Notes

ON

THE PATHOLOGY AND TREATMENT OF CHOLERA.

BY

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THERE are few diseases, the treatment of which has been more influenced by pathological theories than cholera. The theory which has gained almost universal acceptance is, that the worst symptoms of the disease are due to the drain of fluid from the blood. The practice which has been based upon this theory is to check the purging by opiates and astringents, and to restore to the blood its lost constituents by saline injections into the veins. What if this theory be erroneous, and the practice suggested by it injurious? That the theory and the practice are both wrong, I am persuaded; and I propose now to give the reasons for my belief.

First, then, let us inquire, *What is the relation between the symptoms of choleraic collapse and the loss of fluid by vomiting and purging?* It will, I suppose, be conceded by all who are prepared to argue this question, that if the symptoms of collapse are occasioned by a drain of fluid from the blood, there must, as a rule, be a direct relation between the degree of collapse and the amount of liquid which escapes from the blood. Now, so far is this from being the case, that there are few writers of any note or authority upon the subject of cholera who do not either assert distinctly, or record facts from which the inference plainly follows, that not only is there no direct relation between the loss of liquid by vomiting and purging and the degree of collapse; but that these conditions often bear an inverse ratio to each other. In confirmation of this statement, I purpose now to quote some of the best known authors on the subject of cholera.

Dr. Edmund Parkes says: "My cases bear out the observations of Scott, Jameson, Orton, Kennedy, Copland, and, in fact, almost all the English writers of reputation, that there is absolutely no ratio between these two classes of symptoms" (i. e., between the purging and vomiting and the symptoms of collapse) "or that they appear even to observe an inverse ratio to each other. Thus, at a period of the case when the algid symptoms were most fully developed—viz., in the last five hours—the purging ceased; in the cases where the algid symptoms were prominent throughout, and which cases were consequently the most malignant and the most rapidly fatal, the passage of fluid from the intestines was oftentimes trivial in degree and shortened in the period of its occurrence. In cases in which the vomiting and purging were excessive, the algid symptoms often came on slowly, and were less marked and deadly." (*Researches into the Pathology and Treatment of the Asiatic or Algid Cholera.*)

Dr. Parkes, then, in illustration of these remarks, cites some cases in which the frequency of the vomiting and purging was quite out of proportion to the severity of the other symptoms. He afterwards states that "it may be objected to observations of this kind, that the number of stools is, after all, no certain indication of the amount of fluid passed.

This objection would be of weight in cases where the stools were not very different in number; but in some of these cases cited above, we have two or four stools attended by more rapid death than twenty-five or twenty stools; and yet, in the first case, it would be impossible not to suppose the quantity of the fluid passed to be much below that of the second case." He adds, "it may be confidently asserted that 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."

With reference to the varieties in the general symptoms of cholera, Scott makes the following statement: "A frequent variety, the worst of all, is that which is noted for the very slight commotion in the system; in which there is no vomiting, hardly any purging—perhaps only one or two loose stools—no perceptible spasm, no pain of any kind; a mortal coldness with arrest of the circulation comes on from the beginning, and the patient dies without a struggle. This has frequently manifested itself as the prevailing type, and almost all die who are attacked by it." (*Report on the Epidemic Cholera.*)

The testimony of Bell is to the same effect. He says: "It has been found that the more violent the prominent symptoms are, the more likely is a cure to be effected; and that when the disease is attended with rapid collapse, little or no vomiting and purging, and no spasm, the prognosis is very unfavourable." (*Treatise on Cholera, Asphyxia, or Epidemic Cholera.*)

Orton, in several passages of his essay, alludes to the fact that, in the worst forms of the disease, vomiting and purging are slight or quickly cease, or even do not occur at all. (*An Essay on the Epidemic Cholera of India.*)

Twining gives cases which illustrate the same general principle. Thus, he reports (*Clinical Illustrations of the More Important Diseases of Bengal*, p. 10) the case of a gentleman who died nine hours after the commencement of urgent symptoms, in whom there were occasional slight efforts to vomit at intervals of half an hour, and he had only four stools from the commencement to the termination of the attack. And, in another part of his work (p. 37), he says, "We often see patients vomiting violently for hours, and others purged profusely for several days without cholera (i. e., collapse) coming on."

The authors above cited have all observed the disease in India; but the testimony of those who have witnessed it in Europe is in strict accordance with that of the Indian authorities. Thus, Magendie, describing cholera as he saw it in Paris in 1832, makes the following statement: "Some patients have no evacuations, inasmuch that one is obliged to excite them; for the evacuations, though associated with the disease, are not one of the most serious symptoms; and those patients who have had copious evacuations have been more easily treated than those who have had none. This is a remark which has been made by many physicians." (*Leçons sur le Cholera Morbus.*)

Mr. French states, amongst other reasons for believing that the intestinal discharges are salutary, that while "cholera, in its most intense form, produces death instantly without discharges, all those who recover from its attack experience the peculiar discharges more or less." And, again, in favour of the same view, he says, is "the ultimate recovery of persons who have continued in a state of collapse for a considerable length of time, often extending to a period of three days, and who, in all instances, sustained enormous discharges." (*The Nature of Cholera Investigated.*)

