door bodily labour to undergo, they are not so liable to feel the change as the officers." Was this in any degree an indication of scorbutic causes being so soon in operation in the *Alert*? In the *Alert* there was a case of incipient scurvy on January 8th, and another on April 8th, both in men who had not been employed in sledging; and consequently the scorbutic diathesis had developed in them on board the ship. When sledge-work commenced early in March, the first returned in four days with a frost-bitten man, in whom scurvy then developed itself on board, of which he died. The larger sledge-parties left on April 3rd; and on April 4th two scorbutic men were sent back to the ship, one of whom had failed in the first twenty-four hours out. Of the *Discovery's* men, one who had not sledged was taken with scorbutic symptoms on January 3rd; and that was the only case attributable to ship's influences in her, all her other cases being from the long sledge-party to North-west Greenland; and it was noted that the first man taken ill on it had passed the winter on board the *Alert*, and he gave in on the sixteenth day, with no other case till after a fortnight.

These facts indicate that the scorbutic diathesis had grown up in the winter—on the *Alert's* men at least; and that, on going with the sledges, they were less efficient than those of the *Discovery*. That that comparative inefficiency could have been due to any previous want of lime-juice, would not appear to have been the case, as through March a double allowance of it was issued to the *Alert's* men, and not to the *Discovery's* men. Lime-juice was not taken out by any of the extended parties; and they were without fresh animal food also, or, at most, having one or two meals of it during fifty-seven, seventy-three, eighty-one, and eighty-four days respectively; and the deaths occurred on the fifty-fourth, sixty-sixth, and eightieth days, in the first three periods, among forty persons who had travelled, in groups of four parties, across more than 1,800 miles of ice in eighty-six days; in which journeys, that of the *Alert* to the westward lost none; that from her towards the North Pole lost one; and that of the *Discovery* to the north-east lost two men, one of whom had wintered in the *Alert*, and was the earliest of all to fall a victim.

It has been generally admitted that the *Alert's* men, to say the least of it, were more predisposed than those of the *Discovery*, and it must be presumed that the causes of that predisposition were developed within the ship. Whether the differences in the ships' internal economy, alluded to, were the causes of this may be disputed; but it is scarcely tenable that a difference of four days in the duration of the sun's absence can have been sufficient; as, to have scurvy, of the most destructive character, was formerly the rule in the first winter in the comparatively low latitudes of Hudson's Bay, where the sun does not disappear; while there are instances of exemption from it in at least two instances of a winter passed in higher latitudes, even approaching to that of this expedition, viz., in Franz Josef Land and in Spitzbergen, in which the castaway men were compelled to dwell in huts on the shore, and to subsist in great degree, or entirely, on the products of the chase. In these instances, the great difference lay in circumstances compelling them to breathe a less confined, but at the same time, a less agreeable atmosphere with regard to temperature, throughout the winter; while in the spring they were not subjected to any of the extreme labour to which the *Alert's* and *Discovery's* men were subjected, with their thermometers marking below zero; they being then predisposed by the conditions incurred through dwelling between decks, where the air was artificially heated, and, consequently, the more damp. Sir George Nares relates that the difference of heat between the interior and external atmospheres was often 100° Fahrenheit, the former containing an abnormal quantity of carbonic acid, and other excreta of respiration and the skin in a crowded space. These I regard as very productive of that organic debility which precedes the appearance, in ships, of indubitable symptoms of scurvy, whether in the individual or in the community; and I consider that it may be also the cause of the frequent complication of lung-disease in scurvy. In huts buried in snow, and especially in those constructed of it, the moisture exhaled from the lungs is frozen to the walls, while, in a heated between-decks, it is breathed over and over again; and, when condensed on the walls, by admission of the purer external air, it is the constant cause of the most detrimental dampness so frequently complained of, and remarkably so in the *Alert* and *Discovery*.

Sir George Nares has given his opinion of this condition of heat and moisture, as experienced by him, being a very effective cause of the developed predisposition to scurvy.

