Surgery for Rectal Prolapse
Sir,—I was dismayed to read in your leading article (14 November, p. 381) that the operation of recto-sigmoidectomy for the treatment of prolapse of the rectum “has been abandoned”—as if this were simply a matter of history—as one might say “Murphy’s brain has been abandoned in the performance of bowel resection.”

This recto-sigmoidectomy was the operation devised by Ernest Miles. It was carefully described and illustrated by him in his book.1 He gives his own record of the end results of rectosigmoidectomy (termed by him “Author’s Operation”) as follows: “These, so far as my personal experience goes, have been remarkably good. Out of 34 operations performed by myself there has been only one death. There has only been one instance of recurrence, which occurred five years after the primary operation. Amputation of the prolapsed bowel was again resorted to,” and the reason assigned by Miles for this recurrence is explained. In the second edition of his book in 1944, Miles makes no variation in his descriptions of the operation or of the results. He died in 1947 without ever having recanted his belief in the efficacy of this operation.

Seeking to find the reason for this “abandonment” of his operation (which is supposed to have occurred), I consulted all the references given in your leading article. The only one of them having evidence of the “failure” of Miles’s operation is the report by Mr. N. Porter2 in which he quotes a recurrence rate of the astonishing figure of 58%. This was from 97 operations of which Mr. Porter found 58% had recurred within three years; while those relieved of their prolapse still suffered incontinence—or at least only 10% had restoration of continence among those not suffering recurrence. This record of failure seems almost too bad to be true when contrasted with Miles’s own statement of his personal experience. The difference is so profound that the figures of Miles must give us pause. They surely be some explanation of this gross discrepancy, and it may well lie in the postoperative management. The first measure in after-treatment is so primitive and homely that it may seem tedious to describe in length. It is indeed the cultivation of “perineal shrugging,” which is to say the constant voluntary performance by the patient of contraction of the sphincter mechanism. I contend that this should be taught by the surgeon himself in person and not by a physiotherapist or other party. I would suggest that electrical stimulation and physiotherapy are then unnecessary, but the surgeon, in a situation of this nature, must see that his directions are being properly carried out.

Again, in the matter of postoperative care, obesity must be reduced. A protuberant abdomen is likely to defeat all other measures of defence. Thirdly, alcohol must be forbidden entirely in the beginning. It is so well known that alcohol weakens the anal sphincter that this point needs no emphasis. Heavy beer drinkers must accept this measure for a period of at least a month after operation and preferably for considerably longer, if not permanently.

In four of my patients there had been a former procedure such as the Thiess or the Lockhart-Mummery. The most obstinate case of my own experience was the aftermath of an operation for fistula-in-ano. This deined efforts by at least three surgeons, myself being one (encircling a fascial strip after the manner of Thiess on the supposition that the fluid would be resorbed and I was finally able to give relief (now for three years) by Miles’s operation, reinforced by an encircling kangaroo tendon implanted in the peri-anal tissue. This man also had no less than seven operations for the final relief of a left inguinal hernia and perhaps some constitutional defect of the healing was a factor.

Of course, I shall be asked what is my own total experience and I have to acknowledge that it is really quite small. In an experience of 15 cases of prolapse of the rectum, I performed Miles’s operation 13 times. One patient (aged 67) died three weeks later from cardiac asthma. There was one recurrence (after nine months) for which further operation was necessary—the Roscoe Graham procedure being performed.3 This was a woman of 81 years who had had 12 children and had undergone the second operation. In one case, a nursing trainee still only 20, I performed the Roscoe Graham operation as a first procedure, and likewise in another patient aged 61 years I performed this operation after a second operation. In any case, but the frail and elderly any of the abdominal procedures is likely to be formidable. For several of my patients it would have been quite unnecessarily hazardous.

From this experience, I would consider that the advice to abandon Miles’s operation is quite unjustified. I would be very sorry if this became remeasured after the second operation. In other words, that there are other procedures which may be equally effective (some of them a much more severe ordeal for the patient). I remain convinced that the operation of Ernest Miles still has a place in the management of this condition and hope that you and others will be persuaded to reconsider their opinions in condemning it.—I am, etc.,

ROBERT S. LAWSON
Melbourne, Australia.

2 Porter, N., Proceedings of the Royal Society of Medicine, 1942, 86.

Their statement that “there is no correlation between the size of a tumour or a metastasis and its biochemical activity” is too sweeping. The same is true of: “it is a characteristic feature of endocrine paraneoplastic syndromes that they are associated with small tumours”—the reverse is of course true in the case of hypoglycaemia. Their statement is inaccurate also as applied to hypercalcaemia. Generalizations of this nature cannot hold if whole host and complex disorders serve only to confuse.

