Notes

ON

THE PATHOLOGY AND TREATMENT OF CHOLERA.

BY

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THE GENERAL PRINCIPLES OF TREATMENT.

THE account which I have now given of the pathology of cholera, although brief, will, I trust, have been sufficient to convey to those who have carefully read these papers a definite idea of the doctrine which I advocate. If there are any who desire to have further details and proofs and illustrations, these may be found in a book which was published ten years ago, but which has hitherto attracted singularly little notice.

I proceed now to the inquiry, What has this pathology to do with treatment? And I maintain that it is a compass which will guide the intelligent practitioner over a "sea of troubles" and perplexities in the treatment of cholera. We now have a definite view of the nature of cholera, of the relation which the symptoms bear to each other, and of the manner in which the disease tends to a spontaneous recovery. We can see that, amongst a vast number of plans of treatment, some have been unquestionably beneficial by assisting the curative efforts of Nature, while others have been as unquestionably detrimental by opposing those efforts. Others, again, have been of a mixed character; as, for instance, the combination of calomel—a purgative—with opium. And, lastly, some methods have been simply neutral in their effects, except in so far as the knowledge that something was being done for him may have inspired the patient with hope and confidence. As an illustration of this, I may mention one of the many infallible cures which have found their way into the *Times*. I mean the inoculation with quassia. If that which has been satirically said contained the whole truth—namely, that "the chief art of the physician is to amuse the patient while Nature performs the cure"—the inoculation with quassia would be as effectual as any other method of treatment, and it would have the great negative merit of doing no harm.

There is no remedy which has the slightest pretension to be considered a cure for cholera; no drug or agent which, so far as we know, will neutralise the poison or lessen its virulence. I have not the faintest hope or expectation that a specific remedy for such a disease as cholera will ever be discovered. The number of really specific remedies which we possess for any diseases is unfortunately very small. Those who believe in specifics are, in general, ignorant of the nature of disease and of the true methods of cure; and this ignorance renders them quite incompetent to estimate the influence of treatment.

If I have correctly interpreted the phenomena of cholera, the natural method of cure is eliminative. The process of elimination takes place by two successive stages. 1. There is excretion from the blood into the stomach and bowels; and 2. The morbid contents of these viscera are ejected by vomiting and

purging.* Can we do anything to assist this natural curative process? Unquestionably we can; by emetics and purgatives. But is it necessary or beneficial to interfere in any way? Ought we not rather to leave the cure entirely to Nature? I have no doubt that much may be done with advantage. And my conviction of this is based partly upon theoretical grounds, and partly upon the actual results of treatment. If we carefully observe the condition of a patient in collapse, we shall often find that the intestines are more or less distended with fluid; and this, too, while perhaps there is general torpor and very little effort at expulsion. Again, it is often found that, although there has been copious watery purging during life, the small intestines contain after death a large amount of a peculiar viscid dirty white material, having a very offensive odour. A purgative may be useful in removing both these conditions; namely, over-distension of the bowel by liquid, and accumulation of offensive viscid semi-solid secretions. The object of a purgative in cholera is not to increase excretion from the blood into the stomach and bowels; but simply to assist in the expulsion of the morbid secretions from the digestive canal. To fulfil this indication, that purgative is best adapted which acts most speedily, yet with the least amount of irritation. I believe that castor-oil best accomplishes this object; but a great variety of purgatives have been given with more or less success.

When, in September 1854, I first publicly stated that I was giving castor-oil as a purgative in cholera, the announcement was received with a howl of execration. Give a purgative to a patient whose blood has already been drained of all its liquid and thus rendered too thick to circulate! Unheard of atrocity! Statistics must immediately be published to show that this plan is most deadly in its effects.† But at the same time statistics were showing that calomel in frequent and sometimes in large doses is a very successful remedy. How does the calomel act? Is it not an irritant and a violent purgative? Or what is there to deprive it of its purgative properties when given for the cure of cholera?

