THE COLD BATH IN ENTERIC FEVER.

Introduction to a Discussion in the Section of Pharmacology and Therapeutics at the Fifty-Second Annual Meeting of the British Medical Association.

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THE subject on which I propose to make a few observations is the treatment of enteric fever by means of cold baths. The use of cold water in the treatment of disease is not new. It is found amongst the most ancient nations; but, historically, Asclepiades of Prusa, a physician who practised with great success in Rome about the end of the second century before Christ, was amongst the first who brought into general use cold, warm, and hot baths. At this time, the cold water cure became the fashion, and the cold springs of Cutliala in the Sabine mountains had a great reputation. From this period until the beginning of the eighteenth century, I can find no mention of the use of the cold bath in the treatment of disease; but, in the year 1702, it was strongly recommended by Floyer, and in 1729 Hoffmann said "that, employed freely, and not too cold, it is the best remedy in acute diseases, and in chronic diseases one must prefer it to all other means." Up to 1801, it was recommended by Sigismund, Hahn, Unzer, Zimmermann, and others, when a new impetus was given to it by Currie, who employed it in the early part of the present century; but he does not appear to have had any followers.

In recent years, however, it has been resuscitated in Germany; and Brand, Liebermeister, Jürgensen, Ziemssen, and others, have asserted that it greatly reduces the mortality of enteric fever, and more recent statistics even assert that the mortality has been abolished. In these statistics it appears to be assumed that, given cases of enteric fever, the employment of the cold bath, and a low mortality, the conclusion follows necessarily that the low mortality was the result of the cold baths. It is assumed that enteric fever is an unit of equal value, at all ages, at all times, and in all places; and it is forgotten that the disease has a natural history, and that, of any given number of cases fairly chosen, the great majority recover irrespectively of treatment. It is a matter of minor importance what may have been the mortality in a given number of cases, but of the first importance what may have been the nature of the cases upon which the mortality is calculated. This is the great difficulty of medical statistics—probably of all statistics—the unequal value of the units of which the statistics are made up.

The laws of physics are inductions of particulars which are similar and true. Tests of the most varied kind, involving minute and accurate measurements, prove these laws to the satisfaction of every one capable of understanding the subject. But, in medicine, there is much to be desired, both in establishing data and in drawing conclusions from them. The phenomena dealt with are so complex, so variable, and so far beyond the reach of the appliances which man can bring to bear upon them, that it is rarely possible to arrive at the data necessary for a satisfactory induction. Hence a great deal of what is regarded as gospel in medicine has no foundation beyond impressions depending, to a great extent, on the mind impressed, and statistics composed of units of such different values as to be hardly comparable. Even in astronomy, the personal equation has to be allowed for. In medicine, it is nearly everything. From all this, I trust it will be clear that much wider and more minutely differentiated statistical data will be necessary, before even a *prima facie* case will have been made out in favour of the cold bath treatment.

Some of the existing data will tend to show that it is not always successful. Take, for instance, Liebermeister's statistics at Basle for the years 1873-74, and compare them with those at Homerton and Glasgow for the same years.

Of 163 cases at Basle, in the year 1873, the mortality was 10.4 per cent.; whilst in the same year, of 275 cases at Glasgow, the mortality was 9.4 per cent.; and, of 305 cases at Homerton, it was 9.5 per cent. So also at Basle, in 1874, there were 200 cases, with a mortality of 10.5 per cent.; at Homerton, 372 cases, with a mortality of 9.6 per cent.; and at Glasgow, 343 cases, with a mortality of 7 per cent. It is unlikely that the cases of a small country town like Basle, with forty to fifty thousand inhabitants, were more severe than those occurring among the pauper population of Glasgow, with its five hundred thousand inhabitants, and the pauper population of the East End of London, with upwards of one million. And, in this point of view, it is important to observe that it is in the small country towns of the continent that the "antipyretic treatment" has been said to be successful, such as Halle, Jena, Heidelberg, Prague, Zürich, etc. "It does not succeed in Vienna." To this we may add, that it does not succeed in London

and Glasgow. In this there is nothing peculiar. The mortality from acute disease in the hospitals of the large towns is nearly always greater than that of the hospitals of the small country towns; and it will not surprise any one that Jena, Halle, and Basle are no exception to the rule.

