Two lectures on cholera and intermittent fever / [Charles William Bell].

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DR. BELL

ON CHOLERA AND AGUE.

BRIGHTON AND SUSSEX

Medico-Chirurgical Society.

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TWO LECTURES

ON CHOLERA

AND

INTERMITTENT FEVER.

ADDRESSED TO THE
MEMBERS OF THE MEDICAL PROFESSION IN MANCHESTER,
OCTOBER 27, AND NOVEMBER 3, 1848.

BY

CHARLES W. BELL, M.D., K.L.S.,

PHYSICIAN TO THE MANCHESTER ROYAL INFIRMARY,
LATE PHYSICIAN TO H.M. EMBASSY IN PERSIA, ETC.
ETC., FORMERLY HOUSE-SURGEON TO THE MIDDLESEX
HOSPITAL.

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PREFACE.

Owing to the variety of forms in which cholera appeared in Persia, in 1842-3, the author was led to entertain peculiar opinions respecting its nature and treatment. In the course of conversation on the subject, some of his colleagues in the Manchester Royal Infirmary were pleased to say that they would gladly hear the views then advocated by the author, stated at greater length in the form of a lecture.

The following pages are the result of his endeavours to comply with this desire, and their delivery was honored by the attendance of many distinguished members of the medical profession in Manchester and its neighbourhood, who, in a very flattering manner, expressed a hope that the lectures might be published. In venturing now to submit these observations to the profession at large, before the approaching epidemic should have enabled him by further experience more fully to substantiate or to correct his conclusions, he has been moved solely by the hope, that the inquiries here

suggested might prove useful to others in assisting investigation of this obscure disease. He wishes them to be received solely as suggestions, and has therefore abstained as much as possible from detailing the results of his own experience, desirous of attempting to reason on the subject rather than to dogmatise.

Firmly believing in the analogy of cholera with quotidian ague, he has ventured into print thus early, lest by delay the opportunity of confirming or negativing a point of so much importance should be lost to others, to whom circumstances may have been less favorable than to himself in suggesting the points of their similarity.

94, *Mosley Street, Manchester.*

ON CHOLERA.

GENTLEMEN,—

Both as students of the science of medicine, and as professors of the healing art, we are all at present peculiarly interested in investigating the nature of cholera. Thirty-two years have now elapsed since that fatal disease first became known to our countrymen in India, and through them to the inhabitants of other countries; but it was long regarded by the nations of Europe and America, as belonging rather to another world, than as ever likely to become personally interesting to themselves.

It was only on its actual appearance among us in 1832 that we became convinced of our mistake, and were thrown into all the consternation that attends a mysterious and unexpected object of terror. At that time every human attempt made to arrest its progress proved in vain; and, setting almost every variety of climate at defiance, cholera completed the wide circuit of the world.

No sooner, however, was the danger past, than we relapsed into our false notion of security, until this was again dispelled by appalling accounts of its renewed

progress through Asia, and of its having again passed the boundary of the Cancasus, invading Europe by the identical route which it had followed seventeen years before, with no other difference than that its front was now more extended.

It might reasonably have been expected that, after the disease had extended itself over nearly the whole globe, and the press of every country had absolutely teemed with essays upon the subject, some definite agreement would have been arrived at by the profession with regard to its essential nature, and to the principles upon which its treatment should be conducted. It is, then, little flattering to our professional pride, to be obliged to acknowledge that this expectation has been disappointed.

The medical press gives almost daily evidence, that little information of importance has been added to that which had been collected by the earliest observers of the disease; our practice is still almost entirely empirical, the principles on which it should be conducted are as little determined, and the opinions of the profession as little settled as they were on its first visitation.

Even the great question of the capability of cholera being communicated by contagion, is still a moot point, and all the evidence collected on the subject is insufficient to convince our legislature that quarantine regulations in regard to it, are only a useless and cruel grievance. Under such circumstances, and after many of the first medical authorities of the past and

present generation have failed to elucidate its nature, it is only natural that every new attempt to throw light upon the subject should be received with distrust, and that any one who now ventures on the discussion, will do so at considerable risk to his professional reputation; I cannot, therefore, but feel much diffidence in doing so, and that much apology is due to you for requesting your attendance here for that purpose.

My reason for making the attempt is, simply that I believe the circumstances under which I have had an opportunity of studying the disease have been peculiar, and such as to lead to conclusions of great importance, and that I should be acting little in unison with the spirit of our profession, and be neglecting a great duty, if I failed in laying that information which chance has accorded me, before its members. I may, it is true, have overrated the value of my opportunities (this is for you to judge,) but believing, as I do, that what I have witnessed, if fairly set forth, will lead to the determination of a *principle*, rather than a rule of practice, I have felt it imperative on me to submit the subject to your consideration, in the same light in which it had appeared to myself.

After becoming acquainted with cholera, under very favourable circumstances, in Edinburgh, in 1832-3, and in London, in 1833-4, it was my lot to be stationed for several years in Persia, a country, situated both geographically, and in point of climate, midway between India and Europe, and there I had the opportunity of

observing closely the first approaches of the identical cholera, which is now sweeping irresistibly towards us. The disease was there ushered in by a regular succession of epidemics, commencing in a fever apparently continued, but by and by assuming more the character of a remittent, and this very gradually changed to an intermittent of quotidian type; of this the cold stage gradually became prolonged, and assumed all the appearances of an attack of cholera, and then came the cholera, as it has everywhere been known, without any *obvious* stages or intermission. This again in its turn disappeared, and the epidemic resumed the character of remittent and continued fever for a time. These various changes occupied a period of eighteen months.

Having thus witnessed the disease analysed and dissected as it were, into its component parts, and seen continued fever gradually resolve itself into cholera, and cholera into continued fever, by slow gradations, in a manner which does not appear to have ever been presented with the same perspicuity to any author, having, too, been obliged to follow each varying type of that epidemic with appropriate treatment, I have been led insensibly to those views of the nature and treatment of cholera, which I shall endeavour to submit to you, as shortly as may consist with my intention of leading you into the same train of reasoning which I myself pursued.

By considering this fatal disease rather as an exaggerated example of others better known, and

whose treatment is more familiar to us, than as something anomalous and unlike every other disease, we shall, I trust, escape the necessity for vainly searching for some principle capable of reconciling modes of treatment so much at variance as hot air-baths and warm stimulant potations on the one hand, and snow-baths and iced-drinks on the other. Æther, alcohol, tartar emetic, calomel, lead, lunar caustic, croton oil, naphtha, assafœtida, quinine, opium, peppermint, bleeding, blistering, and the actual cautery, are only a few of the remedies that have been proposed for the same stage of the same disease, all equally failing—all equally recommended as infallible.

Still, there is one valuable conclusion to be drawn from the very contrariety of the means employed, viz., that almost all have felt that the more obvious indications (which the well-marked symptoms of cholera would lead us to follow,) are not to be trusted as guides in the treatment of the disease; that some broader principle is necessary to direct our practice than the fallacious one of opposing the cold of the body by external heat, the collapse and sinking of the vital powers by stimulants, or the purging and muscular spasms by opium. Indeed, the abandonment of this method of treating symptoms, and the loss of faith in heat and stimulants are what chiefly distinguish the more recent works of experienced authors from those of more ancient date, and of less practical acquaintance with the disease.

In concluding these prefatory observations, I beg to remark, that I have endeavoured to avoid discussing the opinions of others, because this is unnecessary to the audience I am addressing, who are not only well acquainted with the works of the best authors on the subject, but also possess a large amount of unpublished knowledge of the disease from their own practical experience.

The similarity of cholera to the cold stage of ague is too obvious not to have occurred to many, but there were, unfortunately, two circumstances which contributed much to prevent this analogy from being pursued to its full extent—the first being the want of any *obvious* stage of reaction, relaxation, or intermission; the other, that quinine and arsenic were not found effectual in the treatment.

It was observed that recovery from the cold stage of cholera in this country, was not unfrequently followed by a low form of continued—I should rather say remittent-fever, and this, instead of being regarded as one of the modifications of the disease, induced by climate, or, as a secondary effect of primary disorder, was considered by too many as the hot fit which belonged to the protracted cold stage that had preceded it.

I shall endeavour to point out that this form of fever not unfrequently precedes and follows cholera as an epidemic, even at long periods, before and after the appearance of the major malady, and sometimes occurs as its substitute; and that while it prevails in a country,

the "epidemic constitution" to which it belongs, more or less influences the course of almost every other form of disease, by producing a greater than usual tendency to venous congestion, and diminished or irregular action in the capillary circulation; and that the type of sporadic fever in this country has for several years approached very nearly to this, especially in the last few weeks;—as also in Edinburgh in 1843, in Liverpool in 1844-5, and has been more particularly displayed in those cases of fever that have arisen spontaneously from putrid vegetable matter.

Finally, I hope to convince you that the alliance of cholera with other diseases, of which quotidian periodicity is a marked characteristic, gives us reason to believe that even when apparently most destitute of regularly recurring periods, it is not utterly so in reality. That it is, in fact, by considering cholera as an aggravated congestive ague of quotidian type, that we shall most nearly arrive at just principles on which to conduct its treatment, and that the means by which cure is effected in the one, are both theoretically and practically those which are most appropriate to the other.

DISTINCTION BETWEEN MALARIOUS AND CONTAGIOUS FEVERS.

Before we enter on the consideration of those diseases with which cholera is allied, and of which class it may be considered one of the extremes, it will be well to devote a few minutes to consider the broader

points of distinction between this class and those diseases which are communicable by contagion.

By active disease, we mean either the direct effects of toxical or mechanical injury to some portion of the system, or the indirect effects produced by the resistance opposed by the vital powers to the operation of such causes—or, lastly, the effects of the struggle between the noxious influence and the vis medicatrix.

Fever is the most obvious and most frequent evidence of the activity of the vital powers to resist a noxious influence, and of the attempts of the powers of life to repair the effects or remove the cause of injury; but in considering fever in general, it is usual to limit the term from its wider sense to that condition which follows the absorption of certain poisons into the blood. When, therefore, we find fever to be the consequence of absorbed poison, it is of the utmost importance to endeavour at the outset to ascertain, as nearly as we can, upon what part of the system, and in what manner the effect is produced, and what are the actions by which its influence is resisted.

That the cause of both contagious and sporadic fever is introduced into the system by the blood, few indeed doubt; but the important question is, does the cause produce its effects directly on the blood itself by essentially changing its constitution, or is it only conveyed by the blood, like digitalis, aconite, opium, &c., and without causing any essential change upon the blood, operate by affecting the nervous system?

In an admirable essay on "Spotted Typhus," read before the Provincial Medical Association in August, 1848, Dr. Davies, of Bath, drew a broad distinction between the two classes of poisons which are known to produce fever, viz., animal poison, and malaria. There is one kind of poison, he said, which, arising in the blood, and finding the elements of its reproduction in the blood of another, produces a fever capable of being propagated from man to man; and there is another class of poisons, which, arising in elements extraneous to the blood, does not find its elements there, and though it produces fever, cannot reproduce itself, and is not capable of being propagated from one to another.

The second class is that which is now to occupy our attention, at least, that portion of it which embraces fevers produced by malaria, whose characteristic is periodicity,* and I shall venture to enunciate the proposition with regard to these even more broadly:— That although the first class of poisons—that, namely, which produces the exanthemata,—operates on the blood and changes its constitution, the second operates not on, but through, the blood, and produces its effects by its agency on the sympathetic system.

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* Some malarious poisons do undoubtedly contaminate the blood itself, and require special action for their elimination, as the Egyptian plague by boils; but these form a distinct genus of contagion, and do not fall under our immediate consideration. Those now to be discussed are all characterized by intermitting action till they reach their climax in cholera.

Both classes no doubt produce effects on the sympathetic, but the first, (the animal poison,) acting by a continuous irritation, produces a persistent effort of the emunctuaries to remove the poison from the blood, or continued fever; the second, (malaria,) acting solely by nervous impression on the sympathetic, produces merely a struggle between the impression which is opposed to vitality, and those sensations and actions that are provided for the maintenance of animal life. This struggle is what we observe to take place in the intermitting and congestive fever, &c.

If malarious poison actually changed the constitution of the blood, it would be as impossible to cut short a fit of ague as an attack of small-pox or scarlatina; but it is possible to cut short an ague, by means which (as we shall see when we consider the treatment of intermittent,) are not one of them calculated to produce an essential change on the blood itself. There would, too, in all probability, be some evidence of an elimination of the poison by the fit, as by the skin in the exanthemata, which there is not; or some difference in the blood drawn several hours before and after the fit; but in both cases it is equally incapable of propagating the disease by inoculation. Besides, it appears scarcely possible to account for the leading characteristic of the whole class of these fevers—their periodicity—on any principle but by the law of nervous impressions,—viz., that long-continued impression of an object upon a nerve of sensation impairs its sensi-

bility, but that a change of impression restores it; as the eye long fixed upon a point becomes insensible to the impression of light, but recovers its sensibility on being turned to other objects.

So the continued impression of the poison on the sympathetic subdues its sensibility to its appropriate stimuli,—namely, that excitability which resides in the vital organs, whose action the sympathetic governs and regulates, but its irritability becomes re-excited, and its energies are restored by the new impression caused by change from a natural to a disturbed condition of the organs, which is the consequence of the withdrawal of the influence that combines and regulates their functions. Like the eye, restored to a sensibility to light by change of object, where the energy of the sympathetic is thus restored by the new impression, the recovery of its natural sensibilities would be permanent, were it not again submitted to the same influence by which it had been paralysed at first; but as the optic nerve will again become insensible when the gaze is again fixed as before, and this sooner or later, as the object is more or less illuminated, so does the sympathetic require to be again undisturbedly submitted to the noxious impression after recovery of its sensibilities before the effect is again produced, and that for a longer or shorter time, according to the power of that impression.

If we shall see reason to believe that the effects of the malaria under consideration are not caused by actual change operated on the blood, it will be evident

that they can only be produced through the agency of the sympathetic, for we shall have frequent examples of malaria, affecting at one time the circulation merely, as in ordinary ague; at another only the bowels, as by diarrhœa or nervous constipation; at another the cerebro-spinal system, with convulsion, paralysis, or neuralgia; or, again, we have either two combined, or all three, as in cholera; and we know that with the exception of the blood, there is no other element in the system but the sympathetic, which is in such intimate contact or connection with the organs of circulation, of digestion, and with the cerebro-spinal nerves, as to be capable of producing these effects.