Calomel a purgative! exclaims indignant orthodoxy. No! calomel is an alterative. It acts upon the liver, and restores the secretion of bile. Those who give this explanation of the action of mercury in cholera, are of course quite unaware what is the real cause of the suspended secretion of bile during collapse, and are therefore equally ignorant of the conditions essential for the restoration of that secretion. To call calomel an alterative and a restorer of the biliary secretion does not, like its combination with opium, deprive it of its purgative properties. I therefore claim the reported success of the calomel treatment as evidence in favour of elimination.

Saline Treatment. Another plan of treatment which has unquestionably been attended with a large

* If there are any who doubt the elimination of a morbid poison by the stomach and bowels, no one can doubt the existence of offensive morbid secretions which require to be cast out.

† It was evidently the opinion of some who chanced to be in prominent positions at that time, that my alarming outbreak of heterodoxy must be treated, like cholera, by very active repressive measures. So fierce an onslaught was made upon me, that I began to fear lest what Sydenham said of small-pox two hundred years ago, would be found equally applicable to cholera in the present century. "I venture to assert," said Sydenham, "that the physician who has much to do with small-pox runs many risks with his reputation. The vulgar are ever in the habit of ascribing deaths to the officiousness of the attendant; whilst physicians themselves catch greedily at opportunities for slander. They make out their case before incompetent judges, and procure most uncharitable verdicts. They act thus in order that they may build up a name for themselves, upon the ruined reputation of others; a proceeding disgraceful to even honest artisans—doubly disgraceful to scholars." (*The Works of Sydenham*, translated by Dr. R. G. Latham for the Sydenham Society, vol. i, p. 137.)

amount of success is that which is known as the saline treatment of Dr. Stevens. The theory upon which this treatment is based—namely, that the essential cause of cholera is a deficiency of the saline constituents of the blood—has been shown by accurate chemical analysis to be fanciful and erroneous; but this happens to be one of those rare cases in which a theory may be wrong and the practice based on it not far from right. I have condensed from Dr. Stevens's work (*Observations on the Nature and the Treatment of Asiatic Cholera*) the following account of his theory and his practice.

"The first efforts towards a cure should be directed to assist nature in throwing off the poison from the blood, *per vias naturales*, and, at the same time, supplying the necessary saline stimuli, on which the action of the heart and its vessels depends. In accordance with this view, patients presenting merely the first stage of premonitory symptoms, diarrhoea and vomiting, took, on their admission, a Seidlitz powder; and, if sinking was felt but without bowel complaint, more active purgatives were then employed; or three or four teaspoonfuls of Epsom salts were added to the Seidlitz powder. On the bowels being moved, plenty of thin beef-tea, well seasoned with salt, was given. This simple treatment was so successful, that a great many patients had no further complaint, and were generally dismissed cured in a few days."

"If cramps, coldness, or sinking of the pulse, were present, the patients were considered as cholera-patients in the second stage. The 'non-purgative' salts were administered every half-hour, or more or less frequently, according to the symptoms, and in the following dose: Muriate of soda $\mathfrak{ij}$; carbonate of soda 3ss; chlorate of potash gr. vij. When life seemed rapidly ebbing, the collapse stage having been reached, a strong solution of the same salts, at a temperature of 100°, was thrown into the bowel. The saline mixture was administered half-hourly; and, in severe cases, the muriate of soda was increased to a drachm, or even more, as circumstances seemed to require. In some cases, an enema, composed of a large tablespoonful of common salt in water, at as high a temperature as the patient could bear, was administered every two or three hours."

This plan of treatment has been attended with a large amount of success in the hands of many different practitioners; but, for a very obvious reason, it has not been generally adopted. Its mode of action is essentially eliminative; and, therefore, it is incompatible with that theory of collapse through loss of water which has taken such firm hold of the European medical mind.