So much for the assumption that number of cases and number of deaths prove anything as to the value of particular methods of treatment. Apart, however, from this fallacy, the supporters of the cold-bath treatment appear to have based it upon an assumption for which there is no evidence. This is the assumption that the all-important factor is the heat.

The body is too hot; all the mischief arises from that. Let the heat be abstracted, and all will be well.

The question ceases to be a complex question of biology, and becomes one of pure physics. Clinical experience contradicts this simple view. All the mischief does not arise from the heat. In addition to the heat, there is the individual who is affected by it; and, in this respect, individuals vary. One patient is restless and sleepless, with a dry tongue, and a temperature ranging between 102° and 103°; whereas another, with a higher temperature, is so slightly affected that, but for the thermometer, one would hardly know that he was ill.

Notwithstanding the small reason, *a priori*, there was in favour of this treatment, after reading the existing literature on the subject, I determined, in the year 1872, to give it a trial, moderately and cautiously.

If the patient had not slept for two or three days, even if the temperature fell short of 103°, he was wrapped in a flannel dressing-gown, and placed in a bath of 70° Fahr., gradually reduced to 60° in from ten to fifteen minutes, at the end of which time he was removed to bed, wrapped in a blanket. In nearly all the cases, we thought it prudent to give some stimulant to the patient in the bath, from noting that its effect was to weaken the pulse, which fell but a few beats in frequency, and became thready, compressible, wavered, and sometimes almost disappeared. It required resolution to proceed with the bath in such circumstances, circumstances pointing to impending collapse, a result which occurred in two cases. The general effect of the bath was to lower the temperature two to three degrees at once; and for about three quarters of an hour after the bath it continued to fall, about which time the maximum fall had been reached. It then began to rise, reaching its former height in periods varying from one to several hours. In a large number of cases, sleep followed, varying from half an hour to two or three.

The general conclusion formed of the value of the bath was that, in the milder cases, and in some of the severe cases in the early period of the disease, the bath, given once or twice daily, was an useful way of relieving the discomfort produced by the heat, of allaying restlessness and producing sleep; that, beyond temporary relief of symptoms, it had no effect on the course of the disease or on the general mortality; and lastly, that, in the severe cases, the remedy was inadmissible, owing to its depressing effect on the body generally, to the exhaustion which its administration entailed, but, above all, to its definite and marked effect upon the circulation, which it invariably greatly weakened. In other words, that the bath was useful in relieving symptoms in those cases whose natural termination is recovery; inapplicable, by reason chiefly of cardiac weakness, in those cases whose tendency is towards death; that is, in the cases in which specific treatment is needed. The depressing effect of the bath has been noticed by Liebermeister, who considers a very high degree of cardiac weakness an important contraindication. What exactly may constitute such a condition will be differently estimated by different persons; but it may be inferred reasonably that one who has been the subject of a fever whose definite and specific effect is to produce, and that rapidly, some kind of muscular degeneration and death by asthenia; one who has, let us say, passed ten days without sleep, and with little or no food, is a person whose condition generally is one of weakness, and whose heart in particular is weak in a very high degree. What might be inferred, a look at the patient shows at once. He lies prostrate, unable to turn on his side or help himself to a drink, the impulse of his heart gone, the first sound inaudible, and the pulse quick and feeble; in short, in a condition of exhaustion. Such a person, I submit, is one in whom there is a very high degree of cardiac weakness. To put the case briefly, those severe cases in which there is a probability of a fatal result from bodily weakness and severity of disease are not cases in which the cold bath treatment is admissible.

It is true that cases such as I have described form but a small proportion of the whole number of cases of enteric fever; but it is also true that they are the only cases in which specific treatment of any sort is required.