We find, too, that what we should expect from our acquaintance with anatomy, really is the case,—viz., that the capillary circulation is the part of the system which most frequently, most easily, and most completely, submits to the influence of malaria acting through the sympathetic, because almost every other part of the system,—the heart, stomach, lungs, viscera, &c., are all supplied by other nerves, the capillaries alone depending entirely on the agency of the sympathetic, whose branches are distributed to every, (even the minutest,) vessel in which our instruments enable us to trace it. This view is further borne out by considering the change from the healthy functions of this part of the system that are manifested when influenced by the morbid impression. In a state of health we know that certain chemical reactions take place between the

blood and the terminal structures of the capillaries, by which perpetual slow absorption and deposition of the tissues, is effected, certain secretions are produced, and animal heat is evolved, and that while those actions go on undisturbed, the blood flows freely and unimpeded through the gently distended, but still very minute, veins, without exhibiting the least tendency to adhere to them, as would be the case in inanimate capillary tubes of equal diameter; neither does the contained blood exhibit any tendency to coagulate. We know that this property of preventing capillary adhesion of the blood to the coats of the vessels, and of preserving the fluidity of the blood, is peculiar to the living inner coat, and not to living structures generally, for extravasated blood, in whatever part of the body, immediately coagulates. We are also aware that the capillary circulation possesses local excitability, (independent of the heart,) and the power to resist injury or repair its effects, and that excitability is a reflex action, commencing in sensibility, therefore that this latter quality depends upon the nerves of the vessels.

To prove that all these qualities are bestowed by the sympathetic nerve would lead us too far from our present object; suffice it to remark, that the first stage of ague and cholera is marked by diminution, or cessation of all these qualities and actions. Of the effect of this on the change of the tissues we have little evidence, but we see secretion stopped and converted into exudation, which physiology teaches is directly the

opposite of vital action; the evolution of animal heat ceases; the veins contract like those of the dead body; the blood returns from the terminal structures in a condition very different from that of health—not coursing freely through the vessels, but slowly, and as if adhering to their coats, and in a half coagulated state; and, lastly, by the application of stimuli, which would produce immediate reaction in the healthy skin, we find it almost impossible to excite these vessels in cholera. The direct conclusion from all this is, that impaired excitability, nervous energy, and vitality of the capillary circulation, is the first and most essential effect which we perceive of the impression of ague or cholera, and that the change produced upon the blood is more probably the secondary effect of the cessation or diminution of those changes which are produced upon it in the capillaries of the lungs, and of the system in health, than in any way attributable to direct influence of the poison on the blood. In the whole class of diseases now under consideration, we shall find evidence of disorder of the sympathetic system in one or more of its vital functions, displayed either in the capillary circulation, the digestive, or in the cerebro-spinal system; but in not one of them is any conclusive evidence to be found of a poison eliminated from the system by the fit, in any way resembling that which is thrown off from the blood in fevers produced by animal poisons of the class exanthemata, nor can we conceive any source of constant and universal irritation to the

constitution, such as would result from essentially diseased condition of the blood, to be characterized by intermissions, or to be capable of cure by means addressed to the mere interruption of periodicity. In such a case, on the contrary, we should expect only a continued and persistent action, till the end to be accomplished by the elimination of the poison is attained. Moreover, if it be granted that continued fever is the effect of a constant source of irritation existing in the blood, it would appear to be a corollary to the proposition that intermittent, as opposed to continued reaction, is direct evidence of the absence of a source of irritation in the blood, and produced by means diametrically opposite, which we have fair grounds for concluding to be the absence of that irritability in the very structures which become excited by a source of irritation existing in the blood in continued fever, or by their natural stimulant in health, viz., distension by the *vis a tergo* of the heart*

On these grounds, considering simple ague as the type of the whole class of diseases which we are about to study, we shall proceed to examine its symptoms and the indications for treatment.

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* The above reflections may serve to suggest the question to physiologists,—whether the circulation of the blood by successive pulsations of the heart instead of by a continuous action, may not be a wise provision of nature to avoid the exhaustion of nervous sensibility in the capillaries by continued impression.

SYMPTOMS AND TREATMENT OF INTERMITTENTS.

Intermittent fever has been generally described as consisting of a cold stage, with shivering; a hot or febrile stage; and a stage of perspiration and relaxation, followed by intermission. This description cannot, however, be received as a definition, because of these stages there is only one that is constant,—viz., periodical return of the cold stage, while the febrile and sweating stages are only the consequences of subsequent reaction and relaxation, and sometimes they are altogether absent. But shivering is by no means an essential to the cold stage; on the contrary, in the most dangerous forms of ague, shivering is often absent, as it is in cholera, because the disturbance of the circulation in the cold stage is such as to overpower this symptom.

Shivering may practically be considered not so much a symptom of the impression of cold on the body in health, or of the cold stage of ague, as an evidence that reaction is taking place in the system, and that the heart possesses the power to overcome the obstruction opposed to its action; it is, in fact, the first step of the febrile stage, and in the more malignant forms of ague, is the surest harbinger of recovery, and the best evidence of reaction.

In the medical treatment of ague, little is generally attempted in the cold stage beyond giving a stimulant diaphoretic, or in the febrile beyond cooling diluents.

It is customary to trust entirely to the means of prolonging and confirming the intermission by the use of antiperiodic medicines. But this routine method of treatment is not applicable in all agues, for these differ materially in the urgency of their symptoms, according to their various types, and the duration of their period of intermission.

Thus, both in quartan and in tertian ague, febrile reaction generally runs so high as to cause this stage to be most dreaded by the patient, and the congestive stage is of comparatively little moment, being accompanied with shivering almost from the first; but in quotidian the case is very different, the fever, where it exists, being slight and of short duration, and both this and the sweating stages often absent or imperceptible, and the interval between the fits is sometimes so short as to be insufficient to restore natural irritability to the system by repose. The congestive or cold stage is, on the other hand, extremely severe, and such as to endanger the vital organs, and even to give rise to the dread that the attack may prove fatal before it reaches the stage of reaction. In such a case, then,—and such it will appear is cholera,—not fever, but the opposite condition, is to be feared, and all our efforts must be directed to obviate or relieve congestion.

There cannot be a doubt that this is the most essential part of cholera, and the great source of danger in congestive ague; we shall, therefore, devote the remaining portion of this lecture to the consideration of the

congestive stage in its cause, course, symptoms, and effects, and to the mechanical means of relieving and curing it, whether natural or artificial, reserving what we have to say on the medicinal part of the treatment for a future lecture.

It is customary to date the commencement of a fit of ague from the commencement of shivering, and of cholera from the first purging, but this is an important error, for long before this symptom comes on, a certain shrivelled appearance of the skin may be observed; the expression of the eyes changes; there is a feeling of malaise and confusion in the head; the nails look blue and bloodless, and are marked with a red and white streak, and there is a tendency to yawn, and a feeling of oppression about the heart, accompanied with sighing.

The essential part of ague has begun, perhaps, as much as two hours before the shivering occurs; the blood is gradually forsaking the extremities, and is driven in upon the heart more quickly than it can be sent through the pulmonary circulation, whose capillaries also resist the free transmission of blood; it therefore accumulates in the great veins, so that a great part of that fluid which a short time before was circulating in the extremities, being now rejected by the capillaries, is forced to find room in the venous reservoirs, and great vascular organs of the chest and abdomen, while the arterial system is left comparatively empty.

The consequence of this congested condition of the

great veins is the disturbance of the circulation in the thoracic and abdominal viscera, and oppression of the action of the heart. This disturbance of function produces an excitement in the organs themselves (that is, upon and through the sympathetic nerves,) to resist oppression, by increased efforts to preserve life, or what is called reaction. When this effect is felt, the whole frame partakes of the influence, and manifests it by convulsive shivering. The struggle of the powers of life against the disturbed or oppressed action of those organs whose office it is to sustain life continues, till at length the highly-excited powers of the heart and arteries overcome the inertia of the capillary circulation, and the blood once more courses freely through the veins. But this excess of action does not immediately cease with its victory, a period of fever succeeds, till at length, exhausted, the capillary circulation becomes relaxed, perspiration bursts from every pore, the excitement of the heart subsides, and the circulation returns to its natural condition for a time. This period of repose is not, however, permanent; by and by the capillaries again succumb to the effects of renewed impression, the same effects are produced as before, and the same struggle ensues. The duration of the cold fit depends upon the susceptibility of the sympathetic, to be sooner or later roused to perception of the disturbance of the vital functions which is taking place, and on its ability to excite the organs of circulation to more healthy action, for according to this will the

struggle be more or less severe and protracted, and in like manner the severity of the congestion and abridgement of the period of intermission will be in proportion to the power of the impression to produce the paralytic condition of the capillary vessels by its agency on their nerves, till the climax is reached in cholera where intermission is either wanting, and consequently fatal, or very imperfect.

If, then, the cold stage commences in disturbed capillary action, and contraction of the veins of the extremities, by which the blood is forced into the interior sinuses of the chest and abdomen, distending the spleen, oppressing the heart's action, and disturbing the circulation and functions of all the viscera, it is evident that to prevent or remove this disordered capillary action before it produces the above effects, will be to prevent or cut short the fit.

If again capillary disturbance have taken place, but blood from the extremities be prevented from reaching the interior in great excess, and from producing more than mere disturbance in the circulation and action of the vital organs, the symptoms of congestion, or the fit, will be prevented, or so far mitigated that reaction will take place with comparative ease.

Or, thirdly, congestion and oppression to the heart having taken place so severely as to prevent reaction from being evinced by shivering, the removal of some part of the blood which is producing it will enable reaction to take place, and this last is the use of

bleeding in cholera, as well as in the cold stage of ague. In the cure of ague, therefore, the three intentions we have to fulfil by treatment are—

1st. To prevent disturbance of, or restore natural action to, the capillary circulation.

2nd. To prevent excessive congestion, by anticipation.

3rd. To relieve this if it have taken place.

The first has been accomplished sometimes by producing a sudden general impression on the nervous system,—as for example, by unexpectedly plunging the patient into cold water; but the more usual method is to give those medicines which possess a peculiar power in preventing periodicity, such as arsenic, and quinine, or if we consider their *modus operandi*, we might say those which possess a peculiar tonic effect upon the capillary circulation, or upon the sympathetic nerve in *that* part of its functions, and which, by producing an effect directly opposed to that of malarious poison, act as antidotes to it, in the same manner as diffusible stimulants will oppose the depressing effect of digitalis or aconite on the action of the heart.

The first, then, is chiefly the medicinal part of the treatment, which shall be considered hereafter; the second and third rather mechanical than medicinal, for the second intention is attempted to be fulfilled by the application of tourniquets to the limbs in such a manner as to retain as large a quantity of venous blood in the extremities as possible, but is still better effected by

the early use of venesection before the more prominent evidences of congestion have been manifested. The third is, after the congestion has taken place, to draw blood rapidly from both arms, in order to relieve the congestion.

The mechanical treatment of ague by bleeding, therefore resolves itself into this, that if we can bleed after disordered action in the capillaries has commenced, and as yet has only disturbed but not oppressed the action of the heart, but before the great struggle of the system which ensues on established congestion has begun, the abstraction of blood is easy, safe, and effectual in preventing congestion and producing immediate cure; but after SHIVERING has begun bleeding is unnecessary and may be injurious, because it will then disturb the healthy reaction of which this is an indication. If practised after the commencement of shivering, the first effect of bleeding will be to bring back the congestive stage with tendency to collapse, and afterwards to relieve it; but this is done at unnecessary expense of blood, and although generally successful in cutting short the ague, is practically neither so safe nor so effectual as bleeding just before shivering begins, and therefore it is better to delay till the next antecedent period.

When, however, the evidences of extreme congestion exist without shivering, shewing that it is becoming dangerous, and that due reaction is prevented by the mechanical obstruction offered to the heart by excess of

blood, bleeding from the veins cannot be had recourse to too promptly, in order to relieve it and the other organs from the pressure. But although in an early stage these are perfectly good and intelligible reasons for bleeding where congestion is severe, the practice is not to be recommended after it has existed for a long time, and the reason is this,—

Experience has proved, that often in cholera, where scarcely any external evidence is given of reaction, or restoration to a more natural condition of the circulation, such restorative action is nevertheless really taking place, and if the powers of life can only support the oppression without utterly failing, for a certain period, the disturbed circulation of the capillaries will by and by cease, the heart be freed from obstruction, and the general circulation be more or less restored. Practically, therefore, it is better and safer not to use the lancet if congestion have existed for several hours, lest by bleeding we disturb the natural tendency to recover, so far as to reach a second period of relief at least, if not of reaction; and lest instead of relieving oppression, we should thereby produce syncope. Under such circumstances it is more advisable to trust to medicine and to nature, until the 18th or 20th hour in quotidian disease, or till an hour or two before the next anticipated fit in other forms, shall afford us a safer and more advantageous opportunity of pursuing the same practice with a view to arrest the congestion in its renewed accession. The case, however, is very

different when early called to a patient in whom this condition has existed but a short time, and when there is reason to believe the heart to be in full possession of its energies and only in want of mechanical relief to enable it to resume its powers. In such circumstances there is not only no danger from bleeding, but its effect is little short of miraculous, and in the course of a few minutes, the patient arises from a bed of death and torture, to the full enjoyment of life and health.

In a case like this, no mistake is so fatal as delay, which is unfortunately the most general one, for here the timid practitioner fears to bleed when he can hardly feel the pulse; he hesitates and waits until he observes it rise a little, and then acting on the rule, rather than on the reason of the practice, he opens a vein, cuts short the feeble attempt at reaction, and kills his patient in the attempt to cure him. Some have even carried culpable ignorance of the intention of bleeding in cholera so far, as failing to obtain blood from a vein, to open an artery, little thinking how much reduced is the quantity of properly arterialized blood that amid the disorder reaches the left side of the heart, and that upon the capability of that little to sustain life through the struggle depends the life itself. By taking arterial blood he takes the life of his patient, produces syncope, and having removed the feeble resistance to death which still existed, his patient sinks at once and invariably into complete collapse. Of this fact there is no want of examples on record. Be assured that

the rising of the pulse after it had been lower, in cholera, as in congestive ague, is positive reason against bleeding, and the best evidence that the constitution possesses the power to carry the patient over that days' attack, to enable him to reach a second, if only let alone, and not worried to death with over-treatment.

When, however, we decline to bleed after the patient has been labouring under severe congestion for five or six hours, we must search for the practical reasons against doing so, in the effects produced by bleeding in the hot stage of intermittent. This is a subject on which I can speak from sad experience, and one on which unintentional evidence is borne by many cases reported in various works on cholera, where bleeding has been practised in a late, but yet too early, period of the attack.

