there was no tenderness, redness, or cedema of the skin, but he felt a little pain when he walked. The knee was painted with strong iodine liniment for a few days; no improvement followed. A plaster bandage worn for a few weeks caused the fluid to disappear, and with a leather splint he was soon able to work again in the fields, and quite recovered the use of his knee.

Neither of these patients was a "bleeder." Amongst several medical men with whom I have discussed these cases I may mention that Dr. Genth of Schwalbach tells me that he has met with cases apparently similar; he states that his occurred in nervous young men, and that he found, like myself, that blisters were useless, though the local treatment by baths was very beneficial. As regards pathology, synovitis of the knee-joint, requiring either severe injury or septic infection, seems to be excluded, also rheumatism from the monarthritic nature of these cases and struma from their general nature. The only feasible explanation seems to me to be that they are due to a tropho-neurotic change in the synovial membrane occurring in a debilitated subject from some obscure cause.—I am, Sirs, yours truly,

WALTER G. EARLE, M.R.C.S. Eng., L.R.C.P. Lond.

Tunbridge Wells.

* * We agree with our correspondent in thinking these two cases of interest, but we are inclined to look upon them as examples of a synovitis resulting from very slight traumatism in an unhealthy subject. In each case there was a great likelihood of a slight injury having occurred, and certainly such cases are by no means rare after tennis. The speedy cure which resulted from the use of splints is in favour of this diagnosis.—ED. L.

"ACUTE PEMPHIGUS."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of March 13th, which I received to-day, I read a Clinical Note by Dr. Jón Jónsson on "Acute Pemphigus." It might be of interest to your readers to know that I have seen two cases of "acute generalised pemphigus," one in a young man, aged twenty-two years, in the year 1894, and the other in a boy aged thirteen years, in October last. The young man died on the fifth day. The boy lived, and was at the moment supposed to be out of danger, up to the eighteenth day, when he died very suddenly. The symptoms in both cases were exactly those described by Dr. Jónsson and Dr. Priestley. There were vesicles on the inside of the mouth (buccal mucous membrane), but no eczema.

I am, Sirs, yours truly,

F. G. CORBIN, M.D. McGill.

Chascomús, Argentine Republic, April 4th, 1897.

FRACTURES OF LONG BONES IN SCURVY-RICKETS.

To the Editors of THE LANCET.

SIRS,—At the meeting of the Medical Society of London held on April 11th I showed an infant suffering from multiple spontaneous (so-called) fractures of long bones, dependent, in my opinion, on scurvy-rickets. In your report of the proceedings¹ you mention that Mr. Morgan thought my diagnosis questionable on the ground that hæmorrhages and fractures in scurvy rickets were usually near the epiphyses and not, as in my case, in the centres of the shafts. As the President did not allow me to reply to Mr. Morgan's criticism at the time, may I venture to do so in your columns?

Central fractures of long bones may occur in addition to separation of epiphyses and hæmorrhages beneath the periosteum in the course of scurvy-rickets. I have Dr. G. A. Sutherland's permission to refer to one of his cases of scurvy, as yet unpublished, in which such conditions were diagnosed during life by him, and verified by myself post mortem. I did not see my own patient until several months had elapsed since the fractures had been produced, and at this time there were certainly no sub-periosteal effusions. Yet, from the history of swelling and bruising of the limbs, I have little doubt that hæmorrhages were present at first. Such hæmorrhages are very quickly absorbed. The point which needs further investigation is whether these central fractures are the cause or the consequence of the extravasations. Effusions

beneath the periosteum may lead to fragility of the bones by depriving them of their normal nutrition and support. On the other hand, the general tendency to hæmorrhage in scurvy may cause sub-periosteal extravasation at the time the fractures occur. Opinions on this point were not forthcoming when the case was shown. The time for discussion was limited, it is true; but if in a society like the Medical Society of London discussion of cases is invited at all, I think that answers to criticisms should be allowed. Otherwise the genuineness of cases exhibited must remain for ever doubtful and the value of facts observed may be impaired.

I am, Sirs, yours truly,

LEONARD G. GUTHRIE.

May 3rd, 1897.

"THE HOSPITAL REFORM ASSOCIATION: REPORT OF THE COMMITTEE APPOINTED TO INQUIRE INTO THE WORKING OF THE SPECIAL HOSPITALS OF LONDON."

To the Editors of THE LANCET.

