rid of puerperal fever. As an encouragement to this end, he pointed out what had been done in rendering ovariotomy successful by the simple adoption of sanitary measures.

It was then announced that letters had been received from several gentlemen who were unable to attend, among them Dr. Robert Lee, Professor Simpson (of Edinburgh), Professor Matthews Duncan (whose letter condemnatory of the recent conviction of midwives for manslaughter was read), and many others.

The President (Dr. Priestley) invited remarks from country practitioners who might be present, and called upon Dr. LEISHMAN (of Glasgow) to speak. Dr. Leishman's speech was chiefly remarkable for the frank and open manner in which he announced that he had been led to form conclusions upon the nature of puerperal fever different from those which he had already published; for he has come round to the belief that the various forms of fever included under that term are all or nearly all of a pyæmic or septicæmic character. For, however different in their starting-point, whether it be from scarlet fever contagion, or whether from local inflammations, the later symptoms in all forms are of the same septicæmic character. He thought that the statement that animal poisons from any source could engender the fever should be limited. Puerperal fever in a lying-in hospital is a greater scourge than pyæmia in a surgical hospital; so that it might well be said that a woman who entered the former institution simply to fulfil a physiological function was really exposed to as great dangers as if she were exposed to typhus.

Dr. NEWMAN (of Stamford), in an admirable speech, formulated certain propositions founded upon the results of twenty years' experience in country practice. That there is no such thing as a definite puerperal fever, strictly so called; that a large number of cases of the disease do undoubtedly owe their origin to the exposure of the patient to some definite infective fever (and he quoted a fatal case, in which the only possible source was to be found in a child having scarlet fever at a house where the patient paid a visit shortly before her confinement); that in other cases local inflammations lead to the development of a strictly pyæmic state; that, in his opinion, lowered vitality from exposure to sewer-air in badly-drained houses greatly predisposed to the infection; that possibly the greater activity of the vital processes in a parturient woman accounted for the readiness with which they were infected by poisons against which they would be proof in the non-parturient state; finally, he thought that mental disturbance acted as a great predisposing cause.

Dr. BRAXTON HICKS followed in some remarks upon the necessity of having a clear idea of the clinical position of puerperal fever. A very large proportion of cases were due to contagion from the specific fevers or from decomposing materials in the uterus, to which must be added those in which mental influences had a share,—in this way but 10 or 11 per cent. alone remained the origin of which it was difficult to trace. Sometimes a parturient woman exposed to scarlatinal poison does not take the fever; it would be interesting to know if in such a case the patient had already had scarlet fever. That decomposing clots do give rise to the symptoms of the disease was undoubted, and on their removal and washing out of the uterus the symptoms subside. Dr. Hicks concluded his remarks by stating that there was no doubt as to the contagiousness of puerperal fever, however it be produced, but he questioned if the mere aggregation of puerperal patients in a lying-in ward was *per se* sufficient to give rise to the disease.

Mr. JONATHAN HUTCHINSON, having been called upon, contented himself with giving, in his clear and cogent style, his definitions of "erysipelas," "septicæmia," and "pyæmia." We are unable here, from want of space, to do full justice to his valuable remarks. Suffice it to say that he demonstrated that erysipelas, which is so potent an agent in producing puerperal fever, is *not* a specific fever; that it is simply a local inflammation, to which all its febrile and general symptoms are superadded—a view supported by its unilateral character, by the absence of any incubative period, by it being possible to cut short its extension by appropriate treatment. In this respect it resembled inflammation of the uterus. The term "septicæmia," he said, should be limited to poison introduced into the blood from the patient's own tissues, which may have been primarily inflamed by irritants derived from without, or even without

any external cause whatever; while, as to "pyæmia," he fully believed in its sole origin in a suppurative phlebitis.