This theory of collapse, which now, we trust, has received its death-blow, started into life when cholera first visited Europe in 1832. Before that time, our countrymen in India could not only give purgatives as a part of the regular treatment of cholera, but they might also call them purgatives without subjecting themselves to the imputation of culpable rashness and folly. Abundant illustrations of the use of purgatives in the treatment of cholera may be found in the writings of the following Indian practitioners: Bell, Orton, Scot, Curtis, Twining, Christie, Rogers, Kennedy, Searle, and Corbyn. None of these writers had any very definite pathological theory. They looked upon cholera as a disease chiefly of the nervous system. They called it spasmodic cholera; and they often gave opium to relieve cramps at the same time that they gave very active purgatives to remove the morbid secretions from the bowels. The fact that, in many instances, their treatment was a mixed treatment—a combination of opiates with purgatives—renders their writings the more instructive, since it clearly appears, on a careful study of their

cases, that, in proportion as either the opiate or the purgative plan prevailed, was the mortality increased or diminished.

Several of these authors speak highly of the use of castor-oil, on account of its unirritating properties, and the completeness with which it brings away the viscid and putrid secretions from the bowels.

The very interesting paper by Mr. Watkins (*BRITISH MEDICAL JOURNAL*, Oct. 28th, p. 445), to which I have before referred, contains a most instructive illustration of the effect of various modes of treatment. It will be seen that four distinct sets of cases are there described. In one set opium was given alone; these were all fatal. In a second set, the opium was combined with calomel or with gray powder; of these more than half died. The purgative action of the mercury somewhat lessened the mortal effects of the opium. In a third set of five, calomel alone was given; and of these one died, while four recovered. In a fourth set of twenty-one, castor-oil was given; and nineteen of these patients recovered. Besides these, one case which had been treated by gray powder and opium passed into collapse, and then, castor-oil being substituted for the other remedies, she ultimately recovered. Lastly, one most interesting case in full collapse was treated by castor-oil until he rallied; then, through some mistake, two pills, probably of calomel and opium, were given, he fell again into collapse; he was a second time treated by castor-oil, and ultimately recovered.

It is sometimes argued, that no remedies can possibly be of use during collapse because absorption is suspended. Now, in the first place, Magendie has shown that absorption is not absolutely suspended during collapse, though it is rendered much less active. He injected camphor into the rectum, and detected its odour in the breath in five minutes, whereas in health, he says, it may be detected in one minute. He performed the same experiment with ether, and with a similar result. He reasonably infers that absorption is slowly carried on during collapse. (*Leçons sur le Cholera Morbus*, p. 97.) There is undoubted evidence that opium and alcohol become absorbed, and produce narcotic effects during collapse. But the remedies which are of real use during collapse have no need to be absorbed. Surely a mustard or salt and water emetic need not be absorbed in order to excite the stomach to expel its contents. And it can scarcely be doubted that calomel and castor-oil, by their stimulant action on the inner surface of the bowel, may excite muscular contraction, and so act as purgatives without being taken up by the blood-vessels.

A writer on cholera during the last epidemic stated that "brandy, ether, and ammonia, turpentine, and even champagne, were given to rouse the flagging energies of life, but to little purpose; they were generally thrown from the stomach as soon as swallowed, and draughts containing creasote, chloroform, and camphor, shared the same fate. The stomach had, as it were, lost the power of ministering to the wants of the system."

Now, it may be asked, whether the stomach was not much wiser and more useful than the doctor, and whether it could better "minister to the wants of the system," than by the expulsion of its morbid secretions, together with such a horrible mixture of drugs as could not be retained and absorbed without adding to the perilous oppression of the vital powers.

