Upper gastrointestinal endoscopy

SIR,—While we would agree with the views of Mr V Moshakis (14 April, p 1014) concerning the inadequacies of the conventional barium meal, we disagree with his assertion that endoscopy should be the primary investigation of suspected upper gastrointestinal disease. That conclusion certainly cannot be drawn from our article "Upper gastrointestinal endoscopy: its effect on patient management" (24 March, p 775), to which he refers.

The population we studied was a selected one. There is no evidence that primary use of endoscopy in patients with dyspepsia would usefully increase the yield of treatable disease, and indeed the paper "Open-access endoscopy service for general practitioners" by Dr G Holdstock and others (17 February, p 457) showed no objective benefit as a result of such a policy.

We wish to reiterate our statement that the result of any study of this type "must reflect the degree of selection used in referring and accepting patients for the procedure." It would be unfortunate if our conclusions were misinterpreted as justifying a policy of indiscriminate endoscopy. Radiology as practised to the high standard reached by the Japanese, and increasingly approached in this country, offers a high degree of accuracy, although it can never provide histological material. Where good radiology is available it would be at the very least premature to replace it by endoscopy as the primary investigation, even if this were practicable. There could be a case on grounds of cost and accuracy for the use of endoscopy without a preceding barium meal in some dyspeptic patients, perhaps those over the age of 45 in whom treatment changes as a result of investigation are more likely. This must remain a hypothesis until it is tested.

C D HOLDSWORTH

Hallamshire Hospital,
Sheffield S10 2JF

G E SLADEN

Department of Gastroenterology,
Guy's Hospital,
London SE1 9RT

K D BARDHAN

Rotherham District General Hospital,
Rotherham S60 2UD

G V BALMFORTH

Doncaster Royal Infirmary,
Doncaster DN2 5LT

R A DIXON

Department of Community Medicine,
University of Sheffield Medical School,
Sheffield S10 2RX**Falciparum malaria despite chemoprophylaxis**

SIR,—I fully endorse the views of Drs F J Nye and H E Parry on "Falciparum malaria despite chemoprophylaxis" (21 April, p 1091). It is imperative to maintain a high index of suspicion for this disease in all expatriates working in or returning from West Africa who have a febrile illness. I have just returned from Oyo State in Western Nigeria, where I worked for three years. The majority of expatriates (Americans excluded) take pyrimethamine, 25 mg weekly. It is still a favourite prophylactic and is encouraged by many Nigerian doctors. I have treated at least 20 expatriates with falciparum malaria who had taken the drug regularly, and I myself can be added to

this group. All had resided in Nigeria for at least four months before developing symptoms, which were often insidious in onset, supporting the view of partial suppression of the parasitaemia. However, in some (myself included) there was a classical onset of rigors and high fever. One other patient developed falciparum malaria while taking paludrine, 100 mg daily, regularly.

Personally I would not advocate the use of pyrimethamine alone in Nigeria. My choice of prophylactic would either be Maloprim (combining 12.5 mg of pyrimethamine with 100 mg of dapsone) or chloroquine, 300 mg of base weekly. I never saw a case of malaria on either of these regimens.

S J BENTLEY

Department of Medicine,
Withington Hospital,
Manchester M20 8LR**Continuous lumbar epidural analgesia for labour and delivery**

SIR,—We wish to make two comments regarding Dr J Selwyn Crawford's review paper on continuous lumbar epidural analgesia for labour and delivery (13 January, p 72).

Firstly, there is the author's statement that "the outstanding benefit of abolishing the bearing-down reflex and of pelvic floor relaxation is in cases of breech presentation . . ." (p 73), with which we cannot agree. Since our goal is to promote spontaneous delivery (Bracht's manoeuvre) for this type of malpresentation, we are anxious to preserve the bearing-down powers as much as possible; and this is certainly not attained when an effective second-stage epidural has abolished the bearing-down reflex and has relaxed or even paralysed the abdominal musculature. Furthermore, the pelvic floor relaxation induced by the epidural is not a substitute for a generous episiotomy, which is a must in breech delivery.