Having questioned the case of the hypercalcaemia in our cases, Drs. Von Wichert and Mitchell-Heggs end by stating that no such case of hypercalcaemia was seen among 300 cases of bronchial carcinoma in Hamburg. We are not told how these cases were investigated.—We are, etc.,

J. G. AZZOPARDI
G. POOLE
Department of Pathology, Royal Postgraduate Medical School, London W.12

Scarlet Fever
Sir,—“There was a young lady Ealing Whose friends had so little feeling They refused to receive her When she had scarlet fever. So she paid them a visit when peeling.”

The sting in the last line of this old limerick is the fact that the skin scales during the desquamation phase of the scarlet fever rash are believed to be infectious. This was certainly the teaching when I was a medical student at the end of the 19th century, as one can glean from reading the description of scarlet fever in Clifford Albutt’s System of Medicine (1899). I have never read of any proof that the disease known as “scarlet fever” in those days could indeed be transmitted by the desquamated skin, and I presume that it is now too late to check this old view more scientifically. Nevertheless, this widely-held belief might well have been based on careful observation, so there is a possibility that scarlet fever in the old days was a generalized virus infection which also involved the skin.

Scarlet fever was still a dreaded infection in my childhood, and the incidence of glomerulonephritis after it, often with fatal consequences, was apparently high. When my grandparents arrived in Adelaide in the 1850’s, they had a family of five small children, but two little boys and one girl shortly afterwards succumbed during an epidemic of scarlatina, the cause of death being recorded on their death certificates as “scarlet fever.” Which probably means that they all had post-scarlatinral nephritis. Thus three out of a small family of five children died in infancy from this disease, and their little graves in the Walkerville Cemetery in the Adelaide suburb of that name (like so many other little graves in many cemeteries of that era) are mute reminders of the dangers to children in the good old days.

Is it a heresy to suggest that the scarlet fever in those days was due to a virus and not to a streptococcus? It is generally believed that the glomerulonephritis that may follow the initial infection is due to the remote effects of an exotoxin produced by a streptococcus in the throat, but it is now well-recognized that some

Endocrine and Metabolic Disorders in Bronchial Carcinoma
Sir,—In reply to Drs. Von Wichert and P. F. Mitchell-Heggs (6 February, p. 345) we are not unaware that “bronchial carcinoma frequently metastasizes to bone” and that “tumours which are large . . . are frequently associated with clinically undetected bony metastases”. We would go further—tumours which are small are also unfortunately frequently associated with clinically undetected bone metastases. We made a special effort to exclude bone metastases in our series. The fact that 8 of our 11 cases had thorough necropsies is ignored. But even if we omit this vital fact how do they account for the occurrence of squamous-cell carcinoma specifically, which even they accept. Are they suggesting that oat-cell carcinomas and bronchial adenocarcinomas metastasize to bone less frequently than squamous carcinomas?

2 Porter, N., Proceedings of the Royal Society of Medicine, 1942, 86.

Endocrine and Metabolic Disorders in Bronchial Carcinoma
Sir,—In reply to Drs. Von Wichert and P. F. Mitchell-Heggs (6 February, p. 345) we are not unaware that “bronchial carcinoma frequently metastasizes to bone” and that “tumours which are large . . . are frequently associated with clinically undetected bony metastases”. We would go further—tumours which are small are also unfortunately frequently associated with clinically undetected bone metastases. We made a special effort to exclude bone metastases in our series. The fact that 8 of our 11 cases had thorough necropsies is ignored. But even if we omit this vital fact how do they account for the occurrence of squamous-cell carcinoma specifically, which even they accept. Are they suggesting that oat-cell carcinomas and bronchial adenocarcinomas metastasize to bone less frequently than squamous carcinomas?

Endocrine and Metabolic Disorders in Bronchial Carcinoma
Sir,—In reply to Drs. Von Wichert and P. F. Mitchell-Heggs (6 February, p. 345) we are not unaware that “bronchial carcinoma frequently metastasizes to bone” and that “tumours which are large . . . are frequently associated with clinically undetected bony metastases”. We would go further—tumours which are small are also unfortunately frequently associated with clinically undetected bone metastases. We made a special effort to exclude bone metastases in our series. The fact that 8 of our 11 cases had thorough necropsies is ignored. But even if we omit this vital fact how do they account for the occurrence of squamous-cell carcinoma specifically, which even they accept. Are they suggesting that oat-cell carcinomas and bronchial adenocarcinomas metastasize to bone less frequently than squamous carcinomas?