REMARKS ON THE TREATMENT OF ACUTE RHEUMATIC FEVER:*

Being some Brief Observations on the Discussions lately held at the Royal Medical and Chirurgical Society of London.

BY HENRY KENNEDY, A.B., M.B.,

One of the Physicians of the Cork Street Hospital, Dublin.

WITHIN a brief period a very interesting discussion, relating to the treatment of rheumatic fever, took place at the Royal Medical and Chirurgical Society of London. The point discussed was, however, one of more than ordinary moment, being neither more nor less than the question whether art has any means at command, by the use of which the duration of the disease could be shortened. One party maintained that the disease, if treated only with mint-water, would terminate in about ten days; that is, when no treatment was adopted, the tendency of the disease was to cease about this period; and a number of cases were quoted in proof of this statement. The other held the opposite, maintaining that the treatment known as "the alkaline" did materially shorten the attack. Before stating my own views, I would observe that the question discussed is but one of many of a similar import which have been started within the last few years. The late Sir John Forbes appears to have been amongst the first to bring the matter prominently forward, and to have questioned the power of our art not only over rheumatic fever, but over disease in general, and to have arrived at the conclusion that Nature does everything, and art little or nothing. In keeping with these views, all are aware that what is known as the antiphlogistic treatment has been not only questioned, but even rejected by many of late years. Hence, too, has arisen the question which it is the more immediate object of these remarks to discuss.

Now, I believe that the views commonly held on this point are not sufficiently clear. To assert that the physician cures disease is, I think, in the strictest sense of the words, incorrect. The proper way to put it appears to me to be that the physician treats, and, with the assistance of Nature, cures disease. As to the views advanced by Forbes, and held by many since, I do not believe in them at all; my experience is entirely opposed to them. I have not found that Nature one moment causes disease, and the next sets about curing it. On the contrary, cases either overlooked or neglected, I have not found to get quite well of themselves. Take examples of any of the acute affections—as pneumonia or pleuritis—and, if left entirely to themselves, what do we find at the end of weeks or months? Is it that Nature has cured them, or that we can detect no mischief in the chest? Quite the contrary. And on this point I appeal to the experience of all present, who must have met instances of either disease, which, as I have said, had been overlooked or neglected. Nature, it is true, in the course of time, modifies them much, but she does not cure them; and the system at large suffers seriously from the disease—the latent fire, as it may be called, which keeps still smouldering, and which, if there be a tendency to strumous disease, it is sure to engender; and so it is of other affections, acute or chronic. Who knows not the mischief which often follows a neglected cold?—an expression, by the way, in every one's mouth; and yet this is one of the very mildest affections with which we can be assailed. Or, if we take any of the real diseases of the skin, or syphilis, will any one maintain that these cure themselves? On the contrary, will it not be allowed that the cure of these affections is a distinct triumph of our art? But some one will ask me here, Will not a common cold cure itself? or what control have we over fevers? My answer to the first query is—I believe, indeed I think I may say I know—that a little well-directed treatment will shorten materially its duration, whilst it lessens its severity. The second query calls for more consideration; for here I am quite ready to admit that our control over the specific poisons which cause fevers is little or none. For myself, however, I believe that in barm—and there has been often occasion to state this before—we have an agent of great power in counteracting low states of fever, whether met with in typhus, scarlatina, or small-pox; and with the giant strides which chemistry is now making, it seems to me highly probable the day will come when agents will be discovered capable of counteracting more effectively these poisons. Nor, in the closest connection with this subject, must we overlook the series of most valuable experiments made within the last year or two on the poisons of snakes introduced into the living frame. I confess I look forward to great results from them.

