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A

# TREATISE

ON THE

NATURE, CAUSES, & TREATMENT,

OF

SPASMODIC CHOLERA.

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BY CHARLES F. FAVELL, M.D.

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LONDON:

R. GROOMBRIDGE, FANYER ALLEY, PATERNOSTER ROW;  
G. RIDGE, KING-STREET, SHEFFIELD.

MDCCCXXII.

THE

NATURAL HISTORY OF THE

SPRINGFIELD CHURCH

THE FOLLOWING PAGES ARE INSCRIBED

TO

WILLIAM YOUNGE, M.D., F.L.S.,

*CHAIRMAN OF THE MEDICAL BOARD AT SHEFFIELD,*

*Senior Physician to the Sheffield General Infirmary,*

*&c. &c. &c.,*

BY HIS OBLIGED AND FAITHFUL FRIEND AND SERVANT

THE AUTHOR.





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## ADVERTISEMENT.

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THE following remarks were originally prepared for a society of medical men, who meet together once a week at the Medical Institution of this town.

When the report of Cholera having reached Sunderland first became prevalent, it was suggested that it would be exceedingly desirable to bring the subject forward, as one for discussion, at an early meeting of the members; and by way of introduction, I undertook to furnish some prefatory remarks. The first part of the present essay has scarcely undergone any alteration since that time, but the two remaining parts have been subsequently added.

I am aware that many of the views maintained in the following pages, have been anticipated by works which have recently issued from the press; they are not the less original, however, on that account. I entertain the same opinions at present, as to the nature of Cholera, as I ever did; what I have seen of the disease has only served more firmly to impress upon my mind the correctness of the notions which I had previously adopted.

I have not endeavoured to give any history of Cholera. Most medical men are already sufficiently well acquainted with it, and indeed it has so frequently been the subject of articles in Reviews and Magazines, that there are but few readers of the present day, whether connected with the medical profession or not, who are not to a greater



or less extent acquainted with it. Had I attempted to give even a sketch of its progress, it would greatly have increased the dimensions of the present essay, without materially adding to its usefulness.

The two most interesting inquiries in reference to the disease under consideration are, on what do its symptoms depend? and what is the most rational line of treatment? If I am right in the opinions I have advanced in answer to the former of these questions, I apprehend I shall not be far wrong in that which I have given to the latter.

*HOWARD STREET, JUNE 12, 1832.*

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A TREATISE  
ON  
SPASMODIC CHOLERA.

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## A TREATISE, &c.

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WITHIN the last twelve months there is no subject which has caused more alarming apprehensions amongst the inhabitants of this country, or more discussion amongst medical practitioners, than Spasmodic Cholera. The suddenness of its attack, and the amazing fatality by which it has been attended, are sufficient to cause general alarm. An examination into its nature, causes, and treatment, must necessarily be interesting to every class of society. To medical men in particular the subject is of vast importance, since if they should not experience the horrors of the disease in their own persons, they will at least have to grapple with its virulence in others. It behoves every one who may be called upon to administer to the relief of those who labour under this distressing malady, to prepare himself by every means in his power for so responsible a charge; and if he imagine that, from extensive reading or anxious and serious contemplation, he has acquired some views of the complaint which are not generally entertained; if he feel convinced that some remedies which have been extensively recommended are useless, or hurtful, and that others which have been almost altogether disregarded, are calculated to produce the most happy results, it is his duty to give the utmost publicity to the sentiments which he entertains. It is this feeling which has induced me at present to come before the public. Many of the statements contained in the following



essay will be found scattered through the pages of those who have already written on the disease, but if I mistake not there are some others for which I may at least claim the merit of originality. It has for some months back been my object to obtain every information in my power, and I have exercised as much care and discrimination as I am able in arriving at my conclusions. It unfortunately happens that the influence of great names—the dicta of colleges, and the reports of *boards*, have long been severely felt in medicine, to the prejudice, as I verily believe, of the welfare of the community ; and I cannot but attribute much of the ignorance which prevails on the subject of the present observations, to the unsatisfactory and too often conflicting sentiments which have emanated from these learned bodies.

It will be my object, after having briefly enumerated the symptoms which characterize an attack of cholera, to notice

- 1st. Its remote, or exciting and predisposing causes.
- 2nd. The rationale of the symptoms, and the post mortem appearances, which will also include an examination into what is called the *proximate cause*.
- 3rd. The treatment.

It has often been anxiously asked whether the disease which is at present causing such an alarming mortality in Europe, is the same as that which has occasioned such disastrous results in India ? To this question very different answers have been given ; some have warmly contended, that the diseases are the same, and others have as warmly denied it. Books and letters have been written to prove that the cholera which for some months back has been raging in this country, is nothing more than the disease which usually prevails, to a greater or less extent, in the autumnal season ; and in almost every town where it has



yet broken out, especially at Sunderland and in London, amazing pains have been taken, at its commencement, to convince the public that the horrors of the malady have been greatly overrated. Attempts have moreover been made to quiet the general alarm, by sagely declaring that the disease was familiar to Celsus and Sydenham, and other ancient medical writers. I cannot but think that this is a somewhat singular method of reconciling us to the dangers of so awful a visitation. It surely is no alleviation to *our* sufferings to know that others were similarly afflicted 150 years ago, neither is the danger of the present epidemic mitigated in consequence of the existence of a similar disease in the time of Celsus. But it appears to me that the description which is given by Sydenham, bears a much stronger analogy to the more violent forms of cholera which we are accustomed to meet with in this country, than to the affection which is the subject of the present remarks. That a disease, attended with most distressing symptoms, and a terrible mortality, is at present amongst us no one can deny ; and I think that he must be strangely blinded by prejudice, who on a careful comparison of the reports from India, with the symptoms which have been observed in the cases which have occurred in this country, refuses to admit that in each instance they are substantially the same. No doubt some differences may be perceived, but it would indeed be strange if the change of climate and the different habits of the people did not to a greater or less extent modify the symptoms of the disease. Individuals of different constitutions, we know, are differently affected by the same cause, and therefore, we may reasonably expect to find some variations in the character of the prevailing epidemic, corresponding to the peculiar circumstances of those who become its victims. We must form our judgment



from the great outlines which are drawn of this frightful malady, and not from the minute shading by which the picture is filled up. Many practitioners in this country have rashly denied that the present cholera is the same as that which raged in India, in China, and in Russia, although they never had an opportunity of witnessing the disease till it visited our shores. The opinions of such men are but of little value when compared to the statements of those who had ample opportunities of acquainting themselves with the malady in foreign parts. Those, in fact, who saw it in India, in Russia, and in Germany, are alone competent to speak with certainty on this point; and it is on the statements of such individuals, and on a comparison of the descriptions which we possess from all quarters, that every man ought to base his belief.

That many cases of common Cholera have been reckoned malignant, and that many diseases have been represented to be Cholera, which in fact bore but a very slight resemblance to it, I can readily imagine; but when the public mind is excited, and the opinions of medical practitioners unsettled, and more especially whilst there are men who are willing to exaggerate every disease for the purpose of magnifying their own pretended skill, we cannot wonder that such should be the case. The wonder would be if it were not.

In detailing the symptoms of Spasmodic Cholera, there are some which may be regarded as premonitory—these may first be noticed. The premonitory symptoms are a sense of general uneasiness—slight pain in the bowels, which are more frequently moved than usual—sickness, languor, lassitude, and depression of spirits. For some days the individual feels out of health without having any thing very particular to complain of; he is sensible that the functions of the alimentary canal are deranged,



but he is apt to attribute this to some sudden change in the atmosphere, or to something he has taken as food. He continues in this state for a longer or shorter period of time, perhaps occasionally taking a little Tincture of Rhubarb, the reputed panacea for all abdominal diseases ; till the symptoms set in with violence, and awake the sufferer to a sense of his real danger. It is greatly to be lamented that this first and mild stage of the disease is too frequently neglected. If attention were paid to it, it is probable that a great deal of subsequent suffering would be prevented, and perhaps many valuable lives spared. At the present time, no one labouring under such symptoms as have been described, ought to rest satisfied till he has had the opinion of his medical attendant. Too great a sense of our own security—too entire a reliance on the strength of our own constitution, may, by preventing us having recourse to proper remedies, prove fatal to us. It should never be forgotten that all who are conversant with the disease, have declared that the first stage is that in which medicines are most successfully administered.

In proceeding with an enumeration of the symptoms of Spasmodic Cholera, I shall quote the words of the writer of the Bengal Report, as they are given by Dr. Johnson in his work on Tropical climates, and with slight and unimportant variations, they will be found precisely similar to all the other descriptions which have been published. “The attack,” he observes, “was generally ushered in by a sense of weakness, trembling, giddiness, nausea, violent retching, vomiting and purging, of a watery, starchy, whey-coloured or greenish fluid. These symptoms were accompanied or quickly followed by severe cramps, generally beginning in the fingers and toes, and thence extending to the wrists and fore-arms, calves of the legs, thighs, abdomen, and lower parts of the



thorax. These were soon succeeded by pain, constriction, and oppression of the stomach and pericardium, great sense of internal heat, inordinate thirst, and incessant calls for cold water, which was no sooner swallowed than rejected, together with a quantity of phlegm or whitish fluid, like seethings of oatmeal. The action of the heart and arteries now nearly ceased, the pulse became either altogether imperceptible at the wrists and temples, or so weak, as to give to the finger only an indistinct sense of fluttering. The respiration was laborious and hurried, sometimes with long and frequently broken inspirations. The skin grew cold, clammy, covered with large drops of sweat; dank and disagreeable to the feel, and discoloured, of a blueish purple, or livid hue. There was great and sudden prostration of strength, anguish and agitation. The countenance became collapsed; the eyes suffused, fixed, and glassy, or heavy and dull, sunk in their sockets, and surrounded by dark circles. The cheeks and lips were livid and bloodless, and the whole surface of the body was nearly devoid of feeling.

“The disease, sometimes at once and as it were momentarily, seized persons in perfect health; at other times, those who had been debilitated by previous bodily ailment: individuals in the latter predicament generally sunk under the attack. Sometimes the stomach and bowels were disordered for some days before the attack, which would then in a moment come on in full force, and speedily reduce the patients to extremities. The urine at first was generally limpid, and freely passed; sometimes scanty, and passed with such difficulty as almost to amount to stranguary; and sometimes it was hardly secreted in any quantity, as if the kidneys had ceased to perform their office.”

To this frightful catalogue I shall only add, that Mr.



Annesley, a gentleman who has long been familiar with the diseases of India, regards "a burning sensation between the scrobiculus cordis and umbilicus" as a pathognomonic symptom. It may also be remarked, that the appellation of "*vox cholericæ*" has been given to the peculiar small and feeble whisper, in which persons labouring under the disease speak.

It is needless to dwell longer on the symptoms of Cholera, since they will again be alluded to in a subsequent part of this essay. I proceed then to notice

I. The remote or exciting and predisposing causes.

A great diversity of opinion has existed, and in fact does exist at the present time, as to the exciting cause of the disease in question; and it appears to me that the enquirers have been too intent in endeavouring to find out some *one* cause to which they might be able to ascribe it. Some we find so wedded to the doctrine of contagion, that they imagine that it is propagated in different places solely in consequence of a communication being established with the afflicted—others perceive in the state of the atmosphere the seeds of this malignant malady; whilst others again regard the earth and the waters by which it is surrounded, as the source of its existence. If I mistake not, the partial views which have thus been entertained, and the partial observations which have been made in order to support them, have tended very materially to keep the subject overcast by clouds of doubt and mystery. I am disposed to believe, that if we attribute the origin of the disease to any one of the foregoing causes singly, we shall generally be wrong; and it will be my object to make this appear, in the course of the few remarks which I shall at present hazard. The condition of the atmosphere is subject to great and sudden changes—the human frame in some degree sympathises with the variations



which it suffers, and in its frequent alternations from heat to cold, from humidity to dryness, as well as in its variations in gravity, we observe that it produces very sensible effects upon ourselves. This being the case, we need not wonder that an efficient cause of so general and devastating a malady should have been sought for in the air. But since it is found impossible to regard the common atmospherical vicissitudes as the cause of the disease, recourse has been had to two other hypotheses, in order to surmount the difficulty which otherwise presents itself.—1st. It has been stated that the disease depends on the different electrical state of the atmosphere; it has not fallen to my lot, however, to meet with any experiments which prove this altered electrical condition, even by the very supporters of the hypothesis: true it is that one or two writers have stated that it is in an electro-negative condition, but they do not tell us that they have been able satisfactorily to prove the truth of their assertions; and they bring forward no *experimentum crucis* to show that this supposed altered electrical state is not a coincidence, but a true and efficient cause of the complaint. If the electrical condition of the atmosphere is to be regarded as the cause of Cholera, how is it, I would ask, that a disease of the kind does not more frequently shew itself, since the air differs in respect to its electrical state almost as frequently as it varies in its temperature? But 2dly, it is asserted that the propagation of the disease depends upon some subtle poison which is intimately mixed with the air, or on some alteration in the chemical condition of the atmosphere, which is too minute to be detected by the senses. On this hypothesis I have only to remark, that if the chemical constitution of the atmosphere were altered, the alteration could surely be detected by means of chemical reagents. Analytical chemistry in the present day has attained so



high a degree of perfection, that even the most minute particles of foreign ingredients can be detected by the use of appropriate means, so that we should naturally expect if any such chemical alteration did exist, some of our analytical chemists would have been able ere this to have detected it. But if, as the hypothesis sagely supposes, this alteration is too minute to be detected by the senses, what proof have we of its existence? Surely we have as much right to deny that any alteration has taken place, as others have to assert it, unless they can prove the truth of their assertions by the infallible test of experiment. It may be asked, since the chemical constitution of the atmosphere is the same in all quarters of the globe, and since it is continually mixed by the winds which blow from the different points of the compass, how is it that the Cholera should not be more general than it has been; and how comes it to pass that it should not arise simultaneously in various parts of the world, instead of observing a regular progression from place to place? But I have said, that it has been maintained that the disease is occasioned by a poison which exists in the air. If this were the case, it must exist only in a certain quantity of the air—not in the whole atmosphere, for if it existed in the whole atmosphere, its effects must be generally observable and not confined to particular places. Now this partial adulteration of the atmosphere is physically impossible, unless the adulterating substance had a local origin. *Then* the poison might operate deleteriously in the immediate vicinity of the neighbourhood whence the miasm arose, and it would not affect distant places because it would speedily become so much diluted by mixing with the general volume of air as to become innoxious.—Under such circumstances the air ought not to be charged as the cause of disease and death, but the earth or the



