INTRODUCTORY ADDRESS ON THE HEAT OF FEVER.

*Delivered at the meeting of the Medical Society of London, Monday,
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GENTLEMEN,—My first and most pleasing duty is to offer you a hearty welcome on our reassembling in this room after the summer recess. My next thought is, I must confess, of how we may turn the coming session to best use—of how much mutual brain-picking we may be capable, to the advantage of all, even of the sufferers. But, in logical order, comes before this some looking back upon work already done, particularly the work of the last session. It is hardly necessary to remind you that the intellectual activity of our last session was most vigorous, particularly in that part of it which was passed under the able guidance of my large-hearted and able predecessor in this office. We may remember particularly what dignity was brought to our annual *conversazione* by the remarkable essay on Old Age and changes incidental to it, which formed the subject-matter of Dr. Humphry's oration. And our financial retrospect is not less satisfactory. We may congratulate ourselves that we occupy these new rooms free from sense of indebtedness to anyone outside the Society. The financial genius of Mr. Goodsall has brought it about that all that we owed in respect of the alterations in our building should be covered by debentures subscribed for entirely by our Fellows. If we are satisfied with such progress as has been made, we must be impelled to maintain it; and, in order to facilitate that free communication of knowledge and thought which is the true object of societies such as this, our Council has determined that, in the coming session, a whilome custom, in desuetude for many years, shall be revived. Certain evenings will be set apart, as our programme indicates, for purposes of clinical discussion—of discussion of the cases of patients brought here for inspection and examination. November 9th has been chosen as the first evening for use of this kind; but on November 2nd our meeting will have an allied character. Dr. Gowers has kindly consented to open, on that evening, a discussion on the Clinical Value of the Deep Reflexes. This is a point on which my own experience leads me to believe that we can gain most valuable instruction by comparison of our various observations. I trust that our Fellows will come to our aid in large numbers on that evening, and contribute to some effective determination of the indications which may be drawn from these easily observed signs.

Another part of our proceedings, to which we shall all look forward with certain anticipation of receiving valuable instruction, will be found in the Lettsomian Lectures, this year to be delivered by Mr. Hutchinson.

The volume of our *Proceedings* which I hold in my hand has essentially a retrospective aspect, but it leads me also to speak of the session to come. The volume contains only papers read during the past session, the discussions not being included. The Council, after much deliberation, decided to omit the publication of our discussions until measures should have been taken to ensure fuller and more satisfactory reports of them than have been hitherto available. Arrangements will be made for full reporting of discussions for the future, and it is hoped that the inclusion of them will add great value to our next volume.

And now rises the somewhat of bitterness which runs through all human things. Death has run his line through the names of several of our Fellows since last October. There are so many the less to join in the effort to advance medical knowledge, to join in the friendly intercourse and high aspirations of this Society. I will not ask your forgiveness if I say some few words which may be put on record touching our lost friends.

Daniel Noble, M.A., M.D., F.R.C.P., joined this Society in 1868, and died in January last at the age of 75. Born at Preston, he came to London for medical study at the Borough School and Guy's Hospital. He practised in Manchester, and there attained a position of eminence. He was a man of wide learning, and of good organising power, so that he took a leading part in combating a visitation of

typhus in 1847, and in preparing to resist a dreaded invasion of cholera in 1866. He became M.D. of St. Andrew's in 1853; Honorary M.A. in 1860; and F.R.C.P. Lond. in 1867. He published the following works. *The Influence of Manufactures upon Health and Life*; *The Brain and Its Physiology*; *Elements of Psychological Medicine*; *The Human Mind in Its Relation with the Brain and Nervous System*; *Mesmerism True, Mesmerism False*; *Epidemic Fever of 1847*; *Cerebro-spinal Concussion, with Illustrative Cases*.

