imagining himself to be volumes and so years ahead of the rest of us is that you have erroneously imputed such notions to him; though as between the possibility of the editor of *World Medicine* having delusions of grandeur and that of the editor of the *British Medical Journal* ignoring convention I should prefer not to speculate. Perhaps next time, however, you will put your editorial heads together and so avoid supplying an occasion for their being banded together later.

JOHN S BRADSHAW

How Caple,
Hereford

¹ Fowler, H W, *A Dictionary of Modern English Usage*, 2nd edn, revised E Gowers. London, Oxford University Press, 1965.
² *British Medical Journal*, 1976, 1, 6.

Possible immunological effect of oxprenolol

SIR,—I am writing about a possible adverse reaction to oxprenolol (Trasicor).

A 40-year-old male patient of mine who has severe hypertension is taking oxprenolol and polythiazide. Two weeks after his son's attack of chickenpox he and his daughter developed chickenpox. While the children's illness was of average severity his, however, was of a quite extreme degree.

I reported this on a yellow card to the Committee on Safety of Medicines and in an admirably prompt and courteous reply they mentioned that they had had one previous report of an immunological abnormality in a person on oxprenolol—a woman who developed lupus erythematosus after being on oxprenolol for two weeks.

In view of the fact that people on practolol may have developed immunological abnormalities it is perhaps worth wondering if beta-blockers in general may affect the immunological responses. It would be interesting to hear if any other readers have experienced similar problems.

L F W McMAHON

Whitchurch, S Glam

Cimetidine and ulcers

SIR,—I read your leading article on this subject (27 November, p 1275) with interest. I thoroughly concur that cimetidine is no panacea for all dyspepsia, and share your concern about the possibility of indiscriminate use. The "undeserved ill repute" you mention as a possible consequence of this may also, however, follow incorrect use of the drug in perfectly conventional indications. Confusion may arise on points where your recommendations appear to be at variance with those of the data sheet for the product. As the data sheet recommendations are based strictly on the terms of the product licence under which cimetidine is released for routine clinical use, some comments may be useful.

Firstly, the impression is left that the timing of doses of cimetidine is not important. I should emphasise that the recommended dosage for most indications is 200 mg three times a day *with meals* and 400 mg *at bedtime*. This regimen was used in most of the clinical trials and should therefore lead to predictable results. Secondly, I am puzzled as to the source of your recommendation for two months' treatment, when a month to six weeks

has proved sufficient to allow healing of most duodenal ulcers and evidence on two months' treatment is scanty. I was also surprised by your abrupt dismissal of the evidence concerning the role of cimetidine in the treatment of reflux oesophagitis, one of the main permitted indications listed in the data sheet. Although this evidence may concern small numbers as yet, it is not lacking in quality.

Your recommendation concerning monthly serum creatinine estimations raises the question of what a doctor should do if he finds an increased concentration. The rises in serum creatinine reported in trials were not generally considered clinically significant; patients continued treatment with no demonstrable ill effect. Cimetidine was withdrawn in only three cases for this reason; two of these patients had pre-existing renal disease and one had an abnormal creatinine level before starting treatment. This test would seem, therefore, to be of little practical value for routine use.

Finally, you may be interpreted at one point as implying that a considerable hazard attends the use of cimetidine. No such hazard has been detected, but this should not detract from the desirability of all doctors reporting any possible adverse reactions they see, as this is the only way in which the hazards, or lack of them, of long-term and widespread use can be defined.

E M JACKSON-MOORE

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Welwyn Garden City,
Herts

Screening for breast cancer

SIR,—It was with much interest that I read the report by Mr W D George and others from Manchester (9 October, p 858) of the feasibility of conducting a screening programme for breast cancer using non-medical staff to carry out clinical and mammographic examinations.

If introduced, any such mass screening programme aiming to improve substantially the long-term prognosis of this disease must be capable of detecting the majority of cancers while the primary is still under 2 cm in greatest diameter, when 10-year survival rates of 80-90% may be obtained.¹ Thus the ability of non-medical staff to detect cancers at this stage must be assessed, since the methods being used are capable of doing this in medical hands, as has been demonstrated at West London and Guildford.^{2,3} The staging of the cancers detected at Manchester would therefore be most helpful in assessing the performance of the non-medical examiners, since it is possible that there could be differences between the groups in their detection rates of T₁N₀ cancers even if both detected more advanced cancers.

Presumably the "referred women" at Manchester are equivalent to the usual outpatient referrals and contain a very high proportion of later cancers. Experience at Guildford suggests that of the cancers detected in women attending the screening clinic, those occurring in symptomatic women are mainly over 2 cm in diameter with or without nodal involvement, while the majority of those detected in asymptomatic women are under 2 cm in diameter and node-free, and represent the true screening cases in which most benefit is to be expected.

Considering the Manchester results, in practice "referred women" are seen by a medical team. Of the "invited" and "self-referred," the 782 symptomatic women having 14 cancers represent a group which should ideally consult their own doctors and these cancers are, at least potentially, detectable by a health education programme without sophisticated screening as an initial stage. It is therefore only the nine women having cancers out of the 1944 asymptomatic "invited" and "self-referred" who really benefit from the screening procedure and who are most likely to have the early lesions.