Dr. B. W. RICHARDSON, after showing that the parturient state was physiologically one of great peculiarity, and prone to the development of febrile conditions, proceeded to answer each of the questions propounded by Mr. Wells. He believes that there is no special "puerperal fever"; that it may simply be a fever of reaction or resistance, such as follows amputation of a limb; or that, from the peculiar condition of the blood, it may lead to cardiac thrombosis; or that it may be of the type of a "bilious remittent" fever; or of septicæmic nature from some local injury inflicted during labour; or, finally, that it may be due to poisons derived from without. With regard to the relation of bacteria, &c., to the pyæmic process in the puerperal state, Dr. Richardson said, that for twenty-five years he has always held that septic processes are purely physical or chemical, the association of organic forms being a coincidence, without causal relation; and that antiseptics act solely in a chemical way upon these septic processes. At the conclusion of Dr. Richardson's remarks, the debate was adjourned on the motion of Dr. Barnes.

Correspondence.

"Audi alteram partem."

A NEW TRUSS PAD.

To the Editor of THE LANCET.

SIR,—In August, 1873, I described a new kind of truss pad, which consisted essentially of a hollow pad, in the centre of which was a spiral spring—in fact, a Cole's or a Teale's truss, having the body spring attached to the convexity or convex surface of the pad plate, instead of to the spiral spring in its concavity. The effect of this transposition is that the chief resistance of the pad is transferred from its central and most convex part to its circumference; and whereas in the above-named trusses the surface of the pad which is applied to the skin undergoes no alteration of form under any circumstances, but only of inclination, that of the pad in question becomes changed, under the impulsion of a rupture, from a convex or flat to a concave form. To ensure this result, however, it is necessary that the pad spring should be a little weaker than the body spring, so that, when the patient coughs and the hernia is thrust outwards, the pad, instead of rising *en masse* or at some part of its border, yields only at its centre, while its circumference is kept firmly in its situation by the stronger resistance of the body spring.

A pad of most ingenious construction, similar in principle to the one just described so far as the action of its centre is concerned, but differing from it as regards the action of its circumference, has been recently invented and patented by Dr. Schoevers, of the Hague, and promises to be superior in some respects to the one above alluded to. It is composed of (1st) a certain number of springs arranged in the form of

a circle or of an ellipse. (Fig. 1.) In the centre or point of union of these circular springs, which in a state of rest form a cavity, there is attached (2ndly) a conical spring, furnished

with a metallic plate (Fig. 2), which is only brought into action when the circular springs are expanded by means of

the pressure of the body spring. The above parts are enclosed in (3rdly) a vulcanised india-rubber capsule (Fig. 3),

which yields to the expansion of the circular springs, and also serves to protect them and to complete the pad. Thus constituted, it is connected to the body spring—which is of the half-circle type and of French curve—by means of the ball-and-socket joint seen on the back of the pad in Fig. 2; and the instrument is completed by a connecting strap, which buttons on to a stud at the extremity of the body spring.

The specimen which has been sent me from the Hague is a No. 36, and, notwithstanding that the body spring is very strong, the instrument is so light that it weighs under four ounces. It may also be as well to mention that, although the connexion between the pad and spring is by a ball-and-socket joint, it is not, like Salmon's, which is similarly connected, opposite-sided.

As regards the value of this truss for the retention of difficult ruptures I cannot at present speak authoritatively, not having made trial of it; but, looking to the action of the elastic and expanding arms which form the circumference of the pad, it seems to me likely to have a firmer grip of the parts against which it may be placed, and consequently a greater retentive power than that of any other pad with which I am acquainted. It should be mentioned that the instrument is sold by Messrs. Maw, Son, and Thompson, of Aldersgate-street, who, I believe, are the sole importers.

I am, Sir, yours obediently,

March, 1875.

C. HOLTHOUSE.

THE BIRMINGHAM MEDICAL INSTITUTE.

To the Editor of THE LANCET.

SIR,—I avail myself of your columns to tender my acknowledgments to 460 members of the profession residing in Birmingham and its immediate neighbourhood who, to this date, have placed on record their opinion as to the propriety of the admission of professed homœopaths to the membership of the Birmingham Medical Institute, by returning to me my circular on this subject with their signatures. Of this number, 436 say, without reservation in their judgment, that “professed homœopaths, so long as they assume a mode of practice and maintain a name calculated to mark them from the general body of the profession,” should not be admitted; 19, equally without reservation, say they should; and 5 desire to be neutral.