In ague, when the period of congestion is over, and the febrile stage marks the progress towards a return to natural action, but where the impression which produced the cold stage is as yet only partially recovered from, it is evident, if the theory be correct, that the effect will as readily be renewed as that of light upon the half-dazed eye, which, having been fixed on an object till insensibility is produced, and only half restored to its sensation by removal, is again subjected to the impression, without sufficient interval fully to regain its powers, it becomes almost immediately re-affected as before, and insensibility is nearly instantaneous. So it is with bleeding in the hot stage of ague ;

syncope and return of the congestion are the immediate consequence; or in less severe cases, or later in the hot fit, the intermittent is in danger of being converted into remittent fever of a low, irritative, and dangerous character. Much more might be urged on this subject to show that, independently of the above considerations, that period which immediately succeeds to a long and exhausting struggle of the heart and arteries is not the most favourable time to expect a greatly increased effort, but that on the contrary, *when* the tone of the exhausted vessels, now long accustomed to an overcharge of blood, has become impaired, syncope should be a more probable consequence of venesection than relief and reaction.

If then we have reason to believe that there is a natural tendency to recover from this condition, and to dread and expect its renewal at a certain future period, we must be exceedingly careful not to run into the dangers we have indicated by mistimed bleeding, but rather trust for a time to medicine alone, and await that more favourable period, which, with proper care, will arrive with certainty in by far the greater proportion of cases of congestive ague, and also, I feel assured, from my own observation, in cholera.

We shall have occasion again to return to this subject, but were I called on to give a rule for bleeding in the congestive stage, it would be to bleed early and boldly in the first, or even the second, hour, but no later, till the eighteenth or twentieth, after its first

manifestation, when the same rule will again apply after the twenty-fourth.

The above remarks have been addressed chiefly to the consideration of the cold stage of intermittent fever, and their application to cholera has been only incidental; nevertheless I have stated, that I believe cholera to partake also of this character,—viz., a tendency to intermission, however slight and imperfect. I am unwilling to state a fact of this importance on my own observation only, and prefer to rest the argument on the near relation of cholera to quotidian ague, and on its connection with diseases of an intermitting character, with which it was associated in its first onset in Persia, in 1842-3. This will form the next part of our enquiry. For the present I shall content myself with stating that the perusal of cases by a great variety of authors, has not tended to change this opinion; and I conceive, that if read in the same spirit with which I perused them, this will also become your opinion of the disease, both as it has appeared elsewhere, and as it will probably appear here. In the usual form of cholera, I must allow that the symptoms of this condition are very slight indeed, and I cannot expect them to be conclusive with most, because only to be recognized, if recognized at all, on being sought for with the desire to find them, which undoubtedly is not good evidence. The obvious and external signs of its existence, merely amount to this,—that in most cases of cholera, not rapidly fatal, if visited about the eighteenth

or twentieth hours after the commencement, the patient will be found lying in a sort of lethargic condition, apparently more sunk and more resigned than he had been for several hours before; but on examining the action of the heart with the stethoscope, you find it more natural, and less excited;—you find the warmth a little more extended on the chest, and the patient rather sulky than insensible. If, finding him in this condition, you sit down by him, and watch what takes place, you observe this apparent lethargy cease, he again becomes agitated, throws off the bed-clothes that he had latterly permitted to remain upon him, the action of the heart again becomes tumultuous, and the previous signs of suffering are increased. This certainly is but small evidence of intermission, nevertheless it is some, and considering it as such, the efficacy of bleeding in this stage of repose, or just as the agitation is beginning, has been such in my own practice, as to my mind, to corroborate the opinion; whereas the result of the practice of bleeding, as the pulse begins to rise very slightly previous to the period when this kind of repose might be expected, (as I have seen practised by others, and which you will find recorded in many published cases,) is very much the reverse. I can only endeavour to reason on this subject, which must be left to the general experience of others to decide. I am satisfied, however, that as far as my own has gone, the rule I would attempt to impress, with regard to the most favourable times for bleeding, is correct.

There are, however, other objections to this belief, as, for instance:—Why should not a man recovered from cholera be subject to continued daily accessions? This I confess myself unable to explain, but such is the case with many other diseases, such as ague cured by bleeding; and it is not always the case in cholera, though the character of the accession may differ. It would be too much to attempt to explain everything of what is confessedly so obscure, that even the smallest contribution to its history is looked for with anxiety; but while upon this subject, I may merely mention one point of observation, which, if more extended experience tend to confirm it, may aid in explaining the natural mode of cure in cholera. This is, that in all forms of quotidian ague with which I am acquainted, there appears a marked tendency to run a course of three days; and you will find many recorded cases of cholera which will bear this interpretation, where, after three accessions, or, if you please, three days' continuance, of the symptoms of cholera, the coldness and purging cease. Thus, a man has purging one day and gets better; next day he is attacked with algide cholera, he struggles through this, and there is imperfect attempt at reaction, but the purging, cramps, and coldness again become severe on the third; if this day's accession be not fatal, he goes on into the stage of consecutive fever on the fourth day. Many cases appear to confirm this view, and many to oppose it, as may very naturally be expected, for it is by no means easy to distinguish the

effects of the very active treatment pursued, from those of a very active disease.

In treating of cholera, one of the greatest diffculties arises from its unfortunate and most inappropriate name, which would seem to point to the diarrhœa and disturbance of the bowels as the essential part of the disease; and if we look merely to popular opinion, and to that countenanced by documents issued by authority, it would appear as if no room were left for doubt upon this subject, and that the great matter to be attended to in the cure, is the arrest of diarrhœa, as if that were an invariable symptom, and the most imminent source of danger. It is undoubtedly very right in popular treatises and directions, to fix public attention upon this symptom, because in the epidemics best known, this has been one of the most frequent early effects, and it is well that it should be looked upon as most dangerous, and as a good reason for applying for medical aid before worse follows; but it does not become the professors of medicine, as a science, to adopt such an opinion without examination. We have endeavoured to shew, that, according to particular circumstances, of which we are ignorant, a very considerable derangement of a part may exist without that general disturbnace of the whole capillary system, which we observe in the cold stage of ague or a confirmed attack of cholera,—that exudation may at one time take place from the bowels; at another into the cellular texture of the body generally or locally; at another, that this

peculiar condition may chiefly affect the cerebral system, with extremely little or no evidence of the capillary circulation of the extremities partaking of the influence,—that in fact, the force of the disease may at one time be directed on one class of organs, at another on another, just as we witness the fever of one year to be attended with ulceration of the ileum,—of another in the colon,—of another with pneumonia,—and yet, another with cerebral affection, &c. So when we come to consider the different modes in which the system may be and is affected by the impression of a choleric influence, we shall find it producing the same effect on the general capillary circulation, and the same consequent congestion of the interior organs; but we shall observe that this oppression does not always seek the same mode of relief, although the end attained is still the same—viz., diminished quantity of the venous blood; and that it does not uniformly expend its force by exudation into the intestines, which is the form we are most familiar with, but that in like manner exudation will take place into the serous tissues and from the skin. These are my reasons for not insisting so much as is usual on the diarrhœa of cholera. Even those authors who lay most stress on this symptom, constantly inform us that the most malignant cases are those in which there is neither vomiting nor purging; such, for instance, as those extreme cases which almost always occur on the first arrival of cholera in a district, where the patient is seized with sudden pain in the

epigastrium and expires in a few minutes, with the word "water" on his lips, but without either vomiting or purging,—that is to say, without the more ordinary means of relief to the congestion having come to the aid of the vital powers. Thus, Mr. Parkes, who is one of our most recent authorities in cholera, but whose views are very different from those now advocated, says,—"There is no one who has seen much of cholera who does not know that, exclusive of the mildest forms of the disease, a case with little vomiting and purging is more malignant and more rapidly fatal than one in which these are prominent symptoms;" in other words, this might be expressed, that where natural relief to the congestion does not take place, the congestion proves more fatal. Premising then, that we do not look upon purging as essential to cholera, but only as the most common mode of relief to the circulation, and that it takes place earlier or later in the stage of congestion, according to the peculiar tendency of the epidemic and condition of the individual attacked, we shall trace the ordinary course of the first stage of cholera up to this point, passing over those cases of diarrhœa in which it would appear that the relaxed condition of the enteric vessels proceeds so *pari passu* with the general tendency to congestion, as to prevent any serious amount of internal congestion from occurring, such as is very frequent in the first day's attack of cholera, and from which many recover without passing into the algide stage.

When a man is attacked with cholera there is something in his appearance which another, who has had experience, may recognize, even before the patient himself is aware of it; there is a sunken clay-coloured cast in his features; he looks stupid; perhaps has slight dimness of vision, or dulness of hearing; his hands look shrivelled, the nails blue, and the veins on the back of the hand are dark and contracted, like lines traced with ink upon the skin; it is cold and damp to the touch; if you look at the tongue you find it clean, pale, and watery, as if macerated till deprived of blood, and the conjunctiva is bloodless. He, perhaps, now feels a sudden but slight pain dart occasionally through the left breast and arm, and if you press upon the epigastrium, especially over the spleen, he complains of pain; he has then sudden call to stool, perhaps vomiting, or according to the character or type of the disease, there may be intense pain and sense of burning in the stomach, &c. Let it suffice for the present to trace the course of these symptoms up to this point.

The shrunken condition of the skin indicates the state of the peripheral capillary circulation, and when the minute vessels reject their blood, it accumulates in the vena cava, overcharges the right auricle and ventricle, and interferes with their action; it distends the spleen to its utmost capacity as the reservoir provided by nature to relieve the circulation when oppressed by too great accumulation of venous

blood. Hence pain referable to the heart, and that produced by pressure on the spleen; the pulmonary capillaries partake of the inaction, and resist the passage of blood through them, the accumulation of blood continues from the exterior, and the inferior cava is so distended that it is impossible for blood to enter it by the renal veins; the circulation of the kidneys being thus suspended, their secretion is arrested, hence suppression of urine. Still blood accumulates which cannot revert through the well-valved veins of the extremities, and it must go somewhere. The portal circulation already oppressed, so as to prevent the secretion of bile, now yields still further, and the blood is forced back in a retrograde course along the mesenteric veins till their minutest terminations on the villous coat of the intestines are injected as with a syringe, and their natural action is at an end; a little more blood suffices to make the watery particles filter through the membrane, carrying with it the mucous epithelium, which gives that peculiar character to the cholera evacuation that has been so aptly described as the rice-water purging. No sooner, however, has a large evacuation of this kind been produced, than immediate relief is, in the great majority of cases, experienced by the heart, which had been struggling with the load of blood that oppressed it; the same effect is produced directly, which we endeavour to produce indirectly by opening the external veins, and the heart is thereby enabled to overcome the resistance of the

capillaries, (already as I think disposed to relax,) thus a natural cure is often effected which the well-timed administration of a stimulant may hasten and confirm. If, however, this first evacuation of the fluid particles of the blood does not suffice to give relief, the same effect of increased congestion and attempted relief by purging takes place again and again, till the power of reaction ceases, and the patient dies in the stage of collapse. Perhaps, however, he is more fortunate, and though ineffectual to produce complete relief, and restore the power of the circulation, these frequent drainings of the interior veins may suffice so far to relieve him, that the congestion does not destroy life, and there may yet be sufficient blood pass through the pulmonary capillaries to maintain life through this emergency, and so far to restore more natural action, as to permit of something approaching to repose to the patient from his sufferings, and partial return of the organs to their natural functions. This would doubtless proceed gradually to reaction, and sometimes does, when there is no fresh accession of capillary disturbance to renew the congestion, and the consequence is the low form of fever alluded to; but unfortunately there appears to be a tendency to relapse, which my observation leads me to believe occurs just twenty-four hours after the first commencement of capillary disturbance, the weakened powers now more readily yield to the oppression, and about the twenty-sixth or twenty-eighth hour is one of the most frequent periods of death in

those who have escaped during the first eight hours. After this the period of death must be uncertain. I find that Mr. Parkes gives the average period of death in forty-one cases as twenty-seven and a half hours from the commencement.

Having now considered the subject of congestion at great length,—first, in its probable origin; secondly, in the effects on the peripheral circulation; thirdly, in the effects and symptoms of the distension of the thoracic and abdominal veins and viscera; and fourthly, the artificial and natural means of mechanical relief and cure, we have now, I think, only to consider the symptoms of recovery and of death in cholera, and of the *post-mortem* appearances, before we proceed to the medical part of the treatment.

The symptoms of the disease, if traced in their natural order of succession, are as follow, and those of recovery these reversed :—

1st. Constriction and bloodlessness of the capillaries—of the skin first, and afterwards of other organs, with coldness of surface.

2nd. Oppression of the heart and pain in the epigastrium.

3rd. Suppression of urine.

4th. Suppression of bile.

5th. Vomiting and purging—one or both, or neither.

6th. Muscular spasms in connection with spasmodically contracted colon, and with the state of the bowels.

7th. Especially where the purging has been absent

or scanty, tetanic convulsion of the whole body, from the influence which at first was confined to the circulating system, proceeding onwards to attack the nervous system, as the circulation of arterial blood ceases.

8th. Death by mere cessation of action of the heart, &c., or by affection of the nervous system in tetanus, or both combined.

9th. Returning warmth to the skin after death, which in life had been icy cold. This, as far as my own experience goes, is always the case, commencing in the extremities, but, perhaps, does not occur so remarkably when purging has been very long continued, and the disease much prolonged. Of this I cannot speak with certainty. Partial return of warmth to an isolated part of the body is mentioned by several authors as a sign of extreme malignancy in the attack, and a certain forerunner of death. In all the cases where I have witnessed death from a rapid attack of cholera, whether in this country or in Persia, where there was either no purging, or where it had soon ceased, I observed the feet begin to get warm some minutes before death, and sometimes the warmth spread even above the knee, while the thighs and body remained cold, and the patient yet lived. This I look upon as a sign of death, beginning in the extremities, and I have invariably observed, that after the occurrence of this symptom, the least interference was apt to induce tetanic convulsion. I have twice attempted to draw blood under these circumstances, but am now convinced that this

symptom always is a sign of death, whether the patient appear to live or not, and I feel much inclined to consider that hope still exists, so long as this symptom is absent. We shall again allude to the subject in speaking of the treatment by heat and cold.

