

orthopaedic patients receiving antibiotics.<sup>1,2</sup> The doubling of incidence of pseudomembranous colitis in Birmingham between 1976 and 1977 was probably due to increased diagnostic vigilance.<sup>3</sup>

The word "invariable" has no place in nosographic data processing and accordingly your statement that "diarrhoea is invariable" requires scrutiny. While it is true that the colon is predominantly involved, the ileum can also, to a varying degree, be the seat of the disease. When the colon is completely spared—as in a recent case confirmed by necropsy—diarrhoea can be absent. Clinically this case was dominated by abdominal pain, fever, and leucocytosis. A somewhat similar case was also reported recently.<sup>4</sup> In such cases sigmoidoscopic and proctoscopic examinations—usually useful methods for establishing the diagnosis—cannot be relied upon.

Bearing in mind the topography of the pathological changes, I believe that diagnostic alertness might be enhanced by reverting to the original terminology—namely, pseudomembranous enterocolitis.

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<sup>1</sup> Smart, R F, *et al*, *British Journal of Surgery*, 1976, **63**, 25.

<sup>2</sup> Beavis, J B, *British Journal of Surgery*, 1976, **63**, 299.

<sup>3</sup> Kappas, A, *et al*, *British Medical Journal*, 1976, **1**, 675.

<sup>4</sup> Dane, T E B, and King, E G, *British Journal of Surgery*, 1976, **63**, 305.

### Intravenous diazepam and Cyclimorph

SIR,—I have recently returned from Bridgend, where a brief visit enabled me to observe the use of intravenous diazepam and Cyclimorph (morphine and cyclizine tartrates) in the casualty department and operating theatre as referred to by Mr A W Fowler (22 October, p 1088). I was impressed with the convenience, simplicity, and effectiveness of the method and have no reason to doubt Mr Fowler's assurance of its safety in many hundreds of cases. I would also endorse his choice of the more extensive use of local analgesia (27 August, p 576), a procedure I have been practising many years myself.

Where does this leave the anaesthetist now, I wonder. Some are calling for nurse-anaesthetists (why not nurse-surgeons or clerk-administrators?) while others are taking matters into their own hands so that new areas of anaesthetic work are developing without the guidance of those most qualified to exploit fully the potential of new methods and drugs. For my own part I confess to being a little uneasy on this score, the result of having been rebuffed at times for being too unconventional (or, perhaps, just in advance of current thinking), but it is important for the future of the specialty, and in the long run for patients, that junior anaesthetists receive a complete training. If there are simple methods for dealing with casualties with full stomachs, let us use them more!

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### Treatment of Q fever

SIR,—Dr J R Pinto (10 December, p 1542) is quite correct when he states that in our paper on chronic Q fever<sup>1</sup> we did not mention the use of co-trimoxazole, but we did refer to the survival of the one patient who had

received co-trimoxazole.<sup>2</sup> However, we felt that from the literature available compelling evidence of the effectiveness of antibiotics other than tetracycline and chloramphenicol was lacking. Even in the case reported by Freeman and Hodson tetracycline had not only antedated but had also been continued during the period of co-trimoxazole administration.

I agree that further assessment of co-trimoxazole in the treatment of Q fever is necessary. Following the preparation of our paper there appeared a further report of the use of this drug in Q fever endocarditis, on this occasion associated with glomerulonephritis possibly due to an immune-complex mechanism. Despite co-trimoxazole therapy the patient survived for only three months after diagnosis.<sup>3</sup> Reports of success in the treatment of Q fever endocarditis just one year after withdrawal of antibiotic therapy, although promising, should be accepted with caution. Other patients have died 16 months<sup>4</sup> and almost two years<sup>5</sup> after prolonged courses of tetracycline.

In our experience lincomycin in combination with tetracycline seemed to be more effective in controlling the disease than tetracycline alone. The patient on whom this regimen was originally employed received these antibiotics alone for just over five years. Five years after stopping antibiotic therapy she is alive and in good health, the longest survivor so far reported.<sup>6</sup>

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<sup>1</sup> Turck, W P G, *et al*, *Quarterly Journal of Medicine*, 1976, **45**, 193.