(To be continued.)

THE Secretary of State for India has appointed Mr. David Hooper, F.C.S., of Birmingham, Analytical Chemist and Quinologist to the Nilgiri Government Cinchona Plantations, in the Madras Presidency.

ON THE ANTIPYRETIC TREATMENT OF TYPHOID FEVER.

By J. O. AFFLECK, M.D., F.R.C.P.E.,

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THE discussions at the London Medical Society on the antipyretic treatment of typhoid fever, in connection with the papers of Drs. Coupland, Bristowe, and Cayley, and the leading article in the BRITISH MEDICAL JOURNAL of March 1st, have reopened a subject of much interest and importance, which, it is to be hoped, may engage still wider professional attention. The matter is one deserving careful consideration, as bearing upon the successful management of one of the more common and serious of the infectious diseases; and statements of the case from various sources, and from other than one point of view, are desirable, with the object of determining the soundest and most rational principles and methods of treatment, as well as of estimating the advantage of any particular therapeutic measure.

That this disease owes some of its dangerous characters and damaging effects to the long continuance of pyrexia will hardly be disputed. Few who have observed a severe case of enteric fever going on continuously for four or five weeks, and thereafter followed by a succession of relapses extending over another period nearly as long, can doubt that the patient is in imminent danger of sinking from sheer exhaustion and wasting of his every tissue, notwithstanding it must be allowed that fever-temperature is very differently borne by different individuals. But mere tissue-metabolism cannot be regarded as by any means the sole source of danger. The toxic action of the fever-poison, particularly on the nervous system, the local lesions in the intestines, and their results, not merely in the form of catarrhal discharges, but in structural changes and atrophies which render the assimilative processes comparatively inert, must be held to form a large part, if they do not, indeed, constitute the main part, of the morbid process. In the therapeutical, no less than in the pathological and clinical, survey of the disease, these various factors must be accorded their due prominence.

The treatment of typhoid fever has long been a topic of interest; and whatever is added to our information, or tends to shed light, upon the subject of the rational therapeutics of this disease, cannot fail to be welcomed by all, particularly by those who are specially concerned in the treatment of large numbers of fever-patients in hospitals. Much of our present knowledge of the matter is due to the labours of Gairdner, of Jenner, of Murchison, of Cayley, and of many others; and to the methods of treatment advocated by them must be ascribed a diminished mortality-rate in this disease. The dieting and regimen of the patient, and the recognition and treatment of complications, have all been placed upon a sound basis.

Since the introduction of the clinical thermometer, which has enabled the amount of fever-temperature to be accurately measured, and the course and progress of the disease methodically traced, attention has been turned in an especial manner to the treatment of the pyrexia, with the view of lessening its amount, or, at least, of breaking its continuity. For this purpose, various antipyretic drugs of known potency (quinine, salicylate of soda, salicin, kairin, etc.) have been turned to account, and they undoubtedly, in many instances, succeed in bringing down for a time a high degree of temperature. To most of them, however, there exists the objection that their continuance, upon which, indeed, their efficacy depends, is often ill borne by the digestive organs, and in some cases they appear to be rather hurtful than otherwise.

The modern employment of the cold bath or cold bathing of the body in fever appears to have been first resorted to in Germany by Liebermeister, Brand, and others; and this form of antipyretic treatment has hitherto been more in use on the Continent than in this country. Of late, however, it would seem to have been making its way among us as a therapeutic agent, and the discussion already referred to will probably lead to a wider use of this method of treatment than has hitherto prevailed. In this view of the matter, it might be not unprofitable to inquire a little into the alleged advantages of this system; to consider whether it actually accomplishes all that some of its advocates claim for it; and, further, to see whether or not there exist any serious objections to its general employment.

The advantages are briefly stated by Dr. Cayley to be the following (BRITISH MEDICAL JOURNAL, March 1st, 1884).