I venture to think that here is the “voice of the profession” in our midst on the “difficult question,” and a justification of my protest in the shape of an amendment to the approval of the report of the committee which passed at the recent annual meeting, to the effect that it was to be regretted that the opinion of the profession had not been sought ere donations from professed homœopaths had been received, and their admission determined, only by a committee.

Sir, the loss of my amendment by a majority of twenty-nine, in a meeting composed in the main of members on whose attendance unusual influences had been brought to bear, and who were led up to a vote by threats and denunciations so violent as to be without a parallel, is to me of little moment. I regard in the same light the arrogant and grossly personal attack I encountered at the hands of Dr. Heslop, so long as the true sentiments of our order are on record on this matter, and I therefore cheerfully leave his conduct to the judgment of the profession, as well as to that of his immediate associates, who, whatever may have been their dissent from my views, have one and all confined themselves in their utterances within the bounds

of what is fair and proper in honourable controversy; and I can truly say that it has been my aim and object, from first to last, to avoid any references of a personal character, which are calculated only to weaken the best of causes, and to redound to the discredit of those who utter them.

I shall, however, indeed rejoice if my action in this matter leads to the abandonment of the name of “homœopath.” If Dr. Dudgeon*, who has watched the recent struggle with no common interest, and has appreciated the force and fairness of the test I submitted, can say “that he holds himself free to avail himself of the resources of therapeutics, including homœopathy,” and that the name “homœopath” is a “nick-name,” and to be “abandoned,” then my task is done, and I, for one, fully admit that he claims a consideration at our hands that ought not to be withheld, and manifests an example that, sooner or later, must be followed.

Lastly, let me say that, although I have steadfastly declined any correspondence on this purely medical question in our local press, and have sustained in certain quarters unmeasured abuse, I cannot but recognise the fairness and the wisdom of the comments made by the *Birmingham Daily Post* on the result of the recent meeting, and I take this opportunity to say, in accordance with the spirit of those observations, that the Institute may yet find myself, and, I trust, those who have acted with me, amongst the number of its best friends.

I remain, Sir, yours, &c.,

Birmingham, April 5th, 1875.

OLIVER PEMBERTON.

LITHOTOMY AND LITHOTRITY.

To the Editor of THE LANCET.

SIR,—Sir Henry Thompson's interesting account of the good results of his last 100 cases of stone, together with his previously published cases, furnish a large body of evidence from which surgeons may extract most valuable information.

The whole series leave little to be supplied in the way of what I may venture to call—in no offensive sense—the rough statistics of stone. They indisputably establish their author's claim to an unrivalled experience and dexterity as a lithotritist, and they show to some extent the comparative mortality of the cutting and the crushing operation in the practice of one in whose dexterous hands all advantages that may belong to lithotritry have certainly been fully developed.

There can now be little doubt that in skilled hands and in properly selected cases of moderate-sized stones, the operation of lithotritry exposes those on whom it should be practised to a somewhat less risk of death than those must incur whose larger stones can only be treated by lithotomy. Each operation has, doubtless, its proper domain; still there are many cases which lie, as it were, on the border line, where it is difficult to decide between the respective claims of the two proceedings to a preference.

It is in view of such cases that those most interested in the matter now need further information, not as to the comparative fatality of the two operations, but as to the comparative condition of the two classes of survivors.

An impression exists, founded, so far as I know, on no sufficient data, that a patient who survives after being cured of stone by lithotomy is, with rare exceptions, secure from a recurrence of his malady, and is relieved henceforth from all his sufferings and discomforts. Whether this be so or not, a very interesting line of inquiry suggests itself—namely, the condition of patients some time (say a year) after the removal of stone, whether by lithotomy or lithotritry. This investigation would be difficult to carry out in the case of private patients, who are free to change their surgeons, and are especially prone to seek fresh advice should their unfavourable symptoms persist or their malady recur. Still with the assistance of the private medical attendants of calculous patients such an inquiry as I have suggested could be made, and it would prove a most valuable addition to our knowledge if those who have the charge of such as have been cured of stone, either by lithotomy or lithotritry, would record the condition of each patient when a full year has elapsed since the removal of the stone.

I am, Sir, yours, &c.,

April 7th, 1875.

THOMAS SMITH.

* THE LANCET, April 3rd, 1875.