Post-mortem spasms in the muscles not unfrequently occur, even some hours after death. These do not appear to be the mere effect of relaxation of spasmodically contracted muscles, but rather resemble the jerking spasms produced by galvanism on an animal recently killed, and are probably intimately connected with the cause of warmth returning to the corpse, which in life had felt icy cold, and afford some reason to believe that the condition of the capillaries produced in life by cholera is not a mere cessation of vitality in them, as we have considered it; for the cold, where purging is scanty, exceeds that of any dead body, insomuch as to suggest a question, whether the chemical or electrical actions in the capillaries which belong to health may not be not only impaired, but actually reversed in the disease, and heat absorbed instead of being evolved.

On opening the body after death, the appearances are exactly such as we should expect from the symptoms. The right side of the heart is found gorged and dilated, the dilatation and softness of the coats being greater as the case has been more protracted. The superior and inferior cavæ, and all their larger branches are loaded with dark, tarry, half-coagulated blood.

The minute vessels of the lungs are empty, and the parenchyma often shrivelled and contracted, being pale on the surface, while the pulmonary artery is quite full of blood. The left side of the heart is contracted, and either empty or contains firm coagula, indicating a greater amount of life in the arterial than venous blood, which forms no solid coagulum; and the contraction of its auricle and ventricle a less amount of exhaustion of the muscular fibre in the left than on the right side of the heart, which is flabby and distended. The amount of congestion of the portal circulation, &c., is always considerable in the large vessels, but the smaller ones are more pale and bloodless in cases in which there has been much exudation. The gall-bladder generally contains bile. The spleen is commonly congested, but sometimes found empty, which is, probably, a *post-mortem* effect of its peculiarly elastic structure; when death takes place from consecutive fever, however, it is soft and friable, an almost invariable condition in death from malarious poisoning by tropical fever, &c. The intestines present a peculiar appearance, the colon much contracted and empty, the small intestines containing fluid similar to that passed in life, mixed with a denser material, probably the fibrin and albumen in peculiar chemical combination from which the more fluid part of the blood had been drained off, the sub-mucous coat is distended in different places with the same material, sometimes mixed with broken-down red globules. The kidney is nearly normal, and the bladder contracted;

the brain seldom presents anything peculiar. These appearances require no comment beyond that which has preceded their description in relation to the symptoms.

It would appear, then, that congestion and oppression to the circulation is always the cause of death, and that congestion is a consequence of disordered capillary circulation. We have sufficiently discussed the modes, both natural and artificial, of relieving this congestion, but as yet we have left the means of removing its *cause* in the capillary circulation altogether unconsidered, although it is perfectly evident that this must be the main object of medical treatment. The patient is cold, nothing can be more natural than to try to warm him, and the circulation being nearly at a stand, what is more natural than to rouse it with a stimulant? But then he is cold only to our sensations, not to his own; he is burning and suffocating, and cannot bear even clothing, much less external heat; he is craving for ice and cold water, and the hot stimulant draught which we force upon him is torture; he is vomiting, yet anxious to drink.

Are we then to treat him according to our sensations or to his?—to torture him, or indulge cravings that appear to us so opposite to what his condition requires? —or are we to disregard alike his sensations and our own impressions, and seek for the means of removing the cause of this anomalous condition?

These are questions which must be considered in a future lecture.

LECTURE II.

GENTLEMEN,—

The object of my former lecture was to show that cholera is not a disease *sui generis*, but one of a large class produced by malaria;—that, operating on the individual, either by some agency with which we are unacquainted, or by a poison carried by the blood, its cause does not directly affect the blood itself, and is not capable of reproducing itself in the blood by the elaboration of fresh poison in the system, like that of contagious fevers;—that no poison is extricated from the blood by special emunctuary action, like that of small-pox and scarlatina, which is capable of giving rise to the same disease in another by inoculation or by infection;—but that diseases of this class are all incommunicable from man to man, and as there is no remedial object accomplished by the morbid action, and no noxious poison removed from the system, there is necessarily no crisis and no natural termination; and so far from disease of this class conferring immunity from subsequent attacks, one of its chiefest characteristics is a tendency to relapse, and to render the patient more, rather than

less, amenable to the influence of the same impression for the future. Not being like that of exanthematous fever, (a poison which produces disease in the blood itself,) the cause of this class of disease does not operate as a source of continued irritation to the whole system, so as to produce continued fever, but on the contrary, acting in a manner directly opposite, it induces diminished irritability of the system, instead of irritation, causes depression, instead of excited action of the vital powers, and when fever succeeds, it is that of mere reaction against an impression opposed to vitality, not a struggle to rid the system of a source of irritation which proceeds unintermittingly towards the accomplishment of an end. When, in continued fever, the digestive and cerebro-spinal functions become disordered, they do so in consequence of an all-pervading source of irritation, and in conjunction with excited circulation; but in the class of diseases under consideration, we observe the effects displayed in the digestive, the cerebro-spinal, or the respiro-circulatory functions, separately, or in either two combined, in conjunction with depressed circulation.

In continued fever, death occurs either in consequence of organic disease and change of structure in some vital organ, or from the effects of poisoned blood upon the brain; but in the class of intermittents, the brain is the last to die, death taking place in consequence of mere disordered function of the vital organs, without organic change of structure.

These wide distinctions between the effects which we know to be produced by the circulation of diseased blood in contagious fevers, and those which follow the impression of the unknown cause of epidemic diseases, led us to the conclusion that the phenomena displayed in the latter class, were inconsistent with the belief that actual change in the constitution of the blood could possibly be their cause; and if not on the blood, it is quite evident that whatever produces these effects, must operate on the sympathetic system of nerves, for only the sympathetic and the blood are distributed co-extensively with the effects observed.

We showed reason to believe, that in every disease of this class, the first effect of the unknown cause is an *impression* on the sympathetic, which impairs its power of exciting, regulating, or combining the functions of those vital organs whose unity of action is necessary to life; and that this impaired vitality or disturbed function of the sympathetic, is variously evinced by derangement of the functions, in whole, or part of the circulating, digestive, and cerebro-spinal systems, either separately or combined.

It was argued that, great as the changes are that take place in the appearance and quality of the blood, during a fit of cholera, this does not at all militate against the above conclusions, for these changes are not primary effects of poison on the blood, but secondary effects of functional derangement in the pulmonary and systemic capillaries, by which the blood is pre-

vented from undergoing the processes necessary to its healthy condition, and to the maintenance of its vitality. It is only in consequence of cessation of such changes that the blood in these diseases loses its florid colour, its fluidity, and its vital property of forming a firm coagulum as it dies, for it has been observed that blood drawn during an attack of cholera, as well as that taken from the body after death, shows all the wonted disposition of healthy blood to absorb oxygen from the air; and when we practise bleeding successfully in a severe case of cholera, and thereby restore the patient to immediate health, we find that the blood which at the first was dark, tarry, and adherent to the wound, and made to flow with the greatest difficulty, presently runs more and more freely, and before we bind up the arm we see it acquire all the characteristics in colour, fluidity, and free uncontrolled motion through the vessels, which belong to healthy blood. It is obviously impossible, that by simple removal of a portion of the blood, all the dread poison on which so many authors of repute insist, can be removed from the whole mass of the blood, along with this small quantity; on the contrary, this is rather an *experimentum crucis*, to show that restoration of the functions of the minute circulation in the lungs, and throughout the system, is the cause of the immediate restoration of the blood to its natural condition, which had been altered only by the cessation of these functions.

Having thus endeavoured to prove that the disorder

is not originally, but secondarily, in the blood, and therefore that the primary cause must be morbid impression on the sympathetic nerves, we proceeded to consider the great characteristic of this whole class of disease,—viz., periodicity of attack, and attempted to explain periodicity by the law of nervous impression,—viz., that long continued impression on a nerve of sense subdues its sensibility, and that change of impression restores it, endeavouring to show that when disorder is induced by morbid impression in one part of the circle of vital actions, the derangement of action which results in other associated organs produces a new impression on the nervous system, calculated to counteract the first, and restore to the nerve the power of being excited by its natural sensibilities in all its varied functions.

Having then shown that the first stage of disease is alike in ague and in cholera, it was argued that they are alike subject to the same effects or law,—viz., periodicity of action. Such was the theory of the disease then offered to you. Practically it was shown, that in ague and in cholera, the morbid impression always evinces itself first in apparent cessation of vital action in the capillary circulation, more or less general, and more or less complete, by a greater or less approach in the condition of these vessels to that of inanimate tubes or dead structure; that the vital changes on the blood in the terminal capillaries then cease, animal heat is not evolved, the blood instead of coursing

freely through the minute veins, adheres to their coats, and instead of remaining fluid, assumes a half-coagulated condition, as if the interior coats of the vessels had lost a portion of their known power to maintain its fluidity; that the veins, instead of being gently distended, contract, as in death, and reject their contents. Such was considered the first condition of the cold stage.

It was then shown that the direct effect of this first condition, and of the continued rejection of the blood from the capillary circulation, is its accumulation in the great sinuses and venous organs of the chest and abdomen, oppressing the action of the right side of the heart, deranging the circulation, and impeding the functions of the principal vital organs, and diminishing the quantity of arterial blood distributed to every part of the body; therefore, as upon this the life of the whole body depends, destroying vital action, and causing collapse and death.

But to this fatal effect of derangement of the capillary circulation and consequent congestion, it was shown that the system does not always submit without a struggle, although such is the history of those almost instantaneous deaths from cholera sometimes met with; on the contrary, when from the above cause the cavities of the heart and spleen are distended, (for I believe the spleen, with its sympathetic plexus and elastic structure here plays an important part,) the immediate effect is that produced by the distension of any hollow

muscle,—viz., a stimulus to contraction. This becomes a source of excitement to the nervous energy of the heart, and through it to the sympathetic, in all its functions, and among others, to the resumption of its power over the capillary circulation.

It was shown that in ague the commencement of re-action is evinced in general convulsive shivering of the whole frame, then by fever, and then by return to natural action; but that in cholera the heart seldom experiences relief by simple re-action, without some mechanical aid to diminish the quantity of the blood thrust upon it from the extremities; that this is effected either artificially by bleeding from a vein, or naturally, by the exudation of the watery part of the interior blood into the mucous or serous tissues. It was stated that this mode of relief commonly, but not always, takes place from the intestines, by the watery diarrhœa of cholera, and is then either sufficient at once to relieve the circulation, and conduce to complete cure, or if insufficient for complete cure, it may be adequate to prevent the immediate death which would result without this relief to the congestion, and thereby permit the disease to reach its secondary stages; or, third, failing both these methods of relief, the power of the heart succumbs to the oppression, after which the morbid impression, unopposed by circulation of arterial blood, attacks the cerebro-spinal system, and terminates the struggle in convulsion.

Lastly, attention was invited to the fact that cholera,

like congestive ague, has its diurnal periods of accession and remission,—that, therefore, extreme caution is requisite to avoid attempting to give relief at all times by the same means; for bleeding, which will relieve all the symptoms in the first hours of an attack, will produce the very opposite effect some hours later, unless we wait for the commencement of the new accession, when it will be equally applicable as in the commencement of the first attack.

It was also thrown out as a suggestion worthy of inquiry, whether cholera does not follow the course usual in quotidian agues,—namely, a tendency to run a course of three days' repetition of congestion, where death or cure does not interfere, for in that case the return of the congestive stage will not be so much to be dreaded on the fourth day as consecutive fever.

In dwelling upon these points I was not so presumptuous as to hope that you would receive any one of them solely upon my individual statement, or even consider them proved by the arguments adduced. My intention was rather to state them as points, which, after much reflection and study of the subject, both in its general features and in individual cases, appeared to me to be those to which it was most important to direct attention both in reading and in practice; because, if we are convinced that the disordered action begins in the capillary circulation,—that the congestive stage and symptoms are direct consequences of this disturbance,—that this condition is not absolutely persistent throughout

the whole attack, but goes through certain diurnal changes,—and that the general tendency is for the congestive stage to return on three successive days, and afterwards to run into more continued febrile action, we shall then have a clearer perception of what is wanted in respect of treatment at each particular stage of the disease, and can adapt our remedies accordingly; and from knowing what to expect, we are enabled to distinguish the effect of the remedies from that of the disease they were intended to combat.

I am aware that by many the theory of the disease may be considered to have been unnecessarily dwelt upon, especially in that part of it which attributes the effects to morbid impression on the sympathetic, but this appeared to me important with regard to diseases associated with cholera, and in reference to the *modus operandi* of medicines which are found to exert a beneficial influence, in very different, and apparently very opposite, conditions of the system, arising from the same malarious influence.

I have stated it to be the object of the present inquiry, to search for some enlarged principle for our guidance in the treatment of cholera, which the observation of individual symptoms only tends to confound; and it seemed absolutely necessary, before considering the means employed for cure, to inquire, as carefully as possible, into the essential nature of the malady, in order to comprehend distinctly the intention to be kept in view in administering medicine.

CONNECTION OF CHOLERA WITH OTHER EPIDEMICS, AND THE INFLUENCE OF ITS CAUSE ON THE CONSTITUTION.