1. It fulfils the physiological indications that, by keeping down the temperature, the febrile metabolism of the tissues, and the accumulation in the blood of the products of this metabolism—and to this

Dr. Murchison attributed most of the symptoms of the typhoid state—are much diminished. The granular infiltration and softening of the central nervous system, of the heart, of the liver, of the kidneys, of the voluntary muscles, is, to a great extent, prevented; and, by the stimulating effect of the treatment on the vaso-motor system, that want of tone and general relaxation of the arteries, which is a marked feature of the disease, is counteracted.

2. Where efficiently applied, the treatment produces marked alleviation of the symptoms. When the temperature is kept down, the so-called typhoid symptoms are seldom seen, etc.

3. Complications are rendered neither more frequent nor more severe.

4. The rate of mortality is greatly diminished.

With reference to the first of these points, it may be observed that the mere height of the temperature, and its effects on tissue-metabolism, will not satisfactorily account for the peculiar symptoms of typhoid fever. In fact, the temperature in typhoid is not, in the majority of instances, very high; the type of the fever is somewhat remittent, and cases of hyperpyrexia are certainly rare in this disease. In not a few acute maladies, *e.g.*, rheumatism and catarrhal pneumonia, we find a temperature of higher amount, with less remission, and nearly, or quite as long, duration, as that of enteric fever, and where yet anything like typhoid symptoms are not in general present. There is undoubtedly more than the mere temperature required to account for the remarkable prostration of the muscular and nervous systems conspicuous in enteric fever, and equally so in the shorter fever of typhus.

A septic process is in operation throughout the course of these diseases, of which the temperature is but one of the manifestations, and not always, *per se*, an accurate or reliable one, as regards our estimation of the severity of a case. But, in addition, in enteric fever there is a distinct visceral lesion, which obviously bears some relation to the temperature, the latter being apparently influenced, in no small degree, by the continuance of the ulcerative process.

It can scarcely be contended that the mere combating of the febrile action can counteract either of these great factors in the disease. Whatever antipyretic remedies may do in the way of lowering a high degree of temperature, it is not clear that they have any effect in shortening the progress of a case, nor in preventing or curing the intestinal lesions.

Although I have had but a limited experience in the treatment by the cold bath or cold sponging, I have made extensive use of antipyretic drugs, more especially quinine; and while willingly acknowledging its value in breaking the continuity of a course of high temperature (and no doubt a like effect would follow the cold bath in such circumstances), I have not found it in any way cut short an attack, but rather the reverse, the cases so treated often proving unusually prolonged; and although, of course, this might be due to their severity. A different result could scarcely be expected from a line of treatment specially directed to a mere symptom, and incapable of influencing the still existing cause underlying all the phenomena.

As regards the second point referred to by Dr. Cayley, *viz.*, that, where the cold bath treatment is efficiently applied, it produces marked alleviation of the symptoms, it seems only necessary to remark that the efficient application of this method is limited by requirements which appear to render its accomplishment impracticable in a very considerable proportion of cases. Thus, it seems to be essential to the success of the treatment that it be resorted to from the very first, and that should the first few days have elapsed before it is put in practice, the hope of benefit is materially lessened. To say nothing of the difficulty, even to men of large experience, of recognising typhoid fever "from the first," it is well known that at least in hospital-practice most of the cases have gone on for a good many days before they come under the cognisance of the physician; when, however faithfully the antipyretic (cold bath) treatment might be carried out, the result would be failure, so far as this method is concerned, in a large proportion of instances. Further, even its advocates would appear to discountenance, or at least to recommend great caution in, the adoption of the cold bath treatment where severe local lesions are indicated, although it is just in such cases that the temperature is often highest, and consequently antipyretic action is most called for.

Thirdly, as to the influence of this method of treatment upon the complications of typhoid fever, it is extremely doubtful whether the proof is sufficient to warrant the strong assertion that pulmonary diseases are rendered much less frequent, in view of testimony of a directly contrary character; while as regards intestinal lesions there is evidence which cannot be explained away, that the liability to these is somewhat increased.

