleeding, or to condemn it altogether, object, that if the circulation is in a condition to admit of free bleeding, the case is a mild or favourable one, and would probably yield to other remedies. There is no doubt that fatal collapse has sometimes followed even *large* bleed-

* Madras Report, p. 58.

ings, which has staggered the faith of many practitioners in the general safety of the remedy; but, in the great majority of cases, it is after *small* bleedings that this has happened. There is the most ample evidence also, that cases, especially in Europeans, even under the most favourable appearances, will often, in spite of all internal and external remedies, go on to a fatal issue when bleeding is not practised."

Mr Scott proceeds to exemplify these facts, by extracts from the reports of those surgeons who have had most experience of the treatment of the disease; which are not only illustrative of the importance of the remedy, but prove the difficulty to which the usual doctrines on the subject of bleeding expose the practitioner, in his endeavours to explain the *rationale* of bloodletting in Cholera.

As Mr Scott has truly said, cases often occur in which all remedial means are completely abortive. The remote cause of the disease is apparently in such a state of concentration, or the idiosyncrasy of the patient is such, that he is at once struck with death. In the very outset of the disease, like an animal which has had a full dose of the Upas antiar, the circulation is found to have ceased; the heart no longer beats; and though the sensorial and respiratory powers remain, every effort to restore the pulse and bring back animal heat fails; vitality seems to leave the body gradually, but very quickly; the breathing becomes slower, and the looker-on has a painful consciousness that respiration is becoming more and more useless: Insensibility creeps on, and the transition to death can scarcely be marked. In such a case, every remedy is unavailing. If any blood can be removed, it is gru-

mous and black, and the veins in the immediate neighbourhood only of the operation are emptied. Stimulants produce no effect, and the application of even moderate warmth is not endurable, or perhaps the skin is quite insensible. Vesicatories do not act, and leeches can draw no blood. In these circumstances, it is evident, that the patient's fate is sealed. In watching such a case, however, there will sometimes be discovered an effort, as it were, on the part of the heart, to rally; it will be perceived to act, and the pulse will even begin to flutter; and it has happened, that, almost at the moment of death, there was an evolution of animal heat, so that the corpse became warmer than the patient himself had been, an hour before death. It is therefore the duty of the surgeon to watch such a case most anxiously: the lancet may yet avail; and, as has happened, "the blood was found unexpectedly to flow, the stream improved, and the *dead man* got up and walked away*."

In commencing the treatment of Cholera, then, no time is to be lost in endeavouring to bleed the patient. He should be laid in a warm bed; and the great object being to get blood to flow, it is important that the operation should be performed with as little fatigue to him as possible. He should be kept in the recumbent position; and, as answering the two next indications will be the means of forwarding this important object, immediate recourse should be had to stimulants—applying artificial heat, using friction, &c. The rule as to the extent to which the removal of blood should be carried, is very simple; it has no

* The words of the late Mr Surgeon J. Barton, in describing to me a remarkable recovery.

reference to the prostration of strength, or to the constitutional stamina of the patient. The power of circulation is unable to give motion to the volume of blood which has accumulated in the venous system, and which has been rendered, by the cessation of secretion, the diminished function of the lungs, and the disordered actions, incapable of supporting life; and blood is let, until it appears that so fatal a condition of the vital fluid has been changed. In other words, the blood ought to be allowed to flow, until the natural current in the veins has been restored, and there is evidence, in the improved colour of the blood, that the lungs have recovered their function. If opening one vein be not sufficient, let others be tried; and, until the disease yields, the practitioner must persevere in his endeavours to accomplish this great object. In the outset, perhaps only a few drops of tar-like blood can be obtained; by the use of internal and external stimulants, some good appears to be done, and the pulse rises;—renewed attempts should immediately be made to remove blood; nor should these be desisted from while life remains.

In addition to the lancet, leeches or cupping-glasses should be tried. It is to be remembered, that, added to the general venous congestion, there may be a tendency to morbid plethora in particular organs. There is sometimes confusion of head, and more obtuseness of the intellectual faculties than usual. This is not only a bad symptom with reference to the present condition of the patient, but may be attended with fatal effects, after the primary disease is relieved. In such cases, I should bleed from the jugular vein; and as many leeches as will adhere should be applied to the temples and throat. The abdomen is in every

case the principal seat of congestion; leeches ought therefore to be applied to the belly also, and particularly over the seat of the liver, care being taken to avoid, as much as possible, exposing the patient's body to cold. It is unfortunate, however, that, like blisters, leeches, during extreme collapse, are quite useless *.

2. To stimulate the system. Though classed as the *second* indication of the treatment of Cholera, yet, in their application, bloodletting and attempts to stimulate the system, ought to go hand in hand. The object is to restore the suspended circulation. Blood is let, in order to diminish the quantity of blood to be acted upon, and stimuli are administered, in order to excite the source of power. Indeed, it is often impossible to render bleeding efficient, until after stimu-

* I cannot too strongly inculcate the value of bloodletting, provided it be resorted to *fearlessly*, and with *faith* in its efficacy. Experience has shown that *small bleedings* may do harm, but free venesection, where practicable, has scarcely ever failed. The Indian practitioners, and some others, in the Edinburgh Board of Health, strongly impressed with the value of this remedy, endeavoured to render it popular, by suggesting that it should be recommended in the strongest terms by the Board to the public. But we failed in our attempt, and I was afterwards told, by an individual (not of the Board), that we were *conquered* by the other professional members of the Board. There was, however, no victory. The phlebotomists did not press the point, because they had no wish to force their practice on their professional brethren, who had what they conceived to be good objections to it. But so important do I consider bloodletting as a remedy, that I recommend that no time should be lost in resorting to it in every case in which, during the prevalence of Cholera, there is *a sunk countenance, and blue lips and nails*—the bleeding being persevered in *until the colour of the blood improves, and the patient feels relief;*—and, as was said in the first draft of the Edinburgh Board's Report, Let any one bleed who can perform the operation.

lants have produced some effect. It is not merely by internal remedies that this indication is to be answered : Frictions over the whole surface, rubefacients and sinapisms to different parts of the body, are in truth important portions of the stimulating remedies.

Every practitioner of any experience has his favourite prescriptions. The internal remedies which have been resorted to, with most confidence and success, in the treatment of Cholera, are, opium, æther, camphor, ammonia, peppermint, drogue-amer, spirits, and calomel.

The general mistake in the treatment of Cholera has been, the administering of too large doses of those medicines which have obtained a high character among practitioners. The large doses of calomel and opium, which many are in the habit of prescribing, are especially pernicious. This practice seems to arise from the nature of the disease being misunderstood. A patient is suffering from violent vomiting and purging, and excruciating spasms, and the indication appears to be to allay these high actions. Hence opium and calomel are prescribed in quantities, amounting to what are considered sedative doses. There can be little doubt that more injury than benefit has resulted from this practice ; for, should the disease be overcome, and the medicines not have been evacuated, 10 or 20 grains of opium, and 60 or 100 grains of calomel, must act as poison on the restored functions. The bad effects of the mistaken notions, as to the quantity of opium being not only safe, but useful, in the treatment of Cholera, have not been confined to those licensed to make such experiments on their fellow-creatures, but have been exemplified in the practice of those, who, having no medical man near,

were obliged themselves to prescribe for sick servants and others. It is well known that patients have died from the after effects of over-doses of the narcotics so administered. One gentleman, in particular, of my acquaintance, was rendered seriously unhappy, because a servant, suspected to be labouring under Cholera, to whom in a few hours he had given 500 drops of laudanum, died evidently from the effects of the medicine.

The object being then to stimulate the system, medicines should be prescribed in such quantities as are known to act as stimuli. In fever, a scruple of calomel, no doubt, will very often allay at once the irritability of the stomach; and experiments seem to prove, that in large doses this medicine acts sedatively on the gastro-enteric mucous membrane. It has been ascertained by experience, that most narcotics, in large doses, act as sedatives very soon after being administered; while it is unquestionable that the same medicines, in small quantities, excite the circulation, and are consequently to be regarded as stimulants. Early in my practice in Cholera I found reason to believe, that while there was positive good to be done by small quantities of these medicines, frequently repeated, their good effects were very questionable in the powerful doses which were commonly prescribed.

In the treatment of the disease, this is a point of so much importance, particularly as regards opium, that I think it necessary, in the present edition of this work, to add a few words on the subject. Opium is generally administered in Cholera as an anti-spasmodic; and, as this medicine does not act as a narcotic, so long as it has spasm to allay, it has been thought that the largest doses of opium are

safe during the collapse stage of Cholera. Now the spasms of Cholera are not tetanic, nor of cerebral origin, but evidently result from the condition of the circulation acting on the extreme branches of the nerves; and these spasms are immediately removed by the restoration of the balance of the circulation. When large doses of opium are administered to a patient labouring under tetanus, the function of the stomach being unimpaired, the drug is at once digested and absorbed; but as the cerebral nerves are the seat of the disease, the narcotic effects of the medicine appear to be rendered inert by the state of the nervous system, or are expended in allaying its irritability. In Cholera, however, there seems to be no reason for believing that the nervous system of the brain or spinal marrow is directly in a morbid condition. Opium, therefore, is not indicated as in tetanus; but even if it were so, the function of the stomach being unquestionably suspended, medicines are only partially digested and absorbed. Hence it happens, that, when the stomach no longer rejects what is swallowed, repeated doses of opium remain in it little acted upon; and when that organ recovers its function, if opium have been largely administered, there remains a dangerous, if not a poisonous, quantity of the narcotic to be digested and absorbed. There being now no spasms to neutralize its effects, the consequence is, that re-action is interrupted; the brain, instead of being able to assist the other powers of the system in re-establishing health, has its functions interfered with; congestion takes place in that important organ, and death ensues either from this cause, or from rapid typhus. It is, therefore, in my opinion, one of the most important points in the

treatment of Cholera, to avoid all over-doses of medicine; and, with regard to opium or laudanum in particular, extreme caution ought to be used in administering it, or in recommending it to the public; for both in Asia and in Europe, the most fatal effects have followed over-doses of this medicine.

The condition of the stomach in Cholera generally renders the exhibition of medicines in a liquid shape, in the outset of the treatment, wholly useless, as they are immediately rejected by the stomach or bowels. It is therefore advisable, at least while the vomiting is violent, to administer the remedies in the form of pills. A prescription will be found in the foot-note, which was not only given with great benefit to patients (adults), immediately on my being called to them, but which was distributed in large quantities, for the use of the sick, wherever Cholera was reported to exist in the districts over which my professional charge extended *.

Such pills should be washed down with a small quantity of brandy and water, and as soon as the stomach seems quieted, such a draught as is prescribed in the foot-note should be exhibited †.

* ℞. Calomel. gr. iii.; L. camphor. gr. ii.; Opii gr. ss. Mis. ft. Pil. One of these was given every half-hour, and sometimes oftener, while the urgent symptoms continued. The native assistants, in the districts, had orders not to persevere in this practice longer than three hours, if they had reason to believe that all the pills were retained.

† ℞. Æther. sulph. . . . . gt. xxx.
Tinct. opii, ... gt. x.
Misturæ camphor. . . . . ℥ i.
Mis. ft. Haust.

Draught to be taken immediately, and repeated every quarter or half hour, according to the urgency of the case.

It was at one time the general opinion, that it is dangerous to allow a patient, under the influence of Cholera, to have cold drinks. The prejudice against these (which is still very general), arose from the circumstance of vomiting and purging being very often aggravated, apparently, by the patient's attempting to allay his urgent thirst by drinking cold water. The distressing craving of the patient for cold drinks, and his almost hydrophobic horror of the hot fluids which were invariably offered him, induced me early in my practice to doubt the propriety of rigorously prohibiting the gratification of a desire which was in every case so strong.

Mr Annesley found by experiment, that tartaric acid dissolves the viscid matter which is so often found, after death, lining the enteric mucous membrane; and he latterly allowed his patients lemonade, a beverage which they found delightfully refreshing. He also prescribed a drink containing nitric acid, in the hope that while it was agreeable to the patient, it might communicate oxygen to the system. Whether we admit the importance of dissolving, while the disease exists, the fibrinous matter on which tartaric acid is here said to act, or imagine that oxygen can reach the system in the way Mr Annesley seems to expect, when it is no longer absorbed in the lungs, the practice of Mr Annesley is certainly a real comfort to the patient, and can be attended with no bad effects.

In the very outset of the treatment, friction over the whole body, and shampooing of the extremities, should be resorted to. Friction is best applied by means of hot flannel. It may be necessary, however, to use hot spirits of turpentine; and rubbing with the

flannel should be continued until the turpentine is rubbed dry. Bags of hot sand or salt should be applied to the extremities or other parts of the body; but this will often, while the extremity of collapse continues, prove insupportable to the patient.

Blisters during extreme collapse are quite inert, and the emplastrum lyttæ is too slow in its operation to prove serviceable. Mustard cataplasms are a more valuable remedy: these may be applied with advantage all over the abdomen, and to the calves of the legs, soles of the feet, and even to the chest. Nitric acid has been resorted to, in order to produce an immediate blister—it does not seem to have been attended with any marked benefit, and it is apt to produce an escar which may prove afterwards extremely troublesome. Boiling water is also recommended with the same view, and has apparently, in some practitioners' hands, been found useful. Partial topical applications, however, are not much to be depended on, and, as has been already observed, vesication cannot be produced during Cholera. The disease is a failure of an important *power* of the system, and the object in stimulating the skin is to assist the internal remedies in exciting this suspended power. But as yet there is no local affection to be removed by topical applications, although these may become very valuable in the after indications of cure. In the present stage of the treatment, therefore, an extensive application of sinapisms, or the general embrocation of spirits of turpentine alone, or combined with tincture of cantharides, or of oil and hartshorn, or any other powerful rubefacient, is to be regarded as the best means of assisting the internal stimulants in their operation.

3. To restore the heat of the body. The heat of the body can only be effectually brought back by re-establishing the circulation of the blood. At the same time, remedies, having in view the restoration of the deficient animal heat, have been considered by most practitioners an important part of the treatment; and therefore they are not unworthy of a separate consideration. No one of these three indications, however, although classed *seriatim*, should precede or wait upon the others. The treatment should be commenced with any one of the three which can be most easily answered, and the whole of them ought to be considered as belonging to the first branch of the treatment of the disease.

There is one very remarkable circumstance attending the worst description of Cholera, which has been already repeatedly alluded to, viz. the morbid sensibility of the patient's skin, and apparently of his stomach, to the presence of any thing raised above the temperature of the air. A warm bath, for instance, of a heat some degrees below the healthy temperature of the body, has been found so insupportable, that a patient, after being immersed in it for a few minutes, has sprung out of it, almost with spasmodic energy, and entreated that he might not again be exposed to so painful a remedy. A similar feeling is very commonly produced in the patient by swallowing liquids not more than lukewarm.

Notwithstanding, however, the instinctive dislike to artificial heat, exhibited by so many patients labouring under the disease, still it is advisable that attempts should be made to prevent a fatal reduction of the animal temperature. This is particularly necessary in a cold climate. The patient, therefore, immediately on his being attacked, ought to be placed in

warm blankets, and every endeavour should be used to keep his extremities warm, preventing as much as possible his throwing off the bed-clothes, or removing himself from the heated articles which are applied to him.

The restoration of animal heat has been reckoned so important, that it has led to various contrivances for applying warmth. The hot water bath was at one time much used; but the delay which generally attends the preparation of a bath, the fatigue of using it, and the exposure and fatigue in drying the patient, already alarmingly debilitated, has rendered it the general opinion, that the hot water bath is more injurious than beneficial. It has been found, that the principle which should regulate the application of heat to Cholera patients is, that it should be done with the least possible fatigue to them. With this view, various descriptions of vapour baths have been proposed. The simplest and most expeditious is that which has obtained the name of Dalton's Bath, which is simple enough. It is formed by putting vessels, containing burning spirits, under the cot or couch on which the patient is laid, while the cot is surrounded with blankets, by which means any degree of heat which may be required is obtained. Pots of ignited charcoal will answer the same purpose.

The objection to that method of applying heat is, that if bedding be used, the heat is long in penetrating it; and if the patient be laid on the rattans or on a mat, the heat is partial, and soon becomes insufferable to him. But there is one method of using heated spirit vapour, which is well calculated for applying heat immediately, and generally, to the body. An arched frame is made, rising a foot or eighteen inches, five feet long and two broad; the arched part of this frame is filled up with basket work, and one end

is closed up with a half circle of wood ; the lower part and upper end are open. This is placed over the naked body of the patient, and covered with blankets, the patient's head being left free ; the small and upper end of a curved copper chimney, eighteen inches or two feet long, is introduced through a hole, in the boarded part, the other end of the tube being placed over a cup containing spirits which is ignited. Heat to any degree may be thus introduced within the basket-work, and brought into contact with every part of the patient's body. Another advantage of this method is, that assistants may introduce their hands within the basket-work, and use frictions and embrocations, or apply leeches, without exposing the body of the patient to cold*.

When there are not such conveniences as these at hand, recourse must be had to hot blankets, bottles of hot water, bags of heated sand, &c. But unfortunately it often happens, that the best contrived methods are so painful to the patient, that they cannot be persisted in.

* This bath has been very extensively prepared in Edinburgh. I almost fear, indeed, that it is regarded as of more importance in the treatment of the disease than mere external heat, however applied, has, in my experience, ever proved. Much ingenuity, I find, has been exerted by some of my professional brethren, to improve the wicker-work bath made at the Edinburgh Blind Asylum. None of the alterations, however, which I have seen, are likely to prove of much use, with the exception of an obvious improvement suggested by my friend Dr Christison. He found, in an experiment made with the spirit vapour-bath, that while the parts of the body exposed to the heated air were brought to a high temperature, the parts in contact with the bed felt comparatively cold ;—he has, therefore, contrived a canvass frame-work, which, by raising the body two inches from the bed, admits of the heated air being introduced underneath the patient.

The general result of the steps which must be taken to overcome the disease, is, that venesection is to be immediately resorted to, and every endeavour made to get blood to flow, until the congestion is relieved, and until there is evidence that the function of the lungs and action of the heart are so far restored. For the same purpose stimulants are prescribed; and artificial heat is applied to assist the returning circulation, in raising the temperature of the body to the natural standard.

