as the number of endoscopies increased. The experience in Japan is that the proportion of early gastric cancers increases as the rate of endoscopies and other investigations, such as the double-contrast barium meal and gastro-camera, increases; and there is sufficient evidence to support the view that early gastric cancer in this country is the same as in Japan, where the resectability and survival rates are considerably better.¹

The authors have noticed an increase in patients with "inflammatory changes." However, they do not comment on the exact histological picture. A relation between chronic gastritis and gastric carcinoma has been shown to exist by several workers. It has been said by others that carcinoma develops more commonly when cellular metaplasia or hyperplasia are present. Could this increase in the number of patients with inflammatory changes therefore hide an increase in early gastric cancer?

I think that it would be prudent to follow up the patients with inflammatory changes by repeated endoscopies, multiple biopsies, and cytological examination. It would not be surprising if more exhaustive search and closer follow-up revealed several cases of early gastric carcinoma. Earlier diagnosis is the only factor that would make the resectability and survival rates in this country similar to those in Japan. I would also stress that the conventional barium meal is not only inferior to endoscopy in the diagnosis of gastric carcinoma but also unnecessary, since whatever the radiological findings endoscopy will be needed.²

In conclusion, the authors' statement that their direct-referral endoscopy service has given little objective benefit is premature until the possible connection between inflammatory changes in gastric mucosa and early gastric cancer is explored further.

V MOSHAKIS

The London Hospital,
London E1

¹ Evans, D M D, *Gut*, 1978, 19, 1.

² Moshakis, V, and Hooper, A A, *Clinical Oncology*, 1978, 4, 359.

Emotion and empiricism

SIR,—I agree with Dr A L Cochrane and his colleagues (17 February, p 486) that "informed consent" is an emotive issue and wish to raise a difficult ethical consideration in this context.

It is simple to give "full" information when inviting a patient to partake in a prospective randomised controlled trial comparing a potentially better treatment with a known effective treatment. It is more problematical, however, when no treatment of proved efficacy exists in the context of the study being undertaken. It is usual in this situation to compare the drug under test with an inert substance. If one invites patients to partake in such studies and informs them that drug A is being compared with drug B then one can determine the effect of both the drug under study and the "placebo effect." This placebo effect consists not only of the inert substance but the effect of the doctor-patient relationship. If, on the other hand, one informs the patients that one of the tablets is inert then one may lose the placebo effect from all patients when they are on "active" drug or inert substance.

I believe that in the interest of patients' wellbeing as well as in the interest of scientific progress the degree of information divulged must vary depending on the clinical context

and should never be such as to harm the patient or the doctor-patient relationship. Furthermore, the intended degree of information to be divulged should form part of any protocol submitted to an ethical committee. This body should judge this aspect of the protocol in its clinical context rather than be guided solely by the rarely (if ever) obtainable "free and informed consent."

JOHN F FIELDING

Charitable Infirmary,
Dublin

Colonoscopy for unexplained rectal bleeding

SIR,—I read with interest and appreciation the article by Dr E T Swarbrick and others (16 December, p 1685) concerning the value of colonoscopy for unexplained rectal bleeding. All their data are extremely interesting and helpful. I do question, however, the incidence of ulcerative colitis found at colonoscopy. I believe their data indicated that of the 24 patients with inflammatory bowel disease none had rectal involvement and yet 14 patients were said to have ulcerative colitis. My impression was that current dogma indicated that 95% or more of patients with ulcerative colitis had rectal involvement and I find their numbers somewhat at odds with this dogma. I would appreciate any elaboration of these data or explanation that the authors might have.

M B FRIED

Kingston,
Pennsylvania, USA

* * * We sent a copy of this letter to the authors, and Dr Swarbrick's reply is printed below.—
Ed, *BMJ*.

SIR,—We do not routinely undertake colonoscopy on all patients with inflammatory bowel disease and have no figures on the incidence of rectal sparing; but we agree with Dr Fried's belief that while it may be a feature of Crohn's colitis it is rare in ulcerative colitis. Nevertheless, our results confirm that it does occur.

We would like to emphasise that all our patients had been referred from gastroenterologists, surgeons, or physicians and were therefore a highly selected population. In addition they had already been screened for causes of rectal bleeding by sigmoidoscopy and barium enema, and any inflammatory bowel disease found on colonoscopy would not by definition involve the rectum.

EDWIN SWARBRICK

Gastroenterology Department,
St Bartholomew's Hospital,
London EC1

Bombs and hospitals

SIR,—The leading article "Bombs and hospitals" (24 February, p 506), makes a grave omission in its otherwise common-sense review of how to deal with a major disaster. Any hospital designated in such a disaster will have a recognised accident unit with its consultant. The consultant and the casualty officers (SHOs, registrars, and even senior registrars) are all trained in quick and accurate assessment of all types of injury and deal with them each day. Many designated accident units also have

flying squads, the accident consultant or his deputy attending the scene of the accident and performing triage on site—this again being part of the day-to-day activities.