During the last epidemic, I observed very carefully the effect of brandy in the few cases in which I ventured, in a tentative manner, to give it to patients who were in collapse. I found that it speedily increased the restlessness and feeling of oppression; at the same time the pulse lost power and volume, and

the temperature fell. I infer, therefore, that brandy and all alcoholic stimulants during the collapse stage of cholera add to the obstruction of the pulmonary circulation, instead of passing freely through the lungs and then stimulating the left ventricle, as they do in other states of system. I resolved, therefore, never again to give alcoholic stimulants in collapse, no matter how great the *apparent* exhaustion may be.

In a few cases, the *sesquicarbonate of ammonia* was given with apparent benefit; and this remedy appears to deserve a further trial as a stimulant during the stage of collapse.

It is worse than useless to attempt to *feed* a patient during collapse. The secretions of the stomach are utterly deranged, and the power of digestion is suspended. The mildest nourishment administered at this time only adds to the feeling of oppression and general distress, from which the act of vomiting often gives immediate relief. Upon this point, I have satisfied myself by repeated and very careful observation.

The wife of the English chaplain in Paris has recently obtained notoriety by administering *chlorodyne* in fifty or sixty cases of incipient cholera, and, as it is stated, "has succeeded in arresting the disease in every instance." What would be the effect of *chlorodyne* in cases of choleraic diarrhoea? It would relieve the cramps, and, by its narcotic operation, it would in some degree retard the spontaneous recovery of the patient. Nevertheless, if a lady have a determination to step beyond her own proper sphere, and to play at doctoring, *chlorodyne* would be a much less dangerous drug in her hands than opium and strong astringents.

Dr. Bullar and Dr. Risdon Bennett have recently spoken in high terms of the good effects of *external warmth* to the skin of patients in collapse. Dr. Bullar recommends the hot mustard bath; and Dr. Bennett packing in hot wet blankets. There can be no question that to thoroughly warm a patient in collapse is often a real benefit; the pulse, the temperature and the colour of the skin, and the expression of the features, all improve simultaneously. And we can readily understand this effect of warmth if we have thoroughly comprehended the *modus operandi* of the hot saline injection into the veins. But too much must not be expected from the application of external warmth. The warm bath has often been found to have a very depressing effect. Annesley declares that, in his opinion, it did more harm than good. "The fatigue arising from going in and coming out of it, and from rubbing and dressing the patient, exhausted him." (*Diseases of India*, p. 156.) Christie found the bath injurious; and Dr. Parkes says: "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 clammy skin. I cannot find in my notes a single case in which the warm bath appeared beneficial." (*Parkes On Cholera*, p. 210.)

During the epidemic of 1849, I saw several patients in King's College Hospital placed in the hot-air bath; and the result of my observation was that, while the warmth appeared to be beneficial, by relieving the cramps, and sometimes even improving the pulse, yet, on the whole, the patients appeared to be rather distressed than comforted by the bath. The employment of hot baths in the treatment of cholera obviously requires caution. Faintness is a not uncommon result of remaining too long in a hot bath, even when the bather is in health; and, in the collapse of cholera, a sudden and deadly exhaustion may be thus induced. Then be it remembered, that cho-

leraic collapse is a form of asphyxia. There is an instinctive craving for air. Again and again will the patient toss all covering from his body; and it is likely that surrounding the body with hot water, or hot air, or wet blankets, may cause distress by interfering with cutaneous respiration—*i.e.*, the aëration of the blood through the skin. A very convenient way of keeping up the warmth of the body is by the application of hot bottles, hot dry flannels, or hot sand-bags, to various parts of the surface.

Dr. Murray, an Indian practitioner, obtained very striking results in the improvement of the pulse by *injecting into the rectum* every half-hour, or hour, or two hours, according to the urgency of the case, a pint of a weak solution of common salt and carbonate of soda, at a temperature of 120°. (Roger's *Reports on Asiatic Cholera*, p. 241.) The injections were retained generally from two to four minutes—sufficiently long to impart considerable warmth to the blood in the vessels of the large intestine; and to this, without doubt, the good effects were due. It appears to me that hot injections into the rectum may be used with advantage in cases of extreme collapse.