Such was the practice resorted to by myself and others, with great success in India, and, from all that I can learn, a similar course of treatment has proved equally beneficial in this country. Certain empirical remedies, however, have also found favour in England; and I cannot help fearing that the Central Board of London have recommended some of these on insufficient grounds. This is precisely what happened in India. Remedies which their proposers uniformly represented as specifics, were more than once recommended from authority, and had their day. I greatly fear that the salt-brine taken internally, and the actual cautery to the spine, are of the same description. With regard to all such remedies, it may be observed, that the public has no concern with them until experience has fully established their character, which, certainly, cannot be said of the two to which I have alluded. I am informed, however, that a mustard emetic has proved very serviceable in the north of England. Half an ounce of mustard is given in a cup of water; it produces full vomiting, and appears to stimulate the circulation so as to enable the surgeon to bleed. If such be the fact, it must be regarded as a stimulant, and I should certainly be

inclined to give it a fair trial. My objection, in short, is not to any remedy, the *rationale* of which is explainable, but to those pretended specifics to which accidental circumstances are apt, on all similar occasions, to give a temporary celebrity.

In all this nothing has been said of astringents, antispasmodics, &c. All who restrict their attention in the treatment of Cholera to the discharges and spasms, are in danger of losing sight of the true nature of the disease. They are prescribing for *symptoms*, which, it will generally be found, at once disappear, when the power of circulation is recovered. In fact, the removal of blood, to the necessary extent, has invariably, so far as my own experience goes, put an immediate stop not only to spasms and oppression, but to vomiting and purging, and has relieved the prostration of strength. And in no situation has the physician more reason to be proud of his art, than when, in the course of a few minutes, a patient, from the agonies of Cholera, and from the jaws of death, is placed in safety, and not only restored to a sensation of health, but to one of positive bliss*. The effect of bloodletting would indeed sometimes appear almost miraculous. A patient will be brought in on a cot, unable to move a limb, and, but that he can speak and breathe, having the character (both to touch and sight) of a corpse, yet will he, by free venesection alone, be rendered, in the course of half an hour, able to walk home with his friends.

The following case will illustrate this:—

When marching up the country with a detachment of troops in July 1819, a camp-follower was brought

* A very common expression on such an occasion is, "Oh, sir, I am in heaven!"

into my tent (9 o'clock P. M.) in a blanket. He was reported to have been suffering from symptoms of Cholera during the whole afternoon. His skin was cool, pulse weak and thready; had spasms in the extremities; urgent thirst. I immediately bled him, and with some difficulty obtained a full stream. When about twenty ounces of blood had been removed, the man felt complete relief. Some brandy and water was administered, and he returned with his friends to his own quarters. He was able to accompany the detachment next morning *.

There cannot be a more decided proof that this disease *becomes* one of pure congestion, and that the immediate cause of the remora in the circulation, whatever that may have been, no longer exists, than the almost instantaneous effect produced by removing a portion of the redundant, and, to say the least of it, *inert* venous blood. All know, that however successful bleeding may be, in relieving the more urgent symptoms of inflammation, complete restoration is not the immediate result; that inflammation is not a mere fulness of the vessels of the affected part, but that the arteries are in a morbid condition; and that time is required to restore them to their healthy action, not uncommonly the most difficult part of the treatment. And we are therefore fully warranted in concluding, that if Cholera consisted of a diseased condition of any portion of the arterial system, recovery could not be so instantaneous.

Breathing oxygen, or an air containing more of this gas than is contained in the atmosphere, has suggested itself to most practitioners; and it certainly is well

* Author's Case-book.

worthy of trial, though it is to be feared, that, so long as the lungs are unable to appropriate the usual quantity of oxygen from the atmosphere, little benefit can be hoped for from altering the proportion of that gas. I should also, if possible, resort to galvanism, throwing the galvanic stream through the abdomen.

But having arrived at the conclusion that the great object is to stimulate the sympathetic system of nerves, it is to be hoped that experience may enable us to attain this object more effectually than we have hitherto done.

Before proceeding to what (if the conclusions arrived at above, as to the nature of Cholera, be correct) may be considered the after treatment of the disease, it is proper to say a few words on the Prognosis; premising that, as yet, the disease will not have exceeded a few hours of illness.

Prognosis.—The most satisfactory symptoms, in the early stages of the treatment of Cholera, are, the return of the natural colour of the lips and nails, a rising of arterial action in the extremities, and recovered animal heat. The proof that the disease is overcome, is the restoration of the function of secretion, which is most satisfactorily evinced by the patient's making urine. Such decided symptoms of success may not immediately make their appearance, particularly if blood have been removed with timidity; but let it be remembered, that eight or ten ounces may in one case be sufficient, while in others depletion must be carried to the extent of forty or fifty ounces, before there shall be reason to believe that the overburdened vascular system is relieved.

It frequently happens, that when there is not sufficient confidence in bloodletting, the case is painfully

protracted; that for several hours the patient's life will seem hanging as if by a thread; the pulse at the wrist will repeatedly fail and return; oppression, deafness, vomiting, purging, and spasms will continue; the body will remain cold, and there will be every reason to dread a fatal termination. Of this the following case is an example:—

Dharwar, April 1820.

————, Assistant-Surgeon, Honourable East India Company's Service:—marching up the country in medical charge of a detachment of troops, which, previous to their arrival at this station, had suffered severely from Cholera at different stages in their progress. He had never seen the disease before; and the bad success of every method of treatment which he had pursued, made him view the disease with perfect horror. He was taken ill himself at two o'clock in the afternoon, having marched with the troops eleven miles in the morning. He took some antispasmodic medicines, and when I first saw him, about five o'clock P. M., collapse had made very small progress. Having unfortunately lost every case in which he had attempted to bleed, and being unable to reconcile his mind to the safety of losing blood in the alarming state of debility in which he found himself, he would not consent to be bled. While using every endeavour to overcome his repugnance to this important remedy, stimulants were administered, and the other means pursued which were considered likely to save the patient. The disease pursued its course, the spasms became excruciating, and the sensation of burning heat at the præcordia insufferable. There was almost incessant vomiting, and the purging was very troublesome. About midnight the young man

became hopeless; he was then very nearly in the last stage of the disease; and as persevering attempts continued to be made to obtain his consent to bloodletting, he at last said, nothing could make him worse, and that he would make no farther objection to any method of treatment that might be thought advisable. A vein was immediately opened, and with considerable difficulty blood was made to flow; the stream, however, improved, and, when about fourteen ounces had been removed, the blood was flowing in a full stream, though still black;—the patient, however, became alarmed, he withdrew his arm, and positively refused to allow another drop of blood to be taken from him. But his pulse had become more distinct, and as there was some cessation of the more violent symptoms, it was hoped that, with the assistance of stimulants and warmth, he might yet do well. The relief, however, was not permanent; the spasms, vomiting, and purging, became again severe, and continued more or less so throughout the night, and up to the forenoon of the following day; when the patient was in an alarmingly low state, his body was cold, and his pulse was scarcely perceptible. About two o'clock P. M., twenty-four hours after the accession of the disease, he seemed completely exhausted, all the violent symptoms had for some time ceased, and he fell into a doze, which had the appearance of the torpor which sometimes, in this disease, immediately precedes death. By five o'clock, however, a decided change for the better had taken place, and symptoms of restored action became apparent. Towards night he was out of danger, but it was some time before his health was re-established.

This case illustrates the nature of Cholera:—In

its very commencement, the patient was alarmed and immediately prescribed for himself, and had he consented to venesection at the early stage at which he was seen, the case would have proved a very manageable one. When the patient at last allowed himself to be bled, after ten hours' illness, it was within a very little of being too late, as the blood was found nearly stagnant, and it was only by kneading the arm that the first few ounces were procured. The opinion I formed at the time, and experience has confirmed it, was, that had the vein not been closed until the colour of the blood had been restored, little else would have been necessary to render the patient fit to return to his duty in a day or two. There cannot be a doubt, however, that the bleeding, such as it was, saved the patient's life, although, unquestionably, he owed much to a strong constitution.

In contrast to this young surgeon's case, a portion of a letter is subjoined from an experienced medical friend, in reply to some observations of mine, addressed to him after he had recovered from a second attack of Cholera: — "I agree perfectly with you regarding bleeding (in Cholera), and I have no hesitation in saying, that, if it were possible to take every case of Cholera in sufficient time, so as to produce syncope by bleeding *, the disease would not appear so formidable; but the sad misfortune is, that no one, on the first symptoms (which are so alike to fear), will admit that the disease is affecting him. In the last attack, of the first few cases,

* It is extremely difficult to produce syncope by bleeding in Cholera—the principle which I have laid down in the text, to draw blood until its character improves, and the energy of the circulation is restored, is both a better and a safer rule.

eight I think, every one died; which so completely puzzled me, that I was at a loss what to attempt. I had tried bleeding, and every remedy I had heard of, even to milk of magnesia and the vapour-bath,—and to no one could I give the slightest preference: my patients sunk so rapidly after coming into hospital, that I was almost induced to think the disease had assumed a new character. I suspected that I had been deceived as to the period of the invasion of the disease in those cases; and the most positive regimental orders were issued regarding an early application for aid, by individuals who might have the least reason to suspect themselves attacked. I was soon enabled to resume my old and favourite remedy, the lancet, and with such success, that the Mussulmans even admitted the successful practice of bleeding. I myself was seized about three o'clock in the morning, after having undergone for some days a great deal of fatigue and watching,—and the very moment I passed a watery evacuation and vomited, I bled myself to twenty-four or twenty-six ounces, which brought on syncope; blistered the pit of the stomach with cantharides and turpentine, and took the pill, which I always give, of ten grains of calomel, three of opium, and three of camphor. I used no stimulants of brandy or wine internally, but repeated the calomel, ten grains every three or four hours during the day; and took castor-oil at night, which brought away stools tinged with black bile. Drs Hastie and Edwards came over about five o'clock in the morning, and continued with me the greater part of the day. The turpentine and cantharides acted most violently,—it was rubbed in as is done in blistering horses, which

produced an immediate effect, but was dreadfully painful afterwards *."

These sketches of the cases of two medical men, not only illustrate the effect of treatment, but will, so far as they go, account for the various opinions which are entertained, as to the proper practice in Cholera. In the first, an intelligent young man is left entirely to his own resources, in medical charge of troops: while on a march they are attacked with Cholera, and he thus sees the disease for the first time, at a period when it is impossible to pursue investigations as to its nature, and when his practice must be guided by what he has heard or read. As almost constantly happens on the first breaking out of the disease, most of his cases prove fatal: bleeding having proved unavailing, he has become impressed with the belief, that this operation should not be resorted to in Cholera; and when attacked himself with the disease, by his obstinate resistance to venesection, he brings his life into the greatest jeopardy, and suffers a protracted illness. The second is the instance of a surgeon, who, having had much experience of Cholera, had learned justly to appreciate the value of bloodletting. But even he is staggered in his confidence in his "old and favourite remedy," by its uselessness in the first cases. Having fortunately, however, discovered, that the cause of this is not in the inefficiency of the practice, but in the delay of the application for relief, proper steps are taken to prevent this fatal loss of time, and he is enabled to re-esta-

* Letter from J. Cox, Esq. Assistant-Surgeon Hon. East India Company's Service, to the Author, dated Camp at Kalladgee, 29th July 1821.

blish the character of venesection; and, when attacked by the disease, by immediately resorting to this remedy, renders himself fit for duty in twenty-four hours.

But to return to the Prognosis. The restored circulation, return of animal heat, and re-establishment of secretion, are to be regarded as proofs that the patient is safe. When, on the contrary, collapse continues, and arterial action is no where perceptible; when the temperature of the body becomes more and more death-like; and, above all, when a profuse cold and clammy perspiration breaks out over the whole surface, accompanied with jactitation, there is little reason to hope for a favourable change.

It sometimes happens that, notwithstanding many apparently favourable symptoms, the case suddenly terminates fatally. This fact is illustrated by a case from Sunderland. "Mr B., draper, aged 57, in respectable circumstances, had been complaining for some days of slight bowel complaint, and was not worse yesterday than usual; left his house about three o'clock, and was brought home at six, having been seized with the usual symptoms of Cholera about five. Medical aid was immediately procured, and he was bled to ℔iss. Had neither vomited nor purged since he came home. Drs Barry, Daun, and myself, were taken down to see him about nine o'clock: his pulse then 68; skin generally warm; voice favourable, but the skin of the fingers livid and shrunk; had got an anodyne from Dr Ogilvie, and had respired a bladder-full of oxygen gas. He had no pain at our visit, but seemed restless, and was tossing about in the bed, and his eyes and countenance had rather an unfavourable cadaverous expression. However, as we had all seen

worse cases recover, our prognosis was favourable. A sinapism was applied to the præcordia; warm frictions and heat ordered to be continued. I went this morning between seven and eight to inquire for him, and found he had died at five o'clock. Now, I must tell you, this man was a person of broken down constitution and dissipated habits."—*Extract of Letter from Dr Gibson to the Author, dated Sunderland, 23d November* 1831.

This certainly appears to have been a case of the disease, although some of the striking symptoms were absent. But the patient, both from age and previous habits, was a bad subject for an attack; and in so far as I am able to judge from the above account of the case, I should say that bleeding was not carried far enough; for I should never be satisfied with a case in which, after venesection, the nails remained blue, the skin livid, the fingers shrunk, and where the patient had jactitation.

It has been found that the more violent the prominent symptoms are, the more likely is a cure to be effected; and that when the disease is attended with rapid collapse, little or no vomiting and purging, and no spasm, the prognosis is very unfavourable. But it may be again observed, that, even in the most hopeless case, it is the duty of the surgeon to continue to watch his patient to the last, as instances have occurred in which recovery has taken place, when, to every appearance, the patient was *in articulo mortis*.

4. To guard against local congestion. Most men, either from original organization or accident, have some weaker point in their constitution, which is liable to manifest itself when the system receives any serious shock. Thus, in a London hospital, death

following a great operation, frequently does not appear to result from the local injury; the wound may seem to be going on most favourably, when the patient is seized with low inflammation of the chest, and "he dies of disease of the lungs." So also in tropical climates, a serious injury may not destroy a person by its immediate effects: the fractured limb, or broken head, appears in a fair way to do well; but the patient is attacked with hepatitis, or dysentery, and carried off. These facts illustrate the necessity, in cases of great constitutional derangement, of extending our attention beyond the mere original disease. In Cholera, this is particularly necessary. When the primary disease is relieved, it is the duty of the practitioner to look most carefully to the condition of the head, of the lungs, of the liver, and of the bowels. The great risk appears to be, that a local congestion shall follow the general one. This, from the nature of the circulation in the liver and lungs, is perhaps more likely to happen in these organs than in the head. But should congestion take place in any of those great organs, active measures must be immediately resorted to. Leeches, blisters, and mercurial purgatives, will be requisite; and instances occur in which, from the condition of the hepatic system, it is necessary to administer mercury until it produce salivation.

The affections of the head, after an attack of Cholera, are generally rather of the nature of reaction than of congestion. A mixed and obscure case is occasionally met with, in which there are both conditions of the vessels of the brain;—an instance of this is given at page 77. But, unquestionably, pure congestion sometimes takes place within the head, which is to be

overcome by free topical depletion and active purgatives.

The following case, which was one of those I saw at Haddington, illustrates the danger arising from congestion of the brain after collapse is removed. —— Macguish, ætatis 28, a woman of the town, and a confirmed drunkard, of a spare habit of body, was taken ill early in the morning of 29th December 1831. When first seen by a medical man she had no pulse at the wrist; her countenance was livid and sunk; she had severe vomiting and purging of a fluid resembling meal and water; tongue white; much thirst; cramps in the lower extremities.

Hab$^{t.}$ Tinct. Opii, Sp$^{t.}$ Æther. Sulph.
a. a. G$^{t.}$ xl. in haust.

Sinapisms were applied to the belly, and external heat resorted to. She immediately vomited the laudanum and æther, and the same dose was repeated in about an ounce of brandy.

10 *o'clock*, A. M.—Again vomited the draught. Another dose of laudanum was also vomited. A pill containing two grains of opium was now given and retained. The spirit vapour-bath was used for half an hour, but as it seemed to cause much oppression of the chest its use was discontinued.

11 *o'clock*, A. M.—The sinapism was removed from the abdomen to the feet; the vomiting has ceased; the other symptoms remain much the same.

℞ Calomel. G. x. Opii G$^{t.}$ ii.
F$^{t.}$ Pil. stat. sum.

Half-past 11, A. M.—Sound asleep.

2 *o'Clock*, P. M.—Skin warmer; pulse 96, fluttering; made a small quantity of turbid urine; respira-

tion easy; 24 in a minute; still much thirst; had some soup given her, and warm beer to drink.

Half-past 6, P. M.—Pulse 96, and strong; no vomiting; feels quite well; has had one hour's sleep; skin warm and moist.

Rept^{r}· Mist. Antispasm.

30th December, 9 *o'Clock*, A. M.—Has slept four hours during the night; had three doses of mixture; still has thirst; skin natural heat; pulse 90; vomited once during the night.

Noon.—The above was the detailed account of this case given me by the medical man who attended the patient. When Dr Meikle and I saw her for the first time at noon on the 30th December, she seemed to be dosing, and on being roused sat up in bed, and thought she seemed to understand what was said to her, but was unable to express herself distinctly. Her face was full and unnatural in its appearance, her eyes prominent, and she *had become blind;* her pulse was small and frequent; skin natural heat; there was incontinence of urine, and altogether the woman was in a very unsatisfactory condition. I advised leeches to be immediately applied to the head, and recommended the free exhibition of mercurial purgatives. But I could not help remarking to Dr Meikle that I had no expectation of her recovering. We left Haddington very soon afterwards, but I learnt on the following day that this woman died on the 31st; and her death, I have no doubt, was occasioned by congestion of the brain. She ought, I think, to have been purged smartly on the evening of the 29th, by which time all the symptoms of collapse had apparently ceased; and I have never seen any advantage

from continuing narcotic remedies after reaction has commenced.

When the lungs, and consequently the heart, have their functions deranged after an attack of Cholera, it is generally difficult to discover what is going on: The patient does not recover as usual; he is troubled with depression and debility; his skin is ill-coloured, and perhaps his pulse is irregular. In such circumstances, leeches should be applied to the chest, and a blister over the seat of the heart, and alteratives should be prescribed. As it may be advisable to reach the source of respiration, a blister should be applied to the nape of the neck.

