Place of confinement of women delivered in England and Wales in 1970 and 1975

	% of women delivered	
	1970	1975
NHS hospitals	84.6	95.1
Obstetric departments	66.7	78.4
GP maternity units	17.8	16.3
In hospitals without obstetrics departments ("NHS A")	11.7	7.2
In hospitals with obstetrics departments	6.1	9.1
Other departments	0.09	0.03
Home	13.0	1.7
Other place of confinement	2.4	3.2
All places of confinement	100.0	100.0
No of women delivered	786 586	603 666

GP unit in 1970 and 1975 (the most recent year for which the relevant HIPE data are available).

The estimates of the percentages of women who were delivered in GP maternity beds of 17.8% in 1970 and 16.3% in 1975 are somewhat lower than Dr Bull's estimates of 32.6% and 22.0% respectively. However, Dr Bull's figures must include claims for home confinements, and if these deliveries are added to those in GP maternity beds confinements for which GPs were responsible are estimated to form 30.8% of all deliveries in 1970 and 19.5% in 1975, and only marginally lower than Dr Bull's estimates. This supports Dr Bull's assertion that had I been able to single out births in specialist obstetric beds I should have been likely to find their concentration from Tuesdays to Fridays even more exaggerated than it was for all hospital deliveries.

The figures in the table also support Dr Bull's belief about the decline in deliveries in what he describes as "isolated general practitioner units" and show an increase in the proportion of deliveries in general practitioner beds in hospitals which also have specialist obstetric beds, although it must be added that the percentage of deliveries in "isolated" units rose marginally from 7.2% in 1975 to 7.8% in 1976.

ALISON MACFARLANE

National Perinatal Epidemiology Unit,
Churchill Hospital,
Oxford

¹ Office of Population Censuses and Surveys, *Births Statistics, England and Wales 1976*, Series FM1, No 3. London, HMSO, 1978.

² Bull, M J V, personal communication.

Treatment of the restless legs syndrome with clonazepam

SIR,—Treatment of the restless legs syndrome is usually unavailing, except when the condition is associated with iron deficiency. In the hope that others may also find it helpful I am reporting the successful use of clonazepam in a small open uncontrolled trial. In five patients the symptoms have been abolished or greatly modified as illustrated by the two most severe examples.

A man of 42 had for 14 years experienced this severe discomfort in the legs when trying to relax in the evening. On going to bed the sensations would increase and spread to the arms and shoulders. They could be momentarily relieved by moving the limbs but often he would have to rise from his bed and walk round the house. When the discomfort was particularly severe it would be accompanied by involuntary jerking of the legs, which, according to his wife, would continue for several hours during sleep. Clonazepam 1 mg given an hour before retiring immediately and completely

abolished the nocturnal symptoms. Relief has been maintained by continuing this dose without side effects.

A woman of 65 had suffered from restless legs since childhood. Here again the familiar distressing sensations were relieved by movement and were accompanied by nocturnal myoclonus of the legs. Clonazepam 0.5 mg in the evening and again at bedtime brought almost complete relief of the restless legs and abolished the myoclonus. An unexpected benefit was the complete disappearance of an obstinate pain in the right groin that had been present for over 20 years. This had characteristics that might be linked to the restless legs syndrome as it occurred only at night, waking her from sleep and forcing her to get up and walk about, although this did not bring immediate relief.

Both these patients also had nocturnal myoclonus, but clonazepam has also proved successful in the restless legs syndrome unaccompanied by involuntary movement. I am not aware that this action of clonazepam has been previously reported.

W B MATTHEWS

Churchill Hospital,
Oxford

Shortening hospital stay for psychiatric care

SIR,—May I qualify the conclusions reached by Professor S R Hirsch and others (17 February, p 442) from their study on shortening hospital stay for psychiatric patients? It seems to me from their data that when clinicians attempt to reduce duration of stay by an effort of will only a minority of patients are actually affected, which has only a marginal effect on bed occupancy levels. I am assuming that the only new factor in the treatment programme which they evaluated was the effort of will to get patients home earlier, for nothing else is mentioned about clinical methods. Anyway, the main effects were a small difference in mean duration of stay that was not statistically significant, and a reduction in bed occupancy of, I estimate from their data, about 12%. Let me in no way detract from the value of this study, which set out to determine whether significant economies could be made in this way and showed that they could not.

