tion, which tend to disappear when the mechanical disabilities are adequately treated. In a few of my cases I have had opportunity to establish the fact of such reduction by direct inspection.

The degree of certainty with which one can relieve distressing symptoms (both intra- and extra-abdominal) in cases of colonic insufficiency which is not due to ordinary organic disease, and which react unsatisfactorily to medical therapy, is astonishing. It convinces me that these very common developmental abnormalities are the most important factors in the production of such symptoms. In particular, the evacuation of the colon attained is usually so satisfactory that patients speak of the act with enthusiasm!

I am fully aware of the hostile attitude towards surgical treatment of these conditions prevalent in the British Isles and elsewhere. Sir William Arbuthnot Lane, by advocating treatment which in many cases could only be described as disastrous, "queered the pitch" which he had so carefully prepared. The inadequate measures put forward by others, planned to relieve only parts of the trouble, met with the discouraging degree of failure which one would nowadays expect. In spite of all that the results of the treatment which I advocate (gradually evolved over a period of twenty-five to thirty years) make me wish that some other surgeons would fairly put it to the test. There is indeed ample material available. For that reason I have described the operation very fully in my book *The Colon as a Health Regulator* (Macmillan Company of Canada, Toronto—represented by Macmillan and Co., Ltd., London), which was reviewed in the *Journal* of June 19 (p. 1259).

My book is concerned chiefly with the recording of actual facts. The facts in relation to the results of treatment are so encouraging, and in some cases so surprising, that I am hopeful that those who are interested in the subject will read my book before they accept Professor Learmonth's dogmatic condemnation of surgical intervention when faulty colonic function has to be treated. I am convinced as to the efficacy of proper surgical treatment in these colonic cases. The propriety of the treatment is evidenced by the relief which it affords in the matter of local symptoms (both subjective and objective) and in giving a feeling of general well-being previously absent. One can speak, just as Professor Learmonth does, of "dramatic immediate improvement in function," and very few patients fail to maintain that improvement.

Armchair criticism is to be deprecated in the discussion of this matter, as it is in other practical problems. The fact is that very few can speak with justifiable conviction about the full surgical treatment I recommend because they have not tried it! Most of my patients have undergone careful and prolonged medical treatment by various doctors without relief. A recent very miserable one brought to me a dossier of prescriptions, diet sheets, regulations for daily life, etc., more than an inch thick. He had consulted twenty-two doctors, and had been to a special clinic in Paris—without any benefit. Yet within ten days of operation his outlook on life had become completely changed and has remained so. His remarks are exuberant with regard to his general health and the action of his colon! A similarly affected patient, operated on seven years ago, has been seen unexpectedly, owing to an injury, since I decided to write this letter. His enthusiasm concerning my treatment of his abnormal colonic conditions has not lessened, so that the effect of the operation need not be ephemeral, as some destructive critics would have it!—I am, etc.,

Montreal, Aug. 11. HENRY M. W. GRAY.

SIR,—In a note on *The Colon as a Health Regulator* by Sir Henry M. W. Gray (*Journal*, June 19, p. 1259), it is said that one statistical statement "does not accord" with another statistical statement. The first statement refers to the number of deaths—namely, four—in the series of 205 operations, while the second refers to the number of cases in which complete failure has to be recorded—namely, 1 per cent. in the series. As these figures deal with two entirely different computations the question of "accord" does not arise: on the other hand, there is no contrariety between them. The last sentence, "Other claims unsupported by figures may be even wider of the mark," can have no bearing whatever, as your reviewer up to this point has failed to prove anything wide of the mark.

The impression is inescapable that your reviewer indulges in fault-finding in questioning the data of the book, with the evident intention of disparaging this work, but those who know the character and reputation of the author will give no credence to such misleading statements. The really unfortunate result of such "notes," appearing as they do in a publication of the high standing of the *British Medical Journal*, is that they may turn medical men away from a study of the book and thereby, I am convinced, deprive many patients of the often very great and permanent relief that only the treatment outlined in this book can afford. Having had the opportunity of examining numerous patients submitted to the author's operation, I can assure your readers that Sir Henry is distinctly conservative as to the beneficial results attained. —I am, etc.,

W. M. MACNAB, M.B.

Victoria, British Columbia, Aug. 1.