The good effects of heat are unquestionable. But what is to be said of *ice* or *iced water*, either taken into the stomach, or applied externally? During collapse there is usually a sensation of burning in the region of the stomach, and a strong craving for cold liquids. Iced water relieves that sensation of heat, and it has a powerful influence in checking vomiting. Are these sufficient reasons for giving iced water to a patient in collapse? In my opinion, decidedly not. I am sure that vomiting, when not excessive, is beneficial; and I believe that iced water lessens the burning sensation in the stomach by diminishing the vascularity of the mucous membrane, and thus interfering with the excretion of the morbid poison, upon the ejection of which by vomiting depends the patient's recovery. For these reasons, I believe that the administration of iced water to a patient in collapse is injurious. In most cases, I would allow an unlimited quantity of water of the temperature of the room; but, in extreme collapse, I would persuade the patient to drink *hot liquids*, with a view to add some warmth to the blood.

The application of *ice to the spine* has been recommended by Dr. Chapman. The theory which suggested this practice is a speculative web spun from the projector's brain. There is no evidence to show that this practice has been useful in any case. That some patients subjected to this distressing mode of treatment would recover, is certain; it is equally certain that their chances of recovery would be lessened by the depressing influence of the ice on the nervous system, and its chilling effect on the blood. We know that, if we can thoroughly warm a patient in collapse, we improve his general condition; and this, whether the warming be effected by the external application of heat, or by hot injections into the veins or into the rectum. We therefore can have no difficulty in perceiving that to thoroughly *chill* a patient in collapse must be a painful and a dangerous proceeding.

Venesection, as we have seen (*ante*, p. 607), has often afforded great relief during the stage of collapse; and I have before pointed out that the only rational interpretation of its *modus operandi* is that, by lessening the over-distension of the right cavities of the heart, it increases the contractile power of their muscular walls. The symptom which appears especially to call for venesection, and which has most commonly been relieved by it, is rapid breathing, with an oppressive sense of suffocation. When, with these symptoms, there is a cessation of vomit-

ing and purging, which is probably a result of the almost entire arrest of the blood in the lungs, I believe that venesection affords the only hope of benefit. If during the last epidemic I had known as much of the essential cause of collapse, and of the influence of venesection, as I now do, I believe that I might have saved more than one patient who, for the want of that knowledge, was lost.

In this communication, I have done little more than sketch the general principles of treatment. The only safe guide in conducting the details of treatment is to have a very clear and definite view of the pathological interpretation of symptoms, and of the mode in which remedies may assist the natural process of cure. Without this pathological knowledge, it is scarcely possible to avoid falling into some of the many errors which have affected the treatment of cholera, and which have often led to the neutralisation of the good effects of one remedy by the simultaneous use of other means having an opposite and an injurious influence.

Original Communications.

MEDICAL LOGOMACHY.

By PATHFINDER.

IN the Inaugural Address of Mr. Toynbee, F.R.S., delivered at the opening of the Medical School, St. Mary's Hospital, Paddington, on October 2nd, 1864, we find the following.

"What *purpose* does disease answer in the great scheme of Nature? He proposed to attempt an answer, incomplete as he knew such an attempt must be in an ordinary lecture. Disease, as the word implied, indicated [signified?] an absence of ease; and why does disease visit man?"