water from which the poisonous substances arise. But granting for a moment that the poison does originally exist in the air—that it is not produced from the earth's surface by evaporation—admitting for an instant the impossibility of a part of the atmosphere being poisoned, whilst the rest is pure and healthful—allowing that this is the sole and only cause of the disease, we should naturally expect that in its progress it would observe the course of the wind, and yet we know that it does not—that it as frequently opposes the course of the wind as observes it—that it has often travelled against the most powerful monsoons, and that in certain districts it has scattered its morbid seeds in the large towns and high roads, whilst the immediately adjacent villages and bye-ways have altogether escaped. We know, moreover, that it has commenced and continued its ravages during every variety of atmospherical vicissitude—during dryness and humidity—heat and cold—summer and winter—spring time and harvest, it has shown the horrors of its countenance and done the work of death. If it depended on any peculiar condition of the atmosphere, this would not have been the case. The evidence of its ærial origin is not, in my opinion, sufficiently strong to induce us to favour the hypothesis. But although I deny that any peculiarity or alteration in the state of the atmosphere is to be regarded as the efficient cause of the disease, I would by no means assert that such change may not co-operate with other causes to produce an attack. We know very well that sudden changes in temperature frequently induce disorders in the functus of the chylo-poietic viscera. We know, for instance, that such attacks are by no means unfrequent during the autumnal season, when cold nights suddenly set in after very warm days. We have every reason to believe that these owe their origin to the variations in



temperature much more frequently than to an indulgence in fruits, &c. If then these changes are capable of producing such striking and important effects, especially on the alimentary canal, we are surely entitled to regard them capable when joined with other causes, of giving rise to effects of a much more alarming nature. But let it be distinctly understood, that I by no means wish to convey the idea that any peculiarity in the condition in the atmosphere is of itself sufficient to occasion Cholera, or that any such peculiarity is a necessary concurrent cause in its production. This opinion is considerably strengthened by the observations of Dr. Davy, the talented brother of the late distinguished Sir Humphry. He states that “the disease is unconnected with the direction of the winds, the topography of the places visited, or any sensible changes in the state of the atmosphere.” The idea of its terraqueous origin is, in my opinion, quite as unsatisfactory as the hypothesis which we have just examined. If its ravages were confined to any peculiarity of soil, there would be some propriety in connecting the cause of the disease with such peculiarity. But when we find that it rages equally along the alluvial soil of Bassorah and Bagdad, and the sandy one of Arabia—on the summit of Mount Ararat, and on the plains of Orenberg, we have sufficient evidence to satisfy any reasonable mind that peculiarity of soil or terrestrial exhalation cannot be the sole cause of its existence. But as an evidence of its miasmatic origin, it has been stated to spread chiefly on the banks of rivers. We must remember, however, that the banks of *all* rivers are not equally visited by the pestilence—that the vicinity of *navigable* rivers suffer most, and it may be possible to connect this fact with other circumstances calculated to throw some degree of light upon the subject. This we shall refer to afterwards. We may,



may, however, just ask the supporters of the doctrine of miasm, how it happens that the inhabitants of the neighbourhood of the still small lake whose waters have not depth enough to bear on their bosoms vessels calculated to carry on the traffic of the country, are not nearly so liable to the disease as those who live in the vicinity of the more navigable rivers? If the cause of the malady is the exhalation which is constantly going on, does not evaporation or exhalation go on from the surface of all rivers—*cæteris paribus*—to an equal extent, and with equal rapidity? But again—if exhalation were the real efficient cause of the disease, since evaporation is caused by heat and influenced by barometrical variations—how is it that we find it originating and spreading in spite of the one and in defiance of the other? Nay, if putrid exhalations be the cause of the disease, how can its origin be accounted for in those situations where there are no vegetables to decay and no waters to evaporate? We know very well that exhalations similar to those to which I have referred, are capable of inducing many diseases, of materially impairing the health and vigour of the body, and therefore of rendering us much more liable to the attacks of various maladies. It is exceedingly probable that they may *predispose* us to attacks of Cholera, but it certainly is by no means established that they are its sole and only cause. Putrid exhalations, however, together with a close and confined atmosphere, may be the means of inducing malignant and contagious diseases, as was the case in the remnant of the army of Sir J. Moore, after their defeat at the battle of Corunna; and it is at any rate *possible* that errors in diet, mental anxiety, and the depressing passions joined to such other circumstances as have been enumerated, may be sufficient to occasion the origin of Spasmodic Cholera, or at least to materially increase the degree



of its fatality when it is once established. Thus we find, that its first appearance at Jessore, 100 miles N.E. of Calcutta, was attributed to an intemperate use of rank fish and bad rice; and during the time it prevailed at Ceylon, a place peculiarly subjected to the influence of noxious vapours, and which is exposed to frequent and sudden changes of temperature, it was attended with a degree of mortality, greatly exceeding what it generally occasions. It is notorious that the disease generally attacks those who live in close confined, ill ventilated situations—those who have been subjected to previous fatigue of body or mind—and those who have impaired their health by an indulgence in habits of intemperance. Those on the other hand, who live in open, airy situations, and whose habits are temperate and cleanly, either altogether escape, or suffer but little by comparison: as for example, when the Cholera visited Tripoli, which is described as a very clean well ventilated town, there only were 31 individuals taken ill out of a population of 15,000 souls, and only 5 died. A remarkably striking instance of the deleterious effects of closeness, and a too crowded population, is mentioned by Mr. Jukes. A case of Cholera occurred in one of the apartments of the Barracks with which he was connected, and terminated fatally: another case occurred in a few hours afterwards, and the subject was saved with great difficulty: in the course of the succeeding nine days, nine other individuals became similarly affected in the same apartment. Mr. J. then made an inspection of the state of the ward, when it appeared that it was both ill ventilated and too much crowded; the place was immediately emptied, scoured, and fumigated, after which no case occurred. In the instance which is here related, it does not appear that the affected individual had had any com-



munication with persons who were known to be labouring under the disease, and its origin in Jessore and other places cannot be satisfactorily traced to communications with those previously affected; in fact, Jessore was the place where, in 1817, the Spasmodic Cholera originated.

That the disease may arise spontaneously in places where the inhabitants have been predisposed to it by local circumstances, or by individual excesses, I think can scarcely admit of a question. Circumstances which debilitate the body—which depress the mind—which lessen the force of the circulation, and which impair the energies of the nervous system, will render us obnoxious to the aggression of various diseases; and it surely need not surprise us if some cause, trifling in itself, may be sufficient in such a habit to excite a disease full of danger and of death.

I shall not now stop to detail what the different exciting causes under such circumstances might be, but rather proceed to enquire whether, when the disease is once established, it can be communicated from one individual to another, or in other words whether, from the evidence with which we have been furnished, we can reasonably admit its contagious nature. I need scarcely remark that great difference of opinion exists among medical practitioners who have witnessed the complaint as well as amongst those who derive their information from other sources, on the question which is now to engage our attention; but if I may hazard the remark, it appears to me that the chief cause of the difference of opinion depends on the partial views which the partizans of each side adopt. It unfortunately happens that we are too apt to get blinded by prejudice—to espouse a certain side of a question, and then anxiously to look out for every statement which favours our own opinion, whilst we disregard



whatever may be urged by those who entertain different sentiments. We should not forget, however, that truth but seldom lies in extremes—"in medio tutissimus ibis."

It seems to be thought by some writers that if the disease be really contagious, it must in every instance originate from an exposure to substances, or persons in whom the contagious matter is contained. And it is urged by those who oppose the doctrine of contagion, that if such be the nature of the disease, all persons ought to be affected who are brought within the sphere of its operation. Now in the first place, if we are to argue from analogy, we must deny the truth of the former of these suppositions. Many diseases which are most undoubtedly of a contagious nature, are not capable of being communicated at their origin; and many others whose contagious nature is admitted, arise independent of any communication with those who had been previously affected. As an instance of the truth of the former of these statements, I would refer to cases of fever. We will suppose a medical man is called to visit an individual who is ill of fever, which puts on a slightly typhoid aspect; he recovers: after some weeks the same practitioner may be called on to see some other patient living in the same neighbourhood, who from exposure to cold has got an attack of fever; this resists all the attempts to cut it short—it runs on for some time, and at length symptoms of typhus, with muttering delirium, make their appearance, and in the end the disease puts on the character of real malignant typhus; as soon as this is the case, other individuals who have been about the patient become affected, and they take the very disease which in its milder form was not capable of being communicated from one person to another—as it increases in virulence it spreads from individual to individual, and in the end be-



comes an acknowledged epidemic. In such an instance, then, a real contagious disease originated, how? Not from contact of others, but from cold; and the disease which was not contagious at its commencement, became so during its progress. We have surely many other examples of diseases which spread endemically, or epidemically, and which are generally allowed to be of a contagious nature, arising without the subjects of them being previously exposed to the contact of individuals already labouring under the affection, as the small-pox, measles, &c.

But then, it is urged 2ndly, that if Cholera be contagious at all—if it be capable by such means of being propagated from place to place, and from individual to individual, all those who come within the sphere of its operation—all who are exposed to its attack by being in communication with those who are affected, ought themselves to be seized with the malady. But we should remember, that all persons are not equally liable to the attacks of any disorder; the systems of some individuals have the power of resisting disease to a much greater extent than others. Two persons, we will suppose, go to visit an individual labouring under Cholera; one becomes affected, and the other escapes; are we to infer from the escape of one, that the disease is not infectious? Surely not; the only inference we can legitimately draw is, that the one is much more susceptible of disease than the other. In like manner two individuals are equally exposed for a similar length of time to the influence of cold and moisture; one becomes ill and is laid up of a fever, and the other remains in perfect health: are we, therefore, to conclude that the cold and moisture were not the cause of the illness of the patient? Again, two persons expose themselves to the influence of venereal virus; one becomes infected, and the other does not: are we to infer from



this that the symptoms of him who labours under venereal disease were not occasioned by the virus? Or lastly, two persons wound themselves whilst dissecting the same subject; each takes similar precautionary means, but one has inflamed absorbents—axillary abscess, and sympathetic fever, whilst the other suffers no inconvenience: are we to conclude in such a case, that the matter absorbed by the wound was not the cause of the symptoms of the first? The only conclusion to which in such cases we are warranted to come, in my opinion is, that all individuals are not equally susceptible of disease—that it requires a state of predisposition in order that we may become its subjects. I may receive a wound during dissection to day, and be no worse in consequence; but I may receive another wound under precisely similar circumstances next week, and in consequence of being indisposed at the time, very unpleasant symptoms may result. In the same manner I suppose it is that some individuals escape who are exposed to the contagious matter of Cholera, and others become affected by it. If the exemption of some and the seizure of others is not to be accounted for on this principle, I confess that I am at a loss how to explain it. But it is asked, under any circumstances can Cholera be communicated from one person to another? In my opinion it can. It would be inconsistent with the object of this essay to attempt to give even a sketch of the rise and progress of the disease; and, therefore, I shall content myself by making one or two observations in proof of its being capable of spreading from place to place, by intercourse with the afflicted, without pretending to give any thing like a regular history of its ravages.

We find that it spread gradually from Jessore, the place to which I have before referred as that in which the Cholera commenced in 1817, to Calcutta, and from Calcutta



it spread to neighbouring places. It got to Orenburg in 1829, in consequence, it is supposed, of a man labouring under the disease being brought into the hospital;\* at first it spread gradually through the town, but its progress subsequently became more rapid, till out of a population of 11,000, 1,100 were seized and 200 perished.—After leaving the capital, it spread through the provinces; 3,590 individuals became affected by it, and 865, or nearly a fourth, died.

Captain Sykes writes thus from Punderpoor, in 1818: “It has made its way independent of a S.W. wind from Jaulna to Punderpoor. Its effects were not instantaneous in the country, but its progress may be traced by a slow advance to from 15 to 29 miles a day, as if it had been communicated gradually by persons travelling from town to town. In my Light Company there were three or four men taken ill at once; of course there were attendants upon these men from the same Company, and there have been more cases of Cholera in it than in any other. One of my servants was attacked, and it gradually extended to five. An officer at Punderpoor had seven servants attacked one after another; the gentleman in the next tent had none.” Mr. Coates, in a letter to the President of the Medical Board at Bombay, speaking of the disease in the same place says, “At Punderpoor, it made its appearance at the time of the great Jatra, and was spread at once in all directions by the pilgrims returning to their homes; the number of deaths was 3,000 in a few days.” In 1819, the epidemic broke out at the Mauritius, and it was supposed to have been brought there by a vessel from Ceylon. There has been some doubt and difference of opinion expressed on this subject, but the great mass of evidence is

\* It must be confessed that there are very different accounts given, and that its origin at Orenburg is enveloped in considerable obscurity.



in support of it having been thus imported. It is at least certain, that the disease commenced a short time after the arrival of the *Topaze* frigate. The isle of Bourbon is only 40 leagues distant from the Mauritius, and on the announcement of the disease having shown itself at the former place, means were taken to cut off the communication between the two places ; for some time the island escaped, but a number of slaves were clandestinely disembarked at St. Dennis, and immediately afterwards the disease commenced. In 1823, coincident with the Burmese war and the march of our troops from sick districts in British India, the Burman Empire became affected. In 1820, the King of Siam alarmed by the eruption of Cholera in his capital, convoked his subjects to the sea shore, in order to join in a religious exercise. It is stated that 7,000 persons perished on the spot, and that with the arrival of the fugitives, the Cholera also appeared in all the districts which they visited. The malady broke out at Aleppo, and raged with considerable violence ; about 200 inhabitants left the town and took up their residence in a large garden at some distance. Not one of these individuals was attacked. The disease commenced in Moscow, it is believed, in consequence of a person bringing it from an infected place. It spread rapidly through the city, and from thence into the provinces ; the capital ultimately became affected ; the city prison in St. Petersburg for some time escaped, in consequence of no communication being allowed between the inmates of the prison and those residing in the town ; but a woman, suffering from syphilis, was at length allowed to go out for the purpose of obtaining medical assistance, she became affected with the Cholera, and it subsequently seized the great mass of the prisoners. In the report of Drs. Russell and Barry, it is stated that a *Cordon Sanitaire* was established at Hesse-



burgh ; this for some time kept the neighbouring country free from the disease, but in consequence of the quarantine regulations having been broken by the serfs returning from Petersburg, in three days after these regulations had been set at nought, the malady commenced in the villages. It broke out in the Russian army, and it was carried by the soldiers from Russia to Poland ; all the intermediate towns through which they passed became affected, and the Poles themselves after having achieved a victory over their enemies, paid dearly for the encounter by the invasion of Cholera.