We have hitherto considered the malarious influence by which ague, cholera, and remittent fever are produced, only in its physiological effects on the human body, with little reference to their epidemic character, and without attempting to fathom the origin or nature of the cause. The cholera atmosphere, as it has been termed, is generally believed to pervade chiefly the lowest levels of a tract of country, and I believe this to be correct; but it is singular, that one of the spots pointed out to me as most fatal in Persia, was elevated 10,000 feet above the level of the sea, another 8000, and another 5000; it is also said to follow chiefly the courses of rivers,—in Persia it has uniformly been most fatal in the neighbourhood of the extinct volcanoes which crown the great range that extends from Ararat to Khorasan. Dr. Prout observed, that during the prevalence of cholera, the atmosphere was perceptibly heavier than at other times. In 1834, it was observed in the great London breweries, while cholera was at its height, that fermentation proceeded more rapidly than was usual, and of late an attempt has been made to connect it with certain remarkable electrical phenomena. Dr. Prout's observation has been verified, that during its prevalence a greater than usual tendency exists to the formation of oxalates in the urine. In

both its invasions of Europe, its progress has been very remarkable, and not in the direct commercial route from India, having in its first advance adhered steadily to a course nearly W. NW. from Hindostan; and its second was nearly identical, but with greater extension of its left wing, towards the south.*

Not only to medical men, but to the Government of

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* Without attaching much importance to facts which cannot explain the progress of cholera in other directions, I may mention as a contribution to its history in central Asia, that its first advance upon Persia in 1828-9 was preceded by terrific earthquakes in 1827-8, extending for some hundred miles around the extinct volcano Demawund; that there then succeeded a period of repose from subterranean convulsion till 1840, when a violent earthquake shook down a portion of Mount Ararat, and continued to convulse the country to the south-east of that mountain till December, 1843, when, for the first time in the historical period a new volcano burst out near Shoomacha, thirty miles to the west of the Caspian and perhaps a hundred to the north of Ararat. That in 1841-2-3 the whole tract of country between the mouths of the Indus and the Caucasus had been overrun with unaccustomed dysentery, pestilential fever, and in some places cholera, but after this eruption earthquake ceased and the country became comparatively healthy till 1846, when cholera again broke out with virulence in the same tract. Since then, the main line of the advance of cholera has been in the direction of the volcanoes of Iceland I leave it to philosophers to determine how far it is possible that extensive chemical action beneath the thin crust of this earth, evinced by these earthquakes may have had anything to do, either by disturbance of electrical currents, or the evolution of imperceptible gases, in producing so great an amount of disease, and in determining the main direction of cholera over the line of country which evidently overlies an immense volcanic tract, that extends in a direct line from Cutch, late the scene of extensive subterranean disturbance, over the Elboorz and the Caucasian mountains, towards the principal volcano of north western latitudes

every country through which cholera has passed, its progress has been a subject of great interest in reference to the question of contagion, ever since its first recorded outbreak in Lord Hastings' army in India, in 1817. But although none of the well-contrived barriers opposed to its advance by *cordons sanitaires*, or quarantine regulations, have yet succeeded in delaying its march a single hour, the question is not yet settled. The facts just alluded to all display the general pervasion of something unknown, which influences the physical as well as the animal world, but is wholly beyond the power of man to stay; and, did time permit, others might be added still more convincing, bearing upon the change observable in the features of disease, both in the animal and the vegetable kingdom, long before the actual appearance of cholera. If, then, the connection of these facts with cholera were better determined, and collected, it seems to be impossible to doubt, that the evidence of the existence of a cause, infinitely more general than mere contagion, would soon prove so conclusive as to set the question at rest for ever.

These are points, however, far beyond my depth, and foreign to our present object, which is practical observation of the facts which indicate the relation of cholera to other diseases, and to inquire what useful inferences may be deduced from them.

With respect to the epidemic influence of the cause of cholera in this country, a great change in the character of disease must have been remarked by the

elders of the profession since its first advent; and it is notable to younger practitioners, that there is an obvious difference in the practice of the generation of physicians which is now passing from among us and their own,—those of the old school are much more stringent in the article of diet, more cautious of the use of stimulants, and altogether much more apprehensive of inflammation, than we of more modern days; it is also notorious, that tonics are now much more universally prescribed than they were five and twenty years ago. All this is attributed by some to *fashion* in medicine, but it is evidently not so. In the commencement of the present century, the prevailing fever was attended with high arterial action, and the more acute forms of inflammation were much more general, insomuch that the lancet, calomel, and antimony, were then the sheet-anchors of the practitioner; but since a few years previous to the first inroad of cholera, a great change has occurred, bleeding in fever is now almost obsolete, a much more generous diet has become necessary, and calomel is comparatively disused. Since 1843, the type of sporadic fever of this country has approached more nearly to that of the remittents of tropical climates, displaying an unusual tendency to relapse and to local congestions. Neuralgia has also greatly increased; and Bright's disease, and dropsy after scarlatina, &c., become more general. This particular type of fever has pursued so remarkable a course, that before submitting to you the account of the

various forms in which it appeared in Persia in 1842-3, when obviously and essentially connected with cholera, I think it of some importance to trace its progress from India to this country, great part of which I have myself had the opportunity of witnessing.

In 1841, a fever of remittent and quotidian intermittent type broke out in Scinde, where it destroyed many of our best troops, alternating occasionally with cholera. Both proved severe in Caratchee: it spread through Beloochistan, and appeared at Bunder Abbas in the Persian Gulph, early in 1842; also at Yezd. It thence spread westward to Shiraz, and northwards towards Ispahan and Tehran, proving everywhere extremely fatal; but its further progress to the northwest was arrested for a time, on the high grounds of Sultanieh, by the setting in of winter. Next spring it resumed its course, overspread Aderbijan, Erivan, Georgia, and the whole shores of the Caspian, crossed the Caucasus, and was very fatal in Veronish in the centre of Southern Russia. I here lost sight of it in November, 1843, but was not a little surprised to find it again in December, on my arrival in Edinburgh. It was there modified it is true, and displayed less of an intermittent character, but was fully characterized by other symptoms, especially the tendency to relapse, and the pale tongue. Late in 1844, it appeared in Manchester, especially in Ancoats, where it was very severe. In 1845, it became epidemic in Liverpool, somewhat more modified in type, and the fever of

1846-7 in Manchester, still preserved much of its peculiarites, especially in the frequency with which it was accompanied with jaundice, and in running a course of seven, fourteen, or twenty-one days, and relapsing at these intervals.

For nearly two years, the typhus fever imported from Ireland has rendered this type somewhat obscure, but within the last few weeks, beginning in the middle of September, the same form of fever has become again prevalent and very severe. This will suffice to shew that not only cholera has spread to us from the east, but that since its arrival the very constitution of our fevers has partaken of a similar oriental character, and that not fashion but necessity has demanded the remarkable change of practice alluded to. The best evidence that this is not an *imported* change is to be found in those fevers which arise sporadically from the decomposition of vegetable matters, for these always assume the character of the epidemic then prevailing; as when plague broke out in Oxford, caused by a collection of putrifying cabbages, in the reign of Charles the II., and the most exaggerated form of the type of fever we are now considering appeared in a farm-house in Peebleshire, in 1846, caused by the decomposition of stable manure; so cholera generally makes its first apearance in the neighbourhood of cess-pools, and in the most filthy parts of a city, and judging from my own experience, I should think that ague of a quotidian type must have been remarked by

many within the last three years in badly-drained houses. But it may be remarked, that the epidemic constitution of disease is not only displayed in the fevers of a country, but equally so in every aberration from health, and ever since the first appearance of cholera in this country, these have been such as to require a much more extensive use of tonic medicines. In proportion as the lancet has fallen into disuse, iron has come into vogue in another shape, for although seldom prescribed five and twenty years ago, except for chlorosis, in the form of Griffiths' mixture, the greater proportion of diseases are now treated with chalybeates, a perfect evidence to my mind, not of the influence of fashion, but of necessity, and offering, I think, good grounds for attributing to iron some especial influence over the effects of the prevailing epidemic constitution in this country. For, whereas, thirty years ago this was marked by a highly inflammatory tendency, what we have now chiefly to combat, is feeble and imperfect arterial action, and a great and general disposition to venous congestion, not only in fever but in other diseases.

The usual indication for the use of iron is a pale tongue, and this is a special characteristic of cholera, also in quotidian intermittents and remittents, at particular stages. Its *modus operandi* is generally considered to be to increase the facility with which oxygen is absorbed by the blood, thereby aiding its transmission through the pulmonary circulation, preventing con-

gestion on the right side of the heart, increasing the vitality of the arterial blood, and promoting capillary action in general, but it would also appear that iron exerts a peculiar, and perhaps independent, tonic influence on the nervous system ; hence its effect in tic-doloureux, sciatica, and spasmodic affections, such as hysteria and chorea. Its use as an opponent to mercury is pretty well known, and in moderating congestion, and regulating secretion, in menorrhagia, amenorrhœa, and Bright's disease, while its wonderful effect on spleen disease produced by ague, is well known to East Indian practitioners. These considerations, which would each admit of extended comment, will serve as an introduction to what I have to say of my experience of its effects in the epidemics I have now to describe, beginning in forms very different from cholera, yet ending in that disease, and while their history will demonstrate their near connection with cholera, it will also exhibit the singular effects of iron in various disordered conditions of the nervous system produced by malarious influence, and in restoring their appropriate action to medicines whose operation had been disturbed by the same cause, especially that of purgatives on the intestines, and of quinine on the capillary circulation in general as an antiperiodic.

In our present ignorance of the *modus operandi* of medicinal agents, the efficacy of iron in counteracting deranged action produced by malaria in the several systems,—nervous, circulating, and digestive, over which

the sympathetic exercises its controlling and combining influence, will be looked upon as little more than a curious fact, but as all the diseases I am at present about to bring to your notice occurred in a single year, between January, 1842, and January, 1843, the fact of iron having been found influential in all, must necessarily be taken as strong evidence of identity in the cause of those diseases to which it proved an antidote, and therefore strongly presumptive of a closer connection between tetanus, neuralgia, remittent fever, ague, and cholera, than is generally admitted.

The circumstance which first drew my attention to iron as an opponent to venous congestion, and to mercury, was a severe case of spleen fever, with much cerebral excitement; the symptoms were not such as to induce one to trust to a tonic medicine for the cure, and on the authority of Dr. Abercrombie I treated my patient with calomel, antimony, and salines. He quickly became salivated, an effect as readily produced in spleen disease as in Bright's disease of the kidney. Upon this he got alarmingly worse, fever and delirium increased, and he was only saved by immediate change of treatment to that recommended in Mr. Twining's work on "Diseases of Bengal,"—namely, the combination of sulphate of iron with purgatives. Under this course he quickly recovered from the salivation and fever, and by perseverance for a few weeks the spleen was reduced to nearly its normal size.

As enlarged spleen is the direct consequence of

repeated dilatation of the organ by successive fits of aguish congestion, and as subsequent experience showed that purgatives uncombined with iron did not produce the same effects on the spleen, it seems a fair conclusion that the iron operated by opposing the acquired tendency to venous congestion. In my own practice, too, I did not find quinine exert that miraculous effect in diminishing the volume of the spleen which is attributed to it by American and French physicians, and which has been exhibited by experiments on dogs; on the contrary, it often produced irritative fever, which it never did in combination with iron. The probable reason of this difference in the results is, that I employed it most where the prevailing type of ague was quotidian; others, where the type was tertian, to which the effects of pure quinine seem more peculiarly appropriate.

The next experience I had of iron as a remedy of unexpected power was a very singular form of epidemic disease, that exhibited extraordinary disorder of the nervous system.

A few weeks before this appeared in Tehran a somewhat similar epidemic had prevailed in Bagdad, but there it occurred in the form of angina pectoris, destroying life in a few minutes by spasm of the heart, particularly where any organic weakness existed in the organ, and similar, it may be conjecturcd, to the attack of which Lord George Bentinck and the corpulent Mrs. Armitage appear to have died.

A notice of the disease in the forms in which I witnessed it, may be found in an extract of a letter from me to my late brother, published in the Twenty-sixth Volume of the "Medico-Chirurgical Transactions;" and mention of a similar disease, as occurring in Strasburg in 1842, is made in one of the numbers of the "British and Foreign Quarterly Review."

This disease might be described as an epidemic apoplexy or epilepsy; it was characterized in the slighter cases by sleeping of the hand and foot of one side, coming on periodically every night at a particular hour, and accompanied with palpitation of the heart, nervously excited pulse, and severe headache, but though periodical in its attacks, I could not distinguish anything resembling a cold stage or actual fever. In the graver cases it appeared as coma, with hemiplegia, or epileptic and tetanic convulsions, which, if mismanaged, ended in death, at the same time facial neuralgia was unusually prevalent; in fact there was scarcely a form of nervous disorder which this disease did not assume in the two months during which it continued to prevail.

In two of the earliest cases, misled by acute pain in the knees and shoulders, accompanied with great palpitation of the heart, I mistook it for rheumatism, with commencing carditis, and ordered bleeding, and calomel and opium. By this treatment the symptoms were much aggravated, but they susbequently yielded to repeated doses of iron.

It were difficult to say what induced me to try this

remedy, but I fortunately hit upon it early in the disease, and there was not a single case among very many hundreds in which a few doses of iron did not suffice to effect a permanent cure; whereas quinine, purgatives, blood-letting, and mercury, all did more harm than good, and if uninterfered with, the disease often continued to recur nightly for weeks.

In the treatment of this epidemic, the Strasburg physicians were less fortunate. It does not appear that they had tried either iron or assafœtida, and the conclusion come to when the epidemic disappeared was, that "nothing seemed to do good, but that upon the whole, more of those who were bled rocovered than of those who were not."

This strange disease affords a good instance of a malarious poison producing its effects mainly on the nervous system, and a singular example of a disease so grave as to present symptoms which generally accompany only the most serious lesions of the brain, yielding completely and invariably to so simple a remedy as a drachm or two of sesquioxide of iron. There was here no remarkable change in the secretions, save that so long as it prevailed there was a general tendency to constipation, and nervous irregularity of the bowels, which I shall have to remark, as occurring again in a subsequent epidemic. For nearly two months while the disease lasted, I had occasion to observe that the action of purgative medicines was frequently either null, or it was unusually severe; but I

soon found, that where I was giving iron at the same time, their effect became regular, and for some time I was obliged to combine iron with every purgative, in order to ensure its action, even in cases where there was no other symptom of the malady present.

After the disappearance of this epidemic in March, the season was unusually healthy, with the exception of a few cases of a low form of apparently continued fever, but I believe really remittent, of the same kind with that which afterwards became epidemic in Edinburgh, in 1843, and which exhibited an unusual tendency to relapse.

In August, having few cases of importance to detain me, I quitted our sultry camp, then perfectly healthy, on an excursion into the Elboorz mountains, and pitched my tent at an elevation of 10,000 feet above the level of the sea, near the summit cone of the extinct, but still smoking, volcano Demawund. I was surprised on my arrival to find the tribes already deserting these celebrated pastures, and moving off to the lower grounds, the reason assigned was, that fever and death were busy amongst them, and that they were fleeing from pestilence. I saw several of the sick, and believed the fever to be the usual typhus of the country, but thought it strange to find it in such a locality.

On my return on the fifth day to camp, I found about a dozen of our attendants ill of fever, one man being in *articulo mortis*, but he had newly arrived from the eastward, from which it appeared that this epidemic

was spreading. He died comatose, with a black tongue, and symptoms that I now recognize as those of malarious poisoning, and which similate effusion on the brain,* but which I did not then recognize as such, and believed his fever to be the typhus, such as I had been accustomed to see in the five preceding summers in Persia, for I was not then convinced that eruption is diagnostic of typhus. In the other cases the tongue was thickly coated, all the secretions disordered, and I saw no reason to depart from the routine treatment by calomel, antimony, and salines, which till then had proved successful. None of my patients, however, improved under this system; on the contrary, they were getting worse, and one case seemed rapidly hastening to a fatal conclusion, from excessive vomiting and irritability of stomach.