SIRS,—THE LANCET of April 17th contains a letter signed by the secretary of the Central London Throat and Ear Hospital upon the shortness of our visit to his institution and stating that important matter supplied by him had been omitted in our report. In reporting upon forty-one institutions it was impossible to spend hours over each visit, as he suggests we should have done. In his case we called three times before finding him at home. In regard to the second point, if we had sent to press one-half of what the various secretaries would have liked said about their particular institutions no journal would have published the report on account of its length. Since publishing the report we have been informed by the secretary of the British Mental Hospital that the annual subscription to that hospital in 1895 was £292. We were unable to publish these figures at the time, as this institution has issued no report since 1894.

We are, Sirs, yours faithfully,

W. KNOWSLEY SIBLEY, M.D. Cantab.,

ERNEST SNAPE, M.D. Brux.

May 4th, 1897.

"THE QUESTION OF THE SECOND OVARY IN OVARIOTOMY."

To the Editors of THE LANCET.

SIRS,—The question raised by Mr. Clement Lucas as to the necessity for the removal of the second ovary in ovariectomy is a very important one. As one who has not operated extensively, but who, nevertheless, has seen a considerable amount of gynaecological work, I should feel obliged by your allowing me space to record my views, whatever value they may have. I have in the past, acting on the ordinarily accepted views of eminent gynaecologists, removed ovaries that in the light of my own experience since I would not think of removing now. I have in some cases removed both ovaries, while in others I have removed only one, being influenced, as Mr. Clement Lucas evidently is, by the particular circumstances of each case. I firmly believe there can be no fixed, defined, and inalienable rule in these cases. I remember years ago removing a cystic ovary from a young woman, unmarried, leaving the other, which was enlarged and tender. She made an excellent recovery, but when well enough to be discharged and get about she began to complain of symptoms pointing to the ovary which was left behind, and she was repeatedly sent to me to know if she had not another tumour coming and if it was not advisable to have another operation. I saw no reason for it and refused, and this young woman is now perfectly well. There were pain and tenderness, but I did not consider these sufficient to justify another operation. One cannot avoid the feeling that in this case the second ovary might have been removed had I been strongly impressed with the need for it at the time of operation. We might have had this case to swell the number of double removals, and the tender condition and enlargement would have been employed as arguments in justification.

I know of a case of cyst of the broad ligament which I was about to remove five years ago, but which, owing to the very alarming symptoms occasioned by the anæsthetic, I was obliged to desist from doing when just about to enucleate it. The operation was immediately stopped and the abdomen

¹ THE LANCET, April 17th, 1897.

closed. From that day to this the cyst has not enlarged; indeed, it underwent some degree of diminution. I have seen the patient at intervals of a few months since and she has continued very comfortable and able to attend to her business. During the time I had the patient under observation previously to the operation the cyst had been increasing, and I only decided to operate because of the disturbance and pain it was causing in its neighbourhood, as well as rendering the woman unfit for her duties and giving her great general discomfort. Its presence does not now cause these symptoms in anything like the same degree, and, as I have already said, she attends to her daily duties, which are of an arduous kind. Why the cyst ceased to increase is another question, but it may have been *in some measure* due to the fact that I made her rest absolutely for six months.

I accidentally discovered a cyst of the ovary when attending a woman four years ago for another matter. It had not increased when I last attended her a few months ago, and she at present suffers only a little from it. She does not know of its existence. Why should she so long as it gives such slight trouble and she is quite able for her duties? She has had two children since I discovered the cyst, and if it has remained *in statu quo* for four years (and perhaps for four years previously to that) may it not remain so for four, or even fourteen, more? I may also instance the case of a young married woman in whom a tumour (cystic) of one ovary has existed to my knowledge for years. It has at times caused a good deal of suffering, and on one occasion I almost decided to remove it. Several months of absolute rest, however, with some treatment, helped her greatly, and she has been able to attend to her household affairs with a considerably greater degree of comfort than previously. I have known of the existence of this cyst for six or seven years, and it does not increase.

These, possibly it may be advanced, are not instances quite analogous, except the first, to those mentioned by Mr. Clement Lucas, inasmuch as in his the abdomen was opened, and the advocates for removal would say that it would be better to remove the organ than for the woman to undergo the trouble, anxiety, and suffering of a second operation, with its consequent risk and expense. The risk, of course, is small now, thanks to the progress made in surgery during the last quarter of a century. But there is risk, and it ought to be avoided, to say nothing of the other concomitants of an operation. But if there is a reasonable hope that the ovary may not get worse or may recover, I think the woman, especially if a young one, ought to have a chance and the ovary should be left, and I think it may be fairly said that the cases cited show that ovaries to some extent diseased may be left sometimes with a very fair prospect that they may not only not cause increased trouble in the future, but that they may remain stationary or even recover. If, then, some diseased conditions of the ovaries remain quiescent for years, improve, and even get well, we ought not too hastily to conclude that operative interference is necessary, but should keep our patients under observation and treatment. I am confident that some operations may be avoided by this plan, and the subjects of the disease be enabled to pursue their daily duties with comfort. If the time comes for operation the case can be dealt with then just as well as earlier, provided one does not allow it to go too far until dangerous complications arise. I am quite alive to the necessity for early operation. I am sure, however, we will lose nothing by waiting and watching, selecting the proper cases for such a course.