SIR,—In reply to Mr. E. G. Slesinger's letter (*Journal*, August 14, p. 352), we must express our regret at having inadvertently misrepresented his views on the surgical problems of the elongated pelvic colon. In extenuation we can only say that we were misled by the very words which he quotes from his original paper: "The following remarks are therefore not intended to suggest that elongation of the pelvic colon necessarily results in the production of symptoms, but rather to point out that in certain cases such elongation produces results sufficiently severe to call for relief. . . ."

These remarks seemed to us to indicate that Mr. Slesinger believed that certain cases of uncomplicated dolichosigmoid were capable of producing symptoms requiring surgical treatment. While we are in complete agreement with many of Mr. Slesinger's published views, there is, nevertheless, a considerable divergence in our respective attitudes to some aspects of the subject; the chief difference is that we do not believe that dolichosigmoid in the absence of certain complicating factors can ever require surgical intervention.

Our opinion still coincides with that of Mr. Garnett Wright (*Journal*, April 28, 1928, p. 712), that the production of bands on the outer surface of the mesentery is due to plastic peritonitis set up by repeated attacks of partial strangulation, and not to a natural reaction of the body to the strain of an overloaded sigmoid colon. The cases referred to by Mr. Slesinger in his letter to the *Journal* of July 15, 1933 (p. 129), in which a peculiar type of constipation was produced by a prolapse of the loaded pelvic colon and consequent abnormal angulation at the pelvirectal junction, do not appear to us to require surgical intervention. Our own standpoint is that radical surgical measures should be reserved for patients in whom the clinical and radiological features present a characteristic

syndrome, as summarized by us in our letter to the *Journal* of August 7 (p. 293), and that such cases are by no means rare.—We are, etc.,

D. LIGAT.
St. Leonards-on-Sea, Aug. 18. T. D. OVEREND.

Insects and Disease

SIR,—I have read with interest Major-General W. P. McArthur's lecture (*Supplement*, August 14, p. 126), in which he states that the fever which afflicted Cromwell's troops in Ireland was typhus fever. I give you the true facts, leaving your readers to judge whether the disease was typhus fever or imported plague. On January 30, 1649, King Charles I was executed. In July of that year an English ship arrived in Galway with uniforms and supplies for the few hundred Royalist soldiers of Ormond's army quartered there. This vessel was said to carry in those supplies "the contagious infection of the plague in its first appearance in Ireland. . . . Plague broke out immediately." The *Annals of Galway City* (Historical Manuscript Commission, Tenth Report, Appendix, Part V, p. 500, London, Eyre and Spottiswoode, 1885) gives the following:

"That this Toune of Galway, the last year being the mayoralty tyme of Sir Walter Blake, Knight, was visited with the contagious infection of the Plague which continued from July one thousand six hundred and fortie nine till spring after."

"That all members of the toune have met in the country and have consented to a tax of two thousand marckes sterling towardes the charges of physitians and providing of all other necessaries requisit for the purifing and clensing of the said toune which was with all earnestness pursued by these intrusted with that charge."

The city once freed from infection, the inhabitants returned; Galway had lost 3,900, Dublin 30,000, from the plague. But despite all these sanitary methods, Galway City experienced later a second visitation. Colonel Lawrence, an officer of Cromwell's army, in his *Interest of Ireland* (Part II, pp. 86–7) writes:

"Plague and famine had so swept away whole counties, that a man might travel twenty or thirty miles and not see a living creature, man, beast, or bird; our soldiers would tell stories of the place where they saw a smoker, it was so rare to see either smoke by day or fire or candle by night."