The second point we wish to raise is that the favourable influence of epidural anaesthesia on the acid-base balance of the fetus is in fact a fallacy. True, an epidural block reduces the maternal metabolic acidosis; but the fetus lags behind, whereas it should—at least under normal conditions—adjust itself or follow its mother. We compared two groups of elective labour inductions, one conducted without analgesia or anaesthesia (control group) and the other under continuous epidural block (0.25% bupivacaine without adrenalin) and found that there was no clear-cut difference in the acid-base state of the fetus even though "blocked" mothers were less acidotic than the controls. Obviously, the relationship between the mother and the fetus is changed by the regional anaesthesia, and the fetal-maternal differences—that is, the true indices of fetal well-being—are significantly less favourable in patients given an epidural. That both the anaerobic metabolism (fetal-maternal difference in excess lactate) and the CO₂ gradient across the placenta are found to be increased indicates that the fetus of a blocked woman suffers a slight but nevertheless significant degree of hypoxia and hypercapnia as a result of the epidural block. In a first series of cases¹⁻³ our subjects were free to choose the posture (recumbent or lateral) they considered the most comfortable. Because, theoretically, cava compression due to an inappropriate posture in labour might be responsible for the impairment of the fetal haemostasis a second series was assessed, in which the women were not

allowed to lie on their back except during the delivery proper (to be published). Although the postulated effect of compression of the caval vein could not have occurred, the results were identical to those obtained in our first series.

It is not our contention that these findings warrant restriction of properly conducted continuous epidural analgesia for normal labour and delivery. Provided the mother and fetus are continuously monitored and the obstetric and the anaesthesiological staff are experienced, the slight added risk of the epidural is amply compensated for by the benefits of this type of analgesia. However, the fetal hypoxia and hypercapnia triggered by the epidural, although minimal, should be taken as a warning and lead us to be cautious with epidural analgesia whenever the fetus is at risk with respect to its oxygen supply.

ROBERT DEROM
MICHEL THIERY

University Department of Obstetrics

GEORGES ROLLY

University Department of Anaesthesia,
B-9000 Ghent, Belgium

¹ Derom, R, Thiery, M, and Rolly, G, *Excerpta Medica International Congress Series*, No 396. Amsterdam, Excerpta Medica, 1976.

² Vroman, S, et al, *European Journal of Obstetrics and Gynecology and Reproductive Biology*, 1977, 7, 159.

³ Thiery, M, et al, *European Journal of Obstetrics and Gynecology and Reproductive Biology*, 1977, 7, 181.

Increased eosinophil count in operating theatre personnel

SIR,—Recently attention has been paid in the *BMJ* (24 March, p 779) to the health problems of anaesthetists. An uninvestigated area, however, is the effect of operating theatre work on the workers' haematological state. Prolonged anaesthesia leads to bone marrow depression and changes in the leucocyte and differential counts.¹⁻³ Even subanaesthetic concentrations of anaesthetics lead to bone marrow injury.^{4,5} Operating theatre personnel chronically exposed to trace concentrations of anaesthetics⁶ may therefore have changes in their haematological state.

In an operating theatre we studied 10 healthy nurses and doctors for sedimentation rate, haemoglobin concentration, haematocrit, erythrocyte count, and leucocyte and differential counts. They had been regularly employed in operating theatre work for an average of 5.6±4.2 (SD) years. Nitrous oxide was the main inhalational anaesthetic used. Diethyl ether, halothane, and methoxyflurane were used only occasionally. The mean nitrous oxide concentration in the operating rooms was 560-860 ppm (parts per million, v/v) in the morning and 155-460 ppm in the afternoon, and 70 and 20 ppm respectively in the recovery room, measured by gas chromatography.⁷ After three hours' work the nitrous oxide concentration in the end-expiratory air of the test subjects was 31±16 ppm. The controls were 12 healthy members of the hospital staff outside the operating theatre. The venous blood samples taken after fasting overnight were processed by standard laboratory techniques. The leucocyte count was performed by a Coulter S counter and the differential count by counting at least 200 cells. Because the material was not normally distributed, for statistical analysis the non-parametric Mann-Whitney U-test was used.

The sedimentation rate, haemoglobin concentration, haematocrit, and erythrocyte and leucocyte counts were all within normal range without differences between the groups. The proportion of eosinophils was, however, greater in the operating theatre workers than in the controls. When the total eosinophil count was estimated by multiplying the leucocyte count by the proportion of eosino-