It would be an easy matter to multiply to almost any extent examples of the disease spreading from one place to another, and to show how strikingly in the majority of instances its origin coincided with the arrival of some one labouring under the complaint. This, however, would extend the present essay much beyond the limits by which it should be bounded. The few instances which I have already cited, so far as they go, are sufficiently convincing. But we have got the disease at present much nearer home, and it is interesting to enquire how it got to this country. There are three opinions maintained as to the origin of Cholera at Sunderland. One class of individuals believe that it was brought from Hamburgh by a vessel—another that it was wafted across the German ocean on the wings of the wind—and a third, that it had entirely a local origin. It is somewhat difficult to prove which of these statements is absolutely correct. We may make one or two brief remarks on them *seriatim*. At the time when the Cholera first broke out at Sunderland, it appears that the quarantine regulations which had been adopted at other ports, were by no means rigorously observed there. But there is certainly no evidence to shew that any vessel, *having sickness on board*, arrived at a time immediately



preceding that at which the disease commenced. This fact, however, by no means proves that the disease was not introduced by an infected individual; it only shows that such an one might have evaded the sanitary cordon. There are numerous instances which might be adduced, of persons successfully evading the most vigilant quarantine regulations.

We are told that the King of Prussia and the Emperor of Austria abolished their sanitary lines, because they found that they could not by them effectually prevent the ingress of infected persons into the different healthy parts of the country. It may be remarked that these were inland cordons, and that it is a much more difficult matter to prevent communication between inland towns than between places which are separated from each other by large rivers, or an arm of the sea. This we admit, but at the same time we maintain that it is by no means difficult to evade quarantine at sea. We have already shewn that when the disease was at the Mauritius, although the most active means were taken to prevent it being communicated to the Isle of Bourbon, a number of slaves were *smuggled* over to St. Dennis, and the epidemic commenced immediately after. So, in this country, although it may be very difficult to prove that the disease was so imported, it requires no great stretch of credulity to believe that it is possible it might be introduced by similar means. So long as self-interest is an influential motive with mankind at large, we shall always find individuals ready to brave any dangers, or to place in jeopardy any community, if by so doing, they can secure to themselves what they consider a more than equivalent good.

2ndly. With respect to the disease having been brought by the wind, it need only be observed that we have no evidence to prove it, whilst the strong probability is that



it was not so communicated. In the first place we have no evidence to shew that it *can* be thus propagated. In other places we know its origin could not be accounted for in this manner: nay it has travelled from place to place directly in the "wind's eye," and, as already observed, in opposition to the most powerful monsoons.

In the third place, supposing that the morbid seeds do exist in the atmosphere—supposing that they are conveyed from place to place solely by its influence, how happens it that they were all scattered at Sunderland? Perhaps they were too heavy to be carried further; and since the attraction of gravitation increases as the square of the distance diminishes, it is possible that they may have been gradually approximating towards the earth during the whole of their journey from Hamburgh, and that when they arrived at Sunderland, the force of attraction was sufficient to bring them to the ground. Or, perhaps, the disease did not appear elsewhere, because the soil on which the seeds fell was not sufficiently nutritious to favour their germination!

Fourthly, it is stated that the Cholera at Sunderland had a local origin. This is probable, but even admitting it to be absolutely true, it is not a fact which ought to lessen our anxiety respecting it. We believe that the disease had a local origin at Jessore, but it very speedily acquired a contagious character, and it spread from Jessore to distant places. So at Sunderland, it gradually extended from the obscure point where it first originated, to the surrounding villages and towns. In Sunderland and Newcastle, its ravages were chiefly confined to the lower orders. This appears to have been the case in almost every place to which the pestilence has yet extended.—Few, very few are the instances mentioned, where any of the higher classes of the community have been affected by it.



Such are a few of the facts which have induced me to believe that Cholera is really of an infectious or contagious nature. I am aware that there are strong arguments advanced by those who maintain a different opinion, and we may very cursorily notice one or two.

First, it is stated that a contagious disease ought to be more general and regular in its attack. Certain districts, for instance, have escaped infection, whilst those in the immediate neighbourhood have been labouring under the disease. It is considered that such a fact is incompatible with the laws of contagion. But I would suggest that we know but very little about such laws ; true, we are in the habit of talking of them, but I apprehend it would be a difficult matter to lay them down with any thing like nicety and precision. At any rate, if we cannot account for such exemption on the supposition of the contagious nature of the disease, we can by no means clear away the difficulty by having recourse to the idea of atmospherical influence. Every individual, as I have before remarked, is not equally susceptible of disease, and what is true of individuals, is in this respect true of districts, and therefore we can imagine a particular spot, situated in the very midst of pestilence, escaping altogether.

Secondly, it is asserted that if the disease were contagious, more of the attendants on the sick ought to have been affected by it. We should recollect, however, that attendants on the sick, and especially medical practitioners, are not, generally speaking, so susceptible of diseases as persons of a different occupation. How frequently do we find medical men exposing themselves to malignant fevers, which if not really contagious, at least are epidemic ; and yet how seldom does it happen that these individuals become the subjects of their attack ! From the fact of their escape in such instances, we do not argue



that the disease is not contagious ; we admit that it is so, and yet we observe the medical attendant in daily contact with the sick altogether escaping its infection. This may perhaps be in consequence of his taking abundant exercise in the open air, and not being depressed by the influence of *fear*. But on whatever principle the circumstance may be explained, the fact itself is indubitable. With respect, however, to the affection which is more immediately under consideration at present—Cholera—I am not exactly aware that the attendants on the sick have escaped in greater proportion than others. To assert that they have, is assuming what it will be found no easy matter to *prove*. If I am not greatly mistaken, the number of such individuals who were seized in some places, was far greater in proportion than those who were not so employed. And at Moscow, where at one time, if not at the present moment, the medical men were decided anti-contagionists, and would consequently, we may naturally presume, have been glad to have availed themselves of such a statement in favour of their opinions ; we find them not only not vaunting it, but actually averse to furnishing information on the subject to those who asked them. Dr. Walker distinctly states in one of his reports, that he had been under the necessity of lodging a complaint in an official quarter, that he could not get an account of the exact number of hospital attendants who were taken ill. An unwillingness to furnish information under such circumstances, may generally be regarded as an indication of weakness, I had almost said an avowal of error on the part of our opponent.\*

\* "Out of the 264 medical gentlemen in St. Petersburg, 25 have been seized," says Dr. Russell in a private letter dated Petersburg, July 27, 1831, "and nine have died ; and four others have died at Cronstadt, out of the small number residing there." Dr. Solomon also says expressly, that "of the attendants in the hospital very many died, and few remained altogether exempt—of the women none." The same physician further states, that "three physi-



But thirdly, it is stated that if the disease were contagious, its progress should be stopped by quarantine regulations, and that the Cholera has spread independent of them. Now we have previously remarked that a contagious disease may commence independent of personal communication with any who were previously affected. If this be true, and I think we have sufficient evidence of it, the mere fact of the disease having broken out in places, between which and affected situations there was no kind of communication, cannot be urged as a powerful argument against the doctrine of contagion. What, indeed, do we mean by contagion, but simply that a disease may be communicated from one individual to another by contact? We surely do not mean by it a disease which cannot originate otherwise. I see no difficulty, nor no impropriety, in supposing that a disease may arise spontaneously in a place, and subsequently, having acquired a contagious character, be conveyed from one place to another by infected individuals. If this may be the case in one situation, it may be in another, or in several, and the malady may break out in these different places either simultaneously or consecutively; so that the mere fact of the disease occurring in places to which it could not have been conveyed by persons from previously infected districts, cannot be legitimately considered a valid objection to the idea of the possibility of its being communicated from one individual to another. With respect to the efficacy of quarantine regulations in stopping the ravages of disease, I may only remark, that such regulations are oftentimes and easily evaded. It is a difficult matter to cut off all communication between two towns, or provinces, or countries, and the difficulty will be proportionate to the degree of

cians in Astracan had the most completely formed Cholera, and the rest lost blood for symptoms which indicated the commencement of the disease."



traffic which has previously been carried on between them. If the inconvenience be severely felt, there is sure to be some one found with sufficient daring to break through the sanitary laws which it had been deemed expedient to enforce ; and I need not remark that a small number, or a single infected individual, may as efficiently introduce the disease as a thousand.

It has been urged by the anti-contagionists, that at least 40,000 people left Moscow while the disease was rife in that city, without performing quarantine, and yet that not a single case could be traced as having been communicated by them. Observe, however, it is not said that the 40,000 persons were labouring under Cholera—nay, it is not even said that one single individual of all the 40,000 was ill of it ; and if the whole number were in perfect health, it would have been strange indeed that they should have been capable of communicating the disease. Surely it does not follow as a necessary consequence, that the 40,000 were ill because they came from Moscow—or, it is possible that they might be ill and yet not of Cholera ; or even supposing that they were so affected—that every one of them was labouring under the disease, the fact of their not having communicated it to any others with whom they might subsequently have sojourned, would only go to antagonise some one of those numerous and remarkable instances where the disease in some districts has been distinctly traced from an infected individual having entered—it would by no means prove the impossibility of its being, under other circumstances, so communicated.

But admitting the contagious nature of the disease—granting that it may thus be propagated—we find that there are some individuals who become its victims, and others who escape. Now it is interesting to enquire



how this is to be accounted for. I have already remarked *generally*, that some persons are more susceptible of disease than others, let us now for a few moments consider more *particularly* the causes which predispose individuals to an attack, and the manner in which they operate. I may previously remark, however, that I do not regard Cholera merely as an affection of the stomach and bowels; the derangement which we observe in the functions of the chylopoetic viscera are, I apprehend, generally to be regarded as a secondary affection: but this will more properly come under review in the second division of our subject. The predisposing causes which are generally enumerated are the following:—Exposure to sudden and considerable changes of temperature—a noxious state of the atmosphere,\* arising from effluvia proceeding from animal or vegetable substances undergoing decomposition—a defect of individual or domestic cleanliness—errors in diet—an indulgence in habits of intemperance,† and the depressing passions, as fear, despondency, &c.

It is a fact worthy of particular notice, that in every place where Cholera has yet occurred, it has generally commenced amongst the poor, the filthy, the destitute, and the intemperate. The higher classes of society have comparatively suffered but little from it, especially in this country. But it must not be forgotten, that although the more wealthy in this country have hitherto escaped the disease, in other places the rich and opulent, as well as the destitute poor, have fallen victims to its virulence. In France, for example, we find the Nobles as well as the peasantry enrolled on the list of sufferers; and in Mos-

\* It is stated by Mr. Searle, in his observations on Cholera, second edition, that "the severest form of the disease most frequently prevails during a rainy or moist state of the atmosphere;" and that "during such a condition of the atmosphere, remedies are less successful than usual, even in the milder cases."

† In the Astracan report of the Cholera as it occurred there, it is emphatically stated that "*drunkards when attacked by Asiatic Cholera, always sink.*"



cow, of the 8431 who died, 307 were persons of noble rank, and civil and military officers.

But how is it that, generally speaking, individuals of the higher classes of society have experienced so remarkable an exemption? Surely because they have not been exposed to those causes which have a peculiar effect in predisposing to an attack of the disease. They breathe the same atmosphere as that which surrounds the habitations of the poor, but they have not debilitated their bodies by an indulgence in frequently repeated excesses—they have not weakened the powers of their constitutions by indulging in intemperance, or wallowing in filthiness. The different effects of opposite habits of life are well exemplified by the cases which occurred at Sunderland and Newcastle. We are told by those who have visited these places, that persons who have not inspected them can form no idea of the wretchedness of those amongst whom Cholera has spread. Dr. Holland states in one of his letters to our esteemed townsman, Mr. Ward, that he has seen the bye lanes and back alleys of Edinburgh, Paris, Manchester, and London, but that he never saw during the whole course of his life such scenes of filth and misery as those which he beheld at Sunderland.—Now let it be particularly remarked, that the people who were affected with the disease were those whose circumstances are so described—whilst those who enjoyed more of the comforts of life—whose habits were regular, temperate, and cleanly, escaped. It may be asked in what manner the causes which have been enumerated, act, to be so influential as I have described them? I answer, *simply by deranging the general health.* A person whose health is so deranged, and whose constitution is enfeebled by similar causes, is much more liable to the attack of any disease than one who is differently circumstanced. This



fact is so evident, and must be so universally admitted, that we need not at present enter upon the question.—One suggestion, however, may be deduced from it, viz. that if we are really desirous of escaping the horrors of Spasmodic Cholera, we must sedulously avoid those causes which so powerfully predispose to its attack. All excesses ultimately weaken the powers of the constitution. A moderate indulgence of the pleasures of the table, as well as a moderate use of the innocent amusements of the world, is calculated to strengthen the body and to invigorate the mind; but we might as reasonably expect the fool, who busies himself from morn to night in the pursuit of some imaginary good—who finds neither delight in books nor consolation in thought, should possess as vigorous a mind as he who devotes his days to intellectual employments; as that the man who gives loose to his animal propensities, and who eats and drinks without regard to decency or propriety, should be as robust and hale as he who is habitually more frugal and abstemious.