At this time the Russian Embassy was without a physician, and my attendance was besought for one of the *attachées*, who was suffering from ague, to which, however, he had formerly been subject. Hitherto I had seen a great deal of ague in Persia, but almost always

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* This is identical with the fearfully fatal fevers of the Persian Gulph, which generally prove mortal very rapidly, with symptoms of effusion on the brain; but *post-mortem* examination proves these fallacious, and the only constant *post-mortem* feature is soft and friable spleen. Calomel and bleeding is the usual treatment, death the almost invariable consequence. I can confidently recommend rapidly repeated doses of quinine and sulphate of iron of each a grain, with a drachm of sulphate of magnesia, in three ounces of water, as most efficient in the cure of this much-dreaded form of fever.

of the tertian type, which very rarely indeed resisted the effect of a few small doses of quinine after a purgative. In this case the first two fits were tertian. I prescribed, and promised immediate cure, but in place of cure the third attack took place on the fourth day, and others on the fifth and sixth, and the tongue became dry, glazed, and cracked, such as I had not seen it before in ague, but very like that which we see in the worst forms of nervous fever, without eruption, now prevailing in Manchester. My patient was encamped by the side of a stream, and under the shade of trees. Conceiving that this might be the cause of the unusual severity of the attack, I had him removed to a house on the top of a hill; still, however, he became worse, the shivering less decided, the fever more prolonged, and the stage of sweating and of intermission more imperfect. It was now rather a remittent or a continued fever, like those in our own camp, than an intermittent; quinine was evidently injurious, and I was hesitating whether to adopt the same treatment, with calomel and antimony, that I was pursuing with the others, when, on examining the præcordia, I found that there was much pain on pressure over the spleen; upon this I determined to try iron, in combination with quinine, and gave him pills, composed of sulphate of iron and quinine, of each a grain, combined with one-third of a grain of aloes, every two or three hours. I entertained, however, but little hope of his recovery, for I then expected the next accession to carry him off; it was,

therefore, with no small surprise and satisfaction, that I found him sitting up next day at the hour which I had expected to be his last, and conversing with his friends. The tongue, which had been glazed and dry, and so deeply and painfully fissured as to prevent his speaking, had now become soft and moist, the pulse more free, and the shivering fit, which had been absent for the last two days, came fairly on, and he was well in a few days.

Returning to our camp full of the new light that had thus broken upon me, as to the nature of the fever which was now destroying hundreds in the neighbouring villages, and spreading with great rapidity, and to which this intermittent was evidently nearly related, I re-examined all my patients, and discovered in all, more or less tenderness in the region of the spleen or epigastrium, and on watching more narrowly, distinguished the remittent type, which had hitherto escaped my observation, for I now perceived that all the symptoms became aggravated after three in the afternoon, and that an imperfect remission took place in the morning; that the tongue, which towards evening, and in the fore-part of the night, was dry, black, and coated, was soft and moist in the morning, except in the centre, and the edges pale and translucent. I pursued the same principle of treatment in all, by combining iron with quinine, camphor, aloes, salts, &c., according to circumstances, and adding venesection or leeches to the pit of the stomach in the worst cases,

about the time which I judged to be the commencement of the stage of congestion. No sooner was this change adopted than I had the satisfaction of witnessing immediate improvement in all.

I must, here, however, mention the case of one of the table servants of the Embassy, who had for some days been affected with most obstinate constipation. I had given him castor-oil, senna and salts, calomel, calomel and jalap, colocynth, and croton, all in vain—nothing moved his bowels; and, disgusted with my inefficient treatment, he betook himself to the native practitioners in town.

After two or three days spent in the repeated administration of enemata, only one very small evacuation had been produced. I encountered this man on my return from the Russian camp, looking wretchedly ill, and in complete despair. I now remembered my former experience of combining iron with purgatives, and gave him ten grains of jalap powder, with one scruple of oxide of iron, which relieved him perfectly, and afterwards a grain of sulphate of iron, with three of aloes, always operated freely on his bowels.

At the time of which I have been speaking, while my Russian patient was so ill, I was called to see a poor French girl, whose father was a general in the Persian service, and encamped in a damp and thickly-wooded orchard near the town, about seven miles from our tents. She was apparently suffering from dysentery, passing blood and mucus, the skin hot, and pulse about 130.

I treated her according to the Calcutta practice as given by Mr. Twining, with ipecacuanha, blue pill, and extract of gentian, in which my experience of the fatal dysentery of the preceding year had given me the utmost confidence. No improvement, however, took place, and it appeared to me that there was no resource but to bleed from the arm, for I had frequently proved the efficacy of venesection even in the most reduced cases of dysentery without meeting with a single untoward circumstance to make me dread its effects; my surprise and dismay were therefore great, when, as soon as a few ounces were drawn, she became faint and collapsed, and the body mottled, blue, and cold. It was only now that I learned by cross-examination of her attendants, that in the first commencement of her illness she had had one or two shivering fits, and since then had each night become extremely cold, but without shivering. I had hitherto unfortunately only seen her in the forenoon, when hot and feverish, and none of the symptoms led me to suspect ague, which was not as yet prevalent. She revived a little by the use of stimulants, and I then left her, being called away to another patient, where I was detained so long that the city gates were shut, and I was prevented from visiting her again that night.

The patient I was called to was a man of some importance, who had been out on a hunting excursion the day before, and had slept by the side of a stream. He was awakened by excruciating pain along

the spine and in the chest, so severe that he could scarcely breathe; the pain was now most in the epigastrium, and so acute that he could not bear the slightest pressure; the agony appeared too acute for inflammation, and other evidences of pleuritis and peritonitis were wanting; the skin was warm, and the pulse small, contracted, and very rapid. Much at a loss, I gave a large dose of morphia, and exhausted every means of allaying the pain, with the intention of awaiting further symptoms, but the pain continued to increase until about midnight when the difficulty of breathing became so extreme that I determined to bleed him; blood flowed freely at first, but scarcely had four ounces been drawn when it suddenly stopped, coagulating, black, and tarry on the wound. He instantaneously became blue and mottled, and was seized with terrific tetanic convulsions, bent backwards nearly double in opisthotonos, and in a quarter of an hour he was dead of cholera.

It was not till next morning that I learned that my poor French patient had died at the same hour, with exactly similar symptoms.

The two cases I have detailed were the first of the kind which had occurred, and first pointed to what the epidemic I was dealing with was tending, and made me aware of the fatal mistake I had committed in both; for I had bled late in the hot stage of what might be considered an intermittent, and brought on collapse by the very means which, if used in the

antecedent period of commencing congestion, might possibly have insured safety.

I was not mistaken in believing that the epidemic character of disease which now spread rapidly and extensively over the whole district, was but a modification of Asiatic cholera, to which the fresh cases which occurred became daily more and more assimilated, until the various divergent forms for a time merged completely into cholera, as it has everywhere appeared. In all these different forms it was an invariable feature, that if periodicity was to be traced at all, it was always accurately quotidian, and this was the case in a very large proportion of the sick.

I shall not detain you with a detailed account of the various forms of disease allied to cholera which then prevailed, because I have already published a synopsis of them, in the 32nd No. of the *British and Foreign Medical Quarterly Review* for 1843, under the title,—"Epidemic Ague of Persia, a Species of Cholera." The character of disease was greatly varied, both in intensity and symptoms, appearing as simple tic-douloureux, and intermitting hemicrania; paralysis, general or partial, of every part, and in every degree; in congestion of the brain, simulating apoplexy or epilepsy; in shivering ague, or ague without shivering. By and bye this form was more frequently accompanied with vomiting, purging, and cramps, so as to constitute, in every respect, an intermitting cholera; whereas others had attacks resembling cholera in every particular,

except that instead of the more usual exudation from the bowels, this took place by the abundant out-pouring of the fluid of the blood from the skin, or sometimes into the cellular texture, either of whole or part of the body, producing either partial or general dropsy in the course of a few hours. This latter form was more especially frequent in infants and children. Sometimes, too, this serous exudation occured in a form not less rapidly fatal than the worst kind of cholera, by producing suffocation, in consequence of sudden œdema of the lungs. All these however, and various other anomalous affections, at length gave place to cholera in its ordinary form, with vomiting, purging, and spasms, as little marked by intermission as it ever is, and differing in no respect from that I had witnessed in Edinburgh and London. Its temporary disappearance was then followed by the return of remittent fever. In every one of these forms, however, the urine was invariably scanty, and of a dark porter colour, or wholly suppressed. In its second onset two years afterwards, cholera did not approach so gradually, but after having indicated its advent by a few cases of cerebral congestion and sudden death, it fell at once upon the population, with all the fatality with which its first arrival is everywhere characterized. Suffice it to say, that in all of these I found the same principle of treatment hold good, and even the same doses of a combination of iron with quinine, together with a careful use of bleeding in the commencement of the congestive

stage, universally applicable; whether in the slighter cases of mere nervous affection, in the more decidedly periodical forms, or in those graver cases including true cholera, where the congestion was evidently too extreme to be within the power of medicine to overcome, without the mechanical aid afforded by the use of the lancet, and where the stage of intermission was only to be discovered by calculation of the time, and the most minute investigation.

I shall conclude this division of the subject by the bold assertion, as it may seem to many, even of those who have witnessed cases of the description I allude to, that within the last few weeks, commencing from the middle of September, several cases of quotidian congestion of the brain or other organs, so grave as to threaten life, and sudden unaccountable deaths from affection of the heart have occurred in Manchester, and I believe all over the country; and that these are attributable in my opinion, solely to the already prevailing tendency to venous congestion, and nervous disturbance proceeding from the cause, and perhaps preceding the actual appearance of the more obvious forms of cholera. The cases which I have myself met with lately of this character, have been total insensibility, resembling apoplexy, with extreme collapse, return of warmth, and again a stage of collapse on the following day; also hemiplegia without other symptoms of apoplexy, both yielding to quinine and iron; intermitting hemicrania, neuralgia, unusual pain and tension

of the epigastrium, pains resembling rheumatism in every part of the body, aggravated by the usual treatment for rheumatism, but relieved by iron, and often accompanied with cold chill at a particular hour each day; also sudden attacks of vomiting and purging without tenesmus. These I shall probably have an opportunity of bringing to the notice of the profession in another place; my object here is not to detail cases, which others must have met with as well as myself, but by sketching the course of these diseases generally, to show how maladies, that, taken separately and without reference to the prevailing epidemic character appear to differ in every possible symptom, may yet be found when regarded *en masse*, to be very intimately connected.

I have been particularly desirous that your attention should be directed by the history I have just concluded, to the fact, that in every form of disease there mentioned periodicity has been a prominent characteristic, and that though obscure in remittent fever, in actual cholera and in some nervous affections, the close connection existing between them and decided quotidian ague in contemporaneous occurrence, sequence, and in their essential symptoms and means of cure, would scarcely admit of a doubt that periodicity exists in these also, even if it were more difficult to distinguish in practice than I have found it to be.

There is another point which can scarcely have escaped your observation in these forms of disease,—viz., the great variety of effects produced by the same

exciting cause. Now, on the brain and nerves, with various, or without any, remarkable disturbance of the circulation;—now, on the bowels by constipation;—now, by diarrhœa;—now, by dry vomiting;—on the capillary circulation generally in ague, remittent fever, and cholera;—or locally, by arrest of certain secretions, or by exudations from the compressed and congested blood into a great variety of structures, &c., &c., for this alone I conceive would suffice to show, that such diversity of effect without a single constant concomitant, such as fever, could never be the result of altered blood without nervous impression, and functional disorder.

The invariable efficacy of iron in all these forms of disease, whether simply or in combination with quinine, will also, I think, sufficiently illustrate what it was formerly attempted to argue, that the different conditions of the nervous system and of the circulation, occurring in agues of different type, require a different action in the medicines by which the morbid influence is to be counteracted, according to the type or duration of intermission; for, in speaking of ague, it was remarked, that in quartan the febrile stage is greatest,—that in tertian all three stages are well developed,—but that in quotidian the congestive stage is always the most severe, sometimes even to the exclusion of the others;—that, in proportion as the intermission is prolonged, the congestion is less severe, and the energy of arterial reaction more powerfully excited; but, as the cession of

the vital powers to the morbid impression becomes more complete, the period of repose is curtailed, the febrile stage less perfect, and capillary action more impaired, especially in that part of it which belongs to the venous system. Thus quinine, which almost invariably cures tertian, is often found inferior in effect to arsenic in quartan, and in quotidian we have found it prove positively injurious till modified by admixture with iron, by which its ill effects were obviated, and its antiperiodic action restored,—a fact of no small interest in explaining the failure that has always attended the attempts to treat cholera with quinine and arsenic uncombined.

Little now remains to be said with regard to the nature of cholera, if I have succeeded in convincing you that the disease does not exist essentially in the blood, but in disordered function of the nerves and blood-vessels, and if my arguments have carried any conviction of the truth of the following points—

1st. That an attack of cholera begins, and essentially consists, in disordered capillary circulation.

2nd. That congestion is the effect of this condition, and the cause of the other symptoms, and of death.

3rd. That the diarrhœa is a natural mode of relief to congestion, and thereby tends to the prolongation of life, and that being an exudation, it is not controllable by means applicable to excretion.

4th. That each period of twenty-four hours, in every attack of cholera (which extends beyond that period,) is marked by—1st. A period of capillary disturbance,

2nd. A period of congestion. 3rd. A period of comparative capillary relaxation and partial reaction; and, 4th. A period of renewed capillary disturbance and congestion, commencing at the expiration of twenty-four hours; not from the first purging, but from the first effect of the influence in capillary disturbance; and if the case proceed onwards without death or cure, that similar changes may be traced (often notwithstanding very active interference,) in the two subsequent periods of twenty-four hours each.

5th. Which I have rather asserted from observation, than attempted to assign any reason for, that the fourth day after the occurrence of algide symptoms is not characterized by the return of a distinct stage of congestion, but that the disease then goes on to consecutive fever.

It was my original intention to have selected cases from published works, by writers of authority, to illustrate these points, and to show by examples how the natural stages of the disease may be mistaken for the effects of treatment; to illustrate the evils of bleeding from an artery, or of mistimed venesection, in producing collapse; the injurious influence of stimulants and opium; the torments induced by hot applications; the effect of calomel in causing consecutive fever, &c.; but this course might appear invidious, and I am satisfied would be unnecessary, for it appears to me that a very small proportion of well-recorded cases of cholera will be found which do not bear on

one or more of these five points, and to this test I most willingly submit the opinions which I have attempted to inculcate with regard to its nature and treatment, always providing, however, that due allowance be made in applying the test for the effects of any treatment that is consistent with the views here advocated.