phils, the theatre staff had $221 \pm 99 \times 10^6/l$ eosinophils and the controls $79 \pm 63 \times 10^6/l$ ($P < 0.001$). When those with any kind of allergic history were excluded, a statistically significant difference remained between the groups. When the leucocyte and differential counts were checked six months later, the leucocyte count was $4900 \pm 780 \times 10^6/l$ in the theatre staff and $4500 \pm 1090 \times 10^6/l$ in the controls. The proportion of eosinophils was 0.04 ± 0.02 in the theatre staff and in the controls 0.02 ± 0.02 ($P < 0.01$), and the absolute eosinophil counts were $206 \pm 119 \times 10^6/l$ and $96 \pm 72 \times 10^6/l$ respectively ($P < 0.05$). Summer holidays of one month in the theatre staff had no effect on the leucocyte and eosinophil counts. However, when the direct chamber method of Randolph⁷ was used, the eosinophil count in the theatre staff was $132 \pm 68 (\times 10^6/l)$ and $94 \pm 67 (\times 10^6/l)$ in the controls ($P > 0.05$).

To verify the previous finding of the increased eosinophil count in the theatre staff of Loimaa Regional Hospital, we studied the operating theatre personnel of Turku University Central Hospital. They included 125 nurses and doctors (117 women and eight men). The controls were 70 nurses from the wards. The proportion of eosinophils, 0.03 ± 0.02 in the theatre staff, was higher than in the controls (0.02 ± 0.02) ($P < 0.05$); but the difference in the absolute numbers of eosinophils ($173 \pm 147 \times 10^6/l$ and $144 \pm 122 \times 10^6/l$, respectively) was not statistically significant.

This study suggests that operating theatre work increases the proportion of eosinophils. The same observation has been made by Werthmann,⁸ but in that study the eosinophil count decreased after six weeks' leave. The clinical significance of this finding remains to be established.

MATTI SALO

Department of Anaesthesiology,
University Central Hospital of Turku

PAULI PUUKKA

Computing Centre,
University of Turku

ALLAN RAJAMÄKI

Department of Haematology,
University Central Hospital of Turku

SEIJA AANTAA

Occupational Health Service for
Personnel of Turku University
Central Hospital
Turku, Finland

¹ Amess, J A L, *et al*, *Lancet*, 1978, 2, 339.

² Lassen, H C A, *et al*, *Lancet*, 1956, 2, 527.

³ Green, C D, and Eastwood, D W, *Anesthesiology*, 1963, 24, 341.

⁴ Bruce, D L, and Koepke, J A, *Anesthesiology*, 1966, 27, 811.

⁵ Kripke, B J, *et al*, *Anesthesiology*, 1977, 47, 342.

⁶ Randolph, T G, *Journal of Laboratory and Clinical Medicine*, 1949, 34, 1696.

⁷ Nikki, P, *et al*, *Annals of Clinical Research*, 1972, 4, 266.

⁸ Werthmann, H, *Beiträge zur klinischen Chirurgie*, 1949, 178, 149.

Whooping cough in Shetland

SIR,—In confining his "experience" to the age group 1-4 years, Dr J D MacGregor (28 April, p 1154) is, like Dr Douglas Jenkinson (19 August 1978, p 577), selecting his data. Their overall attack rates are similar (21% and 19% respectively) but the attack rates in unvaccinated children are very different (27% and 79%). Therefore there must be differences other than vaccine protection between their selected samples. Since Dr MacGregor's sample derives from notifications, it presumably includes data from Shetland already reported to you by Dr R K Ditchburn,¹ which show an overall attack rate of 55% in the age group 1-4 and 48% in all age groups.

The truth is, as Fisher² showed in 1935, that data extracted from less than the whole of

the data are misleading if the data relate to mutually exclusive events. In ascribing significance to the differences between the attack rates in vaccinated and unvaccinated children, Drs Jenkinson and MacGregor both assume that prior probabilities of being infected are equal. Their data provide no evidence of this. If one pays attention to the origin of the 1977-8 outbreak in Shetland, as I was able to do through the kind co-operation of Drs MacGregor, Ditchburn, and others during a brief visit early in 1978, it becomes clear that the outbreak began in October 1977 in Scalloway and Walls in schoolgirls aged 8-16, most of whom had been vaccinated. Thereafter it spread to younger children and to other parts of Shetland. A total of 88 cases were notified between October 1977 and the end of that year and it is a matter of record in Lerwick that 38 (43%) of these had been fully vaccinated. Only 21 of these cases were in the 1-4 age group, but it is by no means certain that all cases in this age group were notified, nor do we know whether there was equality of exposure among them. The occurrence of whooping cough in vaccinated children can legitimately be read, other things being equal, as evidence that protection is incomplete because exposure has obviously occurred; the non-occurrence of whooping cough in vaccinated children indicates protection only when there is proof of exposure and, during the same period, occurrence of whooping cough in unvaccinated children.³

G T STEWART

University Department of
Community Medicine,
Glasgow G20 9NB

¹ Ditchburn, R K, *British Medical Journal*, in press.