I repeat once more, that all those causes which have been enumerated as predisposing, act merely by deranging the general health; they exert no peculiar specific action, but they render the body more liable to the aggression of any disease.

I proceed to the second division of the subject—an enquiry into the rationale of the symptoms—an account of the *post mortem* appearances, and an investigation of the proximate cause.

In the science of Physics, a knowledge of the causes which give rise to certain phenomena, is essential to their removal, and their removal is necessary for the cessation of the effects which they produce. In like manner an acquaintance with the *causes of disease* will throw light



upon the treatment, and enable the intelligent physician to prescribe with greater skill and more certainty than one who cannot trace the various complicated symptoms of a malady to their source and origin. When we come to speak of the treatment of Cholera, we shall perceive that the most opposite lines of practice have been pursued by different persons, and that we are not entitled, from what has hitherto been written, to put implicit confidence either in any particular drug, or in any particular class of medicines. Whence does this uncertainty arise? It springs from the circumstance of our being in a great measure ignorant of the pathology of the disease. If we had clear and accurate conceptions of its cause—if we could tell on what the frightful catalogue of symptoms ultimately depended, we should be somewhat more philosophical in adopting or recommending particular methods of treatment. And if the measures which different medical practitioners might have recourse to, should in some respects differ from those which others might adopt, they at least would not be of so opposite a nature as are the remedies which are at present recommended.

Perhaps the best means of acquiring a knowledge of the cause of different diseases, is a frequent and minute inspection of the bodies of the dead. But even in our *post mortem* examinations we sometimes commit great errors, by occasionally regarding particular appearances as *causes*, which would be more properly considered as *effects*. It is oftentimes a very easy matter to shew the dependence of certain symptoms which existed during life, on particular structural changes which we perceive after death; but it is not always so easy to find out the first link in the chain of causes, on which, if we may be allowed the expression, every subsequent link depended. I fear that we are frequently too hasty in drawing con-



clusions from *post mortem* appearances, without considering by what those appearances themselves were ultimately caused. This may, perhaps, in some degree be the consequence of the very limited opportunities which we enjoy of examining bodies after death. If we could more frequently inspect the viscera both in a diseased and healthy condition, it is probable that we should be better able to form a correct judgment on the particular structural changes which give rise to certain functional derangements. It is greatly to be lamented that the prejudice which exists in every grade of society—amongst the rich as well as the poor—the well informed as well as the unlettered, should, by impeding scientific pathological investigation, prevent the acquisition of the most important knowledge which a medical practitioner can possess. It fortunately happens, however, that many opportunities have been afforded, both in this country and abroad, of examining the bodies of those who have died of Cholera; and I shall now proceed, first, to detail the *post mortem* appearances which have usually been observed; and then, secondly, to examine the degree in which they account for the symptoms during life.

The most constant and important morbid appearance which has been noticed, is the large accumulation of blood in the abdominal vessels. None of the viscera appear to be necessarily *organically* affected. M. Chamberet has indeed described an inflammatory state of the mucous membrane after the disease has continued for a great length of time; but it is a condition which is not observed in the more malignant cases, those which run their course in a few hours; the violent symptoms therefore cannot in any degree be attributed to it. The mucous membrane throughout the whole length of the alimentary canal is generally in a healthy condition.



The peritoneal surface is also free from disease, and so far from exhibiting any inflammatory appearance, it is described as being particularly pale. But the vessels which ramify upon the surface of the intestinal canal, as well as the larger veins which return the blood from the inferior extremities and the abdomen to the heart, are perceived to be full of congested blood. The intestinal canal is also irregularly contracted and distended, and contains air and a considerable quantity of fluid, sometimes resembling gruel, and sometimes of a deeper colour. The coats of the stomach and bowels are sometimes thicker and occasionally softer than natural. The liver varies in external appearance but it is generally larger than natural and of a deeper colour, and its vessels are loaded with dark coloured blood. *The Gall Bladder* is almost invariably filled with tenacious bile, and the ducts remain pervious. Mr. Annesley states that although in the cases which he examined, the *hepatic duct* was pervious, the *ductus communis chododius* was not; it required considerable pressure on the gall bladder to force any bile through the common duct into the duodenum. This is an important fact which ought not to be lost sight of. *The Spleen* is generally enlarged, and gorged with black blood, and its texture has frequently been observed to be softer than natural. *The Urinary Bladder* is commonly found empty and contracted. The vessels of the brain are congested. The state of the blood should be particularly noticed; it is always found much thicker and darker coloured than natural. Mr. Annesley says, that in every dissection which he performed, he “uniformly found the *venæ cavæ*, the mesenteric veins, the veins in the vicinity of the heart, the *vena portæ*, the iliac and subclavian veins, and the sinuses of the brain loaded by thick viscid and black blood. The right cavities of the heart were generally



distended with the same description of blood, and when any was found in the left cavities of this organ, it was similar to that lodged in the right. The lungs were always engorged with blood of a pitchy or black appearance, and all the internal vessels presented a greater or less degree of congestion of blood, possessing nearly the same characters. The blood-vessels at the external surface of the body and in the extremities, were generally contracted, or nearly so." A similar condition of the sanguinous fluid is described by all who have given us any account of the *post mortem* appearances which they have observed.

Mr. Searle, in the second edition of his pamphlet, states, "that the congestion is not merely confined to the cavities, it also exists in the veins of the extremities ; but," says he, "the organs, more especially the seat of engorgement, are the stomach, the small intestines, and the brain. The next in degree are the liver, the spleen, and the lungs."

What light then do these appearances cast upon the cause of the disease, and how far do they account for the symptoms ? The first inference which we are warranted to draw from a comparison of the symptoms with the *post mortem* appearances is, that *Cholera is not a disease solely of the stomach and bowels*. We have been somewhat in error in ascribing the symptoms of Cholera to a disordered state of the alimentary canal. In this country we find disorders of the stomach and bowels particularly prevalent during the autumnal season, and we give to the class of symptoms which then present themselves the general name of Cholera. In doing so, we act in strict etymological propriety, because I apprehend, whether we suppose the term to be derived from  $\chi\omicron\lambda\eta$  and  $\pi\epsilon\omega$ , or from  $\chi\omicron\lambda\alpha\varsigma$  and  $\pi\epsilon\omega$ , we are still equally correct, since not only is there



an increased flux from the intestinal canal, but the flow of bile is also augmented. But when we give the same name to the disease which is the more immediate subject of the present remarks, we evidently fall into error, for not only does no bile pass into the alimentary canal, but *the action of that canal is not necessarily preternaturally increased, neither are the excretions necessarily excessive.* Many cases of Spasmodic Cholera are related which were not accompanied either by vomiting or purging. Although diarrhœa often precedes an attack, and although both vomiting and purging generally accompany it, yet we have excellent authority for stating, that the disease frequently occurs without being forerun by such premonitory symptoms, and that it occasionally proceeds to a fatal termination without the chylopoetic functions being sensibly deranged.\* I am aware that some individuals may object that such cases are examples of Cholera, but if not, what are they? They are ranked as Cholera by those medical practitioners by whom they were seen, and who ought, therefore, to be most competent to form an opinion. In the Astracan report it is stated, that "the disease for the most part began with purging and vomiting, to which sooner or later succeeded spasms, &c. The diarrhœa usually preceded the vomiting, and they but seldom both came on at once. Sometimes the disease appeared with diarrhœa, with or without tormina *and without vomiting*; sometimes it began with vomiting, which was succeeded by all the usual symptoms *except*

\* See Dr. Schnur's report of the propagation of Cholera in the kingdom of Poland. See also Mr. Scot's report of the Medical Board at Madras—"A frequent variety," says he, "the worst of all, is that which is noted for the very slight commotion in the system, in which there is no vomiting, hardly any purging, perhaps only one or two loose stools, no perceptible spasm, no pain of any kind; a mortal coldness, with arrest of the circulation from the beginning, and the patient dies without a struggle. This has frequently manifested itself as the prevailing type, and almost all die who are attacked by it."



*diarrhœa* ; sometimes it commenced with sudden loss of strength, *which struck the patient like a flash of lightning* ; and occasionally it was characterised by cramps alone." In short, all who have seen the disease must admit that in different cases the condition of the alimentary canal varies very much, and that in many of the worst cases it is by no means materially deranged. The most invariable symptoms which present themselves are *collapsed countenance, blueness of the lips and nails, shrunken fingers, coldness, suppression of the natural secretions, great feebleness, or total suppression of the pulse at the wrist, and prostration of strength*. Now on what do these depend ? Evidently on the state of the circulation. Dissection has shown us that the large vessels in the chest and abdomen are gorged with blood, and that the blood itself no longer presents its usual characteristics. *It is dark, thick, and ropy*. It is scarcely necessary to remark, that where the blood is imperfectly transmitted, or only partially arterialized, we shall have symptoms more or less resembling those which have now been enumerated. If from any impediment to the circulation of blood the vessels of the face are not supplied as usual, there will be a collapse corresponding to the deficiency of the circulating fluid. If the blood is only imperfectly arterialized, whether that depend on an affection of the lungs or on any other cause, we know that the small capillary vessels ramifying on the lips, &c. will give evidence of it, There will be blueness of these parts. We have a similar blueness in severe cases of hydrothorax, where in consequence of the mechanical force of the fluid in the chest, the blood is with great difficulty propelled through the lungs ; and we have the same appearance in a disease which bears the name of *Morbus Cæruleus*, and which is caused by an organic affection of the heart, in consequence of which only part



of the blood is submitted to the influence of the air in the lungs. With respect to the coldness, I need only remark that the animal temperature is somehow or other connected with the change which the sanguinous fluid undergoes in passing from the state of venous to that of arterial blood. We perhaps are not competent to speak with confidence on what this rise of temperature ultimately depends, but we do know that the function of respiration and the evolution of animal heat are intimately connected with each other. The circulating and respiratory functions also re-act upon each other, and we find the temperature rising or falling to a degree corresponding to the more or less perfect manner in which these two important functions are performed. Thus, if from any cause the action of the heart be considerably augmented, the number of respirations performed within a given time will be correspondingly increased, and the animal temperature will also rise: this is exemplified in cases of fever and inflammation. If on the other hand we forcibly increase the natural number of our respirations, we shall find that we shall also increase the rapidity of the circulation, and the animal heat will likewise in this case be augmented. But, to be brief, we may remark that the same observations which were advanced to account for the blueness, may with equal propriety be adduced to explain the excessive coldness, viz. that it is always present when there is any difficulty to the transmission or arterialization of the blood. Hence in the "blue disease" to which I have before referred, as well as in hydrothorax, ascites, the first stage of intermittent fevers, &c. we have always more or less coldness, and it is proportional to the difficulty or imperfection of the circulation.

Again, there is "suppression of the natural secretions." I shall not enter into any explanation of the function of



secretion, but merely remark that it is universally allowed, that with the exception of ONE, all the various secretions are produced from *arterial blood*. It is evident, then, that if any impediment exists to the conversion of venous into arterial blood, or to the distribution of such blood after its conversion, the function of secretion must be correspondingly impaired. If the tunica conjunctiva and the lachrymal gland are not supplied with arterial blood, no tears can be secreted; if similar blood is not circulated to the kidneys, there will be no secretion of urine,—and so with other organs. Another invariable symptom is, “great feebleness or total suppression of the pulse at the wrist.” Whenever any considerable quantity of the circulating fluid is remaining stationary in any one part of the vascular system, there must be a correspondingly less quantity circulated through the system at large, and the pulse will be the index of the degree of disturbance which exists. Lastly, great prostration of strength is a constant symptom of Cholera. We know that great exhaustion always attends those cases of abdominal affection which are accompanied by excessive discharges. Now since these discharges are for the most part derived from the blood, and since the blood is the “*pabulum vitæ*,” it is easy to conceive that they should produce a very sensibly depressing effect upon the constitution. But in the case before us, as has been already observed, we have *not necessarily* any discharge, and even when there is, the exhaustion is apparent before it has continued long, and indeed bears no proportion to it. How, then, is the exhaustion to be accounted for? If the blood be indeed the *pabulum vitæ*, I apprehend it is only the *arterial blood* which will bear that designation. And if, which is the case, the arterial blood be necessary to the support of the strength and energies of the body, then there must



be depression and exhaustion in a degree proportionate to the difficulty which exists of preparing and transmitting it. We find this to hold good in other cases. In Cholera, therefore, where we have congestion in the large veins, and where respiration cannot be efficiently performed in consequence of the congested state of the pulmonary vessels, the arterialization of the blood cannot properly take place, and the necessary effect of this is, exhaustion of the vital powers. Thus it appears that all the most formidable symptoms may be rationally accounted for from the state of the circulation—from the circumstance of the congested state of the blood in the vessels of the abdomen, &c. It may be interesting to inquire on what the condition of the blood itself depends. It is stated to be "*dark, thick, and ropy.*" It is not difficult to explain why the blood should be dark coloured. We know that venous blood is always much darker than arterial, and if from any cause an impediment should exist to its free circulation through the lungs, since it acquires its original darker hue during the course of its circulation, it is probable that it will become darker and darker in proportion to the length of time which elapses, before it can undergo the change requisite to produce an alteration, not only in its colour, but also in its more vital properties. If from any cause the action of the lungs should suddenly become considerably impaired, so that the process of respiration could be only very imperfectly performed, the changes, which through the medium of the lungs are effected in the blood, could only be very imperfectly produced. The consequence would be that instead of having a fine rich scarlet coloured fluid circulating in our arteries, we should have one possessing different properties; it would have a bluish—or if the difficulty of transmission through the lungs were great—a



blackish colour. I need scarcely remark, that this alteration in the colour of the blood would be equally produced, whether the primary cause existed in the lungs, or in the heart and blood vessels. If, from some particular cause, the vessels should suddenly lose their power of propelling the blood—or if the heart, from a sudden loss of energy, should become incapable of performing its functions, the consequence would be, in the first case—congestion in the vessels *in consequence of their inability to propel the blood*—in the second, congestion might take place *in consequence of the heart not being able to receive the blood from the veins*. In either case, there would be congestion, and the blood would possess an unusually *dark colour*. But it is not only stated that the blood is dark coloured, but also *thick and ropy*. The ropiness, I apprehend, is caused by its greater thickness—its increased viscosity. Now in what manner are we to account for its increased spissitude? For my own part I candidly confess my inability to explain this remarkable circumstance, unless we admit an ingenious hypothesis which has been advanced by Mr. Bell, of Edinburgh, not exactly when he is endeavouring to account for the peculiar state of the blood, but for the nature and source of the discharges which take place from the stomach and bowels. If the explanation which he gives is correct, I cannot be far in error. In speaking of these discharges, he maintains that “they are not secretions—they do not consist of gastric or pancreatic juices—they are not bilious—they are not the mucous secreted by the lining membrane of the alimentary canal, neither are they natural excrementitious matter.” But he affirms that they consist of *a portion of the constituent parts of the blood*. He supposes that exhalation takes place from the ends of the veins—a quantity of the serum and fibrin



exudes, and that these in a great measure form the fluids which are ejected during life, and found in the body after death.