William Martin Coates, F.R.C.S., Surgeon to the Salisbury Infirmary, died on March 28th of this year. A student of St. Bartholomew's Hospital, he took his London diplomas in 1832-3. He was subsequently, for a time, teacher of anatomy and midwifery at the Ecole Pratique de Médecine at Paris. He was, for forty years afterwards, Surgeon to the Salisbury Infirmary, an appointment which he resigned shortly before his death. He is recorded to have, as a surgeon, manifested a remarkably keen power of diagnosis, and to have been a bold and rapid operator. He was the author of several works, namely, *The Nature and Treatment of Talipes Varus*; *Chloroform and Its Safe Administration*; *Treatment of Bronchocele and of Enlarged Glands by Injection of Iodine*; *On Puerperal Fever*; *On Listerism*; and an Address delivered by him as President of the Section of Surgery, British Medical Association, Ryde, 1881, on the *Operative Treatment of Haemorrhoids*. He continued to practise till within a few months of his death, which occurred at the age of 73; and he leaves three sons qualified in the profession, a fourth in process of qualification. He had been a Fellow of this Society from the year 1869, and was, at the time of his death, a member of the Committee of Referees.

In Joseph Bunny, M.D., of Newbury, a very old member of our profession passed away. His medical education was carried on in Edinburgh, at Guy's, and at St. Thomas's. After taking his London diplomas in 1820, he took his degree at Edinburgh in 1823, and then commenced practice in Newbury. There he was for ten years surgeon, for thirty-two subsequent years consulting surgeon, to the dispensary. He was coroner from 1839 to 1873, and was a Justice of the Peace. In him, Newbury is said to have lost one of its oldest and most respected inhabitants. He became a Fellow of this Society in 1873, and died last May at the age of 86. Truly a sort of man making important composure of the backbone of country life, and of this Society.

In John Gay, who died this autumn, on September 15th, this Society has lost a very typical Fellow. Looking backward over the record of his life and work, remembering the gentle friendliness and the absence of self-assertion which characterised him, recognising his love of investigation and his sense of scientific truth, one cannot help feeling that, so far, at least, full justice has not been done to his deservings. He was in harness, with his neck well in the collar, from 1833 to 1883. At the outset of his career, he was the most distinguished student of his year in the then and now great school of St. Bartholomew's. Fifty years later, he read before this Society a paper on Certain Points connected with the Anatomy of the Venous System. In the time intervening, his studies and occupations were various. He wrote many papers, most of which were strongly impressed by original thought and bold induction. Few of them, therefore, failed to excite vigorous debate. Such were his papers on Femoral Rupture, on Indolent Ulcers, on Varicose Disease and its Allied Disorders. He filled many offices. He was elected, in 1836, to be Surgeon to the Royal Free Hospital, and held the office till 1853. Later on, he became Surgeon to the Great Northern Hospital. In 1869 he was elected on the Council of the Royal College of Surgeons; and, besides these, he held, at various times, many public appointments of various importance. He joined this Society in 1849, was treasurer for three years from 1867, and was President in 1870. His last publication was, as we have seen, read here in 1883; but he had also been Orator, in 1860; and had, in 1867, delivered the Lettsomian Lectures, taking as his subject Varicose Disease and Ulcers of the Lower Extremities. His work may speak for him, and to our pride. It will not be forgotten, will probably be better valued as time goes on. But we may, at all events, keep well in mind his genial personality. If he worked, if his eye was always ready to catch some new sight of truth, he was also a thoroughly genial president, companion, and friend. As a companion, I may speak of him from experience. In many a country walk I have known him make the way easy, with pleasant and always kindly talk. I never heard him say an ill word touching anyone, and I have found him alert to see and enjoy with me whatever of interest nature placed to meet us by the wayside.

Of such men I would believe that our Society is made—of large-hearted men, who are content to go on working patiently and faithfully, in the determination to know, as far as may be, and to be useful as far as may be, and, withal, not to be ever feverish in urging their claims to recognition on the passing generation.