Yes, I should declare a vested interest. We in Edinburgh managed to achieve a substantial reduction in stay for unselected psychiatric admissions only after months of thoughtful and laborious reorganisation of a ward, which involved staff having to learn new skills and deploy their time quite differently.

PETER F KENNEDY

Department of Psychiatry,
University of Edinburgh

Hypnosis

SIR,—Your leading article on hypnosis in the NHS (7 October, p 978) implies that resources should be channelled into the training of doctors and dentists in hypnotic techniques, the presupposition being that such techniques have been conclusively shown to be of scientific value. This, I would respectfully point out, is simply not the case, and it is therefore no wonder that most British doctors have declined to take up the option you suggest, especially in view of the possibility that hypnosis may on occasion be harmful.¹

While a body of clinical-anecdotal accounts do support your thesis, controlled trials show

little advantage over placebo techniques—for example, a "hypnosis-producing drug."² Though the subjective experiences may differ in experimental and control groups,³ this is not the case with overt responses. People will go out of their way to please the experimenter⁴ in situations testing hypnotic behaviour. Personality differences may certainly be more important than the effects of trance induction.⁵

As long ago as 1930⁶ "waking" and "hypnotic" suggestions were shown to be equally instrumental in producing the required response. Thus, in the case of posthypnotic suggestion, posthypnotic behaviour may be elicited only when the subject believes that the hypnotist-subject relationship still exists.⁷ Subjects showing a high "need for deference" will comply with posthypnotic-like suggestions, whether or not they are hypnotised.⁸ Again, "regression to an earlier age" seems to be a phantom,⁹ and perceptual changes supposedly induced by hypnosis are reproducible by waking suggestions.⁹

What about pain? Childbirth and toothache are the classical hypnosis-susceptible ailments. But again there is no convincing evidence^{10 11} of an alleviation in physiological responses to such painful stimuli under hypnotically induced analgesia. Natural childbirth patients in one study¹² did not differ from untrained patients in estimates of pain intensity.

In short, the case for the specific usefulness and wide applicability of hypnosis is not proved. It seems not to be of superior efficacy to simpler techniques.

H G KINNELL

Department of Psychological Medicine,
Newcastle General Hospital,
Newcastle upon Tyne

¹ Waxman, *British Medical Journal*, 1978, **2**, 571.

² Glass, L B, and Barber, T X, *Journal of Nervous and Mental Diseases*, 1961, **132**, 539.

³ Orne, M T, *Journal of Abnormal and Social Psychology*, 1959, **58**, 277.

⁴ Orne, M T, *American Psychology*, 1962, **17**, 776.

⁵ Barber, T X, *Journal of the American Medical Association*, 1960, **172**, 680.

⁶ Patten, E F, *Journal of Abnormal and Social Psychology*, 1930, **25**, 319.

⁷ Fisher, S, *Journal of Abnormal and Social Psychology*, 1954, **49**, 503.

⁸ Barber, T X, *Psychosomatic Medicine*, 1962, **24**, 286.

⁹ Costello, C G, *Psychology for Psychiatrists*, p 121. Oxford, Pergamon, 1966.

¹⁰ Shor, R E, PhD Thesis, Brandeis University, Waltham, Mass.

¹¹ Barber, T X, *Psychosomatic Medicine*, 1963, **25**, 303.

¹² Javert, C T, and Hardy, J D, *Anesthesiology*, 1951, **12**, 189.

Evolutions in medicine

SIR,—May I, in the interests of temporal and numerical accuracy, point out three errors in Dr A G Hocken's lighthearted and amusing "Letter from Dunedin" (3 February, p 324)?