<sup>2</sup> Freeman, R, and Hodson, M E, *British Medical Journal*, 1972, **1**, 419.

<sup>3</sup> Dathan, J R E, and Heyworth, M F, *British Medical Journal*, 1975, **1**, 376.

<sup>4</sup> Tunstall-Pedoe, H D, *British Heart Journal*, 1970, **32**, 568.

<sup>5</sup> Meyers, D, *British Medical Journal*, 1968, **4**, 771.

<sup>6</sup> Matthews, M B, personal communication.

### A misleading name?

SIR,—I think it important to bring to your attention and that of your readership the appearance on the market of a compound entitled Napsalgesic.

It may appear to the general practitioner that this is a mixture of Naprosyn (naproxen) and Distalgesic. In fact it is nothing of the sort and contains dextrapropoxyphene 50 mg and aspirin 500 mg.

It is possible that there will be major confusion, because practitioners who are satisfied with Naprosyn may feel that this new compound is better than that drug, whereas in fact it is something entirely different. I think this should be pointed out.

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### Prediction of gangrenous and perforating appendicitis in children

SIR,—Following Mr D F Graham's publication (26 November, p 1375) on the prediction of gangrenous and perforating appendicitis we have applied his "clinical scoring index" to 26 children recently treated at this hospital with proved acute appendicitis. As mortality is related to peritonitis at the time of operation<sup>1</sup> such a prediction might be of clinical value,

and Mr Graham does not exclude children nor does he state an age range for his prospective series of 257 patients. A definition of perforating appendicitis is not included in his paper, but our patients were divided into three groups: six patients aged 3-14 years in group I with a gangrenous or macroscopically perforated appendix, five patients aged 8-12 in group II with purulent peritoneal fluid but no macroscopic perforation, and 15 patients aged 6-14 in group III with acute appendicitis but either serous or negligible peritoneal fluid. The presence of each of the following features scored one point: lower abdominal pain at onset, history exceeding 24 h, pain increasing in severity, presence of guarding and rebound tenderness, temperature over 37.5°C, pulse rate greater than 90 beats/min, and a white cell count exceeding  $14 \times 10^9/l$  ( $14\,000/mm^3$ ), giving a maximum score of 7.

The scores in group I ranged from 3 to 7 and in group II from 3 to 6. In group III only 4 of the 15 patients had a score of 2 or less and as many as 11 scored 3 or more, which, Mr Graham claims, suggests perforation or gangrene; two of these scored 4 and two 5. In particular, three of his seven features were very poor discriminants—pain increasing in severity, pulse rate greater than 90/min, and a white cell count exceeding  $14 \times 10^9/l$ . Only one of the six children with a macroscopically perforated appendicitis complained of increasing pain. Of the 26 children 80% had a pulse rate of over 90/min and there was no significant difference between the three groups. The white cell count was over  $14 \times 10^9/l$  in 60% of group III but only 54% of groups I and II.

We accept that 26 is a small sample, but our conclusion is that this "clinical scoring index" has been of no value in distinguishing a group of children with perforated or gangrenous appendicitis.

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<sup>1</sup> Pledger, H G, and Buchan, R, *British Medical Journal*, 1969, **4**, 466.

### Bromocriptine in presenile dementia

SIR,—We note with interest the article by Dr Christopher Lewis and others concerning the beneficial effects of levodopa in senile dementia (4 March, p 550). Mesocortical dopaminergic neurones are known to exist, and derangement of these may occur in Alzheimer's disease. However, levodopa has been used with conflicting results<sup>1,2</sup> and patients with Parkinsonism and dementia usually deteriorate because of drug-induced psychiatric side effects. Improvement in intellectual function following levodopa has been reported in a few other studies<sup>3</sup> and the possibility that bromocriptine, a dopamine receptor agonist, might have an advantageous and selective effect on mesocortical and mesolimbic systems prompted the present study.

Nine patients with early presenile dementia presumed to have Alzheimer's disease (mean age 64, mean duration of disease 3 years) took part in a single-blind trial. Higher cortical function was assessed by bedside clinical tests and by the Gresham questionnaire (for orientation, memory for past, and personal