I am, Sirs, yours truly,

D. THOMSON, M.D. Glasg.

Lowndes-street, S.W., May 4th, 1897.

TUBERCULOSIS IN DAIRY CATTLE.

To the Editors of THE LANCET.

SIRS,—In the issue of THE LANCET of May 1st your Irish correspondent refers to the resolution passed by the Dublin University Biological Association concerning the prevalence of tuberculosis among dairy cattle. He further states that it is reassuring to find in the report of the Cowkeepers' Association that the result of a veterinary examination by two gentlemen shows only 4 cows out of 1500 examined were affected with tuberculosis. There are some considerations which would lead one to place very small reliance on the results obtained by these gentlemen, to which I wish to draw

public attention. From a return furnished to me by the Veterinary Department of the Privy Council Office, Ireland, I take the following statement: "It appears from the departmental records that the post-mortem examination of the lungs of cattle slaughtered by direction of the Veterinary Department between the years 1888-1893 in the North and South Dublin Unions as having been in contact with cattle affected with pleuro-pneumonia, or otherwise exposed to the infection of that disease, showed that the proportion of cases in which the *lungs* were affected with tuberculosis was almost 5 per cent. The majority of cattle slaughtered were dairy cows." It is evident from this statement that if only four out of 1500 cows are now affected with tuberculosis there must be a most marked improvement in the health of cattle in this district. Indeed, such a marked change in so short a time is sufficiently remarkable to call for some explanation. So far from any explanation of this improvement being forthcoming it is a matter of common knowledge that little or nothing is being, or has been, done to improve the state of the dairy yards, the unsanitary condition of which has been so often pointed out. Taking these facts into consideration, and remembering that the extraordinary prevalence of tuberculosis among dairy cattle in general has been amply proved by numerous statistics, we shall, I think, be justified in doubting these results so long as the methods by which they were obtained are open to suspicion. Professor McFadyean, when summing up the results of his experiments for the recent Royal Commission, states: "The conclusion that appears warranted is that ordinary clinical examination is almost valueless for the detection of tuberculosis in its early stages." Over and over again in his published works this great authority on veterinary pathology has stated that ordinary clinical examination even in the hands of the most skilled and experienced veterinary surgeons is incapable of pointing out all the animals affected with tuberculosis. Unless some more accurate means of diagnosis is used than ordinary clinical examination affords veterinary surgeons merely leave themselves open to the charge of incompetence by certifying animals to be free from tuberculosis. It would appear that Koch's tuberculin in the hands of skilled surgeons is the most accurate method of diagnosing tuberculosis of cattle, and where it cannot be used most veterinary surgeons decline to express a decided opinion. We cannot, then, accept any statement as to the number of tuberculous cattle in our dairy yards which is not based on examination with tuberculin or the results of post-mortem examination. Was this test employed by the inspectors who reported to the Cowkeepers' Association? If so, where are the tabulated returns of the cattle examined? The answer is, I think, that these returns do not exist, because the method was not used, and in consequence the report is valueless.

I am, Sirs, yours faithfully,

T. PERCY C. KIRKPATRICK, M.D. Dub.

Dublin, May 4th, 1897.

"THE SWEDISH SYSTEM OF PHYSICAL EDUCATION."

To the Editors of THE LANCET.

SIRS,—If you would kindly allow me space in your next issue I should be glad to efface an impression which I regret to have made. In your review of my booklet on the above subject exception is taken to one observation. Your reviewer writes: "The English medical profession is charged with 'insular prejudice against anything branded as foreign.'"¹ Reference was here intended to the British nation as a whole. As a general and national characteristic it would be difficult to deny that John Bull entertains a certain scorn for the foreigner and for what is foreign, and has an ingrained belief in the superiority of himself and his opinions. This remark, however, was not specially directed against the medical profession; for does she not gracefully acknowledge that many of the brightest jewels in her crown have been set there by other than English hands? In proceeding I have ventured to state that that learned body is characterised by a conservative spirit, but between "conservative spirit" and "insular prejudice" exists a distinction which certainly constitutes a difference.

Believe me, Sirs, yours very truly,

Bristol, May 4th, 1897.

THEODORA JOHNSON.

1 "The Swedish System of Physical Education," p. 21.