The tendency to local congestion may not be immediately disclosed; particularly when it takes place in the liver. This arises from the obscure nature of the symptoms of derangement in the substance of the liver. It is consequently always advisable to pay the utmost attention to the hepatic system, after recovery from Cholera: many owe permanent loss of health, and much future suffering, to neglect of this great venous organ, while convalescent from Cholera Asphyxia.

5. To guard against reaction. It is remarkable that, in India, reaction should so seldom have taken place after attacks of Cholera. When symptoms of this condition did occur, they were generally partial, and seemed rather to be attributable to irregularity in the balance of the circulation, than to pure reaction; so that even in one and the same organ, there were deranged arterial action and venous congestion.

In India, a patient on being relieved from the symptoms of Cholera, usually falls into a sound and refreshing sleep, and on awaking, has the feeling of

being quite well. There may be still, however, tinnitus aurium, and a slight feeling of confusion of head, and giddiness. These symptoms, no doubt, very often arise from the effects of the powerful narcotic remedies which have been used, and will most likely be removed by freely acting on the bowels. But if there be superadded pain, intolerance of light, a contracted pupil, and a hard pulse, immediate attention must be paid to the head. Local depletion in these cases is perhaps better than general bloodletting. Sixteen or twenty ounces of blood should be drawn from the temples by leeches or cupping-glasses, and active mercurial purgatives should be exhibited. If by these means the symptoms be not immediately relieved, the head must be shaved, cold applied, with blisters to the occiput and nape of the neck; and should the pulse continue hard, general depletion may be advisable, though topical bleeding, when it is possible to carry it to the necessary extent, is as effectual, and safer in cases in which removal of blood has already taken place to a considerable extent. It is unnecessary to enlarge farther on the treatment of such a case, as it can no longer be considered to belong to the subject of this treatise.

Such was my opinion, as the result of my Indian experience of Cholera; and I still think, that, when we have got rid of the state of *asphyxia*, and restored the circulation, we have overcome the disease, properly so called. But in Europe it has been found very generally, that the collapse stage is succeeded by a fever of a typhoid character, which protracts the recovery, and frequently proves fatal. This fever, therefore, demands our particular attention; and, in the outset, I may observe, that, in this country, the

usual treatment of fever appears to have been injured by the doctrine of critical days. It is said that fever must be allowed to take its course. I do not think so. On the contrary, I have seen the greatest benefit result from doing the utmost to cut short fevers of all kinds. In addition, therefore, to constantly regulating the bowels and the skin through the circulation, I have, in the course of my practice both in the East and in Europe, commenced quinine at an earlier stage than it is usually thought of in this country, and in most instances with very happy effects.

In reference to the same subject, I consider it by no means impossible, that the rapid recoveries from Cholera which took place in India, were attributable in no inconsiderable degree to the very decided steps taken by the Indian practitioners. Thus, in that country a patient labouring under Cholera was not only freely bled, but had large doses of calomel administered, followed by active purgation. Dr Mackintosh, of Edinburgh, I believe, has the credit of haing called the attention of the public to the good effects of bloodletting in the cold stage of fever. Before I was aware that such was his practice, I had come to a conclusion nearly similar, for although I have not found this remedy invariably necessary in ague, I look upon it as of great value in relieving the system from a load of unhealthy blood, likely to prove injurious when reaction is established.

On the same principle, I am inclined to regard full venesection, and the free use of mercurial cathartics, during, and immediately after, the collapse in Cholera, as the most likely means of saving the patient from the subsequent fever. In the treatment of this fever, therefore, I should continue to act freely

on the bowels, and lose no time in prescribing quinine and camphor, giving such a pill as the following every two or every three hours :

Sulph. quinæ,
Camphoræ aa. gr. ss. M. ft. pil.

And I should watch for local congestion, resorting to local depletion, in the manner already described, in case that appeared to be necessary.

6. To produce a healthy condition of the bowels. This is an indication which must in every case be attended to ; for, however short the cessation of secretion may have been, and however easily the symptoms of Cholera may have been removed, the restored functions seem to pour into the intestinal canal colluvies, which must not be allowed for a moment to lurk in it.

Calomel is a medicine of the greatest value, when the urgent symptoms have been removed ; particularly if, as sometimes happens, watery alvine discharges continue. As soon as the circulation is restored, ten grains or a scruple of calomel should be exhibited, and followed up, after the patient has had a few hours of rest, with a large dose of castor oil ; a draught of the infusion of senna, tincture of jalap, and tartrate of potasss ; or by a dose of compound powder of jalap. It is, however, advisable not to risk bringing back the irritability of the stomach ; and when there is any fear of this, it is better to give the cathartics in the form of pills *.

Glysters, large and purgative, are also very valu-

* The following formula has been found a valuable one after Cholera :—℞. Extract. colocynth. gr. x. calomel. ext. hyoscyam. a. a. gr. v. Divide in pilulas iv.—et sint pro dosi.

able in cases in which the stomach is still irritable; and they may also be used in order to assist the purgatives taken by the mouth, when slow in their operation.

The object at this period of the treatment is, to procure *fœculent* discharges, and to have evidence that bile is not only flowing freely into the intestines, but that its secretion is restored to its healthy condition; and the practitioner must be satisfied that the kidneys have recovered their function. As soon as there is reason to believe that these important secretions are restored, the patient may be pronounced perfectly safe, although it is always advisable to strengthen him by tonics, diet and regimen; and, as a relapse is to be much dreaded, the patient must not be allowed to be led, by his own feelings of recovery, to undue exertion of strength, or unnecessary exposure to fatigue, cold, or any of the exciting causes of disease.

It sometimes happens, that without being able to trace the symptoms to local congestion or reaction, it is extremely difficult to restore the functions of the stomach and bowels to a healthy condition; and the patient continues to suffer cramps in the extremities, even for many days after the urgent symptoms of Cholera have been removed. In all cases of this kind it will be found, that it is not only the alimentary secretions that are at fault, but that the kidneys have not recovered their healthy function; and the thirst of the patient will shew, that the salivary and gastric secretions are still deficient. These cases may be explained by the supposition, that the sympathetic system has been more permanently injured

than is usual. The treatment in such circumstances should consist in persevering in active purgatives, prescribing a course of alteratives, and freely exhibiting quinine; and it may even be advisable to produce slight ptyalism.

APPENDIX.

APPENDIX.

The following Case from the Russian Reports on Cholera, exhibits the identity throughout, of the Russian and Indian types of the Disease *.

"The disease began at two in the morning, with a dreadful purging, which returned every minute. Although the weather was cold and wet, the patient went out of doors to obey the calls of nature, barefooted and undrest, and without any precaution. About five o'clock he was without feeling, quite powerless, and affected with constant cramps. At six I found him again sensible, but with sunken pale-blue cheeks, dimness of the eyes, coldness of the feet and hands, and bedewed with clammy sweat. He was tossing about, and complaining of trembling of the hands, a sense of oppression at the pit of the stomach, and intolerable thirst. The vomiting, which, according to his own account, commenced much later than the purging, was at this time less frequent than it had been; but the alvine discharges continued to recur incessantly, and were passed involuntarily. The exhausted powerless condition of the patient, in particular, his completely imperceptible pulse, both at the wrist and over the heart, the stiffness of the limbs, the coldness of the tongue, belly, and præcordia, left me no hope of his recovery. The administration of

* Edinburgh Med. and Surg. Journal, No. cviii. 121.

opium with oil of peppermint and ether checked the vomiting only for a short time; anodyne clysters had no better effect on the diarrhœa; and warm frictions, spirituous drinks, and even the hot bath, were resorted to without success to restore the temperature and bring back the pulse. An unsuccessful attempt was in the last place made to draw blood from a vein; and soon afterwards the man expired. Twenty minutes after his last breath, and when the corpse had been already washed and dressed, it was affected all at once with frightful movements. Convulsive motions took place in the hands and feet, like those excited by galvanism, commencing first in a few muscular fibres, especially in the neck and thighs, extending in a vermicular manner, and suddenly producing bending of the head, and agitation and elevation of the feet. These spasms continued with intervals for ten minutes, becoming in the end faint and rare. The same phenomena, though in a less remarkable degree, were observed on another occasion only, but so long as six or seven hours after the termination of the symptoms of the disease *."—*Russian Reports*, p. 115.

INDIAN CASES.

Case in which the Disease appeared to be overcome by the use of Stimulants.

Fort Dharwar, 19th January 1820. Cool season. Range of thermometer from 50° to 75° in shade.

Papdoo —Sepoy, C company 2d battalion 4th Regiment Native Infantry. Admitted into hospital from the lines, (from whence he had walked nearly a mile with assistance), at half-past eight o'clock, A. M. Has had frequent vomiting and purging since gun-fire this morning. His eyes

* "These muscular contractions after death were also occasionally observed in the Cholera of the East Indies."—*Reviewer.*

and countenance sunk; pulse very small and quick; skin cold; much thirst; no spasms.

Habt. stat. submur. hydrarg. gr. x. opii gr. ij. in pil. et spt. vini ℥ij. aq. fervent. ℥ij. in haust.

9 A. M.—His pulse had left him; but he now seems revived by the medicine. His pulse has risen, and his skin is improved.—Cont. brandy and water.

10 A. M.—He has vomited since last report, and his pulse is weaker.

℞. Tinct. opii, spt. æther sulph. a. a. ℥ss. aquæ fervent. ℥i. Mis. ft. haust. stat. sumend, et rept. omni semihora.—Cont. brandy and water.

11 A. M.—His countenance is completely sunk; pulse not perceptible; no spasms; urgent thirst.—Cont. ut supra.

Noon.—After taking a little cold brandy and water he fell into a dose. Skin improved.—Cont. ut supra.

2 o'clock, P. M.—In the same state.—Cont.

4 o'clock, P. M.—No better.—Cont. ut supra.

6 o'clock, P. M.—Voice stronger; pulse is again perceptible; skin warmer.—Cont. brandy and water.

9 P. M.—Pulse easily felt; skin improving.

Habt. submur. hydrarg. gr. x. stat.

20th.—Vomited twice during the night, and had three watery yellow-coloured stools, but slept well; no pain or uneasiness; pulse rather frequent; skin natural; tongue foul; thirst.

Habt. pulv. jalap. comp. ℥i. stat.

Vesp.—Stools still watery.—Rept. submur. hydrarg. gr. x.

21st.—Has had one yellowish feculent stool; pulse small and quick; tongue white; thirst.

Rept. pulv. jalap. ℥i.

22d.—Purged three times yesterday; stools unnatural; pulse small and quick; skin hot; tongue white and parched; thirst.

Rept. pulv. purg. u. a.

23d.—Freely purged yesterday; stools improved in

character; pulse and skin natural; tongue clean; still thirst.—Nourishing diet.

Vesp.—Complains of having severe pain in the chest; breathing oppressed; pulse quick; skin cool.

Imp. empl. lyttæ, p. d. ℞ Calomel. gr. viii. pulv. ant. gr. vi. in pulv. h s. s.

24th.—The blister has risen well; breathing relieved; pain gone; tongue, skin, and pulse nearly natural; bowels free. He gradually recovered, and was discharged well on the 31st of the month *.

Had this man been bled immediately on coming into hospital, he most likely would not have fallen into such imminent danger, and he would have been saved ten days' sickness.

Tanjore, 15th May 1826.—8 o'clock A. M.

——, a native of Tanjore, a middle-aged spare-made man, was disturbed two or three times during the night by calls to stool; since sunrise has had vomiting; slept in the verandah of his house; skin cold and damp; countenance distressed and much sunk; pulse quick and fluttering; lips and nails blue; has cramps in the hands and feet. There is considerable oppression of breathing, and restlessness; urgent thirst; forehead bathed in sweat.

Cholera was prevalent in the district, and within the fort of Tanjore, two miles off; the mortality from the disease was reported to amount to 15 a-day.

Immediately bled to twenty ounces, when the stream had become free, and the patient felt the greatest relief.

℞. Calomel. gr. v, camphor. gr. iii, opii gr. i, ol. menth. pip. gt. v., in pilul. stat. sum.

Apply hot sand in bags to his extremities, and frictions to the whole body; and let him have brandy and water for drink.

9 o'clock A. M.—Pulse steady; countenance improved; heat of the body improving; no return of vomiting; has had one of the peculiar congee-like stools.

* Journal of Cases.

Habt. spt. æther. sulph. ℥ ss. tinct. opii gt. xv. spt. menth. pp^{tæ} gt. x. mist. camphoræ ℥ i. Fiat haust. stat. sum.—Cont. brandy and water.

10 o'clock A. M.—Much the same.—Cont. ut supra.—Noon. Is dozing and quiet; feels quite easy; pulse steady; skin warmer.

Omit. haust. cont. brandy and water, p. r. n.

1 o'clock P. M.—Continues to improve; omit brandy and water.—To have congee (rice gruel) for drink.

Habt. calomel. gr. x. ext. hyoscyam. gr. v. Mis ft. pil. stat. sum.

6 o'clock P. M.—Has slept since last report.

℞. Ol. ricini ℥ i. tinct. opii gt. x. aquæ cinnamom. ℥ i. M. ft. haust. stat sum.

10 o'clock P. M.—Has had some dark-coloured stools, and has made water; feels well, and says he is hungry.—To have mulligatany and rice, and to be kept warm during the night.

16th.—Passed a pretty good night; several dark brown and green stools; complains of nothing but weakness; pulse and skin natural.

℞. Ext. colocynth. gr. xii., hyoscyam. gr. vi. calomel. gr. iv. Mis. ft. pil. iv.—Sign. two immediately, and two in three hours.

Vesp.—Has had several stools, which were latterly feculent. To have nourishing diet. Rept. pil. ut supra, cras mane sumend.

17th.—Convalescent. 20th.—Discharged well.

I consider it so important to establish the character and value of venesection in the treatment of Cholera, that I will take the liberty of adding the opinion of some of the Indian practitioners, whose practice has been most successful in overcoming the disease.

Mr Assissant-Surgeon J. Cox says, "On the evening of the 7th instant (September 1818), I was called to a poor man in the village (Bagracottah, S. Mahratta country), about 6 P. M.: he had been labouring under the disease

since the early part of the morning, and was extremely weak when I saw him, and much affected with spasms of the extremities and muscles of the abdomen; skin cold and clammy, pulse very slow and indistinct. Under such symptoms, I must acknowledge I was a sceptic on the propriety of bloodletting, but was anxious to try its effects in the first case that might present itself. I immediately gave the man 40 drops of laudanum in an ounce of warm Holland-gin, and applied warm chatties of water to the feet, and friction over every part of the body, and opened two veins in the arm, and with great difficulty got 12 ounces of blood away. I was happy to see the man did not sink under the operation, but, on the contrary, appeared revived; and towards the end the blood came away in a stream, and more florid in appearance. On opening the veins, it appeared blacker than venous blood in general, and in a half coagulated state; so much so, that it was only by frequent pressure upwards on the veins from the wrist, that I could procure any stream at all. I remained with him an hour after the bleeding, and was pleased at seeing him no worse than when I first saw him; indeed he appeared more tranquil from spasms, and the pulse if any thing better: the frictions were continued. I now gave him 15 grains of calomel, and washed it down by the draughts of laudanum and gin, and left my dresser with him to superintend the frictions, and to repeat the draughts, should he vomit the former ones up. At 9 o'clock, nearly three hours after the bleeding, he had not been purged or sick once; entirely relieved from spasms; every part of the body felt warm, and the pulse had returned distinctly to the wrist, though faint. The friction was ordered to be continued, and some warm soup was made similar to mulligatany with pepper, and given him to drink in small quantities during the night. On the morning of the 8th, the report was favourable. Had slept a part of the night, or rather towards morning; had no spasms, and felt debility only; pulse increased; skin dry and warm. An ounce of ol. ricini was given him at eight o'clock, which in the course of the day produced three stools—the first bi-

lious, of a dark green colour, and the others more natural. I ordered him to continue the warm soup as his diet, and to take four grains of calomel at bed-time, which was all that was done in this case. The man came to me a few evenings ago quite well, and said he had pursued his usual labours for several days past."—Madras Report, pp. 102, 103. *

Mr Staff-surgeon Trotter says, (Camp near Gudduck, S. Mahratta country, 1st May 1819), " The cases were in general unattended with spasms or vomiting. The patient sinks immediately after one or two watery stools. In these cases, hot spirits with spices, and the application of heat, proved of most use. The purging was always stopped in a short time by the anodyne injections. So great a repugnance had the natives to bleeding, that it was a considerable time before I could prevail on them to adopt it, and not until several fatal cases had happened. The first man on which it was tried, was looked upon by his friends as being completely out of the reach of medicine. They were therefore highly surprised at the good effects of it, especially when, out of the next ten that were bled, not a case terminated fatally. A few were bled at an advanced state of the disease, but it appeared to hasten their dissolution."—Madras Report, pp. 108, 109.

Mr Superintending-surgeon Duncan says, (Bellary, 25th September 1818),—" P. S. I have just seen five men of the 84th regiment brought into hospital with the spasmodic Cholera. All of them were immediately bled, and a cordial draught given them of aromatic tincture of ammonia in camphor mixture : four of them are now asleep, with the pulse full and equal.—The fifth man is in a restless doubtful state, having been ill for some hours. No more than fourteen ounces of blood could be taken from both arms, dark and thick. His skin cold ; his face sunk and livid ;

* This is the narrative of the first trial of venesection in Cholera, by the gentleman whose account of his own case is given at p. 161.

the pulse not perceptible; and he has the appearance of a person labouring under epilepsy, apoplexy, or strangulation. His restlessness prevented the application of leeches. Cordial mixture, with tinct. opii, was frequently given to induce sleep, which is the first symptom of recovery."—Madras Report, p. 110.

Mr Surgeon Owen says, (Bellary, 13th October 1818), —" The two first cases were slight, and yielded easily to laudanum and calomel. But we had soon to observe, that with Europeans these medicines failed, though the disease was taken at the commencement, and the utmost attention paid to the treatment of it. The appearances, on dissection, have been so uniform in their tendency, to prove the great necessity of relieving the circulating system, and preventing that great congestion which has been in every case so apparent, that bloodletting became with me a primary object of attention. I have therefore used the lancet with some degree of freedom. And where the disease would admit of a sufficient quantity of blood being abstracted, I have found it of the most essential service in relieving spasm, and the irritability of the stomach. But how far it can be used with safety, in weak and debilitated constitutions, and old age, still remains doubtful. Yet in these even, if taken with *caution*, and in *sufficient quantity in the first instance*, it will be found a valuable auxiliary.