Edwin Canton, another past president of this society, presents for us a strong contrast to John Gay. He was a man of exceptional brilliance in his profession and outside it. In John Gay, we have seen the man of great social attraction from the domestic side, the man attaining reputation, almost insensibly, by the patient unravelling of knots, which he had set himself to loosen. In Edwin Canton we see the man who was easily first in the lines wherein he sought distinction, and who failed to attain great reputation; I think, for want of earnestness and steadfastness of purpose. Born in 1817, educated at King's College, and afterwards at Charing Cross Hospital, he became Assistant-Surgeon to the latter in 1854, full Surgeon in the next year. Before receiving these appointments, he had been for several years a teacher in the school, as Lecturer on Surgery and Surgical Anatomy. It is recorded of him that, at Charing Cross Hospital, he distinguished himself by excelling in every duty which he undertook. He was brilliant as a teacher and lecturer, brilliant as an anatomist, brilliant as a surgeon, combining readiness with caution, showing deep insight and broad view of the causations of disease, and, in operating, never losing the fruit of his anatomical skill. He was also brilliant as a writer. This power took him, perhaps, too often outside the literature of his profession, yet, when he thus stepped outside, into journalistic or other exercise of the pen, he was still brilliant among his compeers. He has left his mark in *Punch*, and in other weekly journals. His social powers were great, but scarcely of the domestic kind. He loved to mix in society, and was prominent therein, during his heyday. On the one side he enjoyed scientific society, on the other theatrical. And he seems to have been very much at home in both.

His medical writings were not numerous. Perhaps the most remarkable is his paper on the *Arcus Senilis*. This, indeed, is a classical paper, worthy of the brilliant author.

Besides these, I may record, first, an illustrated work, entitled *Surgical and Pathological Observations*; secondly, *Notes on the Morbid Anatomy of Chronic Rheumatic Arthritis of the Shoulder and Other Joints* (illustrated); thirdly, *Remarks on Interstitial Absorption of the Neck of the Femur from Bruise of the Hip*. These notes, as I quote from an obituary notice, represented some of the best pathological work ever done by their author; they were based upon careful examination of specimens which he afterwards presented partly to the museum of the Royal College of Surgeons, partly to the museum of his own medical school.

He held many offices in this Society, and was President in 1863. He gained the Fothergillian Prize for an Essay on Injuries and Diseases of the Spine, which is spoken of as one of the best papers ever sent in for the prize. The date is already far back; it is 1857. I have not seen the paper, but am told it is of such merit that its publication might even now be useful to the Society and the profession.

Mr. Canton retired from hospital-work in 1878, and died this autumn, in the 68th year of his age.

When I pass from these relations, in which sorrow and pride are so intimately mingled, I feel that what I should say is not so clearly laid down for me. I will dare, therefore, to choose for myself, and say a few words to set you thinking, a few words touching a difficulty which occurs to me constantly in the study of disease. We all now use the clinical thermometer. We all, from day to day, see it indicating, in various cases, a heat of the body rising to various degrees above the average, with, as our experience tells us, indications increasing in gravity in some proportion to the ascent of the mercury. We know again that, for the most part, the rise of temperature coincides with the establishment of the process of fever. Now, this increased heat of the body in fever is to me a very constant stimulant of thought. When I ask people how it comes about, I am generally told that it is simply a matter of increase of combustion; that the oxidation-processes of the body go on with undue vigour in fever; that the system is burning its candle at both ends, and that the two flames give more heat than one. When one looks at a patient who has passed through a febrile illness, one is ready to accept the explanation. He may have had no wasting discharge, hæmorrhage, or other obvious drain; yet there he lies, bloodless and emaciated, to a degree which leads one readily to believe that on his bed of fever he has been consumed in all his tissues by an unseen fire.