Consideration of the use of non-medical staff for screening clinics must surely depend largely upon their ability to detect such cases. Here the Manchester data seem incomplete, since we are told only that the non-medical staff detected among the "invited" women, both symptomatic and asymptomatic, 10 out of 16 cancers clinically and 13 out of 16 radiologically, eight of the 16 being symptomatic. The most pessimistic interpretation of these data means that the non-medical staff could have missed three or possibly four (if they missed the only cancer in the asymptomatic "self-referred") of the nine cancers previously defined. This would presumably not be acceptable. Clearly the full data are needed, though even then any judgment based on only nine cases seems premature.

On the other hand there is clearly a high accuracy when non-medical staff assess referred women. Follow-up will be of course of interest to establish any false-negative rate of medical or non-medical staff.

BARBARA A THOMAS

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¹ Cutler, S J, in *Prognostic Factors in Human Breast Cancer*, ed A P M Forest and P B Kunkler, p 20. Edinburgh, Livingstone, 1968.
² Chamberlain, J, *et al*, *Lancet*, 1975, 2, 1026.
³ Thomas, B A, *Lancet*, 1975, 2, 914.

Thoracic duct cannulation for bleeding oesophageal varices

SIR,—In your leading article on "Bleeding oesophageal varices" (11 September, p 603) it was suggested that the least traumatic operation should be performed in order to control the bleeding.

Such a minor surgical procedure, not listed in your article, is cannulation of the thoracic duct and lymph drainage. This operation, carried out under local anaesthesia, was first suggested by Dumont and Mulholland.¹ They demonstrated that in cirrhotic patients cannulation of the thoracic duct and establishment of free lymph flow may reduce portal pressure, decrease ascites and liver size, and control bleeding from oesophageal varices. Since 10 out of 15 patients who bled massively from oesophageal varices stopped bleeding after cannulation of the thoracic duct through a supraclavicular approach it was concluded that this procedure was as effective as a portacaval shunt in decreasing the portal vein pressure and controlling the oesophageal bleeding. A similar enthusiasm was expressed by Bowers *et al*,² who proposed a portacaval shunt operation within 72 h after bleeding has been successfully controlled by cannulation of the thoracic duct.

This simple surgical procedure has been

reported to arrest haemorrhage from oesophageal varices in a proportion of cases for periods ranging from hours to months,¹⁻⁶ but haemorrhage recurred in some patients when the cannula was removed.^{2,4,6}

The effectiveness of thoracic duct cannulation for bleeding oesophageal varices is still a controversial matter. In spite of this, I think that the policy that should be adopted is to perform cannulation of the thoracic duct in patients not fit for major emergency operation. If this minor palliative procedure succeeds in preventing further bleeding the patient can be better prepared for a portacaval shunt operation that can be conducted a few days after his general condition has improved.

DAN ADERKA

Acco,
Israel

¹ Dumont, A E, and Mulholland, J H, *Annals of Surgery*, 1962, **156**, 668.

² Bowers, W F, et al, *Journal of the International College of Surgeons*, 1964, **72**, 71.

³ Cueto, J, et al, *Annals of Surgery*, 1967, **165**, 408.

⁴ Kessler, R E, et al, *Gastroenterology*, 1969, **56**, 536.

⁵ Dumitrescu, I, et al, *Chirurgia (Bucharest)*, 1973, **14**, 529.

⁶ Datta, D V, et al, *Gut*, 1971, **12**, 48.

Diabetic ketoalkalosis: a readily diagnosed non-entity?

SIR,—We have watched with interest the reaction to Drs K C Lim and C H Walsh's recent short report on this subject (3 July, p 19). While reluctant to perpetuate the correspondence, we feel that a challenge should be made to this new term.

We would suggest that in all the cases cited by Drs Lim and Walsh, Dr Elaine B Melrose and others (24 July, p 237), and Mr R Shirley and Dr Jennifer V Martin (16 October, p 943) to support this new entity of diabetic ketoalkalosis an alternative explanation on the basis of concurrent pathology is possible. Naturally the clinicians concerned must remain the final arbiters in a decision as to the validity of our alternative proposals.

In the prime paper Drs Lim and Walsh explain the metabolic alkalaemia on the basis of gastrointestinal losses and, in the second case, the additional factor of alkali ingestion.

In regard to the letter from Dr Melrose and her colleagues we feel that a closer examination of the case is required before it can be used as an example of the condition. While it was clinically similar to the two patients in the original paper, we think the biochemical results and interpretation may be erroneous. Recalculation of the results from the given P_{CO_2} and pH show that the standard bicarbonate should be approximately 25 mmol/l (assuming a normal haemoglobin concentration). Thus this is definitely not a metabolic alkalaemia and a respiratory cause is likely.

In the case reported by Mr Shirley and Dr Martin, unlike Drs Lim and Walsh's cases, the metabolic alkalaemia was not corrected on treatment of the diabetes. Our view is that this set of blood gas results may be shown by a patient with chronic obstructive airways disease whose normal compensated hypercapnic state has been disturbed by measures which reduce the P_{CO_2} . Unfortunately we are given no details of this aspect of the case.