Dr. Gull, in his Report on the Morbid Anatomy and Pathology of Cholera, published by the Royal College of Physicians in 1853, cites the evidence of several practitioners to the effect that, in many cases, "the evacuations appeared to be wholly insufficient to account for the fatal collapse." And one case which came under Dr. Gull's observation, affords a striking illustration of the same principle. "On a post mortem examination, the large intestines contained healthy fæces; whilst in the upper two-thirds of the small intestine, the mucous membrane presented the ordinary changes induced by the cholera process, and the rice-water effusion was abundant." Dr. Gull adduces this case to show that "*cholera sicca*, in a strict sense, does not occur; for although the disease may be fatal without any evacuation, the intestines after death, in such cases, have been found to contain the rice-water fluid." It can scarcely be doubted, however, that when, as in this case, the purging has been insufficient to remove the fæculent contents of the large intestine, the loss of fluid must have been out of all proportion less than in most cases in which recovery takes place. And Dr. Gull, in another part of his Report (p. 211), admits that "the intensity of the symptoms is often in no inconsiderable degree greater than can be accounted for by the amount of the effusion."

With respect, then, to the question whether there is any direct relation between the loss of fluid by purging and the symptoms of collapse, we have a large amount of concurrent testimony to the effect that no such relationship exists.

Even admitting that there were a direct and constant relationship between the loss of fluid and the degree of collapse, further evidence would still be required to prove that they stand to each other in the relation of cause and effect. It might be that they are only the effects of one common cause, which while, on the one hand, it gives rise to collapse, on the other excites vomiting and purging. How far this may be the case, we shall have to consider hereafter. In the meantime, it must, I think, be conceded, that the evidence of there being an inverse rather than a direct ratio between the degree of collapse and the loss of liquid by vomiting and purging, is fatal to the hypothesis so generally received and acted upon, that choleraic collapse is caused by the drain of liquid from the blood.

We will next go on to inquire whether the symptoms of collapse are such as an excessive drain of liquid from the blood might be supposed to produce.

[To be continued.]

THE PRITCHARD CASE. The *Glasgow Herald* says: "The prisoner retains the same amount of self-possession that he has exhibited since the night of his apprehension. A day or so after his incarceration he seemed to feel a little annoyed that he could not be favoured with a supply of pomatum for the trimming of his beard and hair. The prison regimen has not at all suited his taste, but he does not seem to have lost flesh, and his pale appearance may be ascribed to the confinement. In fact, so far as we learn, Dr. Pritchard has all along taken matters very coolly. We are told that to a lady whom he met in the street shortly after Mrs. Pritchard died he remarked 'that his beloved wife was gone, that her death had resulted from an attack of typhoid fever, that he had called in three doctors to visit her on the day prior to her death, but that it had been of no use.' 'Too many cooks, he added, spoil the broth.' In point of fact, only one doctor—viz., Dr. Paterson, saw Mrs. Pritchard on the day prior to her death."

Original Communications.

A WORD CONCERNING THE USE OF BROMIDE OF POTASSIUM.

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THE number of morbid conditions of an uniform character, and allowing of a statistical enumeration of the effects produced by any given remedies, is so small, that we must necessarily, in the present state of science, be extremely sceptical as to any statement averring or denying the efficacy of a drug employed in their removal. There is an inherent weakness in all attempts to tabulate the results of treatment in the great majority of diseases that meet us, not because treatment is useless or the medicinal agents valueless, but because the wise physician does not regard the disease as a nosological abstraction but as a deviation from the healthy standard involving a great variety of elements, each of which may in its turn differ in degree, character, relation, in any given number of patients brought together for comparison. How often do we meet with uncomplicated disease of one organ or tissue? Does it, in fact, ever exist? Or does not rather the whole history of medicine, as well as our individual experience, demonstrate the variability of morbid phenomena according to the circumstances of family history, personal habits, social influences, climate, sanitary conditions, age, sex, diet, that mould our patients?

The duty of rigid inquiry into all the conditions that determine an individual case of disease, becomes the more important the more limited our knowledge is of that particular disease; and if it can be shown that a disease known by a certain technical denomination may not be regarded as a nosological unit even as much as many other diseases that we are in the habit of dealing with familiarly, surely, before determining the value of a new drug or any drug in its treatment, we ought first to agree upon the nature of the cases to which we apply it.

Some time ago, a proposal, very laudable in the abstract, was made, to bring together from various sources and tabulate a large number of cases of pneumonia; and, by comparison, to deduce the results of treatment. It was not surprising, that no conclusions were arrived at; because, although there are certain features of resemblance between all cases of pneumonia, there are so many differences, independently of any bias of the individual observer, that we have not yet attained the common basis of observation. A case of pneumonia occurring in a person with a rheumatic taint, would not be the same as pulmonary inflammation affecting a person in whom no specific morbid diathesis was traceable. Pneumonia in a man inured to hard work, but accustomed to a gallon of beer a day, would demand different remedies, or at all events react differently to the same remedies that might be employed were the patient an overworked and underfed laundry-maid. Every practitioner of experience knows and acts upon this principle, and calls it tact. His daily intercourse with disease convinces him that, although his diagnosis becomes more and more precise, he cannot formularise his remedial effects in such a way as to justify the conclusions of a tabular statement. It is on such grounds that the attempt so often made to determine the value of a given remedy so uniformly fails; and it is for the same reasons, also, that it is