There is one powerful argument which may be adduced in support of this opinion, viz. that in a chemical examination of the blood which has been performed by Dr. O'Shaughnessy, the salts which naturally exist in it were not detected, whilst a large quantity of saline matter was found in the contents of the alimentary canal, where it does not usually exist. The fact now stated, not only throws some degree of light upon the nature and source of the discharges themselves, but also on the subject of our more immediate inquiries, viz. the cause of the increased thickness of the blood. The saline ingredients which the blood contains, we should remember, exist *in the serum*. It is well known that if blood be left at rest for any length of time, it will separate into two parts—the serum and the crassamentum, or clot; the former consists of a large proportion of water, of albumen and of different saline matters; the latter consists solely of fibrin and the red globules. Now if any degree of exhalation or exudation does take place from the ends of veins, it surely should be of the thinner or more watery part of the blood, and the consequence would be that the remaining blood should possess a deficiency of saline matter, whilst the secretions, formed of the exhaled fluid, would exhibit a redundancy. Moreover, the blood which is left, would be thicker than natural, in consequence of having lost its watery part, and this thickness would necessarily increase in proportion as the relative quantity of serum diminished.

If we are to believe accounts, apparently drawn up with great accuracy and fairness, and seemingly without any endeavour to favour any particular or favourite hypothesis,



this is precisely what takes place.\* Thus I have endeavoured, with what success it is not for me to say, to explain the peculiar condition of the blood which is observed in the bodies of those who die of Cholera; and I have attempted to prove that all the *essential symptoms* of the disease arise from the state of the circulating fluid, or of the circulation itself.

I have stated that the first inference which we are warranted to deduce from a comparison between the symptoms of Cholera and the appearances which we observe after death, is that *it is not a disease solely of the stomach and bowels*. Having now shown how intimately the symptoms are connected with, and how naturally they spring from, one of the most usual *post mortem* appearances, viz. congestion in the large vessels, can we legitimately conclude that Spasmodic Cholera is *altogether* a disease of the circulating system? The blood is, indeed, changed in its properties, and its transmission to the different

\* The following is Dr. O'Shaughnessy's statement, which may be found in the number of the Lancet, for the 31st of December, 1834 :—"The blood has lost a large proportion of its water, 1000 parts of Cholera serum having but the average of 860 parts of water. It has also lost a great proportion of its neutral saline ingredients. Of the free alkali contained in healthy serum, not a particle is present in some Cholera cases, and barely a trace in others. Urea exists in the cases where suppression of urine has been a marked symptom. *All the salts deficient in the blood are present in large quantities in the peculiar white dejected matters.*" A more recent chemical examination of the blood taken from patients labouring under Cholera, has been made by Dr. Thomson, of Glasgow, and the result of his analysis is that there is a great deficiency of serum, and a corresponding excess of crassamentum. He states that the average proportion of those two constituents in healthy blood is nearly as follows :—

Serum ..... 55 | Crassamentum ..... 45

"But in blood drawn from Cholera patients these proportions are very nearly reversed."—It appears, however, from the average of 5 cases which he examined that the disproportion is still greater, for he says the ratio between the serum and crassamentum in these was,

Serum ..... 33.2 | Crassamentum ..... 66.8

The salts of potash and soda are evidently diminished, since according to Dr. Marcet 1000 parts of the serum of human blood should contain 8.66 of those salts, whilst 304.36 parts of Cholera serum only contained 1.98. The fibrin of the blood in the cases of Cholera appears to be remarkably deficient, whilst the colouring matter is relatively in great excess.—See Dr. Thomson's paper in No. 65 of the Philosophical Magazine and Annals of Philosophy.



parts of the body is attended with considerable difficulty, but on what does this change of properties depend—what is the cause of the impaired circulation? We find the blood in a particular condition, and we have seen the various symptoms depending on it like the different links in a chain depend on those who are immediately above them. But yet it appears that there is one link wanting, and that is the first. The peculiar condition of the circulation is the cause of much, but yet there is one thing which is the cause of it. We are, therefore, compelled to deduce as a second inference, that *Spasmodic Cholera does not ultimately depend on the state of the circulation.*

We have already seen how intimately the circulation is connected with respiration, and how materially the latter influences the former. Is it probable, then, that the function of the lungs is primarily impaired, and that the congestion of the blood and all the other symptoms result from its abnormal state? Such an opinion has been advanced, and the laborious respiration which is frequently observed, has been adduced as a proof of its correctness. But it should be recollected, that if the other symptoms are the consequence of the disturbed condition of the respiration, the respiratory organs should be invariably affected, and not only so, but *they should be affected first.* This, however, is not the case. It often happens that the function of respiration is but very little disordered. In corroboration of this statement I can adduce my own observation. I have seen numerous cases of Spasmodic Cholera, in which the breathing was altogether or nearly natural; and I can also bring forward the observations of others, whose testimony is entitled to considerable weight; thus in Bell's "treatise on Cholera Asphyxia," page 27, we read as follows:—"That it is not the result of a failure in this important function



(respiration,) 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." Dr. Abercrombie also in his little pamphlet "on the character and treatment of Spasmodic Cholera," says, "the breathing is in some cases soft and easy, in others oppressed and sonorous." And Drs. Lorimer and Burton, when describing the symptoms of the second stage, remark, "the respiration (occasionally natural) is quick or oppressed."\* Now, I apprehend it will readily be admitted, that if any particular condition of the function of respiration were necessary to the production of those ulterior changes on which the symptoms of Cholera more immediately depend, it should be universally present; and since this is not the case, we are entitled to infer, thirdly, *that Spasmodic Cholera does not ultimately arise from any peculiar abnormal condition of the function of respiration.*

Where then are we to look for an efficient cause of this direful malady? I believe the first impression is made on the *nervous system*: not on the nervous system generally, but on a particular part of it—on the ganglionic or sympathetic system. It is too evident to require particular demonstration, that the brain is not greatly implicated in the disease. The functions of the sensorium are regularly performed,—volition and sensation are unimpaired, and the intellectual faculties remain bright and unclouded till the very hour of death.† Such would not be the case if the brain were the seat of disease. The pain arising from the cramps with which the patients are affected is sometimes so excessive that we should regard it a mercy were the power of sensation annihilated, so that

\* See "Observations on the History and Treatment of Cholera Asphyxia, as it appeared at Haddington, by Robert Lorimer, M.D. and John Burton, M.D."

† See Scot's Report.



we might no longer behold the writhings, or hear the cries which are uttered by those who are enduring the most excruciating agony. Perhaps there is no situation more truly distressing, than that in which we are compelled to remain as witnesses to sufferings which we have not the power to relieve.\*

The reasons which have induced me to believe that the nervous system is primarily affected, and that the ganglionic system is the seat of the affection, are chiefly the following :—

- 1st. The influence exerted by the nerves generally,
- 2dly. The distribution of the sympathetic.

I. The influence exerted by the nerves.—It need scarcely be remarked, that the two most important functions performed by the nerves are sensation and motion. We perceive all the beauties of the external world, in consequence of the impression which light, reflected from various surfaces, produces on the optic nerve, which conveys the impression it receives to a distinct part of the brain. If the nerve should be destroyed, or the brain diseased, no light will be perceived. In like manner we hear sounds, in consequence of the impression made upon the acoustic nerve by a certain motion which is given to the air : in this case also the impression is transmitted to the brain, and should that particular part of the cerebral mass be diseased or the nerve destroyed, the impression will

\* Perhaps the reason of the functions of the brain continuing to be so regularly performed, may in some degree be accounted for from its receiving a greater proportion of arterial blood than the other parts. It is nearer the source of the circulation. We have certainly no direct evidence to show that it does actually receive more arterial blood than other parts, but the two following considerations do not render it improbable.—First, venous blood instead of arterial being transmitted to the brain will immediately cause Asphyxia; and secondly, any considerable degree of congestion taking place in the brain during life will occasion apoplexy.



either not be made upon the nerve, not transmitted by it, or not received by the brain ; in either case the sense of hearing will be lost.—We are surrounded by various substances, and we are made acquainted with the several mechanical conditions of bodies, as hardness, softness, roughness, smoothness, &c. by the different impressions made by them on the nerves. In short we are connected with the world by means of the different nerves with which our organs of sense are endowed. But we are not the less indebted to nervous energy for the power of motion than for that of sensation. The muscles, we know, are the instruments by which we are enabled to move about from place to place, but they are dependent for the exercise of this power on the nerves which are distributed to them. The muscles are divided into two great classes—the voluntary and the involuntary ; the former are called into exercise by the will—the latter usually act independent of the will, but they may be influenced by it. In each of these instances the nerves are the instruments by which, if we may so speak, the mind makes known its desires to the muscular system. For instance, I am at present seated at my desk, but I may suddenly form the determination of rising from my seat and walking into the next room. In order that I may rise, the several muscles which maintain the trunk erect upon the extremities, must be made to contract ; the first step in the process is to form the determination,—to exercise the will, then an influence is transmitted by the nerves to the muscles, which must be brought into action—these muscles contract, and they pull up the body. I have said that an influence is transmitted by the nerves ; I cannot pretend to explain the nature of this influence, however, call it nervous energy, nervous fluid, or what you will ; the fluid has never been seen, and the energy is perceived



only in its effects. It is useless to spend time in the discussion of idle speculations; the only thing which is really important, is the fact that the nerves are in such a case *essential*; divide the nerve going to a certain muscle, and however desirous you may be of causing its contraction, you will not be able to do so without having recourse to mechanical or chemical means. But the dependence of muscular contraction on nervous influence is so generally known and so universally admitted, that we need not spend time on its elucidation. I proceed, therefore, to remark that the nerves have a very considerable influence over the circulation. We know that although the heart possesses no *large* nerves, it is nevertheless supplied very freely with small ones, and that a mesh or net work of nerves surrounds the different arteries. Aware of this, and knowing the influence which the nerves exert in other parts of the system, we might reasonably *a priori* conclude that they would modify to a greater or less degree the condition of the circulation. But we have other reasons for believing that such an influence is exerted; we know that the circulation is very materially influenced by mental emotions, and we have every reason to suppose that such emotions act through the instrumentality of the nerves: for instance, some unexpected joyful intelligence is communicated to a person, and immediately his countenance brightens—his respiration becomes quicker—the pulse more bounding—his step more vigorous, and his whole body more elastic; on the contrary, if some doleful news is brought to an individual—if a merchant should be told that a house with which he had considerable dealings had suddenly stopped payment—a farmer that his stacks were burnt down—or a father that his only child had unexpectedly died—the effect produced upon the system would be directly the reverse of that



to which I have just alluded. His countenance would become pale and ghastly—his eye would lose its fire—his pulse would fall in force—his limbs would totter, and his voice would falter. It has been stated that all this would be in consequence of the organs of respiration not properly performing their functions; this, however, is a mere assumption. That the respiratory organs would be affected is true enough, but that they are the first link in the chain of sympathies which we observe, is not quite so evident. It has been before observed, that the condition of respiration and circulation mutually influence each other; if the former becomes suddenly oppressed, the latter will also suffer oppression, and vice versa; and it signifies but little to our present argument which of these two functions in the instances above cited, becomes affected soonest.

*At a period antecedent to the manifestation of any alteration in the condition of either of the functions before alluded to, an impression has been made on the nervous system—this impression is then communicated by the nerves to the organs of respiration and circulation, and we have that peculiar train of effects produced which have been already enumerated.* It may be asked where is your proof that the actions take place in the manner now described? Perhaps I may answer that it will not admit of absolute demonstration, but that we have as much reason to suppose that this is the case, as others have to deny it. We know, however, that in such cases the mind is primarily affected, and the mind in other instances acts on the system at large through the instrumentality of the nerves—we therefore reason from analogy that it does so under the circumstances which have been already referred to. It may be asserted that the first external mark of mental perturbation which we observe, is a long and deep



inspiration ; perhaps so, but it should be recollected that the mental affection has previously manifested itself—that the deep inspiration is merely a sign, a symptom, an evidence, an effect, of the state of the mind, and this effect I maintain is produced through the agency of the nerves. But admitting that the nervous system first suffers in the instances which have been already adduced, it will be a question whether the respiratory or the circulating system comes next in order. I think this a matter of but little importance, but I should decidedly incline to the opinion that the latter is generally first acted upon. There are many instances at any rate, in which we cannot conceive it to be otherwise : as in blushing—in the influence which a certain train of ideas has in determining blood into the corpora cavernosa—in the increased salivary secretion which follows the smell of savoury meats, &c. In such cases we have a certain affection of the mind immediately followed by an increased local circulation—the locality being determined by circumstances, which we shall not at present attempt to explain ; but we have no quickened respiration ; in many instances we have the effect produced without the general circulation being increased—thus in blushing, the cheek is mantled over with crimson in a moment, whilst the blood is not moving with more than its usual velocity in other parts of the body.