We are delighted to see the encouragement of acid-base investigation in all cases of diabetic ketosis and appalled by its absence on occasion. These patients illustrate how mixed and unexpected acid-base disturbances can

present and the fallacy of assuming single pathology which may dictate inappropriate treatment. However, we feel that such cases are the exception rather than the rule and wonder whether the new term "diabetic ketoalkalosis" may not be best avoided in favour of an accurate description of the individual patient's problems, lest it become the latest popular diagnosis.

We would like to end by observing that an alkalosis was not demonstrated in any case, only an alkalaemia.

S JAVED IQBAL
DAVID B WALSH

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Safety for children

SIR,—Apropos your sensible leading article (9 October, p 833) in which you point out that Britain has the worst child pedestrian casualty rate in Western Europe, could I suggest that when a motorist who runs over a child says that the child did not give him a chance the reply should be that he should not have taken or needed one. Surely it is the business of adults to avoid injuring children as they go about their business rather than that of children to be taught prematurely that life is a matter of safety first rather than exploration of the environment.

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Continuous fetal monitoring in the ambulant patient

SIR,—Dr Anna Flynn and Mr John Kelly (9 October, p 842) state that "until now, because of technical limitations, continuous fetal monitoring in labour has meant confining the patient to bed." This is not strictly true, as the technical ability to use telemetry for signals of this nature has been available for quite some time.^{1,2} The real limitation is financial, since a commercial system will cost in the region of £1500 for the telemetry alone, while home-made devices, though low in component cost, absorb considerable resources in terms of technical expertise.

It is not denied that mobility may confer certain advantages to patients in the first stage of labour and that a technique of monitoring which allowed ambulation might encourage obstetricians to keep their patients ambulant. However, we do suggest that the system described is not necessary and because of its expense may be counterproductive to encouraging ambulation. We have developed and are currently evaluating a small battery-powered warning monitor under a grant from the Equipment, Research, and Development Fund of the Scottish Home and Health Department. The fetal heart is detected by electrocardiography and, in its present form, the device is designed as a low-cost portable bedside monitor which has the advantage of not producing large quantities of paper to prove that progress is normal. However, we intend to evaluate a simplified version which could be worn by an ambulatory patient.

While this device simply gives warning of the presence of abnormal fetal heart rate patterns, which then require more detailed

study on a recording fetal monitor, we feel it may be a more cost-effective approach towards extending surveillance to ambulatory patients not in "at risk" categories.

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¹ Weller, C, and Manson, G, in *Biotelemetry*, ed H P Kimmich and J A Vos, p 13. Leiden, Meander, 1972.

² Manson, G, *Electroencephalography and Clinical Neurophysiology*, 1974, **37**, 411.

Is the cholera pandemic waning?

SIR,—Your leading article (14 August, p 390) on the waning of the cholera pandemic notes its involvement of "Africa south of the Sahara (probably for the first time in history)." Lest this suggestion that it is the first time trans-Saharan Africa has been affected is taken as correct, evidence to the contrary should be noted. Richard Burton experienced the ravages of cholera on the East African coast at Kilwa. James Christie in his classic work¹ described the cholera epidemics of East Africa, including their extension to the remote inland parts of Central Africa,² while Pankhurst³ has recorded the many epidemics of cholera in Ethiopia, which must have extended more widely. Finally, for West Africa, which Hirsch⁴ recorded has never been affected, Bowesman⁵ described the epidemic of cholera in Senegal and Gambia in 1869. This was referred to *The Times* while it was raging and Bowesman notes other mentions of this epidemic which, till he wrote, was unrecorded in the *Index Catalogue* and other indexing and abstracting sources. Curiously enough Dr Bowesman's own note seems to have escaped attention.

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¹ Christie, J, *Cholera Epidemics in East Africa*. London, MacMillan, 1876.

² Davies, J N P, *East African Medical Journal*, 1959, **38**, 1.

³ Pankhurst, R, *Medical History*, 1968, **12**, 262.

⁴ Hirsch, A, *Handbook of Geographical and Historical Pathology*. London, New Sydenham Society, 1886.

⁵ Bowesman, R, *East African Medical Journal*, 1959, **38**, 621.

Computer interrogation of patients

SIR,—I wonder whether Dr A L Jacobs's letter (2 October, p 814) is based upon his own practical experience of using computers in this manner or merely an assumption of some fundamental medical ethic.

We have been experimenting with the use of computers in this mode for the past six years in attempting to discover what actually happens during the doctor/patient interaction. It is now being routinely used in one of our medical outpatient clinics. In one of our earlier experiments with bronchitis patients we interviewed the patients as to their opinion. All of them preferred the computer terminal asking the questions to the doctor. The reasons were clearly spelt out: (1) the atmosphere was relaxed and friendly; (2) the machine did not shout at them or become irritable; (3) they did not feel they were wasting the great man's time; (4) they could ask for the question to be repeated without feeling foolish; (5) they could