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.

By E. H. SIEVEKING, M.D., Physician in Ordinary to H.R.H. the Prince of Wales.

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

so dangerous to make positive statements as to the efficacy of a particular medicinal agent.

The pages of this JOURNAL have repeatedly referred to the uses of bromide of potassium in epilepsy. Statements in its favour have been contradicted by writers holding different views. The abstract question as to the value of bromide of potassium in epilepsy was put, as if the disease were a simple perversion of a normal function or a single well definable lesion, which could be neutralised by a given antidote. We have scarcely arrived at a sufficiently precise knowledge of the disease as yet, to reduce the question of treatment to so simple a denomination. We do not know nearly as much of epilepsy as we do of pneumonia, as to the local changes occurring in the body; but, although our knowledge is almost limited to the external phenomena, we are sufficiently advanced in our nosology, to affirm that the circumstances influencing and giving rise to the disease are as complicated as in any disease in the most complete nosological list. It would accordingly be scarcely fair, either to the disease or to the remedy to be tested, to take at haphazard a given number of cases characterised by the epileptic paroxysm, and to determine by the result whether or not the remedy deserved our confidence.

My objections to such a procedure would become still greater, if I were told that the cases were all of considerable standing, and justified the assumption that secondary cerebral lesions had already resulted.

We know of bromide of potassium, that it may be taken in large doses with impunity, and that it passes off by the kidneys; but we know nothing definite of its action on the nervous system. It behoves us, therefore, to be extremely careful not to reject an agent on insufficient grounds; but it is not so much my object to advocate the use of this agent, as to suggest that it does not follow that it is useless because in an asylum containing many confirmed epileptics it fails to produce the desired effects.

I am a firm believer in the general use of drugs in the treatment of disease; but I am habitually sceptical as to the effect produced by any given remedy in an individual instance, knowing the limitation of our powers of observation and analysis. I am not likely, therefore, to over-estimate any medicine; but, unless I have altogether wasted my time and opportunities, I have satisfied myself that there are many cases of epilepsy in which medicines not only benefit but cure the patient; and I believe, also, that in some—alas! but too few—it was something more than a mere coincidence that bromide of potassium arrested the disease. I am, however, equally certain that there are a large number of cases in which it is useless; and that epilepsy, like all other diseases, must not be treated as an unity, but regarded in all its aspects, and treated according to the various phenomena under which it may present itself. Bromide of potassium may be useful in one class of cases, but not in others; just as nitric or nitrohydrochloric acid benefits a certain class, and is useless or injurious in others. The iodides, again, are undoubtedly beneficial in some forms of the disease. Chalybeates, again, are required by some, and aggravate the malady in others.

It is not my wish, nor do I know that I could, with satisfaction to myself or the readers of the JOURNAL, classify the different forms, with the precision that I believe to be necessary; but of this I feel assured, that if we are to arrive at any definite and scientific result with regard to remedies in this particular disease, we must not lump all our cases together, and treat them all with the same remedy, especially with one the physiological action of which is as yet so little determined as that of bromide of potassium.

ON THE NATURE AND TREATMENT OF FEVER.

By A. B. STEELE, Esq., Liverpool.

[Read at the Liverpool Medical Institution, February 9th, 1865.]

I HAVE long been impressed with the conviction that the study of fever (especially with reference to its therapeutics) as pursued in the systematic treatises of authors, differs materially from the study of the disease as read in the book of nature at the bedside. I am disposed to attribute the confused, unsatisfactory, and conflicting conclusions to which the written history of fever leads the inquirer, to two principal errors:—first, that authors have laboured to establish a method of cure; and secondly, that they have based their systems upon a preconceived theory rather than upon clinical observance of the phenomena of the disease. To one or both of these mistakes, may be referred the erroneous but now happily exploded doctrines of Hamilton, Armstrong, Broussais, Clutterbuck, and others; it also explains the ephemeral reputation of those highly vaunted specifics, such as cold effusion, yeast, mineral acids, quinine, phosphorus, antimony, chlorine, mercury, and even alcohol, most, if not all of which, especially the latter, may be useful as remedies under certain circumstances, when given on rational principles, but all of which infallibly become doubtful or dangerous when regarded as specifics. Indeed I hope to be able to show that not only is there no specific for fever, but that, as Dr. Corrigan has observed, in the present state of our knowledge there can be no specific for this disorder.

In Dr. Watson's *Lectures*, we find the following allusion to the opinion of Pitcairn on this subject. "I do not" (says he) "like fever-curers; you may guide a fever; you cannot cure it. What would you think of a pilot who attempted to quell a storm? Either position is equally absurd. In the storm you steer the ship as well as you can; and in a fever you can only employ patience and judicious measures to meet the difficulties of the case." (Vol. ii, p. 843).

This sagacious and profound remark, in my opinion, comprises the whole principles of fever therapeutics. Although it guards us against useless interference and injudicious meddling with the operations of nature, it by no means sanctions or justifies a mere "do nothing" system, which is sometimes disguised under the specious title of "expectant treatment."

Without dwelling upon the now almost discarded doctrines of physicians of a former generation, I propose to point out what seems to me a want of practical soundness in the teaching of some authors of our own time. Let us appeal to Dr. Murchison, whose treatise on fever is considered one of the best and most recent text-books, and which undoubtedly contains a fund of valuable information on the subject of which it treats. In criticising the opinions of a physician of high repute and acknowledged ability, I must be allowed to say, that I do not for one moment venture to question the soundness or the success of his practice. It is the doctrines enunciated in his writings, and the inferences he deduces therefrom, to which I take exception, as calculated to confuse and mislead rather than to guide and assist the inexperienced and inquiring practitioner. Dr. Murchison's theory may be summed up as follows. (Pp. 15-16.)

1. The fever-poison enters the blood.
2. The nervous system (and particularly the sympathetic and vagus) is paralysed.

Here we at once detect what seems to me an important fallacy. In many cases of fever it is not paralysis, but the very reverse; namely, exalted or