Let us now proceed to consider some of the more practical points of the treatment of cholera, especially in reference to HEAT AND STIMULANTS.

When the body is cold and exhibits so great a want of energy in the cuticular capillaries, the most obvious means of removing this condition which suggest themselves to every one are external heat and diffusible stimulants; but as these are not at all times attended with the beneficial effects that might have been anticipated, it is necessary to examine the reason of this failure by enquiring into the particular nature of the object to be gained. Finding that the disturbance of vital action commences in the capillary circulation, and not in the heart, it would appear that to begin by stimulating the heart is to begin at the wrong end of the chain of actions, and that to fulfil the indication some means is required that will act by giving power to the capillary circulation, independently of the heart's action.

Unfortunately all the medicines usually classed as stimulants excite the capillary circulation only by exciting the heart's action; secondarily and slightly,

by sympathy with the heart; but principally in consequence of the distension produced by the increased force with which blood is injected into these vessels; for to them, as to the heart, distension is the most direct means of exciting irritability and action; but if these vessels are not distended, capillary action is not directly excited by exciting the heart.

In the stage of congestion in cholera, we have endeavoured to shew that this increased propulsion of arterial blood does not take place, for the quantity of arterial blood in the system is diminished by the lungs opposing the free transmission of blood to the left side of the heart. We have shewn, that so soon as congestion commences, the heart becomes excited by its most powerful stimulus,—viz., distension of its cavities, at least on the right side, but that its action is opposed by the condition of the pulmonary vessels, and oppressed by the accumulation of venous blood from the extremities; it is evident, then, that to add stimulus to that which is already excited to the utmost in vain, can do no good, unless it can add power, but as it cannot do so in this condition, stimulus can only exhaust the irritability of the muscle, but not propel arterial blood in the quantity necessary to distend and excite the capillaries. If the power of the heart have not been yet excited to the utmost, a stimulant may prove useful by urging it to still greater exertions to overcome the obstruction, but if already excited in vain, it is quite evident that additional excitement can

only do harm, by producing exhaustion of muscular power. If, however, we have reason to believe, that by-and-by the obstruction to the heart's action by the capillaries of the lungs, and impulsion of blood upon it from the veins, will both gradually cease, at least in some degree, it will be evident, that if it only retain sufficient power, the heart will then be in a condition to restore the circulation to a more normal state, and therefore that our efforts should be directed to husband the power of the heart, instead of exhausting it; but the only means of doing so consistent with the maintenance of sufficient systemic circulation, is to endeavour by every possible means to promote capillary action, and so to diminish the labour of the heart by removing the obstruction to its efforts, not by subduing the excitement of the heart by opiates. The whole question, therefore, lies in the means of doing this. We have shewn that when opportunity offers,—that is while the powers of the heart are as yet unexhausted, and its irritability unimpaired,—this may be accomplished by freely opening the veins of both arms, thereby soliciting the blood from the extremities, and relieving the heart from part at least of its oppression *a tergo*, but both experience and reason have warned us against attempting this after the heart has been too long subjected to excitement. What then is our resource in this condition? This is exactly the point which the long history of disease that I have submitted to you is intended to illustrate,—viz., that we actually possess medicines

which have the power of stimulating and fortifying the capillary circulation without first exciting the heart's action. Many of the preceding remarks have been directed to show that the action of antiperiodic medicines is of this nature, only that as the morbid effects evinced in periodical disease of different types are not identical, neither can the antidotal effect required from medicine be identical. I have endeavoured to point out that, although quinine alone does no good in these diseases, the combination of quinine with iron has proved in my own experience to possess this power in quotidian ague and in cholera, in a very remarkable degree. I have found it check the vomiting and purging often in a few minutes, and gradually restore the circulation and warmth, and the secretions, without the assistance of any other applications, not, be it remarked, by exerting an astringent effect in moderating the diarrhœa, for it proved equally useful whether this symptom existed or not, but by removing the cause in the disorder of the capillary circulation.

But it is not enough to consider a question of so much importance as that of the use of heat and stimulants merely theoretically; I must, therefore, add my testimony to that of many authors against the use of diffusible stimuli where congestion is fully established. I have tried it carefully, and anxiously watched the result of each dose with my ear to the heart, and I can positively assert that each dose, in cases where congestion has reached to a dangerous degree, is attended

with increase of all the symptoms, dangers, and sufferings attending congestion.

The first time I was myself attacked with cholera, the symptoms were at once arrested by a glass of brandy taken on the first occurrence of the blueness, purging, and cramps, because congestion, being at once relieved by the diarrhœa, had not reached the point of oppressing the heart's action, and the stimulant aided the action of the heart to excite the capillaries; but on my second attack, the effect of this stimulant was very different, and excessively painful and suffocating, because not being relieved by diarrhœa, the congestion at once attacked the vital functions, and I was obliged to have recourse to quinine and iron, which soon relieved the suffocation.

The case in which I had the best opportunity of carefully studying this effect of stimulants was that of the late Chief Physician of Persia, a native of that country, who had been educated at St. George's Hospital, London, and an intense admirer of calomel. His attack was not one of pure cholera in its usual form, with purging and vomiting, for he was one of the earliest sufferers from the disease, while the digestive organs were as yet seldom affected, and when it presented many of the characteristics of ague. I attended him as an intimate friend, night and day, although no persuasions could induce him to try either the medicines I recommended, or bleeding. He felt so bilious that he was sure nothing would do but calomel. His attack

lasted for several days, in each of which, the cold stage that at first was followed by return of warmth, was more prolonged, till at length intermission ceased; the relief which was denied by the bowels was here effected by the skin, and the quantity of ice-cold watery exudation with which all the body but the breast was constantly bedewed, proved the rapidity with which the iced-water he was incessantly imbibing was both absorbed and exuded. He was much colder than I have ever found cholera patients in whom the characteristic diarrhœa existed. After resisting every proposal I made for treatment, he at length consented to try stimulants, for it appeared he was determined to die a martyr to medical prejudice, accordingly, I administered to him large doses of æther and alcohol. After each dose, on applying my ear to the heart, I heard a sudden increase of confusion in its action, a sort of indescribable churning sound, as if it would burst,—not as if it were getting rid of its contents, but rather constantly agitating the same portion of fluid,—and at each fresh effort of this sort, the icy exudation from the skin burst forth in redoubled quantity. I repeated this experiment until I had again and again proved the effects, and then, as in doing so, it was plain that more harm than good resulted, I left the case to nature, determined to watch the symptoms as they proceeded. It was not till the last day that the brain became materially affected, although, in the course of four or five days, I am certain not above half a pint of urine was secreted;

he then became insensible, the pulse became imperceptible, and at night, being seized with fearful tetanic convulsions, which lasted for two or three hours, he died. The first case of cholera, with purging, occurred on the day of his death in the next house, in a child, and was fatal in two or three hours.

This effect of stimulants in a congestive disease so very nearly allied to cholera, completely confirmed what I had formerly seen of the result of their exhibition in the more common form of cholera at home, insomuch that I have not since been tempted to repeat the experiment unless conjoined with bleeding, or in the very first moments of an attack, when there can be no doubt of their efficacy, or that of any other means of inducing an impression calculated to rouse the sympathetic through any single function, to a perception of its other natural stimuli.

A diffusible stimulant can, therefore, only do good while the heart possesses more power than has yet been called into action, or before it has yet become so completely oppressed as to be unable to respond effectually to the stimulus. The same reasoning might be applied to the question of using external heat as a stimulus, for though heat and pain are as powerful excitants to local action as brandy to the stomach, they unfortunately also act directly as stimulants of the heart's action. Difficult, however, as it may be to explain their effects, I feel bound, in conjunction with many others of great practical experience, to protest against the use of both,

when the case has reached the stage of confirmed congestion; for though in the preceding period, external heat may be as useful as a diffusible stimulus, it is not less injurious in the second. On this subject I cannot do better than quote from one of our most recent authorities:—Mr. Parkes says, "Warm baths, vapour baths, and warmth applied in any way to the surface, never appeared to me to be of the slightest service in true cholera; the spasms were sometimes relieved, but the algide symptoms were almost invariably increased. The depressing effects of the warm bath were sometimes marked and unmistakeable; I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool, but not cold surface, and in five or ten minutes, have seen the same man carried from the bath with a pulse almost imperceptible, and a cold and clammy skin."

These remarks perfectly coincide with my own experience of external heat. I have used the hot-air bath, so much recommended in this country, to such an extent as even to scorch the skin, without producing the slightest reaction, but apparently the worst effect, and have searched in vain for an excuse for the torture to which so many patients have been subjected by such attempts to restore warmth. These would not have been cruelty, whatever the sufferings, if really beneficial; but I am now convinced of the contrary, and that they are not only useless, but injurious. Without knowing more of the actions which take place in the systemic

capillaries in health, and their changed operation in this disease, it is impossible to account for these injurious effects, unless by excitement of the heart, or on the principle which prevents us applying heat to a frozen limb, to which the condition of the circulation is somewhat analogous; practically I have come to the conclusion that external heat is always injurious when congestion is extreme. In my opinion, *medio tutissimus ibis* in this respect, and that it is better to be content with a moderate temperature in the room, sufficient to promote any tendency to reaction, but not to force it.

The question, however, is one of no mean importance,—whether if we are not to yield to our natural impressions regarding the necessity of restoring warmth, we may not cede to the adjurations of the patient to indulge him with cold? This is a question which I am not bold enough to decide. There is I think much more evidence in favour of cold than of heat, yet both may be wrong, and according to my own experience both extremes are unnecessary. One friend writes to me from Tehran, that his practice of immersing the patient to the neck in cold water, till reaction took place, was very successful; another that he cured his patient with frictions with snow, in Erzeroom, where the cholera occurred in the winter season, and where the cold is intense. In Russia, and in Hamburgh, iced drinks have been much used. I am well acquinted with two Persian gentlemen, who were laid out for dead, in the cholera of 1829, but who recovered on

being washed in the open court-yard in a winter's night, as a preparation for interment, with water, to obtain which the ice had to be broken, and I was once called to the public burial ground of the city to see a dead man whose veins had begun to bleed while they were washing his body. The man, it appeared, had been attacked with one of those forms of disease already alluded to—a species of cholera without purging—had dropped down as if struck with appoplexy; an attempt had been made to bleed him in both arms without success, he was left for dead and carried off for interment, when what I have related occurred on his being deluged with cold water; unfortunately no one had presence of mind to bind up his arms and protect him from the cold, and he was really dead when I arrived. These are strong arguments for the use of cold, but may perhaps be met by equally convincing arguments against it. I look upon these as instances of powerful nervous impression made at a suitable stage of the disease, and that if the remarks formerly made of the *post-mortem* return of heat in the corpse be attended to, no such accidents can arise as those fearful cases sometimes mentioned of persons being buried alive. If one, apparently dead of cholera, become warm from the extremities upwards, no doubt of death can exist, and no chance of recovery; if this does not take place, or, on the contrary, if warmth proceed from the trunk to the extremities, there is a possibility that he may be alive.

I consider the question of the excessive external application of heat as fully determined in the negative as in the congestive stage, but open to a certain degree in that which succeeds it, and that the question of cold applications is still *sub judice*, and subject to objection in the stage succeeding congestion, which, however, is so difficult to fix precisely, that both extremes are better avoided, especially as I am convinced that treatment conducted on other principles will almost always prove successful without either.

Now, with respect to the DIARRHŒA,—enough has formerly been said on this subject to show that it has been considered rather as a means of relief to congestion, than in itself a source of danger, as tending to prolong life, rather than as the cause of death in cholera, insomuch, that even if we had it in our power to check it by a word, it is a word which we should hesitate to pronounce, unless we were assured that the cause rather than the effect would be influenced. If, then, it is to be considered in itself as rather a good than an evil, it must, nevertheless, be always looked upon as a sign of imminent peril to life, which, as its direct restraint would endanger immediate collapse, should the more impress us with the necessity of removing its cause without delay; for when the disease is in the neighbourhood, and sudden evacuation of this kind occurs, even though not another symptom of cholera be present, the case is to be considered one of cholera. It is not a secretion from the bowels, for

that is a vital action of the capillaries; this is the very reverse—an exudation, showing that the capillaries of the bowels have succumbed to the same cause of impaired action which we have throughout considered essential to the disease. The capillaries of the skin may yet perform their functions, the disorder may be almost local in the bowels, but it is essential, and were it possible by ligature, or any mechanical means, to arrest the exudation, without arresting its cause, it is scarcely possible to doubt that the effect would be congestion and oppression to the heart; that this would be attended with diminished circulation of arterial blood, and impaired powers in the whole system, which would then succumb to the anti-vital influence, and terminate in collapse. To what purpose then would we use mere astringents to check the diarrhœa? Have we not rather reason to congratulate ourselves that such means are for the most part wholly inoperative? In the perusal of cases it is impossible not to be struck with the complacency with which the authors seem to regard the effect of pills of lead and opium, chalk mixtures, catechu, &c., when administered at the very period of the disease, in which we should naturally expect to find congestion on the decline, (if the disease be really one of quotidian periodicity,) an effect which, if real, instead of apparent, would only be to be deplored. If, however, we check the diarrhœa, by removing the cause, restore their wonted action to the capillaries, and thereby prevent the effect of their

disturbance, we cure, rather than restrain, the diarrhœa. Here, however, a difficulty meets us; the congested condition of the vessels, and their reverse current of blood, afford very obvious reasons for expecting but little effect from any medicine that can act only by absorption into the blood from the alimentary canal; nevertheless, experience proves that absorption does take place to a degree much greater than might have been anticipated. However great the amount of exudation of the fluid of the blood into the intestines, this occurs only at intervals, and paradoxical as it may appear, while the congestion continues unrelieved, there seems to be a great tendency to restore fluid to the blood, during these intervals. Sufficient evidence of the absorption of salts in solution, in the fluids of which the patient drinks with such avidity, is to be found in the fact, that tartar emetic produces its effects when given in this manner, and those I have so often witnessed from quinine and iron, given in weak solution, have been so immediate, as to excite my astonishment. Thirty drops of muriated tincture of iron, with a grain of quinine, in a wine-glassful of water, will often stop vomiting and purging that has resisted large doses of laudanum, and of solution of morphia. That so small a dose of this combination should ever prove effectual is singular enough, but I do not think it very wonderful that opium should rather increase than diminish these symptoms, if they are really mechanical effects of congestion, and the

results of loss of irritability in the capillary vessels, (not of irritation in the viscera,) for opium will evidently tend to subdue whatever irritability may be left in the nervous system, and thereby give more scope to the anti-vital influence, instead of opposing it. Theoretically, such should be the effects of opium, and practically, I am very sure, that it more frequently tends to keep up the vomiting and purging, and the algide symptoms, than to cure them. If the symptoms proceed from partial paralysis, or loss of irritability of the sympathetic, nothing could be better calculated to augment them than opium, for in at least half the cases in which opium is prescribed in ordinary practice, its sole object is to subdue morbid irritability of the sympathetic system. If, however, our reasoning upon the etiology of cholera has had any foundation at all, it goes to prove that the great object of cure is to maintain the vital sensibilities active, and to husband the power of the heart, and neither to exhaust irritability by undue excitement, nor to lull it into insensibility by opiates, for our hopes all depend on the integrity of these qualities, when the period of capillary relaxation arrives.