" I have taken twenty ounces of blood from the arm of a delicate native woman with the happiest effect, where the spasms were very violent; nothing more being required afterwards than a dose of calomel and rhubarb to open her bowels.

" You will observe in the case of Matrosstute, that thirty ounces of blood were taken before the spasms abated: although they afterwards returned with considerable violence, yet his pulse immediately rose, and became soft and full, and the heat returned to the surface. This, I am confident, would not have taken place, had not the circula-

ting system been relieved in the first instance. Nor do I think the same effect would have been produced if this quantity of blood had been taken at two or three times.

" I am happy to say, that *I have not lost one patient who has been bled, or at least when the blood would flow in a moderate degree, so as to relieve the most urgent symptoms.*"—*Ibidem*, p. 122.

In contrast to this strong evidence in favour of bloodletting in Cholera, I shall introduce a short paragraph from Mr Assistant-surgeon Hunter's report, published in the above work; which will also illustrate what I formerly said as to the fact of its having become, from the want of fixed principle in the treatment of this disease, a common opinion that medical men know nothing of its nature or cure. Mr Hunter says, " In one case there was a considerable determination of blood to the head, and suffusion of the eyes; *the temporal artery* was opened, and about six ounces of blood abstracted. The practice appeared pernicious from the patient sinking shortly afterwards *."—*Ibid.* p. 139.

Assistant-surgeon M'Lean, of his Majesty's 53d Regiment, says, (Trichinopoly, November 1818),—" The propriety of bleeding was strongly impressed on my mind, from observing the turgid state of the vessels of the several viscera of the thorax and abdomen, as demonstrated by dissections, together with the advice contained in the Medical Board's circular to superintending surgeons. Of fifty-six cases of spasmodic Cholera, who were bled to fainting, only one died; *small bleedings are prejudicial, inducing debility, without relieving the system;* and I am induced to say as my opinion, that a second bleeding does no good, if

* This is an example of the disadvantages under which a practitioner labours, from incorrect notions of the *rationale* of bloodletting in Cholera. Nothing else was to be expected from an attempt to bleed from an *artery*.— *Vide supra*, pp. 20, 21.

protracted beyond six or eight hours after the first; *we should therefore bleed largely, or not at all.*"—*Ibid.* p. 138.

Mr Assistant-surgeon Mather says, (Cochin, January 1819), " Bleeding was employed with success in one case, in a very advanced stage of the disease. The patient, who was no more than eighteen years of age, complained of giddiness, with a sense of weight in the head; he was restless, and breathed with difficulty; he had been twice on the steaming cot, but the pulse remained undistinguishable, and the skin continued cold. I succeeded in drawing about ℥ xii of blood from the arm; for no pulsation could be felt in any of the branches of the temporal artery, and I did not consider this a case in which I could with propriety open the jugular vein. I was surprised to find the blood had that bluish appearance, indicating the formation of buff; but after coagulation the surface had the tenacity of sizy blood—the colour instead of being buffy was a darkish grey or ash, and exhibited an appearance I do not recollect having ever before witnessed. The weight in the head was somewhat relieved by bleeding; the pulse was just perceptible, though still restless; sleep took place at short intervals; the natural heat was gradually restored, and at length uninterrupted repose succeeded."—*Ibid.* p. 151.

I shall conclude these extracts with a portion of what Mr Annesley says on the question of venesection in Cholera: " That cases have occurred, wherein eighteen or twenty ounces of blood have been taken away, and the patient died, cannot be doubted; but in all these cases it will be found, that bleeding ceased after the vessels had been emptied, whether one or twenty ounces had been abstracted. On the other hand, it will likewise be found, that *if the blood flows freely, till the colour changes from black to red, the patient in general will recover.* When we observe, therefore, the thick black carbonated blood change to a natural colour, and consequently to a thinner consist-

ence, we may rest satisfied that the disease is under our control.

" I saw a very striking instance of this fact in the case of a young lady at Madras, who was attacked with Cholera. The symptoms were decidedly marked, and the usual remedies were applied. The pulse was fluttering and seemed oppressed. I ordered a vein to be opened, and gave particular direction to the gentleman who was in attendance with me, and in whose observation and judgment I had the fullest confidence (Dr Archer), to let the blood flow till its colour changed from black to red, without reference to quantity. At first it came away only in drops, and was exceedingly thick and black. Warm flannels were used to the arm, and in a few minutes the blood began to flow more freely, but still in a very languid stream. After three or four ounces had been taken, the colour changed; it flowed with perfect freedom. The pulse got up, became more regular and soft, and although not more than eight ounces were taken, the patient recovered rapidly.

" Although I recommend bleeding to be attempted at all times, and in every stage of the disease, I am fully aware that many cases have recovered where it has not been used at all, nor do I answer for its universal success; but I do venture to assert, that *if it can be accomplished* in the early stage of the disease, and before the circulation has ceased at the wrist, *in nine cases out of ten it will prove successful*, especially if the colour of the blood change from black to red, if the pulse get up, and the spasms be relieved."—*Annesley on the Diseases of India*, pp. 169, 170.

He, in the next page, strengthens these opinions by a reference to the successful practice of Mr College, surgeon to the General Harris East Indiaman. This gentleman, " whenever he observed any of the crew depressed and low-spirited, at once inquired into their feelings, and, without a moment's hesitation, took twenty or thirty ounces of blood from the arm, gave a scruple of calomel and two grains of opium, and sent them on shore, well wrapt up, to the General Hospital under my charge; and the subsequent

disease showed what might have been expected from less energetic measures."—*Ibid.* p. 170.

Upwards of fifty men were thus treated, and every one of them recovered, so as to be able to join their ship before she left Madras Roads.

The following case proves the importance of fixing our attention on the great pathognomic symptom of Cholera Asphyxia, viz. the suspension of the power of the circulation of the blood; as, but for the absence of that character, the case bore the strictest resemblance to this formidable disease. It is one of several cases nearly equally violent, which I had to treat during last autumn; and it should be studied by those gentlemen who, in their anxiety for the arrival of Cholera, are alarming the public with reports of cases, which they, in ignorance of the true disease, consider to be of the nature of the epidemic.

"*Edinburgh*, 19*th August* 1831.

"Mr G. J. B., æt. 18, was attacked early this morning with frequent calls to stool, attended with much griping, and which were soon followed by severe vomiting. I saw him about 11 o'clock A. M., and found him in a state of great suffering, with severe griping, and constant alvine calls. The discharge from the rectum was squirted out as in Cholera Asphyxia, and consisted of *slightly muddy fluid with flocculent matter floating* in it (the '*congee water*' discharge of the Indian Cholera). He had severe retching, but at this time brought up nothing but frothy mucus. Pulse quick and sharp; skin warm; countenance natural; tongue extremely white. I administered a pill containing ten grains of the submuriate of mercury, which was almost immediately vomited; I then gave the same quantity of calomel with one grain of opium, in a pill; this also was immediately rejected: after a little I gave two grains of opium; this was likewise soon vomited.

Noon.—He was now attacked with spasms in the extremities; continuing to have stools of the same nature, large and frequent, excruciating griping, great irritability of the stomach; thirst; pulse quick, skin warm, colour natural. I covered the abdomen with a mustard cataplasm, and administered a glyster, containing two drachms of laudanum in a quart of arrow-root. This was retained (until next morning); griping and cramps were gradually relieved, and, at 2 o'clock P. M., the patient fell asleep.

Vesp. No return of griping or purging; stomach still slightly irritable; has hiccup; *has passed no urine to-day.*

℞ Ext. colocynth. gr. x. Calomel. Ext. hyoscyam. a. a. gr. v. M. ft. Pil. iv. ij h. s. s. et ii c. m. s.

20th.—A good night; bowels opened, pulse good, tongue still white, kidneys acting.

Vesp. Rept. pil. cathartic. iv. ut heri sumend.

21st.—Quite well.

Here, then, was a case in which there were spasms, total suppression of bile and urine for twelve or sixteen hours, and violent vomiting and purging; and there can be little doubt that any one who had never seen a case of Cholera Asphyxia must have reported this as an instance of the disease. I confess that, had Cholera existed in Edinburgh at the time, I should have bled my patient, and considered it a case likely to terminate in collapse. As it was, had the symptoms been longer unmitigated, I should have resorted to venesection.

The progress of opinion in this country, during the last few months, regarding the contagious nature of Cholera, is curious and instructive, and is so well exhibited in the proceedings of the London Boards of Health, that I think it right to insert here the several regulations which have been successively promulgated from authority.

FROM THE LONDON GAZETTE, Oct. 2.

At the Council Chamber, Whitehall, the 20th day of October 1831,

By a Committee of the Lords of His Majesty's Most Honourable Privy Council,

Their Lordships this day took into consideration certain Rules and Regulations proposed by the Board of Health, for the purpose of preventing the introduction and spreading of the disease called Cholera Morbus in the United Kingdom, together with an account of the symptoms and treatment of the said disease; and were pleased to order that the same be circulated in all the principal ports, creeks, and other stations of the said United Kingdom, with a view that all persons may be made acquainted therewith, and conform themselves thereto.

W. S. Bathurst.

The measures of external precaution for preventing the introduction of the Cholera Morbus by a rigorous quarantine, have hitherto been found effectual; but as the disease approaches the neighbouring shores, not only is the necessity of increased vigilance more apparent, but it is also consistent with common prudence that the country should be prepared to meet the possible contingency of so dreadful a calamity. The intention of the following observations, therefore, is to submit to the public such suggestions as it appears to the Board of Health should either be immediately acted upon, or so far carried into operation, as that, in any case, the country should not be found uninformed as to the best means of providing for its internal protection.

1. To effect the prevention of the introduction of the disorder, the most active co-operation, not only of the local authorities along the coast in the measures of the Government, but likewise the exercise of the utmost caution by all

the inhabitants of such parts of the country, becomes indispensably necessary. The quarantine regulations established by the Government are sufficient, it is confidently hoped, to prevent the disorder from being communicated through any intercourse with the Continent in the regular channel of trade or passage; but they cannot guard against its introduction by means of the secret and surreptitious intercourse which is known to exist between the coast of England and the opposite shores.

By such means this fatal disorder, in spite of all quarantine regulations, and of the utmost vigilance on the part of the Government, might be introduced into the United Kingdom; and it is clear that this danger can only be obviated by the most strenuous efforts on the part of all persons of any influence, to put a stop to such practices; the utmost exertions should be used to effect this end. The Magistrates, the Clergy, and all persons resident on the coast, it is hoped, will endeavour to impress upon the population of their different districts (and particularly of the retired villages along the sea-shore), the danger to which they expose themselves by engaging in illicit intercourse with persons coming from the Continent; and should appeal to their fears in warning them of the imminent risk which they incur by holding any communication with smugglers, and others who may evade the quarantine regulations.

2. To meet the other objects adverted to in the introduction, namely, to prepare for the possible contingency of the country being visited by this disorder, as well as to assist in its prevention, it is recommended that in every town and village, commencing with those on the coast, there should be established a local Board of Health, to consist of the chief and other magistrates, the clergyman of the parish, two or more physicians or medical practitioners, and three or more of the principal inhabitants; and one of the medical members should be appointed to correspond with the Board of Health in London.

Every large town should be divided into districts, having a district committee of two or three members, one of whom

should be of the medical profession, to watch over its health, and to give the earliest information to the Board of Health in the town, whose instructions they will carry into effect.

3. As the most effectual means of preventing the spreading of any pestilence, has always been found to be the immediate separation of the sick from the healthy, it is of the utmost importance that the very first cases of Cholera which may appear, should be made known as early as possible; concealment of the sick would not only endanger the safety of the public; but (as success in the treatment of the Cholera has been found mainly to depend on medical assistance having been given in the earliest stage of the disease), would likewise deprive the patient of his best chance of recovery.

4. To carry into effect the separation of the sick from the healthy, it would be very expedient that one or more houses should be kept in view in each town or its neighbourhood, as places to which every case of the disease, as soon as detected, might be removed, provided the family of the affected person consent to such removal; and, in case of a refusal, a conspicuous mark ("*Sick*,") should be placed in front of the house, to warn persons that it is in quarantine; and even when persons with the disease shall have been removed, and the house shall have been purified, the word ("*Caution*") should be substituted, as denoting suspicion of the disease, and the inhabitants of such house should not be at liberty to move out or communicate with other persons, until by the authority of the local board the mark shall have been removed.

In some towns it may be found possible to appropriate a public hospital to this purpose, or should any barrack exist in the neighbourhood, it might, under the authority of the Commander of the Forces, be similarly applied.

5. Wherever it may be allowed to remove the sick from their own habitations to the previously selected and detached buildings, the houses from which they have been so removed, as well as the houses in which the sick have chosen to

remain, should be thoroughly purified in the following manner :—

Decayed articles, such as rags, cordage, papers, old clothes, hangings, should be burnt ; filth of every description removed, clothing and furniture should be submitted to copious effusions of water, and boiled in a strong ley ; drains and privies thoroughly cleansed by streams of water and chloride of lime ; ablution of wood works should be performed by a strong ley of soap and water ; the walls of the house, from the cellar to the garret, should be hot lime-washed, all loose and decayed pieces of plastering should be removed.

Free and continued admission of fresh air to all parts of the house and furniture should be enjoined for at least a week.

It is impossible to impress too strongly the necessity of extreme cleanliness and free ventilation, they are points of the very greatest importance, whether in the houses of the sick, or generally as a measure of precaution.

6. It is recommended that those who may fall victims to this formidable disease, should be buried in a detached ground in the vicinity of the house that may have been selected for the reception of Cholera patients. By this regulation it is intended to confine, as much as possible, every source of infection to one spot ; on the same principle, all persons who may be employed in the removal of the sick from their own houses, as well as those who may attend upon Cholera patients in the capacity of nurses, should live apart from the rest of the community.

It should here be observed, that the fewer the number of persons employed in these duties the better, as then the chance of spreading the infection by their means will be diminished.

7. Wherever objections arise to the removal of the sick from the healthy, or other causes exist to render such a step not advisable, the same prospect of success in extinguishing the seeds of the pestilence cannot be expected.

Much, however, may be done, even in these difficult

circumstances, by following the same principles of prudence, and by avoiding all unnecessary communication with the public out of doors; all articles of food, or other necessaries required by the family, should be placed in front of the house, and received by one of the inhabitants of the house, after the person delivering them shall have retired.

8. Until the time during which the contagion of Cholera lies dormant in the human frame has been more accurately ascertained, it will be necessary, for the sake of perfect security, that convalescents from the disease, and those who have had any communication with them, should be kept under observation for a period of not less than twenty days.

The occupiers of each house, where the disease may occur, or be supposed to have occurred, are enjoined to report the fact immediately to the local Board of Health in the town where they reside, in order that the professional members of each Board may immediately visit, report, and, if permitted to do so, cause the patient to be removed to the place allotted for the sick.

In every town, the name and residence of each of the members of the district committee should be fixed on the doors of the church, or other conspicuous place.

9. All intercourse with any infected town, and the neighbouring country, must be prevented by the best means within the power of the magistrates, who will have to make regulations for supply of provisions; but such regulations are intended only for extreme cases; and the difficulty of carrying such a plan into effect on any extended scale will undoubtedly be great, but, as a precaution of great importance, it is most essential that it should be an object of consideration, in order to guard against the spreading of infection.

10. Other measures, of a more coercive nature, may be rendered expedient for the common safety, if unfortunately so fatal a disease should ever shew itself in this country, in the terrific way in which it has appeared in various parts of Europe; and it may become necessary to draw troops,

or a strong body of police, around infected places, so as utterly to exclude the inhabitants from all intercourse with the country; and we feel sure what is demanded for the common safety of the state, will always be acquiesced in with willing submission to the necessity which imposes it.

The Board particularly invites attention to a fact confirmed by all the communications received from abroad, viz. that the poor, ill fed, and unhealthy part of the population, and especially those who have been addicted to drinking spiritous liquors, and indulgence in irregular habits, have been the greatest sufferers from this disease, and that the infection has been most virulent; and has spread more rapidly and extensively in the districts of towns where the streets are narrow, and the population crowded, and where little or no attention has been paid to cleanliness and ventilation. They are aware of the difficulty of removing the evils referred to, but they trust that attention thus awakened will insure the most active endeavours of all magistrates, resident clergymen, and persons of influence or authority, to promote their mitigation; and, as the amount of danger, and the necessity of precaution, may become more apparent, they will look with increased confidence to the individual exertions of those who may be enabled to employ them beneficially in furtherance of the suggestions above stated.

BOARD OF HEALTH, COLLEGE OF PHYSICIANS,
October 20. 1831.

The following are the early symptoms of the disease in its most marked form, as it occurred to the observation of Dr Russell and Dr Barry, at St Petersburgh, corroborated by the accounts from other places where the disease has prevailed.

Giddiness, sick stomach, nervous agitation, intermittent, slow, or small pulse, cramps beginning at the tops of the fingers and toes, and rapidly approaching the trunk, give the first warning.

Vomiting or purging, or both these evacuations, of a liquid like rice-water or whey, or barley-water, come on ; the features become sharp and contracted ; the eye sinks ; the look is expressive of terror or wildness ; the lips, face, neck, hands, and feet, and soon after the thighs, arms, and whole surface, assume a leaden, blue, purple, black, or deep brown tint, according to the complexion of the individual, varying in shade with the intensity of the attack. The fingers and toes are reduced in size, the skin and soft parts covering them are wrinkled, shrivelled, and folded ; the nails put on a bluish pearly white ; the larger superficial veins are marked by flat lines of a deeper black ; the pulse becomes either small as a thread, and scarcely vibrating, or else totally extinct.

The skin is deadly cold and often damp, the tongue always moist, often white and loaded, but flabby and chilled like a piece of dead flesh. The voice is nearly gone ; the respiration quick, irregular, and imperfectly performed. The patient speaks in a whisper. He struggles for breath, and often lays his hand on his heart to point out the seat of his distress. Sometimes there are rigid spasms of the legs, thighs, and loins. The secretion of urine is totally suspended ; vomiting and purgings, which are far from being the most important or dangerous symptoms, and which, in a very great number of cases of the disease, have not been profuse, or have been arrested by medicine early in the attack, succeed.