These few familiar illustrations will be sufficient to show the influence which the nervous system has over the circulation. Perhaps, also, since the present is not an essay on nervous influence, sufficient has been advanced on the general agency of the nerves. A word or two might be said on the manner in which they influence secretion, but on this subject I shall only at present remark that they do not *directly*, but *indirectly*, by the effect which they produce upon the circulation, influence the



various secretions. I have at present alluded to only one of the functions of nerves—the power which they possess of exciting different organs to increased action. I pass on briefly to observe, that it is through their agency that the different parts of the system sympathise with each other. The sympathies which take place between distant parts in health and disease, are too evident to need particular enumeration ; or if the connexion which exists between remote organs should, during robust health, be unobserved, as soon as one of those organs has become deranged, it will be made manifest ; thus we have a pain in the knee in cases of diseased hip joint—a pain in the shoulder in hepatic affections, and a pain in the thigh, or in the testis, in cases of nephritis. In uterine affections, we have derangement of the stomach—in intestinal worms, itching of the schneiderian membrane—in some diseases of the stomach, a distressing cough. We might also instance the influence of fear on the different sphincter muscles. This particular affection of the mind operates on these muscles, I apprehend, in a manner similar to that in which other mental affections affect other parts of the system, i.e. through the medium of the nerves.

I proceed to make some remarks, secondly, on the distribution of the sympathetic. The origin of this nerve has given rise to more discussion than any in the human body, and it appears that even at present anatomists are by no means unanimous in their opinions ; for Monro, in his *Elements of Anatomy*, vol. ii. p. 489, distinctly states, “upon this subject anatomists are by no means agreed.” During its course it forms numerous ganglia, which are supplied with small arteries, having corresponding veins, and becomes connected with nerves supplying various parts of the body ;—hence the *reason* why such an exten-



sive chain of sympathetic action should be observed in morbid affections of any portion of this nerve, or of the parts to which it is distributed. It forms ganglia with the intercostal nerve, from which branches are derived to supply the extensor muscles of the body. It also sends branches to the posterior pulmonary plexus. The 6th, 7th, 8th, and 9th thoracic ganglia concur in forming the greater splanchnic nerve, which is distributed to the abdominal viscera; and from the 10th, 11th, and 12th dorsal ganglia proceeds the lesser splanchnic, which unites with the former. The nerves of the chylopoetic and assistant chylopoetic viscera, are derived from the sympathetic, the par vagum, and the rami splanchnici; the latter, we have already seen, are derived from the former. Thus we perceive that all the viscera of the abdomen are supplied with nerves from the same source—no wonder that in their different morbid affections, they should to a greater or less degree sympathise with each other. But we have before observed, that the peculiar symptoms which characterise Cholera do not depend on any particular condition of the chylopoetic viscera, but more immediately upon the state of the blood. It has been shown, I think with tolerable clearness, how directly the most prominent and urgent symptoms depend on the condition of the circulating system; the only difficulty appears to be in accounting for the cause of the congestion. It could not take place without a cause. We have seen how greatly the circulation is influenced through nervous agency; and it appears to me, as I have previously remarked, that we must look to the nervous system, and especially to the ganglionic or sympathetic system, as the only efficient cause of the disturbed circulation, and consequently, as the “fons et origo” of all the symptoms of Spasmodic Cholera. We have every reason to suppose that conges-



tion first takes place in the vessels of the abdomen ; at any rate it is observed more particularly in these vessels after death. Let us notice the manner in which they are supplied with nerves. There is first a large nervous mass called the “semilunar solar ganglion,” which lies on the crus of the diaphragm, *and on the cæliac and mesenteric arteries*, branches of the nerves also accompany the various branches of the arteries. Second, there is the cæliac ganglion *which covers the aorta and the roots of the cæliac and mesenteric arteries*. Third, there is the hepatic plexus, branches from *which accompany the hepatic artery and the vena portæ*. Fourth, there is the splenic plexus *which accompanies the splenic artery*—the superior and inferior mesenteric plexus *accompanying the corresponding arteries*, and there is finally the hypogastric plexus *in which the aorta is included*. Now it appears to me that the simple question is, whether or not the blood vessels which have just been enumerated, are influenced by the nerves by which they are accompanied, in a manner similar to that in which we have seen others to be influenced ; in other words, can these several arteries be excited to increased action through nervous agency ?—Reasoning from analogy, and analogy in such instances must be our only guide, I should say they can. In the examples before adduced, as in blushing, we have every reason to suppose that the increased action in the artery is directly caused by nervous influence. If then the nerves of the abdomen in their functions resemble other nerves, intimately connected as they are with the great vessels, they must be capable of producing similar effects—they must have the power of exciting those vessels to increased action. But in Cholera, the action is not increased, but diminished ; is this owing to nervous influence ? It is caused, I apprehend, either by the direct



agency of the nerves, or by a want of nervous energy. We have already seen that the nerves being in a particular condition, the circulation is increased ; we are surely entitled to infer that if they were in a directly opposite state, the effect produced upon the circulation would be precisely the reverse. Supposing for instance, that in order to produce the excitement, the nerves were brought into a particular state of tension, (which I by no means aver to be the case,)—suppose the excitement depended on this tension, we should naturally expect that when the nerves were in an opposite condition, we should have opposite effects produced—we should no longer have excitement, but depression. We know not in what manner the nerves act—we are only acquainted with the effects which they produce ; but admitting that they are essential to the production of certain effects, and amongst the rest, to the circulation of the blood, they must possess some *influence* which is the cause of the effects which are produced. It matters not what may be the precise nature of that influence ; if it be withheld, the effects themselves will not result. If, for instance, we suppose that sensation and motion depend on the circulation of some very fine subtile fluid, should that fluid cease to flow, we should immediately cease to feel and move ; or, if we imagine them to be owing to a peculiar vibration of the nervous cords, should these cords cease to vibrate, the effects would no longer be produced. This is so evident that it will not require more particular illustration. Suppose then, that instead of the nervous influence being entirely withheld, it were only partially exerted, the consequence would be that the functions dependent on it would only be partially discharged, and the healthy exercise of these functions would be in the inverse ratio to the suppressed nervous energy. Supposing then, that the



circulation of the blood through the vessels is dependent on nervous energy, or capable of being materially influenced by it, no affection of the latter will exist that is not manifested by a disturbed condition of the former. If the flow of nervous energy were entirely suppressed, we should have a *stoppage* of the circulation; if it were only much diminished, we should expect the circulation to be *retarded*, or we should have congestion. If we for a moment admit that the circulation can be increased in force and frequency through nervous influence, I do not see how we can resist the conclusion that it may, by a diminution, or a want of such influence, be brought into a precisely opposite condition. And if we allow that the affection of the nervous system is the primary cause of the disturbed circulation, I think we have reason to conclude that such affection, whatever its nature may be, is first manifested in the sympathetic or ganglionic system. I judge so, first, from the appearances which are observed in the abdomen after death; secondly, from the train of symptoms during life; thirdly, from the distribution of the nerves proceeding from the different ganglia of the sympathetic—the manner in which they are connected with the large vessels; fourthly, from the perfect manner in which the brain exercises its functions; and fifthly, from the want of evidence of any affection of the spinal cord. I am aware that many persons have maintained that the spinal marrow is the seat of the affection, but I am not aware that they have adduced evidence sufficient to convince others of the truth of their hypothesis. Unnatural appearances have been perceived in the medulla spinalis, or in the arteries and veins by which it is accompanied, after death; but such appearances have only been in common with others existing in different parts of the body. The sympathetic system appears to me to be



a system *sui generis*, capable of acting in some degree independent of the brain and spinal marrow, but liable to be influenced by them both, as well as to induce particular alterations in their condition. In the present disease I regard the appearances which have been observed in the spinal canal as the consequence of the disturbed state of the circulation, the state of the circulation being caused by the peculiar condition of the nerves. The rapidity with which the disease sometimes runs its course, and the suddenness of its attack, may also be adduced as arguments in favour of its depending on nervous influence. It is stated by Dr. Burrel, that vomiting, purging, and spasms, were frequently almost or altogether wanting; "all the powers of the system failing at once, and death commonly ensuing in three or four hours after the attack. Several instances were heard of at Hoobly, and other places, of the natives being struck with the disease whilst walking in the open air, and who having fallen down, retched a little, complained of vertigo, deafness and blindness, and expired in a few minutes. At Bellary, a tailor was attacked with what was supposed to be Cholera, and instantly expired with his work in his hands, in the very attitude in which he was sitting."—(Good, vol. 1, p. 308.) It is stated that in a division of troops in India, consisting of about 5000 men, individuals previously in perfect health, dropped down dead by dozens; and those even less severely affected, were generally dead or past recovery within less than an hour.—(Dr. B. Hawkins on Cholera, p. 39.) It is also remarked, that in Ceylon and Bombay, the deaths were occasionally so very sudden, "*as to resemble the effects produced upon the animal frame by a stroke of lightning, or a violent blow on the stomach.*"

Now in all probability the effects in both these cases



are first manifested in the nervous system. We have every reason to believe that when an individual dies immediately after having received a violent blow in the epigastrium, the system of the sympathetic nerve is first affected, and that death is the consequence of the impression which is thus made. It is true that we cannot exactly *prove* this, but I think there are very strong reasons for supposing it to be the case. When we remember the extensive connexion which subsists between the solar ganglion and other important parts of the system—the heart—the diaphragm, and the lungs, we can be at no loss to account for the disastrous effects which frequently follow blows in the epigastric region.

I am aware that the attempts which have been made to prove an unnatural condition of the sympathetic nerve in persons who have died of Cholera, by means of the dissecting knife, have not been successful. But I do not consider that such want of success is a proof that this system of nerves is not the first cause of the disease.—Very considerable functional derangement frequently arises from very minute structural changes, and such changes are often overlooked by even the best anatomists. We commonly speak of disease being organic or functional; by the latter term we mean, that a certain part does not properly perform its duties, whilst we are unable to detect any organic mischief. In Spasmodic Cholera, I maintain that *the function of the ganglionic system of nerves is impaired*, and I repeat, the argument is not weakened by our being unable to detect organic disease.

Having thus attempted to point out what I consider to be the efficient cause of the disease, it would be useless to enlarge on any explanation of particular symptoms. It would be easy to show on what the vomiting, purging, spasms, and the rest of the symptoms which usually ac-



company an attack of the affection depend, but this would only be occupying time by such an explanation as would immediately suggest itself to the mind of every medical man. I prefer at once passing on to make a few remarks thirdly, on the treatment of Cholera.

The various and opposite methods of treatment recommended by different medical practitioners, and the varying success by which they have been attended, serve, at least, to show the want of some fixed principle to guide our practice; and they moreover strongly impress upon the mind a strong conviction, that the cases which have recovered under such diametrically opposite methods of treatment, could not have been identically the same.—The same cause, we readily admit, may be removed by *different* means, but it is not so easy to conceive that it may be removed by *opposite* ones. An attack of inflammation might probably be subdued by bleeding, by continued purging, by Tartarized Antimony, or by Digitalis, but it is not likely that it would be overcome by stimulants and that class of medicines which, instead of lessening the force and frequency of the heart's action, would produce a directly contrary effect. There are two great faults which I think attach to medical men as a body—the first is, assuming the identity of two diseases, merely because they both present similar prominent symptoms, or their non-identity, because the leading symptoms do not precisely resemble each other, whilst they neglect to attend to various minor circumstances which materially modify the whole. The second fault to which I allude consists in confidently ascribing the recovery of individuals from serious and dangerous diseases, to certain medicines which they had previously taken. An unbounded love of generalising is the great evil of the present day;



but they know but little of the œconomy of the human frame who suppose that the numerous diseases to which it is incident invariably present the same external characteristics, or that they are always to be removed by a similar line of treatment. They do not sufficiently remember that every antecedent is not a cause—every sequence not a consequence.

Perhaps there is no disease which has occasioned more controversy, either with respect to its cause, or its treatment, than the one which is the subject of the present observations. Almost every variety of remedial agents has been had recourse to, and the most opposite plans have met with warm and vigorous supporters.—Some place their chief confidence in venesection—others in emetics—others in stimulants—others in bismuth—others in opium, others in tobacco, and others in calomel. If, however, the pathology of the disease had been more carefully attended to—if men had freed themselves from prepossessions in favour of any particular plan, and not placed such implicit confidence in the dogmas of those, who having had some opportunities of seeing the disease, have presumed themselves to be capable of writing on it—if they had exercised in cases of Cholera the same judgment which they evince in other diseases, and had not suffered themselves to be warped by existing prejudices, we should in all probability, ere this, have had a more settled and uniform practice recommended, which would have been attended with the recovery of a much larger number of the afflicted. There is no doubt of the great efficacy of medicine in Spasmodic Cholera, for we know that the mortality is always greater in those places where there is a difficulty in procuring medical aid. In fact, the recoveries appear to be in about the direct ratio to the facility of obtaining such assistance. But whilst we gratefully



own that medicine has effected much good, we cannot but regret that the benefits which it has produced have been materially stinted by the conflicting testimonies of different writers. It will be my object very briefly to notice the remedies, which, from a view of the pathology of the disease, I should think best calculated to afford a reasonable prospect of success.