With reference to the last point—a greatly diminished mortality-

rate—it must be allowed that here too the proof is inconclusive. In few diseases is the numerical test less reliable than in typhoid fever. In the first place, the diagnosis is in not a few cases a matter of considerable uncertainty, even throughout the whole course of the disease; so that, taking hospital-experience as affording the most extensive field for statistical information, it must always be a question how far the numbers represent absolutely cases of typhoid, and may not include other of the many morbid conditions which are known to simulate it. Nevertheless, as Dr. Cayley justly remarks, "a sufficient number of instances, spread over a sufficiently long time, must be collected, so as to exclude accidental sources of error; and like instances only must be compared." Judging, however, even by this standard, it is doubtful whether much has been gained as regards the mortality-rate of typhoid by the antipyretic method of treatment. It must not be forgotten that recent years have witnessed a vast improvement in the management of fever-cases from that which used to prevail. Our nursing, our dieting, our general hygiene, and our medical treatment of fever-patients have all made marked advance, even since Dr. Murchison wrote his famous work; and this fact must, I am persuaded, be credited with much of the diminished death-rate. Notwithstanding Jaccoud's assertion that "while the mortality in typhoid under the old method of treatment was about 19 per cent., under the antipyretic mode it is under 11 per cent.," evidence is not difficult to obtain tending to cast doubt upon the justness of his ascribing this diminution solely to the cause assigned. Thus I have, with the aid of Dr. Wood, the resident in the fever-hospital of the Edinburgh Royal Infirmary, of the typhoid wards of which I have the charge, collected the mortality-statistics of cases treated there during the past four years, where certainly the antipyretic method has not been carried out, but where the greatest attention is paid to the nursing, dieting, and watching for and treating complications, and the following is the result, the details of each year showing remarkable uniformity:

Year.	No. of Cases.	No. of Deaths.	No. of these Deaths occurring within 48 Hours after Admission.
1880	106	12	1
1881	91	12	3
1882	200	23	5
1883	112	13	1
	509	60	10

Thus, under a non-antipyretic treatment the total mortality was not more than 12 per cent., and if cases moribund when admitted, and dying in less than forty-eight hours, be excluded from the list, as they ought to be, the rate is reduced below 10 per cent. This number, too, includes a considerable proportion of cases brought long distances from the country, which, as Dr. Murchison and Sir W. Jenner both point out, often prove to be serious, obviously from the hurtful effects of a long journey. I doubt not that other hospitals not practising the antipyretic method could show a still smaller mortality; but, as already mentioned, it is not desirable that too much should be made of mere death-rate experience; for, unquestionably, the mortality in typhoid fever varies greatly in epidemics, and in relation to the character of the cases under treatment at any given time.

The objections to the cold bath treatment of typhoid fever need not be enlarged upon. Only a few of the more important can now be referred to. One of the most serious is the necessity which the method entails of frequent disturbance of the patient, which, in this fever, is ill borne, and may even be dangerous.

Everyone who treats this disease must notice how trying to the strength are even the most careful movements of the patient when diarrhoea is present, or when the case is a severe and protracted one; and to add to this the fatigue involved in the cold bath treatment, would be to seriously increase the already existing risks. Yet these are the very instances in which fever often tends to persist. Keeping in view the fact that a progressive bowel-lesion is present from the beginning, it can scarcely be doubted, that the less the movement of the patient, the better for his safety. Further, the depressing effect which the treatment is apt to have upon the circulation, renders it unsuitable in a large number of severe cases. This tendency was acknowledged by Liebermeister, and such experience as that of Dr. Collie, at the Homerton Fever Hospital (BRITISH MEDICAL JOURNAL, September 20th, 1879) is abundant evidence that it may be a real source of danger. Lastly, the risk of intestinal congestion, hæmorrhage, and other complications, by the action of the cold in driving the blood from the surface, would appear to be by no means a theoretical one.