It is evident that the most urgent and peculiar symptom of this disease is the sudden depression of the vital powers ; proved by the diminished action of the heart, the coldness of the surface and extremities, and the stagnant state of the whole circulation. It is important to advert to this fact, as pointing out the instant measures which may safely and beneficially be employed where medical aid cannot immediately be procured. All means tending to restore the circulation, and maintain the warmth of the body, should be had recourse to without delay. The patients should always immediately be put to bed, wrapt up in hot blankets, and

warmth should be sustained by other external applications, such as repeated frictions with flannels and camphorated spirits; poultices of mustard and linseed (equal parts) to the stomach, particularly where pain and vomiting exist; similar poultices to the feet and legs, to restore their warmth. The returning heat of the body may be promoted by bags containing hot salt or bran applied to different parts of it. For the same purpose of restoring and sustaining the circulation, white wine whey, with spice, hot brandy and water, or sal volatile, in the dose of a tea-spoonful in hot water, frequently repeated, or from five to twenty drops of some of the essential oils, as peppermints, cloves, or cajeput, in a wine-glass of water, may be administered; with the same view, where the stomach will bear it, warm broth with spice may be employed. In very severe cases, or where medical aid is difficult to be obtained, from twenty to forty drops of laudanum may be given in any of the warm drinks previously recommended.

These simple means are proposed as resources in the incipient stage of the disease, where medical aid has not yet been obtained.

In reference to the further means to be adopted in the treatment of this disease, it is necessary to state, that no specific remedy has yet been ascertained; nor has any plan of cure been sufficiently commended by success to warrant its express recommendation from authority. The Board have already published a detailed statement of the methods of treatment adopted in India, and of the different opinions entertained as to the use of bleeding, emetics, calomel, opium, &c. There is reason to believe that more information on this subject may be obtained from those parts of the Continent where the disease is now prevailing; but even should it be otherwise, the greatest confidence may be reposed in the intelligence and zeal which the medical practitioners of this country will employ in establishing an appropriate method of cure.

HENRY HALFORD, *President of the Board.*

So little, in my opinion, were these regulations calculated to meet the disease, that I used the freedom to address the following letter on the subject to Sir Henry Halford.

LETTER to Sir HENRY HALFORD, *Bart. M. D., President of the Board of Health, &c. &c. &c. on the Tendency of the Proposed Regulations for Cholera; with Observations as to the Nature of the Disease, and the Course to be followed immediately on its Appearance in a Family.*

SIR,

I HAVE perused, with much anxiety, the Regulations lately promulgated by the Board of Health on the subject of Cholera; and I am sure you will do me the justice to believe, that nothing but a deep and overwhelming sense of duty could have induced me to question the expediency of any of those regulations. No medical man in the kingdom can entertain a more unfeigned respect than I feel for the distinguished body over which you preside, and for the high authority by which their regulations are sanctioned. But being impressed very strongly with the conviction, that, in the event of this malady reaching us, the rules prescribed by the Board could not be acted upon; and that any attempt to enforce them would be attended with disastrous consequences, both to the sick and to the healthy, I have taken this method of entreating you to reconsider the subject. It is not yet too late to modify these regulations, or to frame others less likely to be resisted or evaded, and better calculated to save the lives of those who may be attacked with the disease; and in that view it is for the Board to decide, whether any of the suggestions which I have now most respectfully to offer, are deserving of adoption.

In the outset, allow me to observe, that your regulations assume it to be absolutely ascertained that Cholera is contagious; and, on that assumption, some of the rules may

be salutary. But, to say the least of it, we have as yet no conclusive evidence that the disease is contagious; and while uncertainty on that point prevails, nothing, as it humbly appears to me, can be more dangerous than, under the authority of your high name, to excite a degree of panic, the inevitable effect of which must be, immediately on the breaking out of the disease, to deprive the sufferers of those attentions from their families, friends, and servants, which are indispensable to their recovery.

Were these regulations harmless, or did there exist any reasonable expectation, that, if enforced, they would stay the progress of the disease, or confine it to the house or family in which it may first appear, such a sacrifice, for the common benefit, might be expected. But I will venture to say, that, even on that hypothesis, it would be found almost impracticable to enforce the regulations against even one or two families; while, if the disease is to manifest itself here with the wide-spreading desolation which has marked its progress elsewhere, your regulations would not only be nugatory, but any attempt to carry them into execution would be attended with consequences even more fatal than the spreading of the disease itself, and would multiply tenfold the mortality among those attacked.

I do not mean to resume at any length the much agitated question of contagion. I have already stated my views on that subject in a work which the Medical Board has done me the honour to recommend *. But one or two established facts connected with this question will go far, I think, to support the opinions which I entertain as to the best method of alleviating the horrors of this disease, in the event of its arrival in this country.

First, then, I ask, what has our European experience of Cholera taught us? The general body of Indian practitioners had long been accustomed to regard the disease as non-contagious. But as it advanced into Europe, the eminent physicians in the several countries which it has suc-

* Treatise on Cholera Asphyxia, 1831.

cessively ravaged, disregarded the Indian experience; and, perhaps wisely, resolved to protect themselves against its approaches by rigorous quarantine regulations, or by sanitary cordons, enforced with all the power of despotic governments. Look, however, at the result:—In the face of all such regulations the disease has advanced westward with undeviating and irresistible strides; and, so far from the experience of Europe refuting the Indian conclusions on the question of contagion, it would appear, that, as each country becomes acquainted with the disease, the conviction becomes general, that to whatever cause its dissemination is to be ascribed, it is not propagated by contagion, and cannot be confined within any prescribed limits.

Fortunately, the home-bred English physician has hitherto had no opportunity of studying practically the peculiarities of this extraordinary disease, or of pursuing inquiries into the no less extraordinary influence by which it seems to be propagated. But this can be no reason for setting at nought the experience of the large class of medical practitioners who have had opportunities of treating the actual disease; far less can it justify the circulation, by authority, of opinions concerning its contagious nature, so likely to aggravate all its horrors by a deadly and paralyzing panic. The experience of India, of Russia, and more lately of Germany, proves that Cholera travels not with the erratic course of a contagious distemper, but with a march steadily progressive in a particular direction. The absence of quarantine in India did not accelerate its progress; the enforcement of such regulations in Europe has *in no instance* retarded it. Nay more, it is well known that new cases of the disease do not occur in vessels after they have got fairly free of the port where it prevails; and in the thousands of vessels which have performed quarantine on our shores, no well authenticated case of the disease has been reported.

That my apprehensions as to the probable effect of the late regulations are not groundless or imaginary, will be at once apparent, if we attend to the usual course of treatment in this disease. A Cholera patient requires not only the

almost constant attendance of a medical practitioner, but his life, in most cases, depends on the unremitted efforts of non-medical assistants. In India, we see the patient's ordinary acquaintance, free from all alarm, actively engaged in shampooing or rubbing spasmodically affected limbs; while the medical attendant in that country has always the aid of any number of volunteers he may require. But under the influence of your regulations, where are we to look for such coadjutors? It is impossible for an unprofessional person to read the directions lately published in the Gazette, without being impressed with the belief, that, if he touch a patient labouring under Cholera, he does little less than inoculate himself with a mortal poison. The Board themselves, indeed, are already, I much fear, paving the way for inefficiency in hospital establishments; for, by one of the published heads of instruction, the hospital establishments are directed to be *kept low*, that the number of attendants may not tend to spread the disease.

Under the impression of terror likely to be produced by these regulations, it is likewise to be expected that the domestics of the wealthier classes will fly the house as soon as a case of the disease occurs. Then the public are at the same time to be warned of their danger by portentous *boards* affixed to the house; while the wretched members of the family, devoted as it were to destruction, must either trust to the worst description of mercenaries, or, prompted by their affection, must endeavour themselves to render those offices to the sick which their habits disqualify them for performing; and the fatigue attending which, combined with the predisposing effects of fear, must render them greatly more susceptible of the disease. There is, indeed, one other alternative prescribed:—The family have the option of seeing their dearest relatives carried off to a pest-house,—a proceeding so foreign to the customs of this country among the better classes of society, that I fear they would regard this with feelings even more harrowing than those with which they would follow them to the grave.

Nor is this all. The obnoxious surveillance under which

those affected with the disease are to be placed, would lead inevitably to evasions of the rules, even were they sanctioned by the legislature. In order to escape the singular system of *non-intercourse* which is enjoined, the occurrence of cases of the disease would be concealed, or not intimated in time to admit of medical aid being afforded with any hope of success. For it must always be remembered, that this is a disease in which a delay of the shortest period of time in applying for professional advice may be fatal; and yet no one can doubt, that the natural repugnance of the lower classes, particularly in Scotland, to be removed to an hospital, and, with all classes, the rigour of these regulations, will lead to many such fatal delays, or to other expedients, whereby, even supposing the disease to be contagious, the regulations, by their mere severity, would be rendered worse than inoperative.

This is a melancholy, but it is a true picture of the practical working of the late proclamation? And to what advantage is all this? Have such regulations stayed the spreading of the disease in St Petersburgh, in Riga, in Berlin, in Vienna, or in Hamburgh? Assuredly they have not. In Europe, with all the apparatus of lazarettos, and cordons, and plague-boards, and the other means recommended by the Board of Health, the disease has spread as widely, while the mortality has been proportionally greater than in India, where no such precautions are thought of, and where the natives fearlessly brave the disease, and assist one another in mitigating its torments. In India, generally speaking, one in eight of the persons attacked dies; in Europe, with all the advantages of superior medical skill, the deaths, if newspaper reports are to be credited, have been nearly in the proportion of one to two; and my persuasion is, that this extraordinary mortality is to be attributed, in no inconsiderable degree, to rigorous sanitary regulations; which, while they have in no respect arrested the progress of the disease, have increased its evils, both by their direct operation, and by the causeless and

enervating panic which they have been so powerful a means of promoting.

Let it not be supposed, however, that I am rash enough to advocate the abandonment of all precautionary measures. While it is barely possible that quarantine regulations may protect us, every one must wish to see them enforced as they have heretofore been. I greatly fear that they will prove unavailing; but so long as we are free of the disease, no rational person would desire any relaxation in what may be called the *external* means for preventing its arrival. If, however, in spite of all those means, Cholera should make its appearance in this country; and, above all, if it break out, as it probably will do, under circumstances likely to satisfy the most zealous contagionist that it has not been conveyed in that manner; it will well become those to whom the matter has been confided by government, to pause, before they subject the country to an irksome and impracticable system of plague-regulations, likely to prove a more serious misfortune than the disease itself.

I am much deceived if one week's experience of Cholera do not satisfy the medical men in England, that contagion is not the real evil with which they have to contend. But however that may be, I had been led to expect from the Board of Health some practical and popular directions for our guidance, in case the disease should unhappily make its appearance; and I need not say that I had directed my own attention to this subject. It was not, however, my intention to have added any thing to what I have already published concerning this disease; and I should have adhered to that intention, had it not occurred to me, that, as the regulations in the Gazette are directed rather to the prevention of the spreading of the disease by means of contagion, than to the best means of relieving the sufferers, there was still room for some practical directions likely to prove serviceable to the community, and in a particular manner to the inhabitants of the city in which I reside. In offering these suggestions, I make no pretensions to any superior medical skill; but having accidentally seen more of

the disease, I believe, than almost any of my professional brethren in this city, and considering it to be the bounden duty of every man who has had such opportunities, to do his utmost to alleviate the sufferings of those who may be attacked, I venture, under the influence of that feeling, and avoiding all controversial topics, to submit the following hints for your consideration:—

I. It appears to me very desirable, that we should have such a popular description of the disease as may enable us to recognise it immediately on its accession. Much anxiety and needless alarm would be avoided, were the public sufficiently impressed with the distinction between this formidable distemper, and those derangements of the bowels with which, from the name it has unluckily received, and from some of its less important symptoms, it is too apt to be confounded. Had we now to choose a name for the disorder, it would certainly be more appropriate to call it *Morbus Asphyxia*, or the *pulseless* disease,—the failure of the pulse, and a suspension of the healthful circulation of the blood, being much more characteristic of the disorder than any other symptom with which I am acquainted. The approaches of this morbid condition, however, are gradual and insidious. The patient feels lassitude at first, perhaps headach and ringing in the ears, accompanied generally by one or two lax stools. But it must be borne in mind, that the bowels are not invariably so affected; and even that the preliminary symptoms which I have mentioned are hardly deserving of notice, except when Cholera prevails in the neighbourhood.

The symptoms which more properly belong to the disease are blue lips and nails; an earthy or blue colour of the whole skin, the surface appearing shrunken; while the countenance is sunk, and the eyes fall deep in their sockets, and are surrounded by a dark ring. The extremities become cold, and the pulse weak; it may even be imperceptible. There is oppression of the chest, and generally a burning sensation at the pit of the stomach. Severe vo-

miting and purging, of a fluid which is something like rice water; urgent thirst, accompanied probably by violent spasms in the extremities, and excruciating spasmodic pains in the belly, supervene. The patient tosses about his body, and falls into a state of indescribable agony; and although the mind remains comparatively entire, he has very much the appearance of an animated corpse.

A very few hours will reduce the sufferer to this state, which I need hardly say is one of imminent danger, and little likely to be confounded with any of the ordinary maladies to which the inhabitants of this country are accustomed. And no one can see an individual reduced to this extremity, without being satisfied that not one moment is to be lost in sending for medical assistance.

II. Medical advice, however, may not always be at hand; and although safety is scarcely to be hoped for in absence of a professional man, it is nevertheless of great importance to point out to the friends of the patient, what steps may be safely taken between the accession of the disease and the arrival of the physician. The patient ought to be immediately placed in warm blankets, and surrounded with bottles of hot water, bags of hot sand or salt; and every other means of applying *dry* heat which may be within reach, ought to be resorted to. The whole body ought to be rubbed with hot flannel, and the belly should be covered with mustard poultices, which should also be applied to the calves of the legs.

I have seen so little good to result from the application of the hot water bath, that, even if it should be at hand, I should not put a patient into it. One great objection to the application of hot water is the fatigue which it occasions the patient, besides, that any benefit attending it is generally neutralized by the difficulty of drying the body, or by the evaporation which takes place during that process.

Laudanum is usually to be found in every house, and certainly ought to be so during the prevalence of this dis-

ease; but the friends of the patient must be cautious of administering it in large doses without the sanction of a medical man. A tea-spoonful of laudanum, added to six table-spoonfuls of brandy, may be prepared, however, and one table-spoonful of this mixture may be administered in half a wine-glassful of hot water every quarter of an hour,—five drops of essence of peppermint and ten drops of sulphuric ether being added to each dose of the brandy and laudanum. In case the stomach reject the liquid, pills containing three grains of camphor and half a grain of opium may be given; and if the irritability of the stomach be so great that it rejects all that is swallowed, a warm glyster of arrow-root, with a tea-spoonful of laudanum in it, should be injected into the bowels.

As a measure of precaution, all these remedies ought to be at hand; but in towns, of course, medical attendance will probably be obtained in time to admit of their being applied under sanction of a professional man.

Early bleeding is of so much importance in this disease, that, if possible, it should be resorted to even before the arrival of the regular medical attendant of the family. The rule is to bleed until the blood, which is black and thick when a vein is first opened, assumes a red and more natural colour, and until the oppression of the patient is relieved. In the course of my practice I have always found that the danger is, that too small rather than too large a quantity of blood is removed.

The greater part of the course of treatment just prescribed, may be adopted in almost every case, and, if promptly followed, the most salutary effects may be expected. But, as I have already said, the sooner that a medical man is called, so much the better is the patient's chance of surmounting the disease. As to the minute details of medical treatment, it would be foreign to my present purpose to enlarge upon them.

III. Could we make even a plausible conjecture as to the remote cause of Cholera, we might have some hope of being

able to resist the advances of the disease by precautionary measures. But, in the present state of our information and experience, an absolute preventive against an attack of this extraordinary malady, is a desideratum which we can hardly expect to see supplied. At the same time, while we are thus unable to arrest its progress, we are warranted in giving the character of predisposing causes, to certain circumstances which are within our reach. In India, it has been almost invariably found, that regular habits, nourishing diet, and cleanliness, gave those exposed to the disease the best chance of escape; while exposure to fatigue, or to cold, particularly during sleep, poor diet, and, above all, intoxication and dissipated habits, have been found powerfully to predispose to Cholera. There is also reason to believe, that persons living in low ill-aired situations, and sleeping on the ground floors, or in damp floors near the ground, are more liable to the disease, than those who are less unfortunate in their habitations.

In this country, attention to clothing and to within-door comforts, will be of even more consequence than within the tropics. Every endeavour, therefore, ought to be made, by the upper classes of society, to prepare the poor for resisting the disease, by providing them with warm clothing, and, if possible, with warm and well-aired houses. It is said, that in Vienna, where proportionally more of the upper classes have suffered than in other towns in which Cholera has appeared, it is the custom for this class to inhabit the lower storeys, and to leave the upper part of the house to the humbler classes of society. If this fact be established, it would justify us in transferring our beds, where that can be conveniently done, from the lower to the upper flats.

Nourishing diet is unquestionably very useful as a preventive, while crude fruit and undressed vegetables, and in general all food having a tendency to produce disorder in the stomach and bowels, ought to be avoided.

Debauchery of all kinds may be regarded as almost the certain means of provoking an attack of the disease, while it prevails in a town or district. Powerful purgatives are

also dangerous at such a time; and, in general, I should recommend it to that pretty numerous class in the community who are in the practice of dosing themselves with favourite medicines, to abstain as much as possible from this indulgence during the prevalence of Cholera. For although, at such a time, attention to the state of the bowels is of much importance, yet as certain of the ordinary medicines are more likely to be prejudicial than others, or to predispose to attacks of the disease, it is particularly desirable, while the danger exists, that the slighter, as well as the more important derangements of the system, should be treated by medical men.

The disease in India was usually confined to a particular part of a city, or camp, or district, while those residing beyond the tainted limits were safe, for the time, from attack. If the same phenomenon should be exhibited when the disease makes its appearance in this country, safety may occasionally be found in deserting the quarter of the town or district in which the disease particularly manifests itself. But before resorting to this expedient, the habits of the disease would require to be well ascertained; for any thing like a precipitate removal, unless it be to a quarter indisputably beyond the limits to which the disease is confined, might prove more prejudicial than it would be to remain and face the danger—using such precautions against the predisposing causes as have been suggested.