In paragraph 1, p 325, Dr Hocken mentions a "five-floor clinical services block opened in 1972." This was actually a six-floor building opened in August 1968—not as a university building per se but as a replacement for the outpatient and other services of the Dunedin Hospital as part of a long-range redevelopment plan. Existing scattered services thereby came into this very necessary highly utilised facility.¹

In the next paragraph Dr Hocken states: "In 1885 the Otago Medical School started a full medical course in Dunedin with graduation, MB ChB (New Zealand). This remained so until 1974, when Auckland Medical School graduated its first students and reversion to the designatory suffix of Otago was made." Anyone who has read Morrell's history of the University of Otago knows that the University

of Otago Act of 1961 gave autonomy back to the University of Otago, which it had lost to the University of New Zealand in 1869. The MB ChB(Otago) dates from 1962. Professor Morrell describes how "the university celebrated its recovered autonomy by a degree-giving ceremony on 27 July 1962," not, as stated by Dr Hocken, in 1972.²

In the last paragraph of the first column on p 325 Dr Hocken says that "the first 120 students from the self-sufficient school at Auckland graduated in 1974." It was on 23 November 1973 that 40 students graduated from an initial intake of 60 students.³

JOHN BORRIE

Otago Medical School,
Dunedin, New Zealand

¹ *New Zealand Medical Journal*, 1968, **68**, 186.

² Morrell, W P, *The University of Otago—A Centennial History*, pp 194 and 223. University of Otago Press, 1969.

³ *New Zealand Medical Journal*, 1973, **78**, 543.

Oral hyoscine butylbromide for irritable bowel syndrome?

SIR,—Drs J A Ritchie and S C Truelove (10 February, p 376) report an elegantly designed trial of lorazepam, hyoscine butylbromide, and ispaghula husk in the irritable bowel syndrome. This syndrome is difficult to study, but we were surprised that hyoscine butylbromide was one of the drugs chosen, because there is good evidence^{1,2} that this drug is very poorly absorbed and therefore inactive by mouth, although it relieves gastrointestinal spasm when given parenterally. The authors' conclusions therefore seem questionable. We would suggest that any improvement their patients may have derived from the treatment must have been due to the ispaghula and possibly lorazepam, but not to the hyoscine butylbromide.

It is reassuring that the results of therapy with the "real" and "dummy" anticholinergic tablets (their table I) did not differ significantly, as both groups of patients in effect received a placebo. The results of treatment with different combinations (table II) do not support the authors' interpretations. Although the patients given ispaghula with hyoscine butylbromide and those who had all the three treatments did much better than the placebo group, neither did significantly better than the group given only ispaghula.

A HERXHEIMER

Departments of Medicine and
Pharmacology,
Charing Cross Hospital Medical School,
London W6

J J MISIEWICZ

Department of Gastroenterology,
Central Middlesex Hospital,
London NW10

¹ Herxheimer, A, and Haefeli, L, *Lancet*, 1966, **2**, 418.

² Beerman, B, et al, *European Journal of Clinical Pharmacology*, 1972, **5**, 87.

SIR,—Drs J A Ritchie and S C Truelove are to be congratulated on their use of a factorial design in their trial on the treatment of irritable bowel syndrome (10 February, p 376). They conclude that each of the three agents studied is more effective than its corresponding placebo, that a combination of two agents is better than a single agent, and also that the use of all three agents together is better than any two. They based these conclusions on their findings that the triple placebo gave no

successes in 12 patients, the single agents gave either 4 or 5 successes in 12 patients, the two-agent combinations gave 5 or 6 successes in 12, and all three agents combined gave 7 successes in 12 patients.

Three points ought to be made about how the authors have interpreted these results. Firstly, although it can be concluded that most of the active treatments did significantly better than the placebo, none of the differences among the active treatments, singly or in combination, are anywhere near to being statistically significant. It is therefore invalid to claim superiority for two agents over one, or three over two. Secondly, the small sizes of the treatment groups do not allow us to be at all precise about the "true" level of efficacy of any of the treatments studied. For example, although the apparently best treatment—all three agents used in combination—gave 7 successes in 12 patients, a response rate of 58%, this only allows us to be 95% confident that the "true" response rate is somewhere between 31% and 81%. It would also be unwise to conclude that the placebo response rate really is zero, 0 successes in 12 patients merely allowing us to be 95% sure that the "true" placebo response rate is 25% or less.

