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LECTURE ON THE NATURE & TREATMENT OF CHOLERA,

CONSIDERED WITH REFERENCE TO ITS
ANALOGY WITH CONGESTIVE AGUES OF
QUOTIDIAN TYPE.

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LECTURE II.*

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 diarrhoea 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.

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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, inasmuch 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 algid 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 Erzerum, 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 acquainted 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 apoplexy; 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 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, inasmuch, 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 algid 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 diarrhoea 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 pyalism, 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 notable collapse, and unaccompanied with maculæ or typhoid eruption, 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, without, 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 notable 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. Villainous 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 diarrhoea 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 wineglassful 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 assafoetida 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.

[At the conclusion of the lecture the thanks of the meeting were voted to Dr. Bell, on the motion of Dr. Bardsley, senior, seconded by Dr. Lyon.—ED.]

BRONCHITIS AND DEATH

IN CONSEQUENCE OF TWO TEETH BEING DRIVEN DOWN THE LARYNX.

By S. B. DENTON, M.D., Hornsea.

I was called to a Miss Fawcitt, from Sheffield, September 14th, 1848, who had been ill three days. The history she gave me was this:—Feeling tolerably well she went to the sea-side; while there she had an accidental fall; and found on getting-up that two front teeth had disappeared, (they were false, tied together in a gold socket,) they were not to be found about her person, or amongst the shingle. A cough almost immediately came on, which had continued to harass her much up to the time I saw her.

I found the poor sufferer labouring under an acute attack of bronchitis, and although her attenuated frame could ill spare one drop of blood, I durst not omit the application of a few leeches, with two or three emetic draughts of Antimonii Potassæ Tartras.

It was rather late at night when I saw her, and contrary to my request, she left at eight o'clock the next morning for her home, at Sheffield, I did not, therefore, see her again, but was told that she was much relieved.