IV. The duties of Magistrates and of the Heads of the Police, on the appearance of the disease, may, I think, be disposed of in a very few words. The higher classes of society may safely be left to themselves, as they can easily have within their own houses most of the means required for the cure of the disease; and any attempt to enforce against them a system of regulations intended to prevent contagion, would speedily be found impracticable. On that subject I have already said enough, and I have little doubt, that, if we are unfortunately visited by the disease, all such

regulations must and will be abandoned, as vexatious and useless.

It is therefore to the poorer classes that the attention of the Magistracy ought to be directed. For their accommodation, as well as for the benefit of such of the inhabitants generally as may be willing to go into hospitals, certain houses ought to be fixed upon, which may be converted into sick-houses on the appearance of the disease. These houses should be conveniently situated for the service of the densely peopled portions of the town; and there ought to be several of them, so that if one or two are found to be within the *fomes* of the disease, they may, for the safety of attendants on the sick, be deserted.

A sufficient reward ought to be held out to induce persons of good character to come voluntarily forward as hospital attendants; and each hospital should have as many medical attendants attached to it as possible, so that they may relieve each other from the severe duty to which they must necessarily be exposed. I have already adverted to the recommennation of the Board of Health, that the hospital attendants ought to be *few*. This recommendation bears internal evidence, that the individual by whom it was suggested to the Board, *never witnessed the actual disease.* For my part, I should say, with humble confidence, that if the hospital attendants are *few*, it would be much better for the sick that there were no hospitals at all, and that each patient were left to such chance assistance as his friends might be able to procure for him; for, most undoubtedly, in the whole circle of diseases to which the human constitution is liable, there is no one in which the aid of numerous assistants is more absolutely indispensable than in Cholera. My suggestion, therefore, would be, that the Magistrates of each town in which the disease appears, should have a list of all such persons as are willing to enrol themselves for this duty; and that, when occasion requires, they should be sent in sufficient numbers to each hospital.

These hospitals ought, of course, to be well supplied with medicine, and with the apparatus for a spirit vapour

bath, described in my treatise on Cholera. Indeed, it would be a proper regulation to have this apparatus placed in certain marked stations in each town, for the use of the sick generally, whether in hospitals or in private houses. The hospitals ought also to be provided with an ample supply of warm bedding, with the means of keeping up a high temperature by stoves or otherwise, and with a cooking establishment for the benefit of the convalescents.

Lastly, It is the duty of all persons of influence, on the breaking out of Cholera, to exert themselves in allaying panic and alarm. The disease undoubtedly is very appalling; but it will be much more so, if it be not encountered with manly energy and with becoming fortitude. It may afford some consolation to alarmists, that it is a disease which seems speedily to exhaust its violence at any one place. And this is a circumstance, I may add, which is not easily reconcileable with the notion that the disease is of the contagious nature so much apprehended by the Board of Health; since, were that the case, instead of leaving a town, and passing onwards after raging for a limited number of days, it might be expected to propagate itself like the plague, small-pox, and other maladies, which are admitted on all hands to be contagious. This peculiarity in Cholera has been observed by all who have witnessed it either in Asia or in Europe, and is particularly relied on as proving the disease not to be contagious, in the Bengal Medical Report; in which Report, also, it is stated to be the *unanimous* opinion of the whole medical men in that Presidency, that the disease is not communicable by contagion.

It only remains for me to make a suggestion as to the best means of conveying the sick from their houses to the hospitals. For this purpose, a sedan-chair, inclined as those are which are used in hospitals, or a litter, may be employed. The great difficulty in removing a Cholera patient, is to avoid exposing him to any fatigue; for, in this disease, the powers of life are reduced to so low an ebb, that whatever is done about the patient, must be managed

in such a manner as not to require him to make any over-exertion.

I have now brought to a close the practical suggestions to which I have adverted in the outset. In what I have said, I have been actuated by a sincere desire to avert or to mitigate the evils of the pestilence which seems to be approaching us. To some it may appear, that I have animadverted too pointedly on the regulations circulated by the Board of Health. But feeling as I do upon that subject, I have not hesitated to express my opinion. The Board, as well as every medical man in this country, can have but one object in this emergency; and if my endeavours should lead to some modification of the more obnoxious or questionable portions of those regulations, I shall have the consolation of thinking that I have discharged a duty which I hold to be paramount; and no one, I am sure, will look upon my endeavours with more indulgence than the eminent and distinguished individual to whom I have now the honour of addressing myself. I have the honour to be, Sir, your most obedient humble servant,

G. Hamilton Bell.

Edinburgh, 13 Castle Street,
October 28. 1831.

I shall not pretend to say that my remonstrance produced any effect, but undoubtedly the outcry to which the regulations of 20th October 1831 immediately gave rise, produced a very important modification on those regulations, as is apparent from the following official document, issued within less than a month after the date of the former.

CENTRAL BOARD OF HEALTH.

COUNCIL OFFICE, *Nov.* 14. 1831.

SIR,

The Central Board of Health having maturely weighed all the information which has been transmitted to them relative to the progress of the Asiatic Spasmodic Cholera, in various parts of Europe, but more particularly guided by the conclusions on this head, to which Drs Russell and Barry have arrived, after a few months careful and laborious observations of the character of that disease in those parts of Russia which they have visited, beg leave to suggest for your consideration the following sanatory hints:—

I. AS TO PRECAUTIONARY MEASURES.

In order to insure the adoption and realize the benefit of any system of sanatory arrangements in a large community, the first essential point is to divide that community into subordinate sections, and to form distinct Boards of Health, each to consist, if possible, of a resident clergyman and a number of substantial householders, and of one medical man at least.

These Boards should be charged with the following duties in their respective districts, viz.:

1. To appoint inspectors. Each inspector to visit daily, and to inquire carefully after the health, means of subsistence, cleanliness, and comforts of the inmates of, say 100 houses, more or less according to local circumstances.

2. To receive and examine the reports of these inspectors, which should be made up to a given hour on each day.

3. To endeavour to remedy by every means which individual and public charitable exertion can supply, such deficiency as may be found to exist in their respective districts, in the following primary elements of public health-viz.—the food of the poor, clothing, bedding, ventilation, space, cleanliness, outlets for domestic filth, habits of temperance, prevention of panic.

4. To report to their principal Boards respectively, on the above heads, as well as on the actual state of the health of their districts.

The subordinate divisions of each district ought to be numbered or lettered, and each district named; the names of each Board often had not been attached to them, and that of the visiting inspectors employed should be placarded in conspicuous places.

Principal Boards of cities, towns, or parishes, to report directly to the Central Board in London,—

1. On the actual state of health of the whole population.

2. On the precautionary measures already carried into effect.

3. On the measures contemplated.

4. On the suspected sources, if any there be, from whence this particular disease might possibly spring.

With regard to precautions as to intercourse with suspected or really infected persons or places, the Board are confident that good sense and good feeling will not only point out, but morally establish, as may be practicable, the necessity of avoiding such communications as may endanger the lives of thousands.

But they strongly deprecate all measure of coercion for this purpose, which, when tried upon the continent, invariably have been found productive of evil. The best inducements to a prompt acknowledgment of the disease having entered a family, as well as to an early and voluntary separation of the sick from the healthy, will always be found in the readiness and efficiency with which public charitable institutions attend to the subject in S. 3.

It is with much pleasure that the Board feel themselves authorized to declare, and it will no doubt be highly consolatory to the public to learn, that, under the proper observances of cleanliness and ventilation, this disease seldom spreads in families, and rarely passes to those about the sick under such favourable circumstances, unless they happen to be particularly predisposed.

It will not, therefore, be necessary, when there is space, and when due attention is paid to cleanliness and purity of air, to separate members of families actually affected by the disease, not to insulate individual houses, unless in cases of crowded, filthy, badly ventilated habitations, and other contingencies which involve the health of all.

It having been proved, by ample experience, in more than one city in Europe, that the fitting up and furnishing of hospitals for the reception of the poorer classes supposed likely to be attacked by the disease at a period too long before its actual breaking out, has been productive of great waste of means, by the spoiling of various articles, and the consequent want of wholesome accommodation when most required, the Central Board would recommend that proper and sufficient house-room only be secured and prepared in the first instance, and that the charitable be called upon only to pledge themselves to furnish, at a given notice, such articles of bedding, furniture, &c., or the value of them, as they would at once have contributed.

By this means the deterioration of perishable articles will be avoided; and should the district entirely escape, the contributions will be saved.

The situations which the Board would recommend for temporary Cholera hospitals would be those most detached, insulated, and thoroughly exposed to free and open air; the description of house such as would admit of the most perfect ventilation and cleanliness, and the largest space around the sick.

The Board would recommend, where a family is reported to be in an unhealthy state by the Sub-Inspector, and the disease confirmed to be Cholera by a medical member of the District Board, that the head of such family, if unable to afford accommodation at home, be advised to send the sick person forthwith to the temporary hospital, and that the other members of the family be supplied with such additional means and comfort as their state may require, to enable them to resist the influence of the infected atmosphere.

II. MEDICAL AND DIETETIC PRECAUTIONS.

These will be found of considerable importance, from their contributing to prevent or diminish the susceptibility to infection which individuals may possess at the moment the disease breaks out. No sudden nor extensive alterations should be made in the usual modes of living. All changes of food to be useful, indeed not to be absolutely prejudicial, should tend to render it drier, more nutritive and concentrative; moderately costive bowels, the almost invariable consequence of a dry, invigorating diet, will be found more conducive to exemption from Cholera than an opposite habit.

Whenever aperients may become indispensable, those of a warm, aromatic kind, in moderate doses, or domestic means, should alone be resorted to.

What is generally understood by salts—viz. Glauber's salts and Epsom salts, as well as other cold purgatives—should not be taken in any quantities, nor on any account, without the express prescription of a medical man.

The medical members of the Board beg to state, in the most decided manner, that no specific preventive against Cholera is known to exist, and that the drugs hitherto offered with this pretension, in countries where the greatest ravages have been caused by this disease, not only did not possess the negative virtue of doing no harm, but are found to be absolutely injurious.

The two preventives are a healthy body, and a cheerful unruffled mind. Looseness of bowels should be immediately checked, and any thing like periodical chills or cold perspirations should be met by quinine in suitable doses, but habitual drugging, at all times improper, is to be deprecated in the strongest terms when epidemic disease is apprehended.

The Board have been anxious to lay before the public, as early as possible, the above precautionary outlines, which they trust will tend, together with the suggestions emanating from the wisdom of your and other local Boards, if not to exempt the whole population of these realms from

the scourge of spasmodic Cholera, at least to enable them to meet it, in the event of its appearing amongst them, with physical and moral constitutions the least likely to suffer from its virulence.

The Central Board will avail themselves of the earliest opportunity to transmit to you any farther sanatory suggestions which may occur to them on the subject of precautionary measures, as well as an outline of instructions now in preparation for communities supposed to be actually attacked. I have the honour to be, Sir, your most obedient servant,

E. Stewart, *Chairman.*

Council Office, Whitehall, *Nov.* 16. 1831.

In confirmation of the cautionary measures recommended by the Central Board of Health, under date of the 14th instant, with regard to the use of saline medicines during the existence of epidemic disease, we are authorised to publish the following extract of a letter from Dr Daun, dated Sunderland, the 14th instant:—" One of the cases of malignant Cholera which has just occurred—I was called in to see; the patient felt slightly indisposed yesterday morning, about seven o'clock, with slight uneasiness in her bowels. About 10 A. M. she took a dose of Epsom salts before she sent for any medical man to see her; in about an hour after taking the salts she was seized with spasms, and began to sink. I was asked to see her in the evening, which I did, but when I arrived at her house she was *in articulo mortis;* she died in about an hour after I saw her. It is one of the least malignant cases of malignant Cholera which has occurred since my arrival here. Having in India seen the disease in several instances supervene after the administration of a dose of Epsom salts, I have thought it might be of use to caution the public against the use of saline purgatives, and I have caused a notice to be circulated, recommending in their stead rhubarb, magnesia, and ginger, or any other warm aromatic powder, during the continuance of the epidemic."

CENTRAL BOARD OF HEALTH.

COUNCIL OFFICE, WHITEHALL, 13*th Dec.* 1831.

SIR,—Agreeably to the intimation given by this Board in the concluding paragraph of their circular, dated 14th ult., I have the honour to transmit the subjoined "sanitary instructions for communities supposed to be actually attacked by Spasmodic Cholera," with some observations on the nature and treatment of the disease, drawn up by Drs Russell and Barry.

Every individual being deeply interested in the preservation of the public health, it is the bounden duty of all to endeavour to arrest the spread of disease at its very commencement. In order to attain this important object—

1*st*, The most efficient arrangements should be made by the Local Boards of Health, and other authorities, to obtain the earliest and most correct intelligence of every suspicious case which may occur within their jurisdiction.

2*d*, All unnecessary communication should be prevented, as far as possible, between the infected and the healthy.

3*d*, As space, cleanliness, and pure air, are of the most vital consequence, both to the recovery of the sick person and to the safety of those about him, the patient labouring under Spasmodic Cholera should either be placed in a separate well-ventilated apartment of his own house, if it afford such accommodation, and be attended by as few persons as the circumstances of his case will admit, or be induced to submit to an immediate removal to such building as may have been provided for the reception of persons whose circumstances will not afford the advantages at home, of space, air, and separation from the healthy.

4*th*, When an individual shall have been attacked with this disease, and placed under the most favourable conditions, as already pointed out, both for the recovery of his own and the safety of the public health, the room or apartment where he may have been attacked, and from which he may have been removed, should be purified by scrubbing, lime-washing, free ventilation, and fumigation by heated

sulphuric acid and common salt, with black oxide of manganese, or the same acid with nitre, or when these materials cannot be obtained, by strong vinegar thrown upon heated bricks. The bed, bedding, and clothes, should be immersed in water, washed with soap, and afterwards fumigated as above.

5th, To correct all offensive smells, chloride of lime may be applied; but great caution is recommended in the use of this material, its fumes continued for any length of time having been found highly prejudicial to health, more particularly in delicate persons.

6th, A number of steady men, proportionate to the district in which they are to act, should be appointed to lime-wash and purify, as ordered above, under the direction of medical authority, such apartments as may be pointed out by the inspectors of the local board.

7th, Those who die of this disease should be buried as soon as possible, wrapped in cotton or linen cloth saturated with pitch or coal tar, and be carried to the grave by the fewest possible number of persons. The funeral service to be performed in the open air.

8th, It is of the utmost importance to the public health, that an improved diet and flannel clothing, at least flannel belts and woollen stockings, should be given to the poor. No person should ever allow himself to sit down and get cool with wet feet. The most particular attention should be paid to keeping the feet dry and warm. Repletion and indigestion should be guarded against; all raw vegetables, acescent, unwholesome food and drink, to be avoided. Temperance should be most rigidly observed in every thing. In short, no means should be neglected which may tend to preserve individual health. The neglect of any or all of these cautions would not of themselves produce the specific disease called Spasmodic Cholera; but such neglect would most assuredly dispose the individual living in an infected atmosphere to be attacked by this disease, when most probably he might otherwise have escaped.

The most effectual means by which this disease may be

prevented from extending, is to enable the poor, who are generally first attacked, to oppose to its influence, as far as practicable, those ameliorations in diet, clothing, and lodging, which public and private charity will, it is hoped, not fail to produce.

Observations on the Nature and Treatment of the Disease, drawn up by Drs Russell and Barry.

Of the two great classes of functions performed by the organs of which man is composed, one only is attacked in this disease. The operations of the senses and of the intelligence are either left untouched, or are affected but in a secondary manner.

Those functions, on the contrary, by which existence as a living being is preserved; those complicated powers, by means of which we are for ever appropriating and converting into a part of ourselves portions of the matter around us, are all and at once deranged by the attack of this terrible malady. Nutrition is annihilated; respiration becomes difficult, irregular, and inefficient; the involuntary muscles no longer perform their task; the voluntary are drawn into contractions by other powers than the will; the blood ceases to circulate; its physical properties are altered; its serous portion is suddenly thrown out upon the intestinal mucous surface of the body; the secretions are all arrested; the animal heat is no longer produced.

Under such rapidly destructive and almost universal derangement of function, the most energetic efforts should be directed to produce what the disease has rendered nature unable to keep up, viz.—

1*st*, Fluidity, heat, and motion in the blood.

2*d*, Regulated action in the voluntary and involuntary muscles.

Lastly, but above every other consideration, renewed energy in the nervous centre, the source of all vitality and function.

No remedy at all approaching to the nature of a specific has been as yet discovered for the disease. In fact, no one

mode of cure can be usefully employed under all the circumstances of any disease. The grades of intensity, and the grouping of the symptoms with which Spasmodic Cholera makes its attacks, vary with the conditions of the subject; its treatment, therefore, must vary with these grades and conditions.

The leading preliminary symptoms generally are, either diarrhœa, spasms, apoplectic vertigo with nausea, imperfect vomiting, or various combinations of these symptoms.

When the diarrhœa affords time for distinct treatment, it ought to be arrested at once by the most prompt and efficient measures;—by opium in moderate doses; astringents; local bleeding by leeches, if the subject be plethoric; by cordials and sulphate of quinine, if there be cold sweats; by confining the patient strictly to bed, and keeping up heat; by diet; by emetics.

Should spasms be the first and leading symptom, subnitrate of bismuth, cupping along the course of the spine, cordial, and antispasmodic medicines, opium, frictions, and dry warmth, are indicated.

But when the patient is suddenly seized with vertigo, nausea, coldness, loss of pulse, blueness of the skin, shrinking of the features and extremities, with more or less watery discharges and cramps, constituting an aggravated case of the worst type; whether this state shall have come on without warning, or shall have supervened upon either or both of the preliminary sets of symptoms already mentioned, time must not be wasted upon inert measures. Such a patient will inevitably perish, and that within a very few hours, if the paralyzed vital functions be not quickly restored.

Let him then be immediately placed between warm blankets; and should no medical person be at hand, let two table-spoonsful of common salt, dissolved in six ounces of warm water, be given immediately, and at once, if he be an adult. Let dry and steady heat be applied along the course of the spine, and to the pit of the stomach, (if no other means be at hand), by a succession of heated plates or platters. Let the upper and lower extremities be sur-

rounded with bags of heated bran, corn, ashes, or sand, and assiduously rubbed with a warm hand, and a little oil or grease to protect the skin. Energetic, complete vomiting, will probably be produced by the salt; and perhaps bilious purging, with tenesmus.