But, for some years, my acceptance of this ready and most plausible way of accounting for the phenomenon has been hindered by an attentive consideration of an article on the Process of Fever, contributed by Dr. Burdon Sanderson to the Reports of the Medical Officer of the Privy Council, for the year 1875. The article contains an exhaustive notice of the best observations made, up to that time, with reference to heat-production in the body during pyrexia. Now, I am

giving an address, and not reading a paper; I am, therefore, speaking to suggest a subject and line of thought, and not, as you will see afterwards, to go on to set before you conclusions which I hold that I can prove. I refrain from recapitulating the complex and very refined data upon which Dr. Burdon Sanderson sums up impressively. Suffice it to say that, after careful analysis of these data, he writes thus: "The general conclusion to which the preceding calculation leads us is a very important one, namely, that, although as compared with the heat-production of an individual on fever-diet, the heat-production of a febrile person is excessive, it is not by any means greater than the heat-production of health." There is, in fever, it must be admitted, increased exhalation of carbonic acid, increased excretion of urea, but, after calculation, they do not represent a source of heat sufficient to cause the increased temperature of the body. I have read the article again and again, I have referred to various authorities on the subject, and I am compelled to say that the increased combustion explanation, which satisfied me before, has no longer the same value. To what, then, as I felt obliged to lose faith in my first belief, should I turn? Might, as some have argued, the increase of heat in the body be brought about by retention, by some state of the surface which would prevent the liberation of heat from the body and lead to accumulation within? The well conducted observations of Leyden and Liebermeister tend to show that, far from being retained, heat is discharged from the surface in larger quantities during fever than in health. And we all know that intense hyperpyrexia constantly co-exists with profuse sweating, involving the freest possible discharge of heat from the surface of the body, as in severe cases of acute rheumatism. If we are bound to deny the cogency of the two explanations, we are impelled to find a new one. And in illustration of another possibility to which I would draw your attention, let me use an illustration.

Let us suppose that we place over a flame a metal basin containing water, and let the water come to the boil. Let us place a thermometer in the water. So long as there is water left, the thermometer never rises above 212° Fahr., let the boiling be ever so fierce. The evaporation always compensates for the heat introduced. This is, in a way, a parallel to the marvellous heat-regulation of the body, which maintains it at a fixed temperature whatever be the heat or cold of the surrounding air. The heat introduced from the flame is used up in turning water into steam, and raising the steam. Let oil be poured on to the surface of the water, and hinder the extrication of vapour. Heat is then also retained and the temperature rises in the water, as it would in the system on the retention-hypothesis. On the other hand, let the water evaporate entirely. Then the metal basin becomes heated indefinitely, and the thermometer rises in proportion. The water had afforded to the heat the means of further usefulness in which it—to use an old term—became latent, or took another form of energy.

Now, what I am going to ask you to think about is this. Is it possible that the increased heat of fever may be brought about by the cessation of processes in which heat ought to be used up—either as motion, or chemical action, or other kind of energy; so that the processes which may be represented by the boiling water ceasing to exist, like the water when boiled away, the heat generated for maintenance of the process overflows, and warms to excess the body, like the metal basin from which the water has evaporated? Is the increment of heat of body in fever due not only to combustion or other disintegrative process thereto allied, but also to the persistence, in the form of heat, of energy which should have taken another form? This appears to me in a high degree probable. Throughout the body, we recognise two processes ever going on: the building up of tissues on the one hand, their disintegration on the other. The disintegration of tissues is clearly attended by the liberation of heat. Their upbuilding presents itself to me as necessarily attended by the consumption or disappearance of heat, which assumes some other form of energy, kinetic or potential. There is here suggested to me the contrast between evaporation and condensation, between solution and crystallisation. You will ask me if I have any knowledge of experiments demonstrating the using up of heat in the tissue-formation of animal bodies. I have none. So far as I know, all the processes which have been examined have proved to be heat-evolving, even to the formation of peptones. Also, so far as I know, no processes of tissue-building have been investigated from this point of view, and it is difficult to see how, with our present means, any such process comes within the possibility of investigation. But if we have no direct evidence in this matter to help us, we may gain some help from a consideration of the chemical processes of fever. These comprehend, in the first place, an exaggeration of the combustions of health. But they also comprehend changes which exactly reverse those of health, and indicate strongly that there are, first, a cessation of changes which should occur in health; and,