² Fisher, R A, *Journal of the Royal Statistical Society*, 1935, 98, 39.

³ Stewart, G T, *Lancet*, 1977, 1, 234.

Useless drugs

SIR,—It was sad to read the report in Minerva's column (7 April, p 961) of out-of-date and useless drugs being sent to Upper Volta.

The Edinburgh Medical Missionary Society has been operating a "Medicines for Overseas" scheme since 1969 and every item is carefully checked before being packed in 45-gallon (203-litre) steel drums. We have been most grateful to doctors, pharmacists, and others who have enabled us to send abroad an ever-increasing supply of drugs and medical supplies to the third world.

Colleagues in the Edinburgh area can hand in supplies to our headquarters at 14 Mayfield Terrace at any time (even weekends), where they will be gratefully received. We would emphasise that liquids and out-of-date drugs should not be passed on to us.

MARJORY A KEITH

Edinburgh Medical Missionary Society,
Edinburgh EH9 1SA

SIR,—In 1977 when on holiday in Iran I was walking through the bazaar in Hamadan, a city in the north west of that country. My eye fell on a large tea chest full of little glass phials. These had brightly coloured labels and some had equally colourful contents. Closer inspection made it clear I had stumbled on a large cache of time-expired parenteral preparations—mostly antibiotics, mostly of

American origin, and all for sale. The stallholder was doing a brisk trade but became very excited when I tried to take a photograph and broke off his sales patter to get rid of me.

I imagined that he had collected this treasure trove from some clinic dustbin; but the variety of preparations and their apparent lack of application in such a country (a few bottles of gentamicin being the most obvious example) made me think that here was a charity "job-lot"—hence my interest in Minerva's note about the time-expired drugs arriving in Upper Volta (7 April, p 961).

Presumably then, this is the end of the line for such drugs—but I would be most interested to know what the purchasers would do with them. Drink them? Wear them on a necklace? The current state of American popularity in Iran makes me wonder whether my unfriendly stallholder is still in business.

ANDREW BAMJI

London W1P 1FP

Aetiology of appendicitis

SIR,—May we comment on the letter by Mr F T de Dombal and Dr A H Hedley (24 March, p 820), who strongly criticised the view of Mr D P Burkitt and others that "the geographical distribution of appendicitis indicates that it is a disease associated with modern Western culture" (3 March, p 620)? The latter claim simply that while the disease is common in Western populations it is far less common or even rare in primitive or developing populations—a view with which we agree, knowing of no accurate prevalence data to the contrary.¹

Mr de Dombal and Dr Hedley listed 11 references relevant to the appendicitis experience of less privileged populations, from which they concluded that appendicitis is indeed far from rare. Unfortunately, only six of the references are currently available to us. Their reference to Indonesia² is incongruous, since the authors of the particular paper emphasised that in the city population studied appendicitis rate was "low for a community which has quite easy access to medical care." Actually, the rate is very low, only about double that of Africans living in Soweto, Johannesburg. Their reference to the experience of Africans in Nairobi³ is also inapposite. Of 1351 patients presenting with the principal complaint of abdominal pain, appendicitis was diagnosed in only 16 cases (1.2%). The authors stressed that "the low incidence of conditions comprising the acute surgical abdomen deserves note." In the references to series of patients in Nigeria,⁴ Khartoum,⁵ Basra,⁶ and Dhahran⁷ no information was given on the sizes of the populations from which the series of patients were drawn. The papers mentioned scarcely afford unequivocal evidence that there are little differences in appendicitis rates between Western and non-Western populations.

In Africans in South Africa the disease is certainly very uncommon.¹ As an example of evidence, in 1977, among 90 000 mine workers employed by Goldfields of South Africa Ltd, there were 24 cases of appendicitis—that is, 26 per 100 000.⁸ In contrast, in a Western population of somewhat similar age structure—namely, US naval personnel in the last war—the annual incidence rate was calculated to be approximately 850 per 100 000.⁹

With respect, may we make a plea? Rather than employing entirely inadequate evidence to criticise a hypothesis, would it not be more helpful to present much-needed information on the epidemiology of appendicitis as it currently occurs in Western populations? This would at least provide appropriate comparative information for the