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 anionorum) 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.,

Assistant-Physician to the General Hospital, Birmingham.

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."

This theory involves two assumptions: (1) that destruction of a bilaterally associated nerve-centre gives rise to constitutional disturbance, and (2) that the lesion causing herpes zoster causes destruction of the nerve-centre. No proof is afforded of either; and, so far as I know, there are no facts in pathology to warrant them. On the contrary, apart from the pathological process involved in its general destruction, general constitutional symptoms do not follow from the suppression of the function of even large parts of the nervous system. It is, therefore, unreasonable to attribute so much to the "loss of a definite entity" of microscopic dimensions only. If there were actual destruction of the nerve-centre in zoster, we should expect some paralysis of motion or sensation; but the commonest permanent result is chronic painfulness, generally attributed to a lesion of the nerve-trunk, but for the recognition of which there must be a percipient centre.

3. Having assumed that he has met with a case of bilateral zoster giving rise to constitutional symptoms, having further assumed that such an association is usual, and stands in need of explanation, and having formulated the explanation already quoted, he announces the discovery of a "natural law governing the action of the nervous system in disease." This "law" is not formulated; but it appears to be, that neuroses do not attain their acme until both sides of the body are affected. Hemicrania is said to afford illustrations of it; but the "law" needs to be formulated a little more precisely before any opinion of its value can be formed.

4. Finally, he suggests that this case affords an example of the similarity of plant and animal pathology.

He quotes a passage from Sir James Paget's address at Cambridge, which appears to be a sufficient answer to such a suggestion, as in it Sir James points out the fundamental distinction between the elementary pathological processes of animals and plants, caused by the influences of the nervous system. Plants have no differentiated nervous system; and, therefore, they are the last place in which to seek analogies with neuro-pathology.

He thinks that the death of a branch from frost is analogous to this attack of herpes affecting a definite nerve-area after a chill; but the comparison is strained. In the first case, we have to do with the direct effect of cold, as in gangrene after frost-bite; in the second, with the indirect and obscure action of a chill of the surface, determining changes in subjacent structures.

We want thoughtful men to help to organise facts; and I am, I trust, always grateful for well considered speculations, adapted to the outline of our present knowledge. It is from no disapproval of speculation generally, but from doubt of the method adopted in the present instance, that I have ventured to point out what appear to me to be objections to it.

CIRRHOTIC ENLARGEMENT OF THE LIVER IN A FEMALE.

By JAMES OLIVER, M.B. Edin., M.R.C.P. Lond.,
Assistant-Physician to the Hospital for Women, Soho Square.

AN interstitial hepatitis resulting in cirrhotic enlargement of the liver is, by some authorities, denied; whilst others, admitting its existence, express the opinion that the affection is one totally distinct from that of cirrhotic contraction. The active congestion, going on to actual inflammation, and produced by the more or less constant irritation of the organ by the imbibition of alcohol, results in an exudation and proliferation of cells which, in the early stage, are seen to be oval, fusiform, and round in shape. Later on, these so called embryonic cells undergo gradual change, and develop fibrous tissue, which will be found along the course of the blood-vessels, and penetrating between the lobules.

In this stage, the liver is usually enlarged. The newly formed fibrous tissue, however, tends to retract, and cause wasting of the lobules, thereby developing a cirrhotic contraction, and producing the well known hob-nail liver.

The symptoms associated with cirrhotic enlargement, it is to be remembered, are not solely dependent upon the structural changes occurring in the liver, but are rather the result of the cause of the irritation in this all important organ. The alcohol, prior to its passing to the liver through the medium of the portal circulation, exerts its deleterious influence on the mucous membrane and other structures of the gastro-intestinal tract, creating a chronic hyperæmia, which

eventually results in impairment of function, not only of the secreting, but of the absorbing, tissues of the canal. Hence the reason that, even in the early stage, we may have loss of flesh, a marked symptom of the disease.

It is possible—nay, likely—that interstitial hepatitis may result from causes other than alcohol; regarding these, however, little or nothing is known.

CASE.—E. H., aged 28, married ten years, no children, and no miscarriages, four months ago began to notice a slight increase in the size of the stomach, and ever since then there had been complete amenorrhœa. The catamenia appeared at the age of 11, and, hitherto, she had been always perfectly regular, the hæmorrhagic discharge recurring every twenty-eight days, and continuing three or four. For the last three months, she had suffered from sickness, vomiting food at all times soon after its ingestion. The patient had a feeling of intense lassitude, and was constantly more or less drowsy. Two years ago, the appetite began to fail; and, during the last twelve months, she had lost flesh. At times she had suffered pain in the region of the liver, and under both shoulder-blades. During the last two months, headache had been a troublesome symptom, and there had been occasional attacks of profuse epistaxis. Lately there had been diarrhœa, not specially in the morning. Her habits had been intemperate; she had taken two pints of beer daily, besides some spirits. The liver was uniformly enlarged, measuring nine inches and a half in the nipple-line, and five below the ribs; the lower margin, hard and regular, was felt below the umbilicus. The slightest pressure anywhere in the region of the epigastrium caused a feeling of sickness. The urine was thick, and of a deep colour; it contained lithates, bile-pigment, and a small quantity of albumen. For two months, the patient had to get up at least once every night to pass urine. There was slight œdema of the ankles; no ascites.

BRITISH MEDICAL ASSOCIATION.

FIFTY-THIRD ANNUAL MEETING.

PROCEEDINGS OF SECTIONS.

THE MECHANISM AND MANAGEMENT OF THE THIRD STAGE OF LABOUR.

Read in the Section of Obstetric Medicine at the Annual Meeting of the British Medical Association in Cardiff.

By D. BERRY HART, M.D., F.R.C.P.E.,

Lecturer on Midwifery and Diseases of Women in the School of Medicine, Edinburgh.

It may seem, on first sight, that the subject chosen for discussion is too elementary and well threshed out to claim much of the attention of our present meeting; but, although no process has been so much and so often studied practically by the vast majority of our profession, it is yet true that we know only too little of its real mechanism, and are in great variance in regard to its treatment; and this in spite of the fact that the expulsion of the placental membranes entire and without any internal manipulation, in a natural labour, is of the highest importance in relation to after-septic processes, whether the graver septicæmia of puerperal fever, or the less severe and yet troublesome attacks which often leave chronic endometritis, cellulitis, and peritonitis as their sequelæ.

Apart from all this, however, the present time seems to me opportune for considering the subject in our country. The attention now paid to abdominal palpation before and during labour, the increase in our knowledge of the exact relations of the membranes in the third stage—due mainly to the splendid work of my friend Dr. Freeland Barbour, and the application to obstetrics of the principles of antiseptics imperishably connected with the name of Lister—enable us to consider the whole matter from new and commanding stand-points.

In the following remarks, I purpose briefly to relate the facts known to us in relation to the natural and morbid mechanism of the third stage, pointing out what has yet to be discovered in order to make our knowledge complete. The management which seems to me to favour the natural process, or to be necessary when the natural becomes morbid, will also be briefly considered.