Leaving, however, this question of antiseptics for the present, I hold

that our management of a case—say of typhus—within the first few days of the onset of the attack, is all-important. I have the strongest conviction which the nature of the subject admits, that this preparatory treatment—if I may so speak of it—often makes the difference between life and death. In August 1867, when the British Medical Association visited us here, an eminent London physician remarked, in my presence, that he had no faith whatever in treating fever, except so far as sustaining the strength of the patient went. On my telling him that I frequently applied leeches to the head, with the most signal benefit, and more particularly early in the attack, when epistaxis had threatened or occurred, he admitted at once that such a measure might be very advisable. Indeed, as we know that the danger of a case of genuine typhus happens from the ninth to the thirteenth day, and that this danger usually arises from the brain becoming deeply engaged, I cannot see any valid reason why we should not do our best to anticipate what is coming. I repeat, then, that I believe we can mitigate the symptoms of a state which, if left to itself, would prove fatal. Here I almost feel that an apology is necessary for dwelling on points of this kind, and before such a meeting as I now have the honour of addressing; for the profession in Dublin have, I am happy to say, been singularly free from these fluctuations in opinion, as to the treatment of disease, which have obtained elsewhere; still scepticism—and it is nothing short of this—no matter where it may have shown itself, is capable of doing injury to the whole profession, and so I have noticed it.

Though the foregoing remarks are of a general kind, I have thought it essential that they should be made, as they have the closest connexion with my subject, and tend naturally to the question which I would bring before you: Can our art shorten the duration of the disease known as rheumatic fever? I believe it can. I have as strong a conviction on this point as I have that sulphate of magnesia will act as a purgative. Here it may be well to observe that the affection of which I speak is one of great frequency, of a very specific character, entailing much suffering, and one which it is scarcely possible to mistake. It is one, too, which, if I may so say, we can both see and handle; and, lastly, it is one of the exact nature of which, if we have not a complete, we have at least a considerable knowledge. Hence it would scarcely be possible to select a disease more suited to test the resources of our art than this very one. But before speaking of these, it will be necessary to advert briefly to the natural history of the affection; for on this point it is that the gentlemen in London (and very eminent they are) take their stand, by asserting that, if the disease be left to itself, and treated only by that potent agent, mint-water, it will subside in ten or eleven days. I confess I read this statement with great surprise, it was so totally at variance with all my experience on the matter. I could not but call back the time when many, if not most of us, spoke of six weeks and flannel as being the only cures for the disease, shewing, as I think, conclusively, what its natural history then was; and, as years rolled on, and I came to treat the disease myself, I could not help observing that, in by far the majority of cases, a week at least had elapsed before the patients came into hospital with the attack fresh upon them; and when they came in, after even a fortnight, the disease was still at its height, and the suffering intense. And let it be observed, that from the very nature of the disease these patients were compelled to keep their beds, and so carried out the treatment, I vouch for it, systematically, which some would have us now adopt. Yet the disease did not subside, as their coming into hospital fully proved. As to the cases advanced by Drs. Gull and Sutton, in favour of their views, I must say they appear to me very unsatisfactory; but as this has been already shewn by Dr. Fuller, in his last able communication in *The Practitioner*, I need not allude to them further here.

And now to come to the important part of this brief notice—for it is nothing more—what is the best line of treatment for acute rheumatic fever? With our present knowledge, it lies chiefly, I believe, in some one of the modifications of what is known as the "alkaline treatment", which, amongst other effects, there can be no doubt, renders the urine neutral, or alkaline. Though it has been only of late years that the proper way of effecting this object—I mean rendering the urine neutral—has been known, still the plan of using alkalies, and their earths, to as great an extent as has been recently recommended, is, like many other plans brought forward as new, old enough, having been in vogue with several during the last century. It must be repeated, however, that it is a knowledge of the fact that we can positively render the urine neutral which gives us the advantage over our predecessors, and which, to my mind, renders the treatment of the disease almost a certainty.* Which

* When I say a certainty, I would at the same time wish it to be understood that exceptional cases occur where a complete alkaline treatment would be very injudicious. Thus, I have now seen several instances where, in the course of an attack of scarlatina, acute rheumatism of the joints, attended with much swelling, showed itself; and, not long since, I was asked to see a severe case of spotted typhus, where a similar occurrence took place. The remarks mainly apply to the disease in its acute and uncomplicated forms.

