REMARKS

ON

THE USE OF THE COLD BATH IN
ENTERIC FEVER.

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A STUDY of the papers of Professor Liebermeister and others, on the use of the cold bath in the treatment of enteric fever, led me to try it on the cases which came under my care in the Homerton Fever Hospital. I have given, personally, one hundred baths, some in severe, others in moderately severe, cases, taking as my guide not the temperature, but that *plus* the patient; for in this relation, as in others, patients vary much; the temperature with which one man may be highly delirious, another bears with comparative equanimity. In the administration of our baths, then, we had regard less to the temperature than to the age, the state of heart, restlessness, and sleeplessness, and chiefly to the last. 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, taken *per rectum*, 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 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, for 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 natural termination is death; that is, in the cases in which specific treatment is needed. The depressing effect of the bath has been noticed by Professor 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 every probability of a fatal result from bodily weakness and severity of disease are not cases in which the "antipyretic 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 great 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 any specific treatment whatever is perfectly 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, and whose temperatures fluctuate between 104° , 105° , and upwards. 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 "antipyretic 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; and to employ remedies where they are not wanted is, to say the least, a work of supererogation. The "antipyretic 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. 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 febriculæ, 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.

The virulence of epidemics and endemics of enteric varies greatly; at one time nearly all the cases recover, at another time they nearly all die, irrespective of treatment. It will be the experience of every large fever hospital that, not infrequently, a succession of mild cases follow each other for weeks, and one might think that some particular plan of treatment was the cause, but that a succession of bad cases follow and the usual mortality is reached. When this does not occur, some are ready to attribute to treatment what is the result of nature. This error, arising from the varying virulence of epidemics, is not eliminated in the comparison which Professor Liebermeister makes between his "in-different", his "complete", and his "incomplete" antipyretic treatment. It does not follow that the cases of 1843 and of 1874 were like cases, and, until it be shown that they were, the comparison is inconclusive.

The antipyretic treatment of Professor Liebermeister, however, is less successful, tested by statistics, than Dr. Russell's at Glasgow, or my own at Homerton, the comparison of the mortality at Basle, Glasgow, and Homerton for the years 1873 and 1874—the only years in which I have the means of comparing the three places—appears to show this. Thus, of 163 cases in the year 1873 at Basle, 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 Germany 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.

"If fever depend upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin" (Sir T. Watson). I believe the expectant treatment to be the best. There are cases which recover and cases which die; and, in order that we may not prevent the one nor hasten the other, I think the safest course in the present state of our knowledge is to abstain from interference. "Opportunum medicamentum est opportune cibus datus" (Celsus); to which let us add general hygienic measures and good nursing, and I believe the treatment of

acute disease to be almost complete. An occasional opiate to relieve pain, a little wine to help a failing heart, a laxative to relieve constipation, will be useful adjuvants, if judiciously employed; but I do not think they will be anything more.

THE PREVENTION OF BLOOD-POISONING.

By S. MESSENGER BRADLEY, F.R.C.S.,
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MR. SAVORY's eloquent address shows that admirable results are attainable by the simplest forms of dressing, when directed by judgment and care, and is an argument that other methods than that of Lister merit the term of antisepticity. It is also valuable in impressing upon us the importance of the patient's general condition, as well as the state of his wound; and therein, to some extent, points the moral of the parable of the sower, in which the soil determines the character of crops: in some cases the seed, falling upon stony ground, it withers away; in others, falling by the way side, the birds of the air devour it; in others, falling upon a suitable soil, it brings forth (septicæmic) fruit a hundred fold. Doubtless the soil is a factor as important as the seed, and in much that Mr. Savory says there is unquestionable force and truth; but, is he wise in resting contented with such a weak barrier between soil and seed as that afforded by the granulation membrane? I cannot think so.

A chain which holds a mad dog may prevent him from biting; but chains are apt to snap, and his room is preferable to his company. While agreeing with Mr. Savory that antisepticity in surgery is obtainable without recourse to Listerism—as indeed I have long maintained—and while congratulating Mr. Savory and his colleagues on the admirable statistics which he quotes, I cannot agree that the employment of germicides is nought; that drainage-tubes are an abomination; and that the perfection of security, as well as simplicity, are to be sought in water-dressings and bread-poultices.

I will refer to one or two points in the nature of first principles in antisepticity.

First, as to the theory. Antiseptic surgery was inspired by a belief in the germ-theory, which maintains that putrefaction is caused by the presence of organisms (bacteria); their action probably being to pull down the higher and more unstable quaternary compounds, and reduce them to the simpler and more stable binary products. In this process, oxygen is absorbed and free ammonia evolved, and the nature of the change is further evidenced by a characteristic odour: hence it comes to pass that we have a triple test of the existence of putrefaction; firstly, the smell, and the nose is a very subtle chemist in this matter; secondly, the existence of organisms, revealed by the microscope; and, thirdly, the presence of free ammonia, demonstrated by Nessler's test.

The truth of the whole theory essentially depends upon the continued non-putrefaction of highly putrescible fluids, when once the germs present have been destroyed and access of fresh germs prevented. It does not at all signify whether or not the air is excluded; so long as it is filtered free from germs the fluids thus protected never putrefy.