In offering these criticisms and stating these objections, I am far from holding that the antipyretic (cold bath) treatment has no place in the therapeutics of enteric fever; but I contend that the limit of its

applicability is a comparatively narrow one, and that, until much more extensive and satisfactory evidence of its advantages is forthcoming, it is not to be recommended as a general principle of treatment to be pursued in this disease.

At the same time, there may be conditions present, in exceptional instances, which are capable of being materially alleviated by antipyretic treatment.

In ordinary cases of the disease, with moderately high temperature and a distressing sense of heat, together with restlessness and sleeplessness, the cold bath, cold sponging of the body, or cold applications to the head for a time, will often, even at any stage of the disease, lower the temperature and afford relief to the other symptoms; but apart from this, it is not to be expected that they can have much influence on the course or duration of the fever; and, moreover, most of such ordinary cases do perfectly well if carefully dieted and nursed. Again, in a protracted case, with continuous high temperature, and, it may be, evidence of severe intestinal lesions (diarrhoea, etc.), some antipyretic drug, such as quinine, is a safer plan of treatment for lessening the febrile action than the external application of cold; yet should this medicine fail, it might be justifiable to resort to the latter plan, in the form, however, of cold sponging rather than the bath. Still, such treatment, even if successful for the purpose, will probably have but little power to control the progress, or the ultimate issue, of the case. It is to be noted, however, that in neither of the above-named instances would advocates of the antipyretic system admit that it had been properly carried out, seeing that it had not been begun early and persevered in all along.

Lastly, in the rare event of hyperpyrexia, the cold bath should be resorted to, just as in acute rheumatism or other malady in which this formidable symptom may arise.

These observations are submitted as an humble contribution to the discussion of a subject upon which diversity of opinion exists; and as an exposition of my reasons, founded on considerable opportunities for observing the phases of this disease, for declining to adopt as a principle of treatment a practice which, notwithstanding the strong advocacy it receives from eminent authority, has not, in my view, been proved to possess the advantages claimed for it as a method of rational therapeutics. It is not necessary, in order to an estimation of its value, that it should have first been tried, for *a priori* reasons and observations may be quite sufficient to furnish arguments against any system of practice which may entail avoidable risks, and for the alleged benefits of which other considerations may be assigned.

It does not fall within the scope of this paper to deal with other measures of treatment in typhoid fever, but I cannot conclude without quoting some weighty sentences from the memorable address on this subject of Sir William Jenner, delivered before the Midland Medical Society, on November 4th, 1879. "Typhoid fever cannot be cured; but more lives may be saved by the judicious treatment, and more lives lost by the improper treatment, of typhoid fever than of any other acute disease. For a very large proportion of cases no other treatment is really required from beginning to end than rest in bed, quietude, fresh air, pure water, and regulated diet, although most cases are benefited by a little wine in the third or fourth weeks. . . . Temperature so high and continuous as to be a cause of danger, either directly, or indirectly by favouring serious degenerative changes of structure, is present in exceptional cases only, and for such cases alone is the direct application of cold to the general surface required. . . . My experience has impressed on me the conviction that that man will be the most successful in treating typhoid fever who watches its progress, not only with the most skilled and intelligent, but also with the most constant care, and gives unceasing attention to little things; and who, when prescribing an active remedy, weighs with the greatest accuracy the good intended to be effected, against the evil the prescription may inflict, and then, if the possible evil be death, and the probable good short of the saving of life, holds his hand. While admitting, without reserve, that heroic measures fearlessly but judiciously employed will save life when less potent means are useless, the physician whose experience reaches over many years will, on looking back, discover that year by year he has seen fewer cases requiring heroic remedies, and more cases in which the unaided powers of nature alone suffice for effecting cure; that year by year he has learned to regard with greater diffidence his own powers, and to trust with greater confidence in those of nature."

THE Wandsworth and Clapham Guardians have, upon the recommendation of a committee, added £40 *per annum* to the salary of Dr. Field Flowers Sutton, Medical Officer for the Streatham district, in consideration of his arranging to have five dispensing stations, at various points within the district.