The majority of cases recover of themselves; and, as regards their treatment, probably the less of it the better. These cases are cases whose temperatures fluctuate daily between 103° and 104°, rarely remaining any long time at the latter point; and to give them specific treatment is useless, for they recover of themselves, and are, in my opinion, best left alone. The remainder which die, are cases in which the prominent feature is cardiac weakness, cases whose temperatures fluctuate between 104°, 105°, and upwards; or cases with very low temperatures and extreme depression of the nervous system, due to some peculiar effect of the disease.

These are the cases for which treatment is needed; but I imagine it would occur to no one to give such cases—cases which cannot even be “changed” without suffering from exhaustion—a cold bath every two hours. Liebermeister expressly says that “a very high degree of heart-weakness is an important contraindication;” and, further, that many patients do not bear a very frequent use of the bath. Hence, taking together the cases in which the cold bath treatment is inadmissible from cardiac weakness, and the cases which do not bear it, we exclude from its operation all the cases in which it is desirable that active treatment should be used. The cold bath treatment, on the showing of its own supporters, then falls to the ground.

The treatment which we have been considering attracted attention by reason of the very favourable results which were said to be obtained by it. In some cases, the mortality was said to have been reduced to 3 or 4 per cent.; and, more recently, to nothing. Admitting that relapsing fever, the normal mortality of which is about 3 or 4 per cent., that the “typhus” of young people whose normal mortality is very low; that all febriculae, in which the mortality is nothing, were excluded; and, finally, that the enteric was enteric as we know it in London—points on which, by reason of the loose way in which the term “typhus” in Germany is applied to diseases perfectly distinct, I entertain grave doubts—I should still prefer, judging from my experience, to believe that nature of cases, not treatment, determined the favourable results referred to.

Whilst, however, I do not believe in the systematic and routine employment of the cold bath in enteric or any other fever, I do not say that it is of no use at all. On the contrary, as a means of cleanliness and comfort, it will be useful in the milder cases; it will sometimes relieve restlessness, and occasionally promote sleep; and I would not deny that, judiciously given, it might occasionally determine the change to recovery. In a word, largely varied to suit individual cases, it is an useful help in the treatment of enteric fever; but to say this is one thing, and to say that it has abolished death is quite another.

ON THE ANTIPYRETIC ACTION OF SALICIN AND OF THE SALICYLATES; ALSO OF KAIRIN.

Read in the Section of Pharmacology and Therapeutics at the Fifty-Second Annual Meeting of the British Medical Association.

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FROM the earliest ages of medicine, increased bodily heat has been recognised as the essential concomitant of the febrile condition; and its observation, even by the unaided sense of touch, has been employed as one of the most reliable modes of gauging the progress of fever, and of determining the effects of treatment. The use of instruments of precision for the estimation of the bodily temperature has given exactitude to what was previously only an approximation; and, with the aid of the clinical thermometer, we follow the rise and fall of the thermic wave with complete accuracy. This is particularly shown in the action of salicine, and of the salicylates in rheumatic fever; and it is in reference to this that I propose to consider them; for, although possessing a certain antipyretic power in pneumonia and in febrile diseases generally, they cannot be compared to quinine in this regard. Their action in rheumatic fever must be a satisfaction to every practitioner, for it would not be too much to say that they have removed this formidable malady from the list of *opprobria medicince*. In a celebrated discussion, held about a quarter of a century ago, an eminent physician expressed his belief that the only certain remedy for rheumatic fever was “patience, flannel, and six weeks,” and he would doubtless now be surprised to see the most formidable cases mastered in less than as many days; and this is to be accomplished by the free and frequent use of the sodium-salicylate, or, much better, of the natural alkaloid salicin itself. This latter I now altogether employ.

I have given up salicylic acid, on account of its sparing solubility; and if its sodium-salt be used in the large doses which are necessary to prompt and complete antipyretic effects, the most unpleasant results are liable to arise.