To suggest that the first "senior surgeon" called should spend valuable time summoning "the required members of staff" and alerting the blood bank, the x-ray department, and the senior nursing officer is ludicrous. What is the switchboard doing?

The danger in disaster planning is departure from routine. Triage is best carried out by people who perform it each day.

P PRITTY

Accident and Emergency Unit,
Chester Royal Infirmary,
Chester

SIR,—Your leading article on bombs and hospitals (24 February, p 506), in listing the patients who will need operation or intensive care, gives pride of place to blast lung.

In a follow-up study of 1532 consecutive patients injured by bombing in Northern Ireland, there were two cases of blast lung.¹ Multiple contaminated wounds, traumatic amputations, extensive burns, and smoke inhalation are more common indications for such care, but are not mentioned.

W H RUTHERFORD

Accident and Emergency Department,
Royal Victoria Hospital,
Belfast

¹ Hadden, W A, Rutherford, W H, and Merrett, J D, *British Journal of Surgery*, 1978, 65, 525.

Dexamethasone in acute stroke

SIR,—We wish to comment on several points raised by the correspondence about our recent article (7 October, p 994).

Many of the letters have illustrated the problems posed by the publication of "condensed reports." Editors naturally wish to ensure that as many papers are published as possible, while authors wish to ensure that their papers are as complete as possible. In our view, a report of a clinical trial must give enough information to enable readers to understand the basic properties of the patients studied and to be assured that the groups being compared are similar in all respects other than the treatment under trial. The information requested by Dr B G Parsons-Smith (9 December, p 1640) was indeed provided in our original text but was excluded by the Editor for reasons of space. We have made this table available to colleagues who have requested it, but this now makes any subsequent discussion in the pages of the journal difficult to conduct, because some of the letter writers can refer to information which the other correspondents and the general readers have not seen. We believe that this is a general problem and that correspondence about many other papers illustrates the point well. In short, there is a limit to the condensation which can be applied to a paper without depriving the reader of information which we, as authors, feel he should have.

We turn now to specific points from the letters. Dr Parsons-Smith states that dexamethasone is often effective if given in large doses immediately after the onset of stroke, but this has not been confirmed by controlled trials. Moreover, many stroke victims are not seen by a doctor immediately

after the onset of the stroke: in our study the mean interval between the ictus and arriving in hospital was 5½ hours.

Dr J W Norris (6 January, p 56) questions the validity of clinical evaluation at one year but our article makes clear that we also assessed the patients at 10 days and at three months. We found no significant difference in morbidity and mortality at these intervals.

To Dr F Clifford Rose and his colleagues (6 January, p 55) we would reiterate that the purpose of our study was to determine whether unselected patients with stroke would benefit from dexamethasone since the previous literature is inconclusive on this point. We agree that further studies are needed to see if a subgroup of such patients will benefit from steroids, but this requires the use of CAT-scanning, which is not available to physicians in district general hospitals, where the majority of stroke victims present.

GRAHAM MULLEY
R G WILCOX
J R A MITCHELL

Department of Medicine,
General Hospital,
Nottingham

* *Dr Mulley and his colleagues have made an important point about "Condensed Reports," which are a compromise between authors and editor enabling articles to be published for which we would not otherwise have the space. In future, we shall routinely ask these authors to comment before publication on letters to the Editor about their articles.—ED, *BMJ*.

Premenstrual tension syndrome

SIR,—Although many claims have been made for progesterone deficiency in the pathogenesis of the premenstrual syndrome, evidence is in fact lacking. In your recent leading article (27 January, p 212) two studies were cited. In one only 30% of patients were shown to have lower than average progesterone levels.¹ Of this small number 70% were effectively treated in an uncontrolled study of dydrogesterone. In this study and that of Backstrom² the onset of symptoms was found to precede the changes in progesterone secretion, suggesting some other agent to be causative in this syndrome. Moreover, if progesterone deficiency, or a decrease in the progesterone-oestrogen ratio, were responsible for the syndrome symptoms would be expected to occur during the preovulatory rather than the premenstrual phase.

The administration of progesterone, however, may be expected to relieve symptoms by virtue of its natriuretic action and its central nervous depressant action. Even so, there is not yet a double-blind controlled study to support the effectiveness of the drug despite enthusiastic claims.³ Other studies, in fact, have suggested that progesterone excess is responsible for the syndrome. This is a more logical hypothesis. Gillman showed that the administration of progesterone actually gave rise to premenstrual symptoms.⁴ In a recent study in Nottingham⁵ higher levels of progesterone were shown in symptomatic subjects during the postovulatory phase of the cycle: the difference preceded the onset of symptoms. The levels of prolactin and aldosterone were similar in symptomatic and controlled subjects. Even so, spironolactone reduced psychological symptoms and

reduced weight in more than 80% of subjects in a double-blind crossover trial.