ANEURYSM OF THE POPLITEAL ARTERY: CURE BY DIGITAL COMPRESSION IN SIX HOURS AND A QUARTER, AFTER FAILURE OF INSTRUMENTAL COMPRESSION AND LIGATURE.

By ARTHUR E. J. BARKER, F.R.C.S. ENG.,

Assistant-Professor of Clinical Surgery at University College, and Assistant-Surgeon to University College Hospital.

IN the BRITISH MEDICAL JOURNAL of June 30th, 1883, I recorded a case of popliteal aneurysm, cured in about eight hours and a half by digital compression; and to this I may now add another, with an equally favourable result, after a still shorter treatment. Besides affording encouragement in the adoption of a mode of treatment for this disease, to which we are, perhaps, becoming a little too cool of late, the case offers some special points of interest. In the first place, the patient had been subjected to instrumental compression for five weeks before he came under my care; then, almost immediately afterwards, I ligatured the femoral artery in Scarpa's triangle with a twisted silk ligature, entirely obliterating the vessel at that point up to the present, and apparently curing the aneurysm; then, four months later, pulsation appeared again in the tumour, which became as large as before, but was this time cured finally, by digital compression of the common femoral artery, in about six hours and a quarter.

The case is briefly as follows.

J. D., aged 36, was admitted into University College Hospital, under my care, on August 29th, 1883, suffering from an aneurysm of the right popliteal artery. For this he had been already carefully treated, by Mr. Gandy of Norwood, with a Skey's tourniquet, applied almost continuously for five weeks. This compression had had no effect upon the tumour. The patient was a particularly healthy, fresh-looking, cheerful man, whose personal and family-history were excellent, and showed no evidence of constitutional disease of any kind. He had always been a gardener, working for the last eleven years in a very hilly garden, and doing all the work (which was very heavy) himself. This overstrain appears to have been the only exciting cause for the aneurysm in this case.

The appearance of the tumour dated from eight weeks before admission, when he first noticed pulsation in the ham. He was unaware of any special strain or other cause for it, and it gave him at the time no pain. Its size had remained the same since first observed. On admission, the swelling was of flattened oval shape, about two inches in diameter; it was tense and elastic, and pulsated strongly. It was seated exactly opposite the middle of the knee-joint, and was slightly red on the surface, having a distended vein on its outer side. There was aching pain on flexing the leg, but none when the limb was at rest in extension; some tenderness on pressure on the tumour was complained of, but none in the thigh or leg. Pressure on the superficial femoral artery arrested all pulsation in the sac.

Instrumental compression having failed, and the man being extremely anxious that something radical should be done, I ligatured the superficial femoral artery in Scarpa's triangle on September 6th, 1883. The operation was done in the usual way under spray, and the vessel was tied with a twisted silk ligature well carbolsed, which was cut short and left in the wound. The first ligature broke in drawing the second half of the knot; the next piece of silk bore the strain well, and was placed a quarter of an inch above the first spot chosen. The pulsation in the aneurysm was now found to be completely controlled, and no pulsation was felt in it until about five hours later, when it was just perceptible. The tumour gradually shrank, while over it a small artery could be easily felt. The wound healed, without any trouble of any kind, by first intention throughout, the ligature showing no signs of coming away. The patient left hospital on October 1st, looking and feeling very well. At this time there was no pulsation to be felt in the tibial arteries, and no discomfort or pain anywhere. In this condition the patient remained at home until the second week in January, 1884 (about four months). He then noticed a return of pulsation in the right popliteal space, with pain in the knee as this gradually increased. A week later he came up to see me, when I found the aneurysm almost, if not quite, as large as before the ligature of the femoral artery, although the latter, below the seat of ligation, was now pulseless, as were also the tibials. Above the ligature the vessel pulsated strongly. Pressure on the common femoral, below Poupart's ligament, completely controlled the expansile stroke in the aneurysm, and from this there could be no doubt that it was fed by branches of the profunda, which had been presumably enlarged during the five weeks of instrumental compression of the common femoral, which had preceded ligature. Of course here