Now, ease is feeling—comfortable feeling, arising from the normal condition of the nerves, which have no injury to proclaim—i. e., no sympathy to express. But is the "expression"—the feeling that indicates injury—really that injury? The purpose of the watchman is to give notice of, where he cannot prevent, injury. But, when he cries out "Fire", is his bawling the "injury"—the veritable *ill-health* (or bad condition) he wants mending or abating? All the pertinent meaning to be got from the word "disease" is this—that it denotes a feeling of uneasiness, which in turn denotes or implies a *state of injury*—i. e., a *not-just* state. This malady is an effect or sequence of other conditions; and hence the inquiry of medicine is, first, In what does this mal-condition consist? and second, What are its causes? This opens out three practical courses; viz., one preventive, one curative, and one palliative. Our duty is, with a diseased person before us, first to cure; secondly, to prevent for the future; and, thirdly, when we can do no better, to palliate. We *prevent* only by avoiding causes. We *cure* either by dislodging removable causes of malady, when the effect vanishes, or by applying counteractives (i. e., remedies which promote right action and lessen wrong). We *palliate* symptoms (i. e., procure ease) when we can do nothing else, or even sometimes when we are also touching causes. If it be asked, How do we dislodge causes? we answer, by strengthening the vital system—first, through the sole direct conditions, those of assimilating food and supplying natural elements of normal action; and second, by administering medicines possessed of specific powers to suppress injurious function.

But let us return to the question, "Why does dis-

ease visit man?" If that at all differs from the questions, "Why does a blister inflame?" or "Why does fire burn?"—I am incapable of perceiving the difference. It appears to me the same as asking, Why is action followed by reaction? But, accepting the first definition given of disease (as implying, indicating, or meaning *absence of ease*), what intelligence is there in the query, "Why does absence visit us?" If we are ever to have any science of medicine, we must learn to define our words more accurately, and to employ them with less ambiguousness. There is, indeed, one answer to be given to the question, "Why disease becomes?"—viz., because the cause of it becomes. But that can hardly have been the meaning of Mr. Toynbee, who proceeds:

"There was one circumstance common to all diseases, however various or utterly unlike [in what?] they might be; all imply a previous influence operating upon the body, disarranging its natural condition; all imply some antecedent injury [antecedent to what?] He agreed with Hunter and others, that disease [the *disarrangement*] had a different object from that of destroying life; and that there was a more satisfactory course to be pursued with respect to it than treating it merely as an enemy to be subdued and stopped."

Here we are plunged into chaos! If disease have the common property of (in other words, essentially is) "disarrangement", which is "injury", how can its purpose be good? Is a broken leg, or a bullet-perforation in the chest, or a bleeding lung, or strangulated hernia, or a fractured skull, or a jerking heart, neither to be mended, stopped, nor subdued? All injury must tend to destroy life; all disarrangement must be bad; and all that is bad is the work either of an enemy or of an ignorant friend. Was Mr. Toynbee thinking of some "healing process", or of some "vital reaction"? I answer, in that case he was not thinking of the malady, but of an instinctive, blind effort of the vital power to resist or repair it. Ought we to confound together in one indiscriminate term the fire and the firemen—the disease and the symptoms by which we recognise Nature's associated effort to cure it?

But, in the next paragraph, Mr. Toynbee absolutely disavours disease from injury altogether! It is no longer a disarrangement or injury, but the *Vis Medicatrix Naturæ*, which re-arranges and re-adjusts! In short, that which was the result of a previous influence, disturbing the natural condition, is now the old chronic Vitality of the body; the suffering Patient has become the active Doctor! I wish he would explain how, consistently with his theory, is justified the subduing of the heart's jumping and jerking action by the administration of digitalis.

"Mr. Toynbee contended that the just interpretation of all this was, that, in disease, Nature was always attempting to remedy an injury; that sometimes without, but often with, the aid of Art, her purpose was accomplished. Sometimes the injury was too great to be remedied even by Nature and Art combined, and life was lost in the contest. It was unwise to attempt to cure a disease without first seeking to ascertain and remove the *injury*, for the repair of which the disease existed. He was disposed to regard an ordinary cold, not as a scourge, but as a remedial process. . . . Assuming, then, that disease was the consequence of an injury, the question before them was—What is to be the end, the aim, of the disease which was thus induced? Could the object be to add further injury? This question was answered in the affirmative by some, who recognised in it, at least in its progressive stages, a process essentially destructive. It must be manifest to every medical man that, in many cases at least, Disease, in