It is unlikely that the syndrome is due to a single hormonal change. The combined effects of the many hormones (perhaps through shifts in neuronal electrolyte state) should be considered. It is probable that the high levels of progesterone following ovulation give rise to a diuresis and natriuresis. This is followed by a phase of a salt and water retention as the aldosterone becomes secondarily raised. This flux in electrolyte state is coincident with the onset of psychological symptoms, and possibly causative. Depending on the individual aldosterone response, there may be a return to the original sodium and fluid state or it may go beyond this to cause retention and oedema. This would account for the variable findings of previous workers, showing that weight increase and fluid retention are not universally associated with mood change.

Complete ablation of the menstrual cycle using a oestrogen-progesterone combination may improve symptoms in certain patients, though symptoms are made worse in many, especially during the phase following norethisterone withdrawal.

SHAUGHN O'BRIEN

Royal Sussex County Hospital,
Brighton, Sussex

¹ Munday, M, *Current Medical Research and Opinion* 1977, 4, suppl 4, p 16.

² Backstrom, T, and Carstensen, H, *Journal of Steroid Biochemistry*, 1974, 5, 257.

³ Dalton, K, *The Premenstrual Syndrome and Progesterone Therapy*. London, Heinemann Year Book Medical Publishers, 1977.

⁴ Gillman, J, *Journal of Endocrinology and Metabolism*, 1942, 2, 157.

⁵ O'Brien, P M S, et al, *British Journal of Obstetrics and Gynaecology*, in press.

Epilepsy and learning

SIR,—Your leading article (3 March, p 576) usefully drew attention to an important aspect of the management of children with fits. However, your statement that "sodium valproate is the first choice in new patients" is more categorical than seems justified on present evidence—apart from taking no account of the type of fits being treated.

There is no doubt that sodium valproate is a very useful new anticonvulsant, but it has been in widespread use in Britain for only a few years. Reynolds¹ has pointed out the long delay which may occur between the introduction of an anticonvulsant and the recognition of all its long-term unwanted effects. Your editorial implies that sodium valproate is less likely to cause school problems than phenytoin; but you quote no evidence to support this view, and you admit that evidence about long-term effects of sodium valproate on behaviour is scanty. A trial comparing learning performance in matched epileptic children taking phenytoin, sodium valproate, and carbamazepine (which also deserves consideration) is needed before such a recommendation can be made.

ROGER ROBINSON

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Guy's Hospital,
London SE1

¹ Reynolds, E M, *Lancet*, 1978, 2, 721.

SIR,—The British Epilepsy Association was pleased to see in the conclusion to your leading article (3 March, p 576) the sentence "Epilepsy is not just fits."

One of the things epilepsy may also repre-

sent is the need for specialised educational help. It is with this in mind that we wish to publicise more widely the establishment of a new school for children with epilepsy, which will take its first pupils in September. It is Mossbrook School, Sheffield, which will accommodate about 35 pupils with epilepsy on a residential basis. The age range will be primary and middle school and the ability range "dull to normal," with epilepsy as the principal disability. It is envisaged that Mossbrook School will be a "feeder" school for the David Lewis School, Cheshire, which caters for older pupils with epilepsy.

Doctors can help the association and the Department of Education and Science to judge the need for this new facility and identify children likely to benefit from its existence. We would be very pleased to hear from any doctors, especially paediatricians, in the Sheffield area—or elsewhere—who could give a rough estimate of the numbers of children known to be in need of a special school because of a primary disability of epilepsy. Numbers only are needed at this stage, and confidentiality will be absolutely respected.

ANDREW GORDON CRAIG

British Epilepsy Association,
Wokingham, Berks

Glossopharyngeal neuralgia with syncope

SIR,—Drs R R Jacobson and R W Ross Russell report relief with carbamazepine in the rare combination of glossopharyngeal neuralgia, arrhythmia, and syncope (10 February, p 379). Unfortunately, this drug may lose its effect, as in our patient.¹

A woman who had repeated attacks of pain, bradycardia, and syncope responded at first to carbamazepine 200 mg, twice daily, but then stopped her treatment after three months. When her symptoms recurred with great frequency and intensity 16 months later both carbamazepine and atropine were ineffective. Finally, in January 1975 the right glossopharyngeal nerve was excised, with lasting relief; and at review four years later she was entirely free from symptoms. Thus surgery may be curative in this uncommon, bizarre, and disabling condition when medical treatment has failed.

J RUSSELL REES
P G BICKNELL

Bristol Royal Infirmary,
Bristol

¹ Taylor, P H, et al, *Journal of Laryngology and Otology*, 1977, 91, 859.

Priorities in road accidents

SIR,—Dr J C Allen in his letter (24 February, p 548) is mistaken in interpreting silence for apathy. There has, after all, been very little recent correspondence in your journal, or any other scientific journal for that matter, on the flat earth theory because most sensible people have long ago realised that this theory is untenable.

Dr Allen, of course, is perfectly entitled to exercise his personal liberty when driving a car with regard to the wearing of a seat belt, just as I am sure he drives on the right-hand side of the road in England (and on the left-hand side in France) and, of course, drives through all red traffic lights, particularly when a juggernaut is crossing on the green. What