In the second place, let us see what antisepticity in surgery aims at. Naturally, as Lister urges, not merely at preventing the access of these mischief-making organisms, but at destroying the germs of those already present. Consider the phenomenon of a putrid egg; how has putrefaction taken place in this instance? Manifestly by the development of the pre-existent germs, and not by the admission of germs from without; for, while "stony limits cannot hold *air* out" any more than *love*, it is not the air which works these changes, as proved over and over again; and even if germ-laden gases could penetrate the egg-shell, it is certain that bacteria would not pass through the membrana putaminis. In this instance as elsewhere death may take place, but without putrefaction. Boil the egg, and so destroy existing germs; and then, although it necessarily dies, it does not putrefy. It is true that chemical changes are involved in death, of a nature not unlike those taking place in putrefaction, but they are not identical, and therefore this is not to the point. Professor Lister seeks to secure the double object by the employment of the antiseptic spray and his well-known dressings, and certainly has conferred the greatest boon upon the surgical profession in all his work in this direction. Amongst other facts elicited during the progress of this work, it has been abundantly proved that there are no more prolific sources of infection than water and such articles as poultices; yet these are the very agents on which Mr. Savory so much relies. How, then, can we reconcile Mr. Savory's treatment with scientific surgery? True his statistics show admirable results; but are there not still better results forthcoming from the master Lister and his followers? I pause for a reply.

My belief is, and long has been, that Lister's principles are invulner-

able, and his practice excellent; but that the latter is unnecessarily complicated. I have argued that, scientifically the same results—by which I mean equally certain results—are to be obtained without the spray and without the employment of patent dressings; but I certainly should not look for these results if I used water-dressings and bread-poultices. To satisfactorily solve the question, some other things than even such statistics as Savory advanced are necessary; and among them are the presence or absence of putrefaction, as proved by the triple test I have mentioned, and the temperature of the patient given from day to day after the operation. I hope these points will be answered by Mr. Lister. In the meantime, I should like to be allowed to allude to three cases where I have employed with success similar dressings to those described in my paper, on Antiseptic Surgery, in the BRITISH MEDICAL JOURNAL for February 23rd, 1878.

The first is that of the organisation of a large blood-clot, which formed sequentially to removal of a loose cartilage from the knee-joint, and a report of which appeared in the JOURNAL, November 30th, 1878. The second is the case of a lad in whom I excised the entire spine of the scapula and head of the humerus, removing, at the same time, much granulation tissue. In this case, repair took place without rise of temperature and without putrefaction—the absence of the latter being rigorously tested by the means referred to above. The third is the case of a youth in whom I removed the os calcis, with similar results to those in the second case.

I mention these instances because they are pre-eminently the class of cases in which putrefaction is prone to occur; and, therefore, as tending to show that any treatment which averts such a catastrophe merits the term antiseptic.

Mr. Savory makes a bold, straightforward attempt to prove the same of his method. It remains to be seen whether he has established his position.

BLOOD-POISONING AND ANTISEPTICS.

By WILLIAM THOMSON, F.R.C.S.,
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I WAS one of the many who listened with deep attention and delight to the oration of Mr. Savory at Cork. The subject was skilfully chosen; and, although the orator's words were, as I thought then and think now, only the declaration of a section which clings to old habits and methods that are gradually being swept away, I could not but admire his address as eloquent and brilliant. But when I have said that Mr. Savory left unused no aid of the advocate's art to assail the position held by so many British surgeons, and to defend his own, I have said all that I can say in favour of it; for the result, I fear, will be to establish many in their hostility to antiseptics in the ordinary sense, and to turn back the timid who are on the threshold of conversion to more modern surgical ideas.

I am not about to enter into an elaborate criticism of that remarkable address; but there are some points in it which deserve observation. Mr. Savory is a believer in the germ-theory. He admits that there are such things as "agents of putrefaction" commonly called germs, which abound in the air, and notably in animal decomposing fluids. It is against the noxious influence of these that antiseptic surgery is directed. It is quite beside the question whether fluids potent for the gravest mischief may be produced within the body, "far away from any direct communication with the air". An abscess slowly forming in some remote region of the body is an entirely different thing from an amputation wound suddenly inflicted, or an incision into a knee-joint, or into the peritoneum. Lister's method is intended to prevent the access of these germs to freshly exposed wounded surfaces. It would be an excellent thing if we could also influence those distant collections of pestilential fluid; but, not being able to do so, is there any logical ground upon which we ought not, therefore, to limit the chances of blood-poisoning as much as possible by using an antiseptic spray? It is quite true that under the crudest system of dressing wounds we may see good results, and that a poisonous fluid may be in contact with a raw surface without producing bad effects. But now and then we see a case in which the "terrible transmission" to the blood takes place, and the patient is probably lost. Under what condition does this occur? Mr. Savory answers: "First of all, this mischief is least likely to happen in healthy wounds during the process of repair; and when it does occur, it is either in a wound unhealthy in character or flagging in action, or in one so recent, or in which the process of repair has been so delayed, that little or no new and healthy tissue has been produced on the surface. In other words, when the newly formed tissue is healthy and entire, it is tolerably impregnable, and whatever interferes with its integrity gives the opportunity for mischief. I say the mischief is less