In speaking of the treatment of Cholera, it is convenient to divide an attack into three distinct stages. 1st, that in which we have vomiting and purging; 2nd, that of collapse; 3rd, that of reaction. The first stage we may term one of excitement—in many respects it resembles the disease to which we give the name of *Gastro-Enteris*—there is vomiting, purging, and pain on pressure. These symptoms, I apprehend, are caused by the increased action of the vessels supplying the lining membrane of the alimentary canal. The pain on pressure, however, is not general over the whole abdomen, but confined to the epigastric region; it is most severe when applied immediately over the situation of the semilunar solar ganglion. This first stage may be of greater or less duration, and the appearances of the mucous membrane after death will be modified by the length of time which it continues, as well as by the severity of its symptoms. The indications of treatment in this stage are obviously to lessen undue action, and to allay excessive irritability. For these purposes we may have recourse, first, to venesection. Many are the instances in which bleeding, for what we may term the premonitory symptoms, has been followed by the happiest results. Mr. Cass, of Goole, states his conviction that a considerable number of persons whom he saw, were preserved from a more formidable attack of the disease, by having a timely recourse to the lancet. It appears, in fact, now to be the established



practice, to abstract blood as soon after the onset of the symptoms as possible. It is in this stage also that opium may be beneficially administered. The effects of this medicine in allaying irritability of the stomach and bowels, are too well known to need any lengthened remarks.—Opium may be given with advantage either *per os*, or *per anum*. In the former case it will be better to give it in a tolerably large dose, in the form of pill, because in that form it will be less likely to occasion vomiting. If the tincture is prescribed, it must be mixed with more or less of some other fluid; but it may be regarded as a general rule, that where there is great irritability existing in the stomach, the less foreign matter we introduce into it the better. The practice of frequently exhibiting small quantities of gruel or other liquid, for the purpose of allaying thirst, is reprehensible; the relief which it affords is only temporary, whilst it often occasions mischief which it is by no means easy to overcome. If we wish to administer the narcotic *per anum*, it is best done in combination with the Decoctum Amyli; 60, 80, or 100 drops of the tincture may be added to about four ounces of the decoction, and injected in the usual manner. Every reflecting person will immediately perceive the necessity of the quantity of the injection being small. There are two methods, I apprehend, in which opium may be serviceable: first, by lessening irritability, and second, by allaying pain. If the irritability of the stomach and bowels should continue for any length of time, and in consequence thereof considerable discharges should take place, a corresponding degree of exhaustion will be the inevitable result. It is manifestly therefore a desideratum to allay that irritability—to restrain those discharges, and thus to prevent the excessive debility which they must necessarily induce.



Secondly. Opium is valuable in consequence of the power which it possesses of allaying pain. The pain arising from cramps in Cholera is, we well know, occasionally excessive. I need scarcely remark that continued pain even although it be slight, produces derangement of the circulating system, and if it should continue long and be violent it is followed by languor, feebleness of the powers of the circulation, and general prostration of strength.

It is in this stage of Cholera that calomel is beneficial. I may here state my decided conviction that the powers of this remedy have been very much over-rated, not only in the disease which is the subject of the present remarks, but also in others. With what intention is calomel given in Cholera? too generally, I fear, for the purpose of acting on the liver. It is even yet a favourite notion with some persons that this dreadful malady is caused by a vitiated secretion of bile, or a suppression of its secretion; although perhaps there is nothing more easy of demonstration than the fact that the peculiar condition of the biliary organs is a consequence and not a cause of the disease. The practice of exhibiting large quantities of this drug for the purpose of exciting these organs to increased action, is, in my opinion, absurd; and I think that that medical practitioner who, in the malady under consideration, makes calomel his sheet-anchor and chiefly confides in its wonder-working powers, must regard himself as particularly fortunate if the death of his patient is not the result of his misplaced confidence. I by no means wish to underrate the value of this powerful medicine,—I am only desirous of seeing it more properly and judiciously employed. I grant that it may be serviceable in Cholera when given in the first or third stage, but I cannot imagine how it should produce very beneficial effects



in the far more dangerous one of collapse. When administered in the first stage, I conceive, that it may act either directly on the stomach, and thus cause a cessation of the vomiting, or it may act as it sometimes does, by equalizing the powers of the circulation. The latter effect is more readily and certainly produced by joining calomel with opium. The power of calomel in allaying irritability of the stomach, especially if given in large doses, must be familiar to every medical practitioner. In these respects, then, the medicine now under consideration is valuable, but to give it with the idea of its producing any peculiar specific action on the liver, is exceedingly ridiculous. But, say the mercurialists, is not the liver affected? Most certainly it is, but it is so only in common with the rest of the chylopoetic viscera, and the condition of this organ is similar to that of the spleen, the lungs, &c.\* When calomel is given in the third stage of the disease, its action is precisely similar to what it is when administered in cases of fever. On the whole, then, my firm impression is, that the powers of this medicine have been too highly extolled—that it may be advantageously exhibited in cases of Cholera, but that it is both improper and dangerous to regard it as a specific.

I shall pass over without observation several other remedies which have been recommended, simply because I have no confidence in them. Individuals labouring under the disease, may very likely have recovered after being treated with oxide of Bismuth, or cajeput oil, but we have no evidence to show that their recovery was in consequence of

\* That there is no suppression of the secretion of bile is evident from the fact, that in *post mortem examinations*, the gall bladder has generally been found full; and that the disease cannot be owing to the bile not passing into the intestinal canal, may reasonably be inferred from the fact, that when an obstruction exists in the ductus communis choledicus, which is often the case, we have not as a consequence of such obstruction the symptoms of Spasmodic Cholera induced.



the administration of those drugs; and so far as I can learn, when they have been employed, it has only been in combination with other means. To which of these means the happy result is to be attributed, I leave those to decide who have carefully perused the publications in which these medicines have been so highly extolled. I have formed my own opinion. Emetics have been recommended by some practitioners, in this stage of the disease, but my own impression is, that they are calculated to do more harm than good. The symptoms which we wish to relieve are excessive vomiting and purging, not dependent, it should be remembered, on any thing which has been previously taken disagreeing with the stomach and bowels, nor yet on any vitiated secretion, but solely on increased irritability. This increased irritability is not very likely to be allayed by the exhibition of a stimulating emetic.

What would be the effect of considerable counter irritation in the first stage of Cholera? We have evidence of increased action in the stomach and bowels; and if great external irritation were produced, as it might be by friction with Tinct. Lyttæ and Ol Terebinth, it is possible that the internal vessels might be relieved, and the subsequent congestion prevented. But I have had no opportunity of seeing this practice tried, and therefore I speak of it with diffidence.\*

I pass on to make a few observations secondly, on the treatment of the stage of Collapse.

It has been already observed that the symptoms of this stage of the disease depend on the congestion of the blood in the larger vessels. Those remedies therefore which are most likely to overcome the congestion will be most serviceable. What they are I shall briefly point out.

\* Since these remarks were written, I have seen the practice which I have ventured to recommend, in some measure adopted, and I think with beneficial results. I am satisfied that the plan deserves a fair and extensive trial.



The class of medicines chiefly to be relied on in the treatment of this stage of the disease is stimulants.—Æther, Ammonia and Brandy may now be freely exhibited. In addition to these friction should be employed, either with flannel, the flesh brush, or other stimulating substances. Let it constantly be borne in mind that the blood is stagnant in the vessels of the cavities, and we can scarcely forget that effectual relief can be obtained only by procuring an increased circulation on the surface. The various means which are usually adopted for inducing an “external circulation,” should be unremittingly employed. We must not rest satisfied with having done all till we have availed ourselves of every expedient which is in any degree calculated to promote the end we have in view. Immersion in hot water and the use of the hot air bath have both been recommended in this stage of the disease, but I have never seen any decided good result from either one or the other. I regard the water bath as hurtful, and the air bath as almost useless. Continued friction with a stimulating embrocation, as mustard and oil of turpentine, and the constant application of bags of hot salt to the extremities, are much more efficacious in restoring the animal temperature than either of the remedies before alluded to. But if it be judged expedient to try the effect of the hot water bath, the greatest care should be taken to have the patient well dried when he comes out of the bath by means of friction with rough cloths, and he should be surrounded by such substances as are the worst conductors of caloric. By no means should a linen night dress be worn since it rapidly abstracts heat from the body; cotton is much preferable, and flannel better than either; hence he should not lie between the sheets of the bed but be placed betwixt the blankets. Bottles of hot water or hot bricks wrapped in flannel should be constantly



applied to the feet, and large mustard poultices to the legs and thighs. In addition to the remedies already suggested, I am disposed to place some degree of confidence in warm enemata, which may be composed of mutton broth, beef tea, or warm milk. Dr. Reid Clanny made use of injections of the former. There are two reasons which induce me to think that such substances may be advantageously employed. First, the warmth being in the immediate neighbourhood of the congested vessels, may possibly stimulate them to increased action, and, Secondly, since we have every reason to believe that the process of absorption continues, such substances may afford some degree of nourishment to the exhausted and sinking body. It may be objected that they do not undergo the usual change, they are not converted into chyle, and that therefore they cannot produce the effect which is here supposed. But it yet remains to be proved that various animal matters cannot afford nourishment before they have undergone such a change. If I mistake not, the evidence which we possess would lead us to an opposite opinion. Chylification, we believe, takes place in the small intestines, chiefly in the duodenum, and is the consequence of the mixture of the bile and pancreatic juice with the chyme which has been propelled from the stomach. If this be the case, the process cannot take place in the large intestines; and yet we know that persons have been kept alive for considerable periods of time, solely by nutritious substances being injected *per anum*. It is probable then, that the powers of life might be sustained in some cases of Cholera by the employment of similar means. At any rate, since such a practice could not possibly do harm, there can be no reasonable objection to giving it a full and fair trial.

In this stage of the disease, stimulating emetics, as



common salt and mustard, should also be given. By exciting a powerful action in the stomach, they may also excite the neighbouring parts, and thus overcome the congestion. If they produce their usual action on the stomach, they will at the same time cause a determination to the surface, and relieve the overloaded vessels of the cavities. Much has been said on the propriety of bleeding in this stage—one set of persons warmly advocating the employment of venesection, and another as warmly condemning it. It requires considerable care to decide on the line of practice to be adopted, “when doctors,” who have enjoyed ample opportunities of obtaining information, so entirely “disagree.” If, however, we bear in mind the condition of the circulating system, it will probably afford some light to guide us through the difficulties which are created by the danger of the patient, and the conflicting testimonies of medical writers.

First, then, let it be borne in mind, *that there is no excess of blood*. Many diseases we know depend on the existence of an excess of blood, which we designate by the term Plethora. In such cases, the abstraction of blood will obviously be attended by relief. In the present instance, however, since we have no such redundancy of the sanguiferous fluid, we can scarcely expect that the practice of venesection will be followed by equally happy consequences. Secondly, let it be remembered, *that there is no increased vascular action*. The action of the blood vessels may be preternaturally increased, without the fluid which they circulate being in greater quantity than usual. This is the case in many of the acute inflammations. In such instances we bleed to relieve increased vascular action. If I do not greatly err, we seldom make use of the lancet except for the purpose of fulfilling one or other of these two indications. On what principle then is bleed-



ing practised in the cold stage of Cholera? Obviously on neither of those to which I have just alluded, and yet it is possible that it may be attended by beneficial effects. I conceive the operation to be as follows:—A great quantity of blood, as has been previously remarked, is congested in the large vessels of the abdomen and thorax. If we open a vein in the arm, and a quantity of this fluid should escape from it, the column of blood which was situated below will move onwards to supply the place which it previously occupied, and a slight progression of the blood throughout the system will be the consequence. This salutary action will not be confined to the vessel which is opened, but it will take place to a greater or less extent through the whole vascular system. There will be a kind of vacuum formed in the vessel from which the first drop of blood is obtained, and the pressure of the atmosphere will force on the blood situated below the opening, to prevent what the ancients taught us to believe nature abhors. If we can obtain a full stream of blood, the internal congested vessels will be relieved, and the condition of the patient materially improved. I think the idea which is here suggested, is in some degree strengthened by the reports which we possess on the treatment of Cholera. It is stated, that when first a vein is opened, it sometimes happens that a considerable period elapses before the blood will flow through the orifice; when, however, after the employment of friction and various other means the blood does begin to flow, it is first of a very dark colour, and a thick consistence. In this it resembles the rest of the blood in the system; it has not been subjected to the influence of the air in the lungs, and it has lost some of its serum. After it has flowed for a short time, its colour changes—it becomes brighter and bears a greater resemblance to arterial blood.



This is caused by the large vessels gradually becoming unloaded, and the circulation through the lungs being restored. The difficulty which occurs in first procuring blood, is caused by the congestion in the veins of the extremity, for we have it on the evidence of Mr. Searle, that the congestion occurs not only in the large vessels of the cavities, but also in those of the extremities, and in fact, congestion in the latter is the necessary consequence of the condition of the former. It may appear somewhat paradoxical to state, that we employ blood-letting in the stage of collapse for the very same reason that we employ external and internal stimulants ; but yet this is the case, we use it for the purpose of *promoting* the circulation. They conduce to the same end in a somewhat different manner, but I apprehend it signifies but little by what means the circulation is restored, provided the effect is produced. My own impression is, that stimulants of the most diffusible kind should be first resorted to, and that the animal temperature should be maintained by every artificial means in our power ; but that the utility of bleeding should be constantly borne in mind, since its occasional usefulness is established on the most clear and satisfactory evidence.\* The great prostration of strength is no argument against the employment of venesection, since it depends on the state of the circulating fluid, and if the use of the lancet is capable of putting that fluid in motion, it will also remedy the prostration of which it is the cause.