Should a medical man be on the spot, a moderate bleeding, if it can be obtained, would be desirable, previously to, or immediately after, the administration of the salt, or of any other emetic which may be preferred.

The extensively deranged action of those organs, whose nerves are chiefly derived from, or connected with, the spinal marrow; the anatomical characters found about that great source of vitality after death, in many cases of this disease; together with the success stated by Dr Lange, chief physician at Cronstadt, to have attended the practice mentioned below, founded upon these views, in twelve out of fourteen aggravated cases, fully justify the following recommendation:—

In cases such as those just described, let the cautery be freely applied to one or two, or more places, on either side of the spine, as if for the purpose of forming good sized issues. Should the heated iron have produced any excitement of the nervous power, and the salt-emetic have caused any portion of the bile to flow through its proper duct, a great step will have been accomplished towards recovery from the stage of collapse. Cordials and opiates judiciously administered; sinapisms and other external stimulants, mercurials, with mild aromatic aperients, which the intelligence and activity of British medical practitioners will not fail to adapt to the actual circumstances of each case, will conduct the patient safely to the stage of re-action.

The organs during the collapse of this disease, probably owing to deficient vitality, often give no indication of having been acted upon by repeated doses of certain powerful medicines, which, under other circumstances, would have produced the most pronounced effects. It is, therefore, suggested that this temporary insensibility of the system

could not inculcate the administration of such repeated quantities as could, by accumulation, when the organs begin to recover their vitality, give rise to unfavourable results.

Thirst being a most distressing symptom of this disease, the quality and temperature of the drink should perhaps be left to the choice of the patient; but the quantity taken at a time should not exceed four ounces, and should be acidulated with nitrous acid, if the patient will bear it.

Should the disease prove extensively and rapidly epidemic in a large community, it would be prudent to establish stations at convenient distances from each other, where medical assistance and medicines might be procured without the risk of disappointment or delay. The details of these arrangements are left to the wisdom of Local Boards of Health.

As the symptoms of the consecutive stage of feverish reaction in Cholera differ but little, if at all, from those of ordinary Typhus, except perhaps in the greater rapidity with which they but too often run to a fatal termination; and as this kind of fever is treated in no part of the world with more success than in England, the entire management of this stage of the disease is left to the zeal and science of the profession at large.

Attentive nursing, and assiduous, well-directed rubbing, are of the utmost importance; a strictly horizontal position, however, must be maintained until the heart shall have, partly at least, recovered its action. An erect, or even semi-erect, position, during the collapse, has been often observed to produce instant death. Warm baths, therefore, for this and other reasons, are worse than useless; evaporating fluids, and indeed all moisture applied to the skin, seem to be contradicted for obvious reasons. Hot air-baths, so contrived as to be applicable in a recumbent posture, and admitting access to the patient for the purpose of friction, may be of use. I have the honour to be, Sir, your most obedient servant,

E. Stewart, *Chairman.*

REPORT OF THE EDINBURGH BOARD OF HEALTH.—16*th November* 1831.

MEMBERS OF THE BOARD.

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The Right Honourable the Lord President.
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George Meikle, Esq. late Superintending Surgeon to His Highness the Nizam's Army.

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- Dr Maclagan.
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Mr Bell,
} *Medical Secretaries.*

George Forbes, Esq. *Treasurer.*
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REPORT, &c.

The Edinburgh Board of Health having maturely considered what steps should be taken for checking the Epidemic Cholera, if it should appear in this city, are of opinion that the proper time is now arrived for making public the following directions for the guidance of the Inhabitants.

I. The Board are satisfied, that the disease may arise spontaneously from hidden causes; and that it may also become contagious in circumstances not yet ascertained. But they are fully warranted in declaring, that, when it does become contagious, the risk of its spreading in that manner is very much diminished if due attention be paid to cleanliness and sobriety. And they therefore entreat the inhabitants of Edinburgh, in the event of the disease appearing here, not to be misled by exaggerated notions of its contagious nature,—the inevitable tendency of which would be to leave the sick helpless, and without that attendance from friends and others which is more pressingly required in this disease than in any other.

II. Experience has shown, that the most essential precaution for escaping the disease is sobriety,—that intoxication during the prevalence of the epidemic is almost sure to be followed by an attack,—and that those addicted to drinking are the most subject to take Cholera, and the most likely to sink under it. In like manner, strict attention to personal cleanliness,—to cleanliness and ventilation of dwelling-houses,—to warm clothing,—to regularity of hours for sleep,—to keeping as much as possible within doors at night,—and to taking food before going out in the morning,—may be relied on as important means of security.

III. The Board are providing, that, should the disease appear, several Hospitals shall be opened over the city;—

also that stations shall be established, where, as well as at the hospitals, medicines and heating-apparatus may be procured at all hours of the day and night;—and likewise that, if necessary, all night-watchmen shall be supplied with medicines for the immediate use of any who need them. By these means it will be in the power of every one to procure instant aid.

Notice of the situation of hospitals and stations for medicines will be given the instant the disease may appear.

IV. But to prevent the possibility of delay, especially in the dead of night, when the disease is very apt to begin, the Board recommend families to provide the following articles.

1. *Mixture.*—Take sulphuric ether and aromatic spirit of hartshorn, of each half an ounce, compound tincture of cinnamon, one ounce: Mix and cork up carefully.
2. *Laudanum.*—An ounce to be kept in readiness.
3. *Pills.*—Take of opium twenty-four grains; camphor one drachm; spirit of wine and conserve of roses enough to make a mass of proper consistence: Divide into twenty-four pills.
4. *Clysters.*—Take of laudanum an ounce, tincture of assafœtida two ounces: Mix for keeping. Before using, mix three tea-spoonfuls with a wine-glassful of thin starch: and retain it, when injected, by pressure below with a warm cloth, as long as possible.
5. *Mustard-Poultices.*—Have always at hand four ounces of powdered mustard. A fourth part of this, spread over porridge poultices, will be sufficient for one patient.
6. *Hot-air Bath.*—The Board have approved of a hot-air bath of a simple construction, which may be seen at the *Blind Asylum*, and made by any carpenter, price about ten shillings. Every family who can afford it ought to have one.

V. No time should be lost in sending for medical aid. But when the disease commences suddenly in a violent form, it is dangerous to lose even the time which must pass before such aid can be had. This form is at once known by sudden weakness, a contracted, ghastly countenance, blueness of the lips, and general coldness, accompanying or preceding vomiting, purging, and cramps. In such cases, the friends will without delay give a table-spoonful of the *Mixture*, No. 1, with 60 drops of laudanum, in half a wine-glassful of *cold* water. Follow this with a table-spoonful occasionally of warm spirits and water, or strongly-spiced wine. Repeat two tea-spoonfuls of the *Mixture*, with 30 drops of laudanum, every half hour, if the first dose fail to relieve.—If the mixture be vomited, then give two *Pills*, No. 3, and repeat one every half hour, if the first two fail to relieve or be vomited. But after the vomiting and cramps cease, the Mixture or Pills must not be repeated without medical advice.—The *Clyster*, No. 4, should be resorted to also from the first, and repeated once if not retained.—[N. B. The doses of the Mixture, Laudanum, Pills, and Clyster, must not be exceeded. For children of fourteen, half the doses mentioned; and for children of seven, one-fourth is sufficient.]—The *Hot-air bath*,—or, if it is not at hand, dry heat over the whole body in any shape, such as by hot blankets and hot bricks, sand, salt, or bottles of hot-water,—together with constant rubbing of the whole body,—should likewise be resorted to from the first.—The *Mustard-Poultices* should, as soon as possible, be applied over the belly and on the soles and calves, and kept on till the patient complains of their smarting.

By the time these measures have been put in force, opportunity will have been given for procuring medical advice, which is indispensable for the treatment afterwards.—The Board think it necessary to apprize the public, that where this disease has prevailed, bloodletting, when resorted to within the first, second, or third hour from the commencement of the attack, has been very generally found useful along with the other remedies, notwithstanding the

appearance of sudden weakness and excessive sinking already mentioned.

VI. By following these rules, and taking prompt advantage of the provision made at the Station-points, the Board are convinced that very many cases will be checked at the outset. But, at the same time, they strongly exhort the labouring classes to convey their sick friends with all speed to the hospitals, rather than try to cure them at home, where they can seldom have the proper means at command. The hospitals will, it is hoped, be so numerous, that one shall be near every man's habitation; and carriages, to serve at the same time as dry-heat baths, may be found always ready at the hospitals and stations, for the instant removal of patients at all hours of the day and night.—The Board feel assured, that, by quickly availing themselves of these provisions, working people will get their sick friends brought sooner and far more effectually under treatment than in any other way; and they must not forget, that every minute's delay is highly dangerous.

VII. The moment the disease is suspected to have appeared, information must be given to William G. Cuningham, Esq. Clerk to the Board, at the City Chambers. And, should it establish itself in the town, medical men are expected to send to the same quarter, every morning before half-past nine, a report of each new case, death, or recovery, —specifying the name, residence, age, employment, date of seizure, and date of the event. The Board anxiously look for punctuality in this respect from every medical person.

While the Board think it right to express their apprehension, that in certain circumstances the disease may be communicated by personal intercourse with infected persons or goods, they wish it to be distinctly understood, that, as they are satisfied it may also be diffused in other ways, not yet ascertained,—as they think it will be impossible in this country to establish a system of internal quarantine, or ab-

solute seclusion of the sick,—and as the attempt to do so would throw very many persons into great distress,—they do not mean to recommend that any attempts should be made to prevent its diffusion in this city by seclusion of the sick, otherwise than as detailed above.

N. B.—To Apothecaries.—As an inferior laudanum is sold in some shops, apothecaries are cautioned to use only the strong laudanum of the Edinburgh Pharmacopœia. Copies of this Report, and printed labels for the articles recommended by the Board, with directions for their use, may be procured by apothecaries from the Clerk.

By authority of the Board,
JOHN LEARMONTH, *Lord Provost.*

Specimens of the Labels are here annexed, for the information of medical persons and others in the country who may be applied to for the articles recommended by the Board.

No 1.
CHOLERA MIXTURE.

A table-spoonful, with 60 drops of laudanum, in half a wine-glassful of cold water. If this fail to relieve, repeat two tea-spoonfuls, with 30 drops of laudanum every half hour.

Half these doses of mixture and laudanum for children of 14. One-fourth for children of 7.

Do not exceed the doses prescribed; and stop when the vomiting and cramps cease, unless you have medical advice.

No. 3.
CHOLERA PILLS.

To be used if the mixture No. 1. be vomited. Two pills at first, and then one every half hour, if the first fail to relieve. Half these doses for children of 14; one-fourth for children of 7.

Do not exceed the doses prescribed; and stop when the vomiting and cramps cease, unless you have medical advice.

No. 4.

CHOLERA CLYSTERS.

Inject three tea-spoonfuls in a wine-glassful of thin warm gruel; and retain as long as possible by pressure below with a warm cloth. If not retained, repeat immediately, but otherwise not.

Half the dose for children of 14; one-fourth for children of 7.

No. 5.

MUSTARD FOR POULTICES.

A fourth part is enough for one person. Dust it thickly over porridge poultices, of which apply a large one on the belly, and others on the soles and calves. Remove when the patient complains much of the smarting.

I may add, that the Edinburgh Board of Health have been for the last two months very actively engaged in making every arrangement likely to mitigate the evils of Cholera, in case the disease should make its appearance in Edinburgh. The whole city has undergone a thorough process of cleaning, and every tangible nuisance has been removed. Soup-kitchens are established in different parts of the town, and clothing is in the course of being provided. Coals are also ready for gratuitous distribution. Temporary hospitals for the reception of Cholera patients are prepared in three different quarters in the most crowded parts of the city, with complete establishments of medical men, apothecaries, &c. &c. The police wards have been taken as convenient subdivisions, and medical men are appointed for each ward. Precautions have been thus taken against the worst; and the energy and unanimity with which these steps have been taken are in the highest degree creditable to the Board.

On its being reported to the Edinburgh Board of Health, that Cholera had broke out at Haddington, Dr Meikle and I, at the request of the Board, in company with Major Mac-

donald of the Central Board of London, proceeded to that town, and the following is our official report :—

On our arrival at this place, accompanied by Major Macdonald of the Central Board of Health, London, we found the Haddington Board of Health sitting, to which we were introduced by Mr Riddell, Sheriff-substitute. The medical men of the town were in attendance (five*). The letter from the Secretary of the Edinburgh Board having been read, the President of the Haddington Board assured us of their willingness to co-operate with us in any inquiries we might think necessary, and we take this opportunity of observing, that this assurance was acted upon to the fullest extent.

We were informed that no new cases of Cholera had occurred since yesterday morning, and that there had been no deaths since the 28th. That six cases remained under treatment, three of which were almost well, and the others convalescent. That the medical men were convinced the disease was pestilential Cholera; but that every endeavour to trace its source had failed. At our request the medical men accompanied us to visit their patients.

1. The first place we were taken to was the suburb called the Nungate, on east side of the river Tyne. The case was that of a woman of the town, a confirmed drunkard. She had been attacked, we were informed, with symptoms of Cholera, during the night of the 28th–29th. When first seen at half-past 8 o'clock A. M. of the 28th, she had no pulse; countenance sunk; skin cold and livid; cramps in lower extremities. Tongue white; much thirst; vomiting and purging of fluid having the appearance of meal and water. These symptoms disappeared under the use of stimulants. When we saw her (noon 30th), pulse 96, and weak; skin about natural temperature. She was *blind*, and was apparently suffering under the influence of narcotics. The blindness had just been discovered.

* Drs Black, Lorimer, Howden, Cruikshanks, and Burton.

2. The next case we saw was that of a cabinet-maker, living in a clean and comfortable house in Haddington, on the west side of the river, opposite to the residence of the above mentioned case, and about 100 yards in a direct line from her house, though, by the bridge, the distance must be a quarter of a mile. It was in this quarter of the town that the disease first broke out. This man is reported to have been attacked at 3 o'clock of the morning of the 29th. When first seen, half-past 8 o'clock A. M., countenance pale, skin below natural heat, but not cold; spasms in lower extremities, vomiting and purging of watery fluid; thirst, heat, and oppression at chest; pulse feeble and quick. Recovering under use of stimulants. No symptoms of Cholera when we saw him; no fever; had made water.

3. The next case to which we were taken was that of Wilson, a carter, a very powerful man, of a dissolute character, living in the centre of the town. He is reported to have been attacked with cramps in the extremities on the 28th, after having been engaged in sinking a well. When first seen, the cramps were severe, and he had pain at the pit of the stomach; neither vomiting nor purging; no collapse. Immediately bled to 24 oz.; and opiates continued to be administered during the night. When seen by us, pulse high and tongue foul.

In all this we of course have seen nothing to authorize us to say that Cholera prevails at Haddington, on our own observation; but, from the reports of the medical men, who have treated the cases which have occurred, we have no hesitation in saying, that these must have been cases of the pestilential disease at present prevailing at Newcastle. The case No. 1. is the only one of those we have seen, presenting the character of a patient recovering from Cholera; and we are of opinion, that, in this case, the woman is in great danger of a fatal state of congestion in the head.

Our next object was to examine the evidence on the question of the source of the disease.

We found that three cobblers had left Newcastle in

search of work on the 14th instant, travelled on foot, and arrived in Haddington on the 19th. They had not fled from Newcastle to escape from the Cholera, but to escape starvation. For though they had heard a good deal about it, no cases had occurred near to where they lived; they had never seen any one ill of the disease, and none of their acquaintance had suffered from it so far as they knew. This was the statement of one of them whom we examined, and we were informed similar statements had been made by the others. The first case, that of William Craig, occurred on the 18th, the day before these men reached Haddington. They had formerly been acquainted with this man, but did not see him after their arrival. These men live in the neighbourhood of the parts of the town where the disease has prevailed.

The Board at Haddington have only been able to trace communication among those attacked in one instance. Two of the women, whose cases proved fatal, live in different storeys of the same house, or *land*. And it appears, that although not intimate acquaintance, they had spoken to each other on the previous day, or on the day on which the first was attacked, and that on the day following the second was seized with the disease.

All the cases have occurred within a circle of about 100 yards in diameter (but No. 3), and in a low and filthy part of the town on the banks of the river.

GEORGE MEIKLE.
G. HAMILTON BELL.

HADDINGTON, 30*th December* 1831.

CASES.

The following is the Note, kindly given me by Dr CRUIKSHANK, *of the first case of the disease at Haddington.*

Haddington, 19*th December*, ½ *past* 10 A. M.

Visited William Craig, æt. 36, spare habit, and addicted to habitual intoxication. Was attacked with bowel complaint and slight vomiting on 17th, after mid-day. When

visited, had pale face; lips livid; no pulsation at the wrist; spasms and coldness over the whole body, particularly complained of in the lower extremities; eyes sunk, and surrounded with a livid areola. Tongue white, and resembled that of a dead animal, with the papilla enlarged. Respiration 23 in a minute, but not laborious, though he complained of tightness across the chest. Appearance of the egesta resembled a mixture of meal and water.

Hab. statim T. Opii gtt. xxx.
Spt. Æth. Nit. ʒij.
Aq. Ammoniæ gtt. xv.
T. Valerian. Ammon. ʒij.
Aq. Menth. Pip. ℥iij. M. pro haust.
Enema cum G. R. Assæfœtid. gr. xxx.

6 P. M.—Repr. haust. cy. vini rubri cal. ℥iv.—No improvement.

20th, 11 A. M.—No improvement.
Habt. pil. opii cont. gr. ij.

Evening.—He died.

The attack of this disease cannot be traced to contagion, as, after the most minute inquiries, it cannot be ascertained that the patient had any communication with any person from infected districts.

(Signed) JOHN CRUIKSHANK, *M. D.*

The following Cases have been kindly communicated to me by my friends Messrs Meikle and Stevenson.

Four Cases of Cholera Asphyxia treated at Haddington by Drs BURTON *and* LORIMER *of Haddington, and Messrs* MEIKLE *and* STEVENSON, *Surgeons, Madras Establishment, from* 31*st December* 1831 *to* 3*d January* 1832.