With respect to most medicines given in pill, I consider them nearly inert in the congestive stage of cholera, more especially those which are not soluble in water, but require the action of the secreted juices of the stomach for their solution, for we know that such secretion does not then take place. Calomel is fortunately one of this class, and in cholera passes

through the bowels almost unchanged, and I am told on good authority may often be found at the bottom of the bed-pan in the form of a grey powder. With respect to the influence of calomel in this disease, I am fully satisfied that the most mistaken opinions prevail. We have already alluded to the effect of tartar emetic, which is said to be eminently curative, and on the principles of nervous impression, we can well conceive that it will be far from inactive, if it produce its characteristic effect, because that effect is a vital one of great influence on the whole of the sympathetic system, and were we destitute of safer and more appropriate medicines, it might be well worthy of trial. In like manner croton oil has been recommended: it may act like the diarrhœa of cholera, or venesection, in reducing the volume of the blood; but if it produce secretion, which is a vital action of the bowels in opposition to exudation, may operate as a stimulant to the vital energies of the sympathetic, and prove curative. (The danger of inducing syncope by these means is, however, too evident to require comment.) So also may any other medicine or means whatever cure, which produces a powerful impression on the nervous system, provided it do not act by injuriously stimulating the heart's action,—by impairing the force of capillary circulation, or the excitability of the nervous system,—or, in other words, if it be not calculated to produce the very effects which are caused by the exciting cause of the disease. Upon this principle CALOMEL, if it act as a purge, may

prove beneficial, but if it affect the constitution, it can only be detrimental This, I think, will become evident, if we consider the action of mercury but for a moment; its chief value is in subduing inflammation, but all our observations tend to shew that the condition, both of the blood and of the capillaries in cholera, is diametrically opposed to that of inflammation. The medicines most useful in cholera and allied diseases,—viz., iron and quinine, are those most opposed to the effects of mercury, and most curative of them. All experience goes to prove that mercury, if an irritant, is at the same time a cause of positive weakness of capillary circulation, with the single exception, and that a doubtful one, of its effect upon the liver. There is no point in medicine on which I feel more entirely convinced, than that the constitutional effects of mercury are invariably injurious in cholera, and tend to produce consecutive fever. Soon after cholera appeared to have ceased, in 1843, I submitted two patients to a course of mercury for chancre at the same time,—one, a groom, died collapsed as soon as the gums became affected, though a very healthy powerful man; the other, the Persian High Treasurer, fell into the same condition, and was only saved by large and quickly-repeated doses of quinine and iron, and went through a severe attack of consecutive fever.

Mr. Parkes mentions, that two syphilitic patients, while under the influence of mercury, were attacked by cholera in his hospital, as proof that mercury is not

prophylactic, I might mention several instances to prove it really a predisponent; but so many points of difference will suggest themselves in the condition of one labouring under cholera, from that in which we should be anxious to produce ptyalism, that it is unnecessary to pursue the subject farther.

There now remains I think but one material point in the treatment of cholera, viz., that connected with the use of FRICTIONS and RUBEFACIENTS. Of these, it may be sufficient to say that their efficacy will depend upon the period at which they are applied, which is always difficult to decide, and on the degree of fatigue thereby occasioned to the patient, they partake much of the nature of heat and stimulants; but if applied without disturbing the patient, may perhaps be beneficial, although I have not myself very much confidence in them. Disturbance of the patient is a question of much more importance. In a report of the Commissioners of Public Health, much evidence is adduced by the examination of several medical men practising in London during the former visitation of cholera, as to the ill effects of removing cholera patients to temporary hospitals. There is very obvious reason for desiring to remove them from the locality in which they have actually acquired the disease, because, undoubtedly the cause exists there, and will continue to operate powerfully against the effects of remedial measures, and will also endanger the health of those who are in attendance, and if any of them happen to be attacked, it is sure to

give apparent countenance to the popular belief in contagion; but if the disease have been acquired elsewhere than in the dwelling, no object is to be gained by removal unless to a very decidedly more healthful locality, while on the other hand, the evidence is conclusive that danger often attends the practice. In considering the stages of cholera, it was remarked that there is a time when the patient appears rather indifferent than stupid—not asleep—for sleep never occurs in cholera, but as if resigned to fate rather than willing to make the slightest exertion. This condition marks that stage in which the tendency to *cessation of morbid inaction* exists rather than reaction of the powers which circulate the blood, that, namely, in which the influence that causes the disorder is so equally balanced against the resistance of the vital powers, that the slightest disturbance or source of exhaustion will turn the scale, and it is the merest chance whether a stimulant will now rouse the vital powers to throw off the oppression, or by disturbing the circulation then in progress to partial recovery, renew the congestion. It is at this period that the greatest danger of syncope and collapse is incurred by an attempt at removal, and even frictions or anything that tends to disturb the patient may induce syncope and collapse; but sinapisms and blisters that can be applied without disturbance, may prove beneficial.

It is almost needless now to state the practice that I should myself pursue in this disease. If asked what

I should do in a case of cholera, I should answer that, that depends very much on the stage in which I found my patient, but in all cases and in all stages I should certainly give a largely-diluted solution of some salt of iron and quinine, with a view to counteract the morbid inactivity of capillary circulation, and repeat it as often as the patient would drink of it till warmth was restored. If called to one of those cases of sudden collapse, with little vomiting and purging, reported to have frequently caused death in eight hours in Scotland, I should be satisfied that mechanical assistance was required to relieve actual congestion, and if I arrived sufficiently early would open the veins of both arms, trying to induce sneezing and coughing, or almost any powerful vital action, likely to give an impulse to the blood, and cause it to flow. If, however, I arrived too late to bleed, I should not hesitate to conjoin Epsom salts with the mixture, however heterodox such practice may appear, because such cases are generally without purging, and purging is a safer and more direct source of relief than bleeding late in such a case; and I have followed this practice largely with success. I should neither give stimulants nor apply heat, should limit external applications to a blister or tincture of capsicum to the spine, be careful of disturbing my patient unnecessarily, and endeavour thus to carry him over the first twenty hours with life; if his condition was then such as to give little hope of his being able to sustain a fresh attack of congestion, I

should risk everything to obtain blood to such an amount that the colour should become florid and its motion free, and should continue the medicine till the secretions became fully re-established—till the hue of health returned to the complexion and the lips and tongue had lost their pallor.

In all cases of consecutive fever my experience points to the use of the same remedies conjoined with salts and diminished quantity of quinine, which in excess, or even if not in very small quantity, tends to produce dryness of the tongue, but which I think necessary, nevertheless, for a time. Perseverance in this treatment long after the secretions are restored, no doubt sometimes tends to produce a red, furred, and bilious tongue, very different from the clean, glazed, and dry red surface, or pale-edged darkly-coated appearance that preceded it, but then the fever has become bilious, and may safely be treated with a smart dose of calomel, or what I find still better, sweating in the wet sheet of the hydropaths. The same treatment is appropriate to the prevailing form of remittent, commonly called low fever unpreceded by cholera or noteable collapse, and unaccompanied with maculæ or typhoid erruption, but characterized by the appearance of the tongue. If the spleen be tender on pressure, a short trial of sulphate of iron with sulphate of magnesia, withont, or with not more than half a grain of quinine to each dose, with occasional application of leeches to the epigastrium, (in the forenoon,) will, I think, show

that there is something in the treatment recommended, if nothing in the theory. *Post-mortem* examination in this type of fever constantly exhibits more or less softening of the spleen, a characteristic of tropical remittents, and in my opinion a positive indication for the exhibition of iron.

I must now conclude, but I confess I leave the subject with much reluctance, for there are many points of much practical importance left untouched; among others, the influence of the epidemic constitution (while cholera prevails) upon other diseases apparently unconnected with it, is full of interest.

Of these I cannot refrain from mentioning one example before concluding. While cholera prevailed I had two patients with pannus oculi, in which I had tried every possible local application in vain; one of them, however, had on one occasion a slight shivering fit when he visited me, and I gave him a mixture of quinine and iron, the pannus immediately began to diminish, and I tried the same medicine on the other, omitting local applications, and in both a cure was effected in the course of ten days. Similar effects were observed in a great variety of other diseases. I am very well aware, that in this laudation of a particular combination of drugs, I have subjected myself to the charge of empiricism, and in a certain degree I must acknowledge the charge to be just, for nothing could be more purely empirical than the grounds upon which

I was first induced to make trial of it, whatever my subsequent endeavours to explain its operation; but my object now has been only to recommend the combination of iron with quinine to your consideration, as a means well fitted to attain the end in view,—viz., to promote the oxygenation of the blood, to fortify capillary action and nervous irritability, to confirm the tendency to remission (whether apparent or too obscure to be noteable but presumable,) and to prevent periodical return of venous congestion. I have endeavoured to shew that these are the primary objects requiring attention; relief of congestion by mechanical means the secondary one.

If I have over-rated the effect of the medicines recommended, I must be allowed to say that I have not been induced to do so by theory alone, for that which I have offered to you is solely the result of reflection after I had used it in many thousand cases effectually, but purely empirically. While the epidemic prevailed in Tehran, my court-yard was daily crowded with hundreds of poor wretches, praying for the love of God for a supply of the bitter water, or water of life as they called it. It was served out to them in pint-and-half mixtures of water, containing twelve grains of quinine, nine of sulphate of iron, and thirty drops of dilute sulphuric acid, with directions to take a coffee-cupful for a dose; these orders, however, were not always attended to, and in some of the most remarkably rapid cures I witnessed, the whole was swallowed at a draught. One of these I particularly remember.

In visiting one of the princesses, the wife of the Chief Astrologer, I observed a wretched object, who had dragged himself to the tank in the outer court of the house; with difficulty I recognized him as the tutor of the family. He had been vomiting and purging, and agonized by cramps, and his thirst had impelled him to the position in which I found him. I immediately sent him a quart bottle of the mixture described, and I was never so much surprised as to see him in the afternoon at my house with his bottle, whither he had walked nearly a mile, to beg a fresh supply, having swallowed the whole at once, and felt immediately revived. I had previously known him as, both from religious prejudice and as an epicurean, an abhorrer of medicine; and this certainly was not nectar. Villanous though the compound may seem, it appears to be peculiarly grateful to the patient, by appeasing the thirst and sense of heat in the stomach, which even ice fails to allay, and I have seldom seen it vomited after the first dose or two. I have met with one or two instances in which it was found impossible to obtain blood from the veins, but where, shortly after a few doses of the medicine had been taken, bleeding was practised with success.

In the cases of simple diarrhœa without tenesmus now prevailing, I find the most immediate effect produced by a grain or two of quinine given with half a drachm of the muriated tincture of iron, in a wine glassful of water. With respect to the cramps and spasms, I have been in the habit of looking upon them as mere

symptoms, only to be obviated by removing the cause; it would, however, appear from the observation of others, that, apart from the more fatal symptoms of suppressed arterial circulation evinced in convulsions; the cramps in the legs, &c., are closely connected with the contracted condition of the colon, and I should therefore feel inclined to try the effect of assafœtida injections.

I have now, gentlemen, only to thank you for the attention with which you have listened to my remarks. The treatment which I chanced to adopt on purely empirical grounds appeared to me more than usually successful, and I was led to enquire whether there was anything in the nature of the disease, or the remedies, that was explicable on known and acknowledged principles of physiology and therapeutics; the result was, that there existed a deficiency of facts by which to determine the question, and, till these were settled, it was impossible that all should agree on the treatment or deductions, at the same time it was evident that there were facts which, if generally admitted, would necessitate universal concurrence in a broad principle of treatment, to the exclusion of a vast amount of erratic practice, that might be termed a blind groping for facts on which to reason.

In my endeavour to ascertain what were the established facts, it appeared that beyond a rehearsal of symptoms there was not one universally admitted,

save the constant existence of venous congestion,—contagion, the effect on the blood, the cause of death, the influence of particular treatment, were all subjects of hot contention, and the medical profession was divided into as many parties, and more, than there were sections in the materia medica. Under such circumstances it appeared that the weight of any single evidence could not possibly turn the scale of opinion, and that the only chance of arriving at a useful conclusion was to examine the disease in all its forms, as they relate to its cause, its course, its terminations, and its connection with other diseases. The result of this I have endeavoured to lay before you, in the reduction of the essential facts to six, not one of which is generally admitted, but if they be proved to be facts, the deductions from them are too evident to require comment. These which have previously been stated at length are shortly as follow:—

1st. That cholera is the effect of impression on the sympathetic system,—that its cause does not primarily affect the blood, is incapable of reproduction in the body, and, therefore, incommunicable.

2nd. That its effects begin by impaired vitality and disordered function of the capillary circulation.

3rd. That congestion of the thoracic and abdominal veins is the consequence of the above condition, and the cause of other symptoms.

4th. That exudation of the watery particles of the blood, whether through the mucous, or into the serous

tissues, constitutes the natural mode of relief to this congestion.

5th. That cholera goes through certain diurnal stages, of which capillary disturbance is the first, venous congestion the second, more or less intermission or cessation of capillary disturbance the third.

6th. That the natural tendency of cholera is to go through these successive changes for three days, and then cease, or to run into consecutive or remittent congestive fever.

It appears to me that if these are facts (the establishment of which will depend on the observation and reading of every practical man,) a principle of treatment is inevitably established along with them, because they are facts on which the whole pathology of the disease may rest, and the whole course of treatment depend. If they prove otherwise, I have wasted your time and my own, in a feeble attempt to recommend a combination of quinine with iron, as only one of the numerous medicines said to be beneficial in cholera, but the action of which will then be totally inexplicable; and I shall owe you all much apology for inviting you to listen to the arguments by which I have attempted to substantiate as facts, what another process of reasoning may, perhaps, demonstrate as fallacies.

THE END.