It was remarked in an early part of this essay, that the train of symptoms which characterise an attack of Cholera, directly depend on the state of the circulation, but

\* I think that if even we had no direct evidence of the benefit which occasionally results from the employment of bleeding in the stage of collapse, we should still be fully justified in adopting the practice, from the knowledge we possess of the effect of venesection in the cold stage of intermittent fever.



that the peculiar condition of the circulation is the effect of nervous influence, or rather perhaps in the stage of collapse, a want of nervous influence. I have already brought forward the reasons which induce me to think, that the ganglionic system of nerves is the "fons et origo" of the disease, so that I need offer no further observations on the subject at present. But supposing that I am right in my conjecture, a remedy presents itself which promises to be of inestimable value—I allude to galvanic electricity. I am aware that in the treatment of Cholera, this powerful agent has not been altogether overlooked, but I at the same time feel satisfied that it has not been so extensively tried as its vast importance demands. If it be true that the paralysed state of the blood vessels and the consequent congestion of their contents are owing to a want of nervous influence, then galvanism and electricity are the two most valuable remedial agents which we possess. We well know that in cases where nerves have been divided, and the organs to which their ramifications are distributed have in consequence become paralysed, their functions have been restored by connecting the extremity of the nerve with a galvanic or electric apparatus. Cases of paralysis, which have been materially relieved or effectually cured by means of electricity, must be familiar to every medical practitioner; and there is an instance related by one of the most popular and intelligent writers of the present day, Dr. Abercrombie, of the beneficial effects of Galvanism on the intestinal canal in a case which had resisted all the remedies which had been previously employed.\* The case to which I allude was very different I readily grant, to the disease which is the subject of the present observations, but since it shews the in-

\* See pathological and practical researches on diseases of the stomach, &c. p. 131.



fluence of the agent which I am advocating, in cases of intestinal derangement, I adduce it as a strong argument in favour of its employment in Cholera. Does the congestion of the blood, I again ask, depend on the want of nervous influence—*is it possible that this should be its cause?* If so, galvanism or electricity ought to be extensively and fully tried. They offer a more reasonable prospect of success than any other remedy with which I am acquainted. By other means we are attempting to remove the effect of an unknown cause. By this we strike at the very root of the malady—we put to rights the first link in the chain of disease, and we may reasonably expect, having done so, that every subsequent link will fall into its proper place. Every reflecting mind will immediately confess that the peculiar condition of the circulation is the effect of something. Of what? What so likely as a peculiar state of the nerves, which under all circumstances so materially influence its condition? Admit this, and at the same time grant the power of the agents to which I allude, and who will deny the propriety of submitting them to a most rigid trial? All other means have repeatedly failed in the hands of even the most skilful practitioners. This may also be unsuccessful, but the chance of failure is no argument against the propriety of trial. In the treatment of cases so full of danger, we ought not to rest satisfied with having made use of any *single* remedy, however likely we may conceive it to be to effect the object which we have in view. It is certainly desirable that several means should be tried at the same time, provided they do not interfere with each other, and that their mode of action is not opposite. It fortunately happens that electricity or galvanism may be used in combination with most of the other remedies which have been suggested, as calculated to afford relief in this stage of the disease. One or the other may be



employed, for instance, at the same time that we are giving internal stimulants, and applying heat or friction to the surface of the body. In short, there is not a remedy which I have mentioned but may be used in conjunction with the means of which I am now speaking. But even granting that I am too sanguine in my expectations of the good to be effected by the agents now under consideration—granting that they are not likely to be followed by the beneficial results which I anticipate, they will at least do no harm, and therefore there can be no well grounded objection to submitting them to the test of experiment. If they are unsuccessful, it is only in common with every other remedy which has previously been recommended. How is it that galvanism and electricity have been so little employed in the treatment of disease? I fear it must be confessed that, generally speaking, medical practitioners are so ignorant of the agents, and so incapable of employing them, that they are either altogether unacquainted with their amazing importance, or unable practically to avail themselves of their use. Would it not be well that the minds of the rising members of our profession should be deeply impressed with their vast importance, by their being made the frequent subjects of rigid examination?

Since the above remarks were written, I have had an opportunity of perusing the paper of Dr. Thomson, of Glasgow, which appeared in the 65th number of the *Philosophical Magazine*. I differ with him entirely in his opinion of the seat of the disease, which he believes is confined to the eighth pair of nerves, but I cordially agree with him in the remedy which he proposes, and I rejoice that I can adduce the weight of his name in support of the means which I have feebly advocated. From the different opinions which we entertain of the seat of the dis-



ease, it is evident that we should apply the remedy somewhat differently. My object would be directly to affect the solar ganglion—his to affect the eighth pair of nerves; but I agree with him so entirely as to the means to be adopted, that I shall quote the concluding paragraph of his communication.

“From the researches of Dr. Wilson Philip and others,” he observes, “it seems established that when the eighth pair of nerves is cut, the function of respiration is so much impeded, that death is speedily induced. But if a current of galvanic electricity be made to pass through the lungs, the function of respiration is renewed, and becomes as perfect as ever, as long as the electrical current continues to pass through the lungs. Is it not likely that if a current of galvanic electricity were made to pass through the lungs of those which labour under Cholera, it would revive the energy of that indispensable organ, and enable it to produce the requisite changes on the blood? The heart would be stimulated, the circulation renewed, and very probably the other organs, when supplied with blood in the proper state, would again begin to act. Might not the kidneys be in this way induced to do their duty, and renew the secretion of urine? And might not the whey-looking matter, which is thrown out of the body by vomiting and purging, and which obviously consists chiefly of the serum of blood, cease to be thrown into the stomach and intestines? Were a current of galvanism passed for a few hours at a time through the lungs, it is probable that such a change would be induced in the blood as would lead to a restoration to health.”

“All the medicines tried in Cholera, in Glasgow, were of little or no value. Emetics, opium, stimulants, heat, and blood-letting, were all tried in vain. Galvanism, which I consider as the most promising remedy of all,



was scarcely attempted, yet it surely deserves a fair trial, and seems to be more likely to produce beneficial consequences than any other mode of treatment hitherto proposed."

In further corroboration of the opinion which has been advanced on the importance of making trial of galvanism in the stage of collapse, I am induced to quote a case which is related by Drs. Lorimer and Burton.\*

"A woman, ætat 65 or 70, was attacked with Cholera on Thursday afternoon, the 1st of March. At nine in the evening, she was in a state of collapse, without pulse, &c. During the night, small quantities of stimulants were administered at intervals, warm enemata thrown up, and attention given to the use of external means. No benefit resulted from their use. She was brought to the hospital on the forenoon of the 2nd. The collapse was then most perfect, having existed for sixteen hours and a half. The vomiting and cramps had ceased; the pulse was quite imperceptible; the voice inaudible, except in a whisper; the extremities blue, cold, and shrivelled. The galvanic arrangements being completed, one wire was connected with the trunk at the scrobiculis cordis, the other attached to the left ancle. After a few minutes a very feeble pulsation was perceived in the radial artery; but as it was not permanent, the wire was removed from the ancle and applied round the lower part of the neck. The effect was immediate and powerful; the pulse became very distinct at the wrist—96; the eye and countenance generally much improved; the voice strong, and bitter complaints were made of the severity of the suffering occasioned by the wires. The patient occasionally raised herself in bed, but the effect of any such exertion was to

\* See "observations on the history and treatment of Cholera Asphyxia, as it occurred in Haddington, by Robert Lorimer, M.D. and John Burton, M.D."



render the pulse more indistinct, or to suspend it altogether. The galvanism was kept up for five or six hours, (other remedies being also employed) during the whole of which time the pulse continued steady. As the energy of the trough was exhausted, the pulse became feeble,—a deeper collapse recurred, and in four hours more she died.”

Now this case is exceedingly valuable, in so far as it shows the power which galvanism has over the circulation. The effect appears to have been manifest almost immediately, and was commensurate with the energy of the trough; in proportion as it became exhausted, the circulation was enfeebled, and finally the poor woman died. I should think it by no means improbable, that she would ultimately have recovered, had galvanism been sooner resorted to. The “most perfect collapse” had been existing for sixteen hours and a half before she was taken to the hospital. This, I apprehend, had so debilitated the vessels, that when the galvanic agency was withdrawn, they were no longer able to propel their contents. Another circumstance which is mentioned in the case deserves notice—I allude to the situation of the wires. When one of them was at the *scrobiculus cordis*, and the other attached to the left ankle, “a very feeble pulsation was perceived in the radial artery,” but as soon as the wire which had been attached to the ankle was connected with the lower part of the neck, an immediate and powerful effect was produced, and “the pulse became very distinct at the wrist.” Does not this fact throw some degree of light upon the seat of the disease?

I pass on to say one word on the treatment of the third stage of Cholera. This stage is one of reaction—it is in short, fever; and the remedies most likely to be of service, are those which we are in the every day habit of



employing in febrile affections. Bleeding, emetics, purgatives, sudorifics, salines, and narcotics, may be used as the urgency of the symptoms appears to demand. If local determinations should take place, as frequently happens, local means must be had recourse to, in addition to general remedies. Every practitioner may be left to follow the line of practice which he has generally found most successful, and we need not fear that the skill, the watching, and the patient attention which has been exerted in overcoming the most alarming and dangerous stage of the affection, will be unsuccessful, when it has to grapple with the far milder one of reaction. It must not be for a moment thought, however, that this stage is free from danger; many have died in it, and therefore it demands the utmost care and attention.

I reserve one general remark as a conclusion to my observations. *It is absolutely necessary, in every stage of the disease, that the patient should constantly be kept in the recumbent position, he should not even be raised to a sitting posture to obey the calls of nature. Mr. Searle states, that three patients under his immediate care, lost their lives from want of attention to this particular.*



## APPENDIX.

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Since the preceding observations were written I have availed myself of the opportunity, which the existence of Cholera in Dublin afforded, of personally inspecting many cases of the disease. The result of my investigations in Ireland has been still more strongly to impress me with the conviction of the justness of the views which are maintained in the present essay. Every facility of obtaining information was afforded to my friend Mr. Turton and myself, and during our stay we had ample opportunities of seeing the disease in all its stages. There was no particular plan of treatment adopted which demands especial notice. Bleeding, emetics, friction, either with the bare hand, with flannel, or with stimulating embrocations, the internal exhibition of stimulants, as Brandy, Wine, Whiskey, Æther, Ammonia, and the administration of calomel and opium, were the means chiefly confided in. The hot air bath was but seldom used—bags of hot salt were constantly kept to the feet and legs.

Bleeding was occasionally had recourse to in every stage of the disease and generally with good effect. Many persons experienced relief from venesection, in whom before the performance of the operation no pulsation could be perceived either in the radial or humeral artery. Emetics are, I think, chiefly useful at the commencement of the stage of collapse—a stimulating emetic composed of mustard, being exhibited at that time is often productive of considerable benefit. The stimulants which are given should be of the most diffusible kind ; Æther, Ammonia,



and Brandy are those on which I should chiefly rely. Opium is sometimes administered as a stimulant, but it ought to be given with great care in this stage of the disease; if it should not produce a stimulating effect we shall have its sedative action, and thus rather aggravate than diminish the class of symptoms for which it was prescribed. When pain is severe, when the cramps are violent, and the sickness and purging are excessive, opium may be safely and beneficially administered; but when these symptoms are not urgent—when the powers of life are almost extinct, and the exhausted patient more strongly resembles a lifeless corpse than a living being, the exhibition of large doses of a powerful narcotic is extremely injudicious and calculated to produce the most distressing consequences. It should not be forgotten, that although the primary action of opium is stimulant, the stimulus which it occasions is generally very transitory, whilst its more permanent effect is sedative.

There are very different opinions maintained in Dublin with respect to the power of calomel in cases of Cholera; some give it in large doses and appear to place considerable reliance in it, whilst others think it of but little value. I did not see a case in which it appeared to produce any very decided effect. The combination of Bicarbonate and Chlorate of Potash, and Muriate of Soda, was given in several cases, but its action was not such as to entitle it to any great share of confidence.

I am not aware that the effect of galvanism has been tried in a single instance in Dublin, but from what I saw of the cases there, I feel confident that it would prove a most valuable remedial agent.

Many of those who recovered from a severe attack of Cholera, suffered afterwards from gastric fever, or affections of the head. It appears as if the stimulating substances



which were given during the stage of collapse, had induced some degree of inflammation of the mucous membrane of the stomach ; this viscus becomes exceedingly irritable, and rather painful on pressure, and there is more or less accompanying fever. In order to subdue these symptoms, general or topical bleeding is employed, blisters applied to the epigastrium, opiates given, and laxative enemata administered. The affection of the head is not inflammatory, it appears more like congestion in the vessels of the brain, and it is treated by bleeding and blistering, and stimulating purgatives. It by no means unfrequently happens, that these consecutive affections terminate fatally. One case was brought to a fatal termination by Malæna coming on ; the patient, a poor man, very rapidly sunk.

In conclusion, I may revert to a subject which was briefly noticed in an early part of this essay, viz. the difference between the present disease, and the epidemic, to which we are every year more or less accustomed.—Many of the most prominent symptoms are common to both diseases, but I consider that they materially differ from each other in the following particulars :—

1.—English Cholera comes on after a person has been taking something calculated to disagree with the stomach and bowels—after having undergone unusual fatigue, or been exposed to considerable alternations from heat to cold, moisture, &c. The present epidemic, on the other hand, does not appear to be caused, in the great majority of instances, by such means ; it seems to arise entirely independent of them. I do not wish it to be understood, that I consider it impossible that it should originate from such causes, but merely that it cannot usually be traced to them ; they appear to act chiefly as *predisposing causes* ; in the former affection they are the *exciting ones*.



2.—The exhaustion which occurs in cases of English Cholera, depends on, and is proportional to, the discharges which take place from the stomach and bowels; whilst in the Cholera of India, the exhaustion appears to be independent of, and bears no proportion to the quantity of matter eliminated from the body. It often happens that the exhaustion comes on at once, without being preceded by either vomiting or diarrhœa.

3.—There is no evidence that the English Cholera can be communicated from one individual to another, whilst every one must own that the facts adduced to prove the contagious nature of the Cholera of India, are exceedingly powerful.

4.—In the Indian form of the disease, there is a remarkable and sudden sinking of the eye, which is surrounded by a very dark coloured areola; this is not the case, so far as I know, in the common Cholera of this country.

5.—The coldness of the tongue and breath are striking symptoms in the Indian Cholera, but not in the English.

6.—The blueness which is observed in nearly all the bad cases of the Indian Cholera, is but very rarely to be met with in any other form of disease.

7.—There is a peculiar appearance of old age given to a patient labouring under Spasmodic Cholera, which I never perceived in any other disease. A person appears to age 20 years in less than as many hours.

8.—The appearance and chemical constitution of the dejections are exceedingly different in the two diseases,—in the English Cholera, they for the most part contain an excess of bile—in the Asiatic, on the contrary, they show a deficiency.