secondly, a production of changes not occurring in health. The proportions of soda and potash which should be eliminated from the body in health are reversed in fever. The same holds of chlorides and phosphates. The potash and the phosphates are the associates of the highly organised principles; the chlorides and the soda of the introduced and further organisable principles. On the view that there is, in fever, arrest or default of the building up of the tissues, we can imagine the retained chlorides and soda waiting with the organic substances on promotion, like salmon at the foot of a fall, till at the end of fever they part with their associate organic matters, and pass them on to the elevating influence of the potash and phosphates. We can imagine the potash and phosphates during fever swept away as useless, because in the arrested ascending metabolism they have nothing wherewith to combine, and are, for the time, useless, fit only for the draught. It is, in fact, presented to me strongly that these chemical variations indicate the cessation, in various degrees, of that process of tissue-building which should, in health, use up heat, and which, ceasing in fever, leaves heat to run wild.

Thinking over such problems, and failing hitherto to find any possibility of experimental investigation of the using up of heat in ascending metabolisms of the body, I turned myself to the vegetable kingdom. In fruits we have, as it seems to me, two processes, of meaning exactly opposed one to the other; the building up of the fruit, wherein we have the formation of cellulose, starch, etc., and the ripening, wherein we have the breaking down and the production of sugar. I determined to investigate fruits of rapid growth, and test their temperature as compared with that of the surrounding air. Before doing this, I consulted botanical books, and questioned great living botanists. But information was not forthcoming from either source. The kindness of a friend who has large hothouses near London enabled me to make some experiments, which I venture to say have some importance in vegetable physiology, as well as in their relation to the question of pyrexia.

The cucumber was the fruit which I chose for my observations. It is a fruit which grows very rapidly, and a fruit in which the signs of ripening can be readily seen. It is grown in houses, at a fairly fixed temperature, and in an atmosphere of considerable moisture. Having chosen my growing cucumbers at a stage free from any fear of ripening, I had a glass bottle, with wide open mouth, filled with water, suspended by the side of each fruit; the bottle equalling the fruit as nearly as possible in size. After twenty-four hours or more, I commenced observation. I used a delicate pointed thermometer, lent to me by that skilful constructor, Mr. Hawksley. With this, I took, first, the temperature of the air of the hothouse around the fruit; next, the temperature of the water in the bottle; next, the temperature of the cucumber at different points of its length. This was done by plunging the sharp end of the bulb containing the mercury to a fixed depth, marked by a line on the bulb. I took the temperature of the cucumber at various points in its length for a definite reason. Cucumbers begin to grow at the base or stalk-end, and further growth is beyond this at the tip or flower-end. If any difference of temperature between the fruit and the air should be found, it might be argued to be due to evaporation if the difference were equal at all points; but, if the difference should vary at the several points tested, the influence of metabolism might be recognised.

On May 28rd, 1884, a very warm day, I examined a young growing cucumber thirteen inches long. The temperature of the house was 86.1° Fahr.; the temperature of the water in the bottle was 85.3° Fahr. The cucumber gave the following readings: at the stalk, 84°; two inches along, 85°; middle, 85°; two inches from tip, 84.6°; tip, 83.9. The experiment was repeated with another cucumber on the same day, and with similar cucumbers on other days. All the observations were to the same effect. I note one or two other observations.

On June 7th, the weather being much cooler, and the air of the house being at 75.6°, the water in the bottle was at 76.9°. A ripe cucumber was found to be at 77.5° in the middle; a young cucumber, pendulous, 74.6° in the middle; a young cucumber, horizontal, 74.7° in the middle.

On June 14th, the weather being again warm, the water stood at 86° in a bottle on one side, at 86.3° in a bottle on the opposite side of the cucumbers to be examined. A cucumber, nearly ripe, gave 84° close to the stalk, 84.2° in the middle, 83.2° at the tip; a small, but evidently growing cucumber, 83.5° in the middle; a nearly ripe cucumber, on the preceding day, and placed close to the others, 87° at all parts.