Finally, although this study does not provide useful data from which we can judge the merits of the various treatment options, it would seem rational none the less to use the form of treatment which at least appears to be the most effective—namely, all three agents in combination. However, as, a priori, fewer agents are always preferable to more, the costs and risks of using three agents must be set against the weakness of the evidence in favour of this option. What these costs and risks are, a statistician, must leave for others to judge.

K D MACRAE

Charing Cross Hospital Medical School,
London W6

SIR,—The choice by Drs J A Ritchie and S C Truelove (10 February, p 376) of oral hyoscine butylbromide as an antispasmodic because of its effectiveness in a similar dose intravenously was unfortunate. Early work with this drug indicated very slight absorption from the gastrointestinal tract.¹ Further studies using the labelled compound have confirmed that it is taken up very poorly throughout the gut.² Even when given parenterally, it causes effects of very short duration³ and this makes it unsuitable for long-term use. The controversial reports about the spasmolytic effects of oral hyoscine butylbromide² illustrate the difficulties in evaluating clinical trials with these types of drugs, and it would seem of great importance to study first the gastrointestinal absorption for each substance recommended for oral use.

D GIBBONS

West Cornwall Hospital,
Penzance

¹ Herxheimer, A, and Hafeeli, L, *Lancet*, 1966, **2**, 418.
² Helström, K, Rosén, A, and Söderlund, K, *Scandinavian Journal of Gastroenterology*, 1970, **5**, 585.

³ Möller, J, and Rosén, A, *Acta Medica Scandinavica*, 1968, **184**, 201.

Benzodiazepine pharmacokinetics

SIR,—Though I appreciate that one of the reasons for using lorazepam as a tranquilliser was the availability of matched placebos in the

excellent trial by Drs J A Ritchie and S C Truelove (10 February, p 376), I would disagree with the statement "Lorazepam has a longer duration of action than other benzodiazepines. . ."

Lorazepam possesses an elimination half life of about 12 hours and has no pharmacologically active metabolites.¹ Twice-daily dosing would therefore maintain fairly stable steady state plasma concentrations. Diazepam and its major active metabolite desmethyldiazepam have elimination half lives of over 24 hours,² and clinical effectiveness would therefore be expected after once-daily dosing with diazepam. Potassium clorazepate is marketed as a once-a-day tranquilliser. It is hydrolysed in the stomach to desmethyldiazepam, which is then absorbed.³

There are now more than a dozen different benzodiazepine derivatives available on the market. Choosing from a very similar group of compounds should perhaps be influenced largely by cost.⁴

J COOKE

St James's University Hospital,
Leeds 9

¹ Elliott, H W, *British Journal of Anaesthesia*, 1976, **48**, 1017.

² Garattini, S, Mussini, E, and Randall, L O, *The Benzodiazepines*. New York, Raven Press, 1973.

³ Shader, R I, and Greenblatt, D J, *American Journal of Psychiatry*, 1977, **134**, 652.

⁴ Smith, A, and Rawlins, M D, *British Medical Journal*, 1977, **2**, 447.

Open-access endoscopy service for general practitioners

SIR,—We were interested in the recent paper by Dr G Holdstock and others (17 February, p 457) regarding experiences with an open-access endoscopy service for GPs. The authors suggest that the open-access approach has the advantage of avoiding the need for clinical referral entirely, and that it is a logical progression from our instant endoscopy clinic.¹ We feel, however, that a clinical assessment before endoscopy has a distinct advantage, because it may lead to the diagnosis of other, non-gastrointestinal disease, which would otherwise be missed.

During the first year of our "one-visit clinic," in approximately 240 patients seen we diagnosed rectal carcinoma (1 case), cholelithiasis (2), hepatic disease (2), diverticular disease (2), hypolactasia (1), and pancreatitis (1), all in patients with dyspeptic symptoms but no gastroduodenal pathology. These diagnoses were made as a direct result of the clinical assessment