CASE I.

Margaret Thomson, aetat. 15, a delicate girl of spare habit, niece of the man Pearson who died this forenoon, and who occupied a closet adjoining her uncle's bed.

December 31.—Was affected this morning at 6 o'clock with slight sickness at stomach, followed by vomiting and purging of serous fluid; heat scarcely diminished. Pulse strong, some calomel and colocynth was given in the morning. At 2 P. M. she became much worse, and had slight spasms in the legs; she was now bled to 14 ounces. The blood was dark-coloured, with scarcely any serum; 4 P. M. she was visited by Dr Morries and Messrs Meikle and Stevenson from Edinburgh; found her tranquil, with neither vomiting nor purging. Pulse very feeble at the wrist, moderate heat of surface, which feels natural. Countenance dejected; orbits surrounded with a dark circle, exhibiting that expression so characteristic of Asiatic Cholera. Has no pain anywhere. Is urgent in her demands for cold drinks. The evacuations were inspected and found of the colour of whey, with albuminous flakes interspersed. Has passed no urine since the commencement of the attack. At the recommendation of Dr Morries, a mustard emetic was given and the following draught:—

Tinct. Opii gt. xx.
Æther. Sulph. gt. xxx.
Aquæ ℥ i.

It was instantly rejected, another was given, which was retained ten minutes.

Tinct. Opii. gt. xx. and Calomel gr. v. were given, and ordered to be repeated in half an hour. A mustard cataplasm applied to the epigastrium.

7 P. M.—Appears rather worse; very restless; and tossing about the bed; spasms in the legs; frequent retching of white glairy fluid; and vomits every thing fluid that is given.

Tinct. Opii gt. xxx.
Acid. Nitric. gt. x.
Aquæ ℥ij. M. ft. haust. statim sumend. et repetatur si opus sit. Brandy and water for drink.

9 P. M.—Much worse; constant jactitation and retching;

spasms in the leges; pulse not perceptible at the wrist; at the middle of the arm it is 120, and small.

Enema ex Tinct. Opii ℥ij.
Aquæ ℥iv.

Emp. Epispast. Epigastrio.

Calomel. gr. x.—Tinct. Opii gt. xxx. in a tea spoonful of gruel. This latter and acid for drink.

1832. January 1, 8 A. M.—Had no sleep; and has been vomiting frequently; had only one stool of a white milky colour; pulse now felt at the wrist; weak and tremulous; 120; skin moderately warm; expression of countenance more favourable.

Tinct. Opii gt. xxx. Aq. ℥i.

Enema purgans. Cal. gr. iiss. Ext. Colocynth. gr. v.

10 A. M.—Still vomiting; but improved in other respects; the injection was retained half an hour.

Repetantur Pilulæ. Ol. Ricin. ℥i.

1 P. M.—Two stools somewhat feculent and bilious; much better.

Repr. Enema.

3½ P. M.—Comatose; still retching; vomited the oil.

7 P. M.—Three dark feculent stools; has had some sleep; pulse 115; still very weak, retching diminished.

10 P. M.—Rather worse; much exhausted; circulation very languid, and skin of extremities cold; eyes suffused; blister of epigastrium was displaced, and did not rise.

To have a table spoonful of mulligatany and hot spiced brandy and water occasionally.

Ol. Ricin. ℥i.

Emp. Lyttæ Capiti, necnon Epigastrio.

2d January, 9 A. M.—Much improved; heat diffused all over, but the pulse is feeble; three very dark bilious stools, and made urine last night.

3d.—Still doing well. From accounts up to the 5th inst. this girl was recovering.

Case II.

1831. December 31st, 7 P. M.—Dunbar, a shoemaker, residing in the same house as the former patient, has been affected since 11 A. M. with purging and pain of abdomen; great oppression at the præcordia; pulse 115, full and strong; skin hot.

V. S. ad ℥ x. Cal. gr. x., Opii gr. iss.—Tinct. Opii gt. L.

1832. January 1, 8 A. M.—The bleeding produced a tendency to syncope, and the blood drawn appeared very dark, and without serum; one very dark pitchy stool this morning, much pain of abdomen below the umbilicus.

Ol. Ricini ℥ iss.

10 A. M.—Much the same: to apply a blister to the belly if not relieved.

Calomel. gr. x., Opii gr. i.

7 P. M.—Much better; pain of abdomen gone; pulse 102, softer; two or three very dark olive coloured offensive stools.

Repr. Pilulæ.

January 2d.—Much better this morning, and is now sitting up; two or three dark bilious stools.

Accounts from Haddington up to 5th represent this man doing well.

Case III.

James Wingate, ætat. 37, a nailer.

1832. January 1st, 8 P. M.—Complains of severe spasms of the legs which seized him only half an hour ago, his hands are tremulous, with starting of the tendons of the arms; pulse 115 and strong; skin warm; reported to have been drunk last night, and this forenoon vomited once or twice some fluid described to resemble saliva, or the liquor he drank. Bowels said to be unaffected.

Venæsectio ad ℥ xx.

Calomel. gr. x. Opii gr. i. Hyoscyam. iii.

Turpentine frictions to the legs.

11 P. M.—Spasms have ceased as well as the vomiting, and he seems composed; pulse 96; heat natural; a mustard emetic was given at the commencement, and is still retained.

2d.—Considerably better, no stool, has had no return of the spasms, pulse 88, heat natural.

Calomel and Jalap.

3d.—Improving.

5th.—Doing well.

Case IV.

Lawrie, an old sailor, a man of dissipated habits, has been ill of diarrhœa for the last eight days.

January 1, 11 P. M.—Being called by Dr Lorimer, visited this patient. Is in a complete state of collapse, skin cold, and pulse imperceptible at the wrist or temporal artery. Countenance cadaverous. Was taken ill suddenly this afternoon at five, and seen by Drs Lorimer and Fyfe, who prescribed a mustard emetic, which operated copiously, and a cataplasm of the same was applied to the feet. At this period the pulse is stated to have been distinct and frequent, and skin warm. The veins are now so shrunk it is impossible to bleed. His wife has been giving him large quantities of cold water to drink, which is strictly prohibited.

Tinct. opii gt. x

Calomel. gr. x.

A sinapism to the spine and a blister to the belly, brandy and water, ammonia, &c.

2d.—Slight reaction, but no pulse is perceptible at the wrist; heat is nearly natural over the whole body. Died in the evening.

Remarks.—Of these four cases, the girl Thomson and Lawrie are the most marked examples of Asiatic Cholera. Wingate's was also decidedly so, and cut short by prompt early treatment, the lancet being the chief remedial agent in this case. With regard to Dunbar some doubts may be entertained; but, in our opinion, it was a case of incipient Cholera, as Pearson, the uncle of the girl, died in the same house that day, and he appeared considerably fatigued, by having attended upon him, and much agitated in mind. I have reason to believe, that, in most cases, there is a pe-

riod of febrile excitement some hours previous to the accession of the symptoms of vomiting and purging, and that it is frequently overlooked both by the physician and patient. Bleeding in this stage, in general, prevents the disease appearing in its usual form, which is, in the majority of cases, a state of collapse.—*Quer.* In this case of Dunbar, what was the cause of the want of serum in the blood? It surely could not have depended on the abstraction of serum by profuse discharges from the stomach and bowels; from the former there was none, and very little from the latter. This would seem to countenance the idea, that some remarkable change takes place in the constitution of the blood, previous to the invasion of the vomiting and purging. I merely hazard this conjecture from the observation of this case, as few cases are treated at so early a period. It is not improbable that the freer use of calomel, in the Indian practice, may in some measure modify the disease, and prevent those typhoid symptoms that are the sequel of the disease, as it appears in Europe. I hold, therefore, the indispensable necessity of purgatives, combined with smaller doses of calomel, as soon as re-action is perfectly established, by which means large quantities of vitiated bile are discharged, to the great relief of the head, which now becomes affected; and it may be remarked, that, until bile appears distinctly in the stools, the patient is not out of danger. I would remark, in the case of Lawrie, the imminent danger of leaving the patient, even for two hours, until re-action is fully developed. The medical gentleman who treated him at the commencement, was persuaded that he would be in a condition to be bled on his return three hours afterwards, but in that short period complete collapse had supervened, which rendered all other means unavailing. Bloodletting should therefore be the first thing resorted to.

I believe the object of exhibiting this mustard emetic, a recent practice, is to give a temporary excitement to the heart and arteries, so as to enable the practitioner to bleed: for my own part, I must declare, that, after the pulse became imperceptible at the wrist,—a state that a great ma-

jority of patients are in when first seen, I never succeeded in abstracting blood in sufficient quantity, so as to be of the smallest benefit. Bleeding from the external jugular vein, when the extremities are cold, and pulse very feeble, would appear a more rational practice. A friend of mine had frequently recourse to that mode of extracting blood, and with great success.

With respect to the mode of propagation of this disease in Haddington, all attempts to prove its introduction from Newcastle and Sunderland have completely failed; the disease first appeared on the Haddington side of the town, close to the river Tyne; at this part the river is dammed up and stagnant. At Nungate, Mrs Macgleish was attacked, and died. Pearson's wife was attacked on the Haddington side of the town, close to the river, and died: he then went with his children and niece Margaret Thomson, and took up his residence in Dunbar's house, which is also near the river, in an ill ventilated dirty lane. Here Pearson immediately took the disease, and died after an illness of only eight hours,—his niece was attacked while he was under treatment, and Dunbar also complained that evening, but, on more minute inquiry, it was found he had been indisposed all the day before. I see no proof whatever, therefore, of contagion in this case. The day after the death of Pearson James Wingate was seized: his house is in the same lane, three houses removed from Dunbar's: all communication was denied. On the opposite side of the river (Nungate) the next case makes its appearance (Laurie) in a lane behind Mrs Macgleish's house, it is positively stated by his wife that there was no communication whatever between the people of the two houses; in fact, Laurie had been ill of diarrhœa, and confined to his house for some days. This subject is still undergoing strict investigation on the spot, and it is to be hoped some light will be thrown on this obscure subject. As far as we are warranted in inferring from these few cases, the disease would seem to be more under the influence of locality than of contagion, especially when we see people whom terror induces to shut their doors

and seclude themselves, are not exempt from that dreadful malady.

In these sentiments Mr Meikle entirely concurs.

JAMES STEVENSON,
Surgeon, Madras Establishment.

20. EAST CUMBERLAND STREET,
5th January 1832.

I have been so fortunate as to obtain Mr Steele's permission to publish the following case, which I regard as interesting, for several reasons. It is a decided case of Cholera Asphyxia. Was exceedingly well treated from first to last,—the woman's life having been evidently saved by the judicious treatment of Mr Steele. It proves, so far as one case can prove, the advantage of bloodletting during collapse, and of free purging after that stage, in warding off fever. It shews the necessity of watching for local congestion after the danger from collapse is over. And it must be considered as an undoubted instance of the disease arising spontaneously.

ADAM'S ROW, PARISH OF NEWTON.

Jan. 6, 2 P. M.—Mrs Ross, æt. 35. Complains of severe pains in the bowels, coming on at short intervals, with contraction of the muscles at the umbilicus;—has been vomiting a fluid, resembling in its appearance barley-water, and evacuating per anum a fluid more nearly resembling perfectly pure water, with a small quantity of mucus diffused through it;—complains also of sickness and vertigo, with feeling of weight and burning heat at the præcordia;—severe spasms in the feet, legs, thighs, hips, and hands;—great prostration of strength, with thirst, and an urgent desire for cold water. Features sunk, livid, and death-like, eyes dim and heavy; hands and feet cold; the other parts of the body not cold, but considerably below the natural warmth. The mouth inside is warm, but the breath is cool—nearly cold—respiration unaffected. Pulse 116, very small, at the wrist scarcely perceptible. Tongue white and moist. Has voided no urine since the attack came on.

For the last eight days she has been troubled occasionally with diarrhœa, accompanied with pains in the bowels,—last night, however, on going to bed, she felt perfectly well, but was awoke at four this morning with griping pains in the belly. She arose from bed at six, for the purpose of commencing her daily domestic labours, and felt a call to go to stool,—the evacuation was copious and natural, but rather loose. Before having time to dress, she became so sick, that she was under the necessity of immediately returning to bed;—her feet, legs and hands now became cold, and affected with spasms;—she had another alvine evacuation about two hours after the first, which was dark-coloured and watery, and since that time has had constant sickness, with vomiting and purging of the peculiar watery fluid, as stated above,—the spasms at same time becoming more severe, and extending to the thighs and hips. She had, about an hour ago, an opium pill of a grain and a half, with a glass of whisky, and at present feels rather easier.

I immediately removed about 14 ounces of blood from her arm, which was all that could be got away, and even that with much difficulty. The blood was thick and dark coloured; and during its flow she became very sick, and vomited about 8 ounces of the whitish watery fluid before mentioned. Her pulse at the same time sunk, and could not be felt at the wrist. When the retching had subsided, she swallowed a pill, consisting of 2 grains of camphor and ½ grain of opium, washing it down with half a glass of brandy mixed with water,—was ordered to repeat the same every half hour,—to apply a large sinapism to the belly, with bottles filled with warm water to the feet, legs and other parts of the body.

5 P. M.—She has had no evacuations per anum since last report, and has vomited but little; what has been ejected from the stomach, however, has still the same appearance, but is imbued with something of a brownish colour, probably the brandy or dissolved opium. Has taken six pills —has had severe cramps confined to the feet, with constant

sickness and thirst—feet and hands are now warm—pulse a little improved,—ordered to take one pill every hour.

9 P. M.—Three pills taken—has vomited only once since last visit, which was about two hours ago—felt a call to evacuate the bowels, but without effect—no urine—some cramps occasionally in the toes—complains much of sickness, thirst, and headachs. Lips blue—eyes sunk, and whole appearance of countenance *very cadaverous*. Pulse 116, a little stronger. Surface of body warm and clammy.

She was again bled to about nine ounces, when she became sick, as in the morning; her pulse *fell*, and the flow of blood stopped. Blood still thick and dark-coloured.

Pills and brandy to be continued.

Jan. 7, 10 A. M.—At one in the morning she had taken six of the camphor and opium pills as above, when they were discontinued, and one-half ounce of the mist. camphor, with five drops laudanum and a little oil of cloves given every hour. She is now improved in her appearance; has had no spasms; body of natural warmth, and covered with a clammy perspiration. Pulse 112, soft, and of natural strength. Tongue white. Has vomited none, nor has she had any alvine discharge. Took some tea and biscuit for breakfast. She is at present giving suck, but the breasts have become quite flaccid. Complains of no pain except some degree of headach, which she describes as not very severe.

Intermitt. mist. camphor.

℞. submur. hydrarg. gr. xxx.

Ext. colocynth. gr. x.

Syrup. simp. q. s. ut ft. mass. in pil. x. dividend; quarum sumat i. omni hora.

7 P. M.—Continues to improve: countenance resuming its natural appearance, but seems a little tinged. Only three of the pills taken, and no evacuation either upwards or downwards. Voided about fourteen ounces of high-coloured urine at 11 A. M., and again a smaller quantity in the afternoon. Had beef tea for dinner, and tea and biscuit in the evening.

Jan. 8, 10 A. M.—All the pills taken. Has been vomiting and purging copiously since six this morning. The fluid ejected from the stomach is tenacious, and of a dark yellowish-brown colour; that per anum is stated to have been "black and green," and in smell highly offensive. She complains of pains in the epigastric and umbilical regions. Countenance anxious and clammy; eyes heavy; conjunctiva of a yellowish tinge. Skin of natural warmth, dry; pulse 132, thready; tongue brown and moist; voids urine occasionally. States that on the afternoon of yesterday her milk was abundant, but that to-day it flows less freely.

8 P. M —Has had several very dark bilious evacuations from the bowels; and twice since the morning visit an attack of retching with the discharge of a little frothy mucus. Urine flows freely; breasts flaccid; pulse 138, thready; tongue whitish; moist. Thirst urgent. Complains of considerable pain in the epigastric and lower part of the right hypochondriac regions, where may be felt a diffused hardness, tender to the touch. She also feels it uneasy when she moves her body, and lies easiest on the left side. Appearance of countenance as in the morning. Head uneasy, but not pained. Has had hiccup twice during the afternoon, and had an attack of it at the time of visit.

Jan. 9, 10 A. M.—Countenance resuming its natural appearance and expression. Complains principally of weakness, thirst, and the abdominal tenderness: no appetite for food. Pulse 128, soft. Tongue whitish. Slept occasionally during the night. No vomiting. Two dark-coloured alvine evacuations. Bilious, but not feculent. Urine voided in much the same quantity as when in health, and of same appearance. Has no milk in the right breast.

8 P. M.—Was cupped this afternoon over the pained part of the belly, but with little success; not more than two ounces of blood having been got away; pain still continues; pulse 112; soft and weak; no alvine evacuation; other symptoms as in the morning.

Applicet. Emp. Lyttæ parti dolenti abdom.

Extract of letter enclosing above case.

"*Craighall*, 10*th January* 1832.

"In reference to the above case, I may mention, that contagion seems to have had nothing to do in its origin. The residence of the patient is at least 12 miles from Haddington, with which place I have not been able to ascertain that communication of any kind had occurred. The woman is the wife of a collier; has had eight children; is cleanly in person; of temperate habits; of healthy constitution; and in so far as propriety of conduct in every respect can constitute a ground of exemption from the disease, in so far was she entitled to consider herself safe. She has had an attack of ordinary Cholera of this country every autumn for some years past.

"It is perhaps not unworthy of remark, that upon bleeding her, the blood which flowed readily at the first, became languid in its current as the flow went on, and at last stopped altogether. That she became deadly sick and vomited, and the pulse died away; in this particular, differing, so far as I have read, from most other cases of Cholera, when the pulse is said to rise with bleeding.

(Signed) GEO. STEELE, *Surgeon*."

I may observe on this point, that it was found in India, that, unless the flow of blood from the vein became more free, and its colour improved, the symptoms described by Mr Steele very frequently followed. And there can be little doubt, had he been able to remove blood to this extent from Mrs Ross, the first bleeding would have sufficed, and little else would have been necessary to remove the collapse. Compare this with case in text, p. 159.

Mr Steele informs me, under date 13th January 1832, that the blister effectually relieved the pain and symptoms of hepatic conjestion. "She is now," he adds, "nearly well, debility being her only complaint."

FINIS.