* Read before the Medical Society of the King and Queen's College of Physicians, Ireland.

preparation of potash should be given, or whether different salts should be combined, I must say I have not been able to determine. As we know that they almost all leave the system as carbonates, the presumption is that no important difference exists as to their effects on the system. This point, however, must still be considered *sub judice*. The alkaline treatment, then, of rheumatic fever, I consider as taking the very first place; of course modified in the extent to which it is carried by the demands of each case. On this point I go the entire way with two London physicians, Drs. Garrod and Fuller, who have each written most ably on the matter. The paper, indeed, of the former appears to me absolutely conclusive; and yet, strange to say, I believe it was not even alluded to in the late discussions in London.

Forcibly, however, as I would wish to bring under your notice the alkaline treatment of the disease, I am very far from saying that it is all that is required. You all know that the affection is one which very generally, though not always, entails severe suffering. All suffer pain, but some a great deal more than others. Now, in all these cases I have been in the habit, for some years past, of giving anodynes in union with the alkalies, opium being the chief, and, when it agrees, by far the best. The opiate treatment has been long known; but I am not sure I have anywhere seen the combined plans recommended; and yet, with my present experience, I believe it to be the best and safest plan which can be adopted. It acts *tuto, cito, et jucunde*.

In these very brief remarks on the treatment of acute rheumatism, I have purposely omitted many points both of moment and interest, as well as treatment. To one point only, before concluding, I may allude—that is, whether the alkaline treatment lessens the probability of the heart becoming engaged. I believe it does. But this is a point on which it must be allowed a great caution would be required, before giving an opinion. If, however, a certain line of treatment lessens the duration of this disease, it would be fair to infer that it must, by so much, diminish the chance of the heart getting engaged; and this would seem to be confirmed by any statistics yet brought to bear on the question. Within my own observation, the heart, when attacked at all, has, in the great majority of instances, been affected before the patients came into hospital; and it has been the valves, not the pericardium, which were involved. It is worth noting that I have, in four instances, seen pericarditis precede the affection of the joints.

To sum up, then, I hold that acute rheumatic fever can, by our art, be materially shortened in its duration, and that those who are sick need a physician.

REMARKS ON THE TREATMENT OF DIABETES.

By F. W. PAVY, M.D., F.R.S.,

Senior Assistant-Physician to, and Lecturer on Physiology at, Guy's Hospital.

CONTAINED in the number of the BRITISH MEDICAL JOURNAL for April 10th, I noticed a communication from Dr. Basham, entitled "Observations on the Successful Treatment of Diabetes by Alkalies and the Phosphatic Salts of Ammonia, with Limited Restriction of Diet." A case which I brought forward at the Clinical Society towards the close of last year, that had been treated successfully without restriction of diet, is there alluded to. The medicinal agents administered in this case consisted of opium in increasing doses until the quantity reached twelve grains *per diem*, and a mixture containing the bicarbonate of potash and aromatic spirit of ammonia.

What is said by Dr. Basham leads me to offer a few remarks, because the inference is conveyed that the result might have been attributable to the alkalies, and not to the opium. I am quite ready to admit that it would have been more satisfactory for the sake of conclusiveness if the alkalies had not been administered; but I am at the same time convinced, from what I have seen in other cases, that they had little or nothing to do with the rapid arrest of the discharge of sugar that occurred.

I know of no medical agent that is capable of exerting a controlling influence over the complaint like that exerted by opium. In common with others, I have employed alkalies and ammonia largely in diabetes, and am well familiar with the kind of effect to be looked for. I have been of opinion that they produce a slowly beneficial action in the complaint; but never have I observed any immediate effect of a marked nature that could be attributed solely to their influence. With opium, on the other hand, the effect is so striking, that there can be no question of its reality. In my communication to the Clinical Society, besides the case above alluded to, two others were mentioned, in both of which the sugar was removed—in the one instance by opium, and in the other by morphia. In these cases, there was no other agent administered. They were cases of the disease in middle-aged subjects. The

sugar was reduced as far as it could be by a restricted diet; and then, under the influence of the morphia and opium, it was entirely removed.