I trust that you will find in these observations, as they stand, something of interest. They show, at least, that the traditional coolness of the cucumber is not a mere creation of fancy; that the

growing fruit is actually cooler than the medium in which it is growing. So far as these observations are concerned, a new fact has been established; but the observations are not numerous, and the inferences are not sure. I hope to extend the observations; to introduce many controlling experiments; and so go on to safer inference. Nevertheless, arguing upon what we have before us, and upon parallel experiments made upon bananas, with which I will not now trouble you, we may acknowledge that the comparative coolness of the growing fruit may have been due to evaporation; and that the warmth of the separated fruit may have been due to the comparative cessation of evaporation. On the other hand, I may urge that the air of the hothouse was loaded with moisture, as is the case in properly managed houses of this kind, and that moisture was deposited in beads on the surface of the fruit. This, of course, is against the evaporation explanation. And, so far, I should urge that the difference in the temperature of the fruit at various points in its length is against evaporation. Further, it is in favour of the hypothesis of metamorphism in metabolism, in that the temperature was most reduced where, according to all appearance, tissue-formation should be most rapidly proceeding. If time serve me during the coming year, I hope to institute experiments which shall eliminate the evaporation difficulty, and to investigate other fruits; but, so far as I have gone, I think that I am entitled to argue that there is indication that the metabolisms leading to formation of tissues from juices do actually use up heat.

Now, let us apply what has been said to help us in understanding the causation of pyrexia and hyperpyrexia. Dr. Broadbent has written thus. "If a theory of the febrile process is to be formed, it must be based upon a theory of the relation between the nervous system and the processes of nutrition and oxidation, and especially the latter." When I read this I can read that my distinguished friend's thoughts go much with mine. I believe that, in the production of fever-heat, there is a first factor of increased oxidation, or combustion, or disintegration, setting free heat. I cannot, however, find this sufficient to account for all the increase of heat observed in pyrexia, and still more in hyperpyrexia. The further increment I believe to be furnished by heat going astray in default of correlative change in metabolism. And, like Dr. Broadbent, I am inclined to recognise in the nervous system the power, inciting, on the one hand, to disintegration, controlling, on the other, the nutritive functions. In the progress of febrile diseases, there is a manifest correspondence of deeper and deeper affections of the nervous system, with higher and higher degrees of temperature. As temperature rises, headache, excitement, insomnia are followed by delirium; on delirium follow various involuntary movements, and finally convulsions; to these succeeds coma. As death by coma approaches, the temperature rises rapidly, and, in many cases, is increased, or, for a time, maintained after death, when the oxidation-processes dependent on the circulation of blood must have ceased. The coincidence up to death may be argued to be due to effects of increased heat of the body upon the nervous tissues, but this argument is strongly met by the difficulty of explaining the rise of temperature without invoking the aid of the nervous system. What I would suggest is that in all fever, slight or intense, there is superadded to the combustions which we recognise an influence of the nervous system, a trophic influence, arresting processes in which heat should be transformed; and that the increasing temperature of fever is determined by increase of this inhibitory influence. When death occurs, inhibition, a partial condition, must be replaced by annihilation, a complete condition; and at that stage, while we cannot allot any part to disintegration, we can claim that complete cessation of constructive metabolism must set absolutely free all heat previously generated.

All that I have been urging is, you will say, only theoretical. It is a new contemplation of things daily under our eyes. The practical objects of this Society justify me in going on to indicate how in some degree these speculations may bear upon treatment. We have lying ready to our hands a great armoury of drugs useful, in various ways, in the fight with pyrexia. Of these I will not now speak, not from want of respect, for I am conscious of the great value of many of them; but I wish to speak particularly of the usefulness of another remedy, also always ready to hand—baths—in the control of high fever, as its causation is here presented to us. I have for many years studied the use of baths in the treatment of diseases of various kinds, and have had personal experience of them in most of the important wells of Germany and France. My belief is that baths—large applications of fluids of various temperatures to large surfaces of the skin—come in as moderators; that their main beneficial use consists in attracting, so to speak, the attention of the nervous system, and drawing it off from certain pernicious paths upon which it has en-

