SIR,—I have read Dr. Fletcher Shaw's paper on "The present position of the treatment of carcinoma of the cervix" with much interest. Whilst appreciating the compliment Dr. Fletcher Shaw pays to Mr. Victor Bonney and myself in alluding to our work on carcinoma of the cervix, I think when writing a paper with such an important title he really should have taken the trouble to ascertain whether any later figures of ours than those he quotes were available. The reference giving our operability rate as 67 per cent. is taken from the first edition of our *Gynaecological Surgery*, and refers to patients operated upon between 1900 and 1909, that is twelve years ago, whilst the reference quoted second-hand from Professor T. Wilson's article alude to work he published in 1913, nearly nine years ago. As Dr. Fletcher Shaw presumably went carefully into the subject, it is surprising that he is unaware that in 1916 Mr. Victor Bonney and I published in the BRITISH MEDICAL JOURNAL a series of 100 radical operations for carcinoma of the cervix, based on a five years' freedom from recurrence, the first that had appeared in this country. Again in 1920 the same figures are set out at length in the second edition of the *Gynaecological Surgery*. The old figures that Dr. Fletcher Shaw quotes in his paper, so far as our practice and results are concerned, give an entirely erroneous impression of the *present* position of the treatment of carcinoma of the cervix, and it is indeed fortunate that Mr. Victor Bonney's paper appears in the same issue, which will enable your readers to make the necessary corrections.

Figures dealing with the radical operation are of no value unless they include a considerable series of cases which are founded on a three years' freedom from recurrence with reference to the life-prolonging effects of the operation, or on a five years' freedom from recurrence, constituting an "absolute cure" (though Mr. Victor Bonney's paper shows that seven years are really required to make such a claim). In this country my colleague and I are the only surgeons who have published such figures, and I think it is most desirable that others practising this operation should defer publishing their results until they can produce a similar series of cases, as only by such can the real value of the operation be determined.

It may interest Dr. Fletcher Shaw to know that in 1913 we first performed the radical operation on an "inoperable case" made "operable" by radium, the patient being sent to us from the Radium Institute in London. Since then we have from time to time had opportunities of repeating such an operation, and our experience coincides with his, that the dense fibrosis produced by the previous application of radium makes the procedure extremely difficult.—I am, etc.,

London, W., Jan. 2nd. COMYNS BERKELEY.

PEPTIC JEJUNAL ULCER.

SIR,—Your interesting reference to this lesion in an annotation published on December 24th, 1921 (p. 1092) will, I dare say, reawaken in the minds of some, as it has done in mine, the very cogent question: To what extent is the surgical technique of gastro-jejunostomy to blame for the formation of peptic jejunal ulcers? That the lesion may be the direct result of some defect in the method of operating is, I believe, generally accepted; and the more likely is this view to be correct when not only pathologists, but surgeons themselves are willing to acknowledge its probability. Thus, Dr. Georges Loewy, of the surgical clinic of the Salpêtrière, as quoted by you, gives as the first of the three possible causes for the formation of these ulcers, "errors in surgical technique." It has been pointed out that these ulcers may be classified into two sets, those occurring at the line of union of the bowel to the stomach and those limited strictly to the jejunum, that is, distal to the line of suture. It is only with the former that the question of a purely operative cause can be entertained.

Apart altogether from the preventable "errors" committed by inexperienced or careless operators, are there any defects in the details of recognized efficient methods likely to conduce to the formation of these post operative ulcers? As far as I know, there are only two primarily essential methods of accurately uniting together the cut edges of the stomach and the jejunum to form the fistula bimucosa—and, of course, it is assumed that every experienced operator does accurately unite these edges—the one is by the use of a clamp devised specially for the purpose, and the other by simple suture without the aid of clamps. As a matter of operative convenience, and as a means of execution with greater rapidity, the use of clamps is a distinct advantage but this advantage is possibly considerably outweighed by the effect which the clamping of the tissues has in interfering with the proper supply of blood to the edges of the artificial opening; it is more than likely that the clamping of the visceral walls during the process of suturing causes the small vessels passing to the wound edges to become thrombosed, so that the resulting delay in greatly needed rapid repair leaves a weak line of defence against the powerful digestive action of the gastric secretion. It is on similar grounds of interfering as little as possible with the supply of blood to the cut edges that—without the aid of clamps—the too vigorous and multiple employment of forcipressure forceps to bleeding points may crush and devitalize tissues to such an extent that they may be just as easily digested by the gastric juices. It has been in order to avoid these possible devitalizing influences upon the line of suture that, in my own practice, I have always sought to ligature as few vessels as possible along the wound edges, using a continuous suture of absorbable material that embraced all coats of the stomach and bowel, and therefore, at the same time, occluding any bleeding points.

Whether I am right in concluding that the use of clamps may, in the way indicated, be a cause of the formation of that particular class of ulcer occasionally found at the line of suture, could be easily proved if it were possible to ascertain, in any case of the kind that occurred, whether clamps were used at the operation of gastro-jejunostomy. When it is remembered that the existence of these ulcers is usually only made known to us by sudden perforation, and therefore that the case may fall into another hand than that which originally performed the operation, statistics of any practicable value become very difficult, if not impossible, to obtain. We are more or less compelled, therefore, to act on purely theoretical considerations, based, however, on grounds that are both pathologically and surgically reasonable.—I am, etc.,

Glasgow, Dec. 26th 1921. A. ERNEST MAYLARD.

TREATMENT OF ACUTE GONORRHOEA BY ELECTROLYSIS.

SIR,—Without entering into the merits or otherwise of the treatment of acute gonorrhoea by electrolysis I should like to offer a criticism of Mr. Russ's article in the BRITISH MEDICAL JOURNAL for December 31st, 1921.

In the first place he appears to revive the ancient pathology that the gonorrhoeal process as it occurs in the urethra is accompanied by ulceration, and that this is the basis of stricture. It is true, he says, that "probably the use of strong caustic chemicals, either as bougies or as instillations, produces the initial chemical lesion which initiates the ulcer formation."

The urethroscopist knows quite well that ulceration, as ordinarily understood, is a pathological curiosity in gonorrhoeal inflammation of the urethral mucous membrane, caustics or no caustics. The process is a thickening one and not a destructive one. The deposit of young cells called forth by the irritation is Nature's proper response to limit the affection, but in the case of a tube like the urethra the final result may be disastrous, owing to the contraction of the deposit. Nature is more concerned with the immediate arrest of the infection than the final result to the individual.

The superficial thickened layers of epithelium which cover the masses of young cells—the precursors of stricture—necrose and are cast off, but the process does not denude and necrose the subepithelial tissue to form an open ulcer. Correct conception of the pathology will lead to treatment directed to expand the canal and so bring about absorption of the young deposit before its conversion into hard and contracting fibrous tissue (stricture).

The statement that in the series of 500 cases no stricture occurred is not very convincing. Presumably the clinical signs of stricture is meant, as no indication is given that an examination of the canal was made for commencing stricture. It takes many years in most cases for the urethra to be so narrowed by stricture as to give clinical evidence of its presence; when this happens the time for cure is passed. It is indeed difficult to believe that of 213 cases of chronic gonorrhoea which were treated, not one of less than two months' duration, some did not require dilatation treatment. It would be quite contrary to my experience.

Again, with regard to immunity from arthritis and fascial complications in his series of cases, I may say that during