There is also at the present time a patient at No. 1 bed in John Ward, Guy's Hospital, a most instructive case bearing on the point in question. It is the same patient to whom I administered the ozonic ether without observing any beneficial effect, and whose case, in reference to this agent, I mentioned in the *Lancet* a few weeks ago. The man, a middle-aged person, was passing, under a restricted diet, about six pints of urine, containing upwards of three thousand grains of sugar, in the twenty-four hours. At first, after the ozonic ether had been discontinued, morphia was administered in gradually increasing doses, beginning with half a grain three times a day. The quantity of urine and sugar from day to day declined, and, in the space of about three weeks, disappeared altogether; the quantity of morphia administered at this time being a grain and three-quarters three times a day. After a little while, the morphia was taken off; and in a week's time the sugar began to reappear, and went on from day to day increasing. This state of things was allowed to continue for about a fortnight, when opium was given; and, under its influence, the sugar has again disappeared. I merely here allude to this case; the patient, being still in the hospital, may be seen by any one desirous of doing so.

Of what service as a curative agent, opium, in larger doses than it has been customarily given, may prove in cases in general, I do not at present pretend to say. Much more extended observation is required before this point can be determined. Whatever may happen to be disclosed by subsequent experience, it cannot be denied, I think, that we have before us an important therapeutic fact. The woman whose case formed the basis of my communication to the Clinical Society has been seen by me from time to time up to as recently as a fortnight back. She has taken no opium or other kind of medicine since October last; has been living (as she did when under treatment) upon an ordinary mixed diet; and has been passing urine devoid of sugar. Her case, in fact, up to the present time, forms an example of a direct and perfect cure.

I have a patient under treatment, also in John Ward, to whom I have been lately giving the carbonate of ammonia to a much larger extent than it is usually administered. One hundred grains in the twenty-four hours have been taken. At the commencement, it was given without a restricted diet; then discontinued; and, after the patient had been for a few days upon a restricted diet, given again. At first, the effect observed was an increase both in the amount of urine and sugar. Since, however, a marked improvement has occurred; but, if space be accorded me in the BRITISH MEDICAL JOURNAL, I will, in one of the forthcoming numbers, supply a detailed report of the case.

DISLOCATION OF THE SEMILUNAR BONE OF THE CARPUS.

By R. P. B. TAAFFE, M.D., Brighton.

A SHORT time since, the sprite at the Brighton Theatre, in one of his performances on a trapeze, fell from a height of about twenty feet. He fell on his side; and, according to the statements of his fellow-performers, he rebounded from the stage about two feet. In his fall, he struck the back of his hand violently against the stage. On looking at the wrist, there appeared to be some amount of deformity. On close examination, it was found that there was dislocation of a single bone of the carpus, which projected prominently forwards and upwards between the radius and ulna; it was not wedged between the bones, but projected in front of them on the anterior aspect of the limb. The radius and ulna were not displaced; nor was there any evidence of fracture.

I saw the case within ten minutes after the accident, and before any swelling had taken place. I was enabled to reduce the dislocation by means of strong extension and firm pressure, without chloroform. When last seen, the movements of the wrist were not impaired, and there was no recurrence of the dislocation on flexion or extension of the joint.

From the situation of the dislocated bone, and its being quite free, without any displacement or fracture of the radius or ulna, and its being reduced by pressure inwards and downwards, it could have been no other than the semilunar bone.

Mr. Erichsen records a case of dislocation of the semilunar bone on the dorsal aspect of the carpus. The peculiarity of the case I relate is, that the bone was displaced anteriorly, and thrown some distance above the carpus (about half an inch), and into the forearm.