barked. Some years ago, I was much struck with some remarks made at a meeting of one of the learned societies by Dr. Thudichum, in which he threw doubt upon the idea that the reduction of temperature in fever by cold baths was a purely physical phenomenon, dependent simply upon abstraction of heat. I had been using graduated baths in hyperpyrexia, placing the patient in water at 95° Fahr., and cooling the water very evenly down to 70° or 75° during half an hour. The result had been, in some cases, an abatement of temperature as large as ten degrees; and with this had come sleep, replacing insomnia; calm, replacing delirium; easy breathing, replacing panting. The change in the nervous state from excitement to tranquillity had been as pronounced as the reduction of temperature. Soon afterwards, I made an experimental test of the power of such baths to reduce the temperature of a dead body to such an extent. I placed the body of a person dead the day before, after rigor mortis had ceased, in a bath heated to, and maintained at, 107° Fahr., until it was ascertained by the thermometer that it was heated through. Then I placed thermometers in the body, at various points and at various depths, taking care so to implant them that the water would have no access to their bulbs; and then I cooled the water to 95°; and, during a subsequent half-hour, cooled it gradually to 70°. The reduction of temperature in the superficial parts of the body was much less than that brought about by a bath, and the temperature in the deeper parts was scarcely affected. Even if the circulation had been going on, no lowering of temperature at all approaching a lowering of six degrees could, I think, have been produced. It must be acknowledged that the experiment was incomplete, in that I did not maintain a circulation of warmed water in the blood-vessels; but, so far as it gave results, they were adverse to the simple physical theory.

In dealing with this question of baths, we must remember the enormous influence exerted upon the central nervous system by slight impressions made over a large peripheral distribution of nerves. A scald sufficient to produce but a slight vesication, when it involves the whole surface of the body, will kill. And I believe that the application to the whole surface of the body of water at a different temperature from that of the body acts in fever by exerting a reflex inhibitory influence upon the nervous system, leading to a relaxation of its trophic constraint. Of course, few remedies are simple in their action, and I should be prepared to admit the assistant influence of the lower temperature of the bath from the physical side. Anyway, in the treatment of hyperpyrexia, I know of nothing so trustworthy as the bath—cool, cold, or graduated. For my own part, I prefer the latter, and have again and again seen it reduce temperature and save life, when quinine, salicin, salicylate, and other febrifuges had failed to arrest a steadily rising temperature already above 106° Fahr. And now my speculations are at an end. I have wished to say something that might give you food for thought, perhaps set some of you on the path of investigation, which I should myself like to follow more closely than circumstances are likely to permit. I cannot claim to have proven much, if indeed anything. I have not sought to do so. I have only sought, on a day when a new session opens, to suggest some new explanations of phenomena coming under our daily observation.

A NOTE ON TWENTY-SEVEN CASES OF PERINEORRAPHY.

By FANCOURT BARNES, M.D.,
Physician to the Chelsea Hospital for Women, etc.

In the Chelsea Hospital for Women, during the last twenty months, I have operated for ruptured perineum twenty-seven times. Of these cases, sixteen were treated by the old method of dissecting off a certain area of mucous membrane, and bringing the surface thus freshened into symmetrical apposition. In eight of the sixteen cases just mentioned, the sphincter ani had been torn through; in the remaining eight, the sphincter was intact. The result of the operations in all these cases was successful. In all these cases, a suppository containing one-sixth of a grain of morphine was given at the conclusion of the operation, and it was necessary to continue the administration, in the form of suppositories or otherwise, during the days between the operation and the removal of the stitches. As a rule, no enema or purgative was given until the seventh day. The stitches were removed on the tenth day. In three (out of the eight) cases where there had been rupture through the sphincter, the tear had been sewn up immediately after the laceration, by the medical man in attendance. The result of this manoeuvre, however, was not successful; none of the patients, although apparently possessing fair perineæ, were able to retain their faeces. The only result of the operation was