In Bartholow's *Materia Medica* (1882, p. 528), the dose of salicin is given at from 10 to 30 grains, and of the salicylate from 15 grains to a drachm. Dr. Mitchell Bruce, in his *Handbook* (1884, p. 332), recommends from 5 to 30 grains of salicin, and from 10 to 30 of salicylate; while Dr. Whitla, in the last edition of his admirable work (1884, p. 420), speaks of 15 grains of salicylate every two hours for 3, 4, or 6 doses, and this is undoubtedly a safe and useful line of treatment as far as it goes. Dr. MacLagan, in his work on *Rheumatism* (1881, p. 200), recommends from 20 to 40 grains of salicin or of salicylic acid, at first every hour, and subsequently at more distant intervals, according as the symptoms begin to subside.

From a very extensive experience, both of the natural alkaloid and of the artificial phenol derivative, it appears to me that there is no use in giving either in doses of 15 grains every third hour, and that the proper treatment of rheumatic fever is, having carefully noted the temperature, to begin boldly with from 30 to 50 grains of salicin, according to the severity of the symptoms, and to twice renew this dose at hourly intervals. If, at the end of three hours, the temperature be found reduced by at least one degree Fahrenheit, the dose may be persevered in for three hours more, when a reduction of from two to three degrees Fahrenheit will probably be found, with a corresponding improvement of symptoms.

If, on the contrary, at the end of the third hour the temperature should be found unaltered, or even increased, an addition of 10 grains should be made, and persevered in for two hours, at the end of which another 10 grains should be added if necessary; and, unless in some very exceptional case, the struggle must and will end in the victory of the alkaloid, and that without the slightest unpleasant symptom. I have several times given 50, or even 60, grains every hour, but this is seldom necessary. When I began early last year with these heroic doses, it was suggested to me that there might be a risk; and I accordingly proceeded to test what was the largest dose that could be borne without inconvenience.

Catherine C., aged 42, an averagely strong adult, was convalescent from acute rheumatism. She was up every afternoon, and was taking 30 grains of salicin daily, to prevent the relapse which is liable to occur if the drug be abruptly abandoned. On March 14th of last year, I commenced a daily increase of five grains; and on March 24th, she took an eighty-grain dose of salicin, and continued it four days without any apparent inconvenience. This experiment was interesting at the time; but I have since given healthy patients, who were not on a course of salicin, as much as 120 grains at a time, without any apparent result.

It has been asserted that cases of rheumatic fever treated by salicin, or by the salicylates, are more liable to cardiac metastatic troubles; but this is entirely contrary to my experience of 102 cases so treated, in which there were only five cardiac cases. One of these, Margaret D., aged 23, terminated fatally; but for this there were circumstances fully accounting. The other four made perfect recoveries; one of them, Henry McD., a cabdriver, aged 27, being of very great severity and persistency; and, in his case, it may be remarked that the salicin for the rheumatism, and the grey powder for the endo- and pericarditis, were given together. In any case in which there is the slightest want of tone or fullness in the systolic sound of the heart, I am in the habit of applying a large fly-blister over the whole cardiac region; and this I regard as a most important adjuvant to treatment, and a prevention to cardiac complication. Salicin is best taken on the empty stomach; and the handiest mode of administering it is either a capsule or wafer-paper. Suspended in milk, it is easily swallowed. Dr. MacLagan has remarked its tonic after-effects during convalescence; and this I can fully confirm.

No practical physician will for a moment doubt the antipyretic powers of salicylic acid, or of its more handy and soluble sodium-salt, in the treatment of acute rheumatism. To promptly master this disease, however, they must be given in hourly doses of from twenty to fifty grains; and I have seldom so employed them without producing nausea, vomiting, general depression, drumming of the ears, and, occasionally, temporary interference with sight or hearing. It is difficult to account physiologically for the marked difference in the clinical action of salicine and of the salicylates; and if we admit the hypothesis, generally received at present, that salicin is converted, in the system, into salicylic acid, the difficulty is proportionately increased. I see many objections to this view; and, without advancing any speculative opinion on an obscure point of therapeutical chemistry, would suggest the consideration whether both salicin and the salicy-