Lawrence estimated that five-sixths of the Irish population had perished, an estimate adopted by Prendergast in his *Cromwellian Settlement of Ireland* (1863). The plague ran its course until 1653. Twelve years later, in 1665, it raged in London. General Ireton died of plague in Ireland. One would not expect that he would suffer from louse infection (typhus), and beasts and birds do not die of typhus fever or of "Buchan's malaria." Yet Galway City for nine months successfully resisted the Cromwellian siege, a proof of its powers of resistance, even with the loss of its best fighting men in two visitations of the plague. —I am, etc.,

Athenry, Co. Galway, Aug. 16. C. H. FOLEY.

Injection of Umbilical Vein in Retained Placenta

SIR,—Mr. David Currie, in his paper in the *Journal* of July 10 (p. 58), once more emphasizes the dangers of manual removal of the placenta—45.2 per cent. morbidity and 15.4 per cent. mortality are indeed staggering figures. Since the publication of his first paper (1932), in which he pleaded for a more extended use of cord injection, I have been employing this method with certain modifications for the treatment of retained placenta. Starting at St. Mary's Hospital, London, in that year, I have continued to use this form of treatment in the obstetric unit of Hong Kong University.

The technique is even simpler than that advocated by Mr. Currie, and has been reported elsewhere (1936). All that is required is a 200 c.cm. metal ear syringe. This is filled with warm sterile saline or water. The cord is re-cut and the nozzle of the syringe is pushed into the umbilical vein. One can tell if the nozzle is inserted in the vein, for when it is in this vessel it can be easily pushed down the cord. The nozzle being held firmly in the cord with the left hand, the contents of the syringe are rapidly injected. An assistant refills the syringe and a further 200 c.cm. are quickly introduced. Following this, Credé's manœuvre is attempted. In one case 800 c.cm. were injected without any deleterious effect before the placenta was expressed. One of the causes of failure of this treatment is the presence of a contraction ring.

The method as outlined above is so simple that even a midwife can undertake it. There are occasions when medical aid is not at hand and a midwife has to deal promptly with a major emergency. Rather than wait till the doctor's arrival and severe exsanguination of the patient, the midwife could at least try cord injection before putting her hand in the uterus to remove a retained placenta. In the Tropics manual removal of the placenta is even more hazardous than in England, for many of the patients who come to hospitals for their deliveries are suffering from a marked degree of secondary anaemia, malnutrition, infections, etc.

Lack of conservatism in obstetric practice is now recognized as a contributory factor in the high maternal mortality which is found in certain countries. Mr. Currie is to be commended for reviving a conservative method of treatment which has been allowed to remain in desuetude for a century.—I am, etc.,

Hong Kong, Aug. 6. W. C. W. NIXON.

Currie, D. W. (1932). *Lancet*, **1**, 1087.
Nixon, W. C. W. (1936). *Chinese med. J.*, **50**, 1832.

Tennis Elbow

SIR,—In the past the term tennis elbow was used in rather a vague sense, and many authorities included under it a heterogeneous collection of disabilities in the region of the elbow-joint and forearm. In order that the results of treatment may be of more comparative value it is necessary to have a standard definition of the term. Tennis elbow must, in virtue of its name, stand for the commonest cause of disability in the elbow of a tennis player.

According to Mr. G. P. Mills (*Journal*, July 31, p. 212) the commonest condition in his series of fifty-eight cases was one characterized by radio-humeral tenderness, presumed to be due to the nipping of the frayed edge of the coronary ligament. He does not mention what proportion of the cases were tennis players, however. In my own experience another condition is responsible for the majority of cases of tennis elbow, and is best designated as traumatic periostitis at the origin of the extensor carpi radialis brevis muscle. Both conditions may occur, and often do occur, in people other than tennis players. Mr. Cyriax, in the *Journal of Bone and Joint Surgery* (October, 1936) points out no fewer than twenty-six pathological conditions that have been described as responsible by various authors, all of which goes to show that tennis elbow is an incredibly bad term. Nevertheless, in order