the manufacture of a bridge of tissue, masking a fistula, leading from the vagina into the rectum, with the sphincter intact. This bridge of tissue (or, as I have sometimes called it, the pons anodynorum) had of course to be divided before the ordinary dissection could be proceeded with. It is to my mind an argument, among others, against the practice recommended by some obstetricians of invariably "putting in a stitch," as they say, at the time of rupture. For years past, I have had the opportunity, in the British Lying-in Hospital, of observing that perineæ, whether ruptured through the sphincter or not, will always heal in varying degrees; it may be, from the bottom of the wound. In this way, I have often seen tears up to or through the sphincter, left to themselves, heal entirely, and this without the knees being tied together.

Last May, Mr. Lawson Tait was kind enough to show me his operation for restoring the perineum on two of my patients in the Chelsea Hospital for Women. In one case, the sphincter was torn through; in the other, it was not. Mr. Lawson Tait's operation consists in splitting the recto-vaginal septum with the scissors, and closing the wound thus made with silver sutures. Lawson Tait's operation has the following advantages over the old plan. The splitting of the vagino-rectal septum, and the introduction of the sutures, can easily be done in from five to eight minutes. The old plan, in the hands of most operators whom I have seen, occupies seldom less than twenty, sometimes fifty minutes. It is needless to point out that, in this one respect alone, the patient undergoes less danger. The stitches are inserted within the margins of the wound instead of through the skin, as in the old plan. There is no loss of tissue. If the operation fails, the patient is no worse off than she was before.

Since seeing Mr. Lawson Tait do these two operations, which were both successful, I have repaired the ruptured perineum by his method in eleven cases; two torn through the sphincter, nine not through the sphincter. All these cases have done perfectly well. In my opinion, Mr. Lawson Tait's operation is a great improvement upon the old plan. The patient, instead of being in pain until the stitches are removed, and requiring a continuous administration of sedatives, is free from pain. This is owing to the sutures being placed inside the wound instead of through the skin. The new perineum is thicker and more solid than that obtained by the old method. Mr. Lawson Tait advises the daily use of enemata, and does not prohibit the patient living on her ordinary diet.

THE SYMMETRICAL DISTRIBUTION OF HERPES ZOSTER.

By ROBERT SAUNDBY, M.D.,

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I HAVE been reading the paper on "Symmetrical Completion of Herpes Zoster," in the JOURNAL of August 8th, and would ask to be allowed a little space for comment upon the views expressed in it.

1. The case recorded is put forward as one of symmetrical completion of herpes zoster. The completion is not a fresh outbreak of herpes on the opposite side of the body, but, nine months later, certain ill-defined pains radiating from the nape of the neck, oppression over the eyes, and vertigo, in some respects resembling Meniere's disease, with general constitutional disturbance.

This cannot be regarded as an indisputable instance of symmetrical completion; and, in the absence of any precise information as to the patient's previous history and present condition, especially without reports on the digestive system, urine, and ear at the time of the occurrence of these symptoms, it is not at all a satisfactory foundation upon which to build a superstructure of transcendental pathology.

2. The author quotes the ancient doctrine, that bilaterally complete zoster is fatal, and suggests that, if not fatal, it gives rise to constitutional symptoms, of which the present case is an example.

Without going further, I would say that, in my own experience, bilateral herpes has run a course in no way distinct from that of the one-sided affection; but the generally received explanation of the old aphorism is, that it sometimes depends upon serious organic disease.

Bilateral zoster is often said to be syphilitic; but this is not true of all cases, I am convinced.

So much for the alleged fact; but the author goes further, for, assuming such constitutional disturbance to occur, he seeks an explanation in the theory that, when half a nerve-centre is affected, the resulting phenomena are local only; but, when the whole centre is involved, constitutional symptoms are superadded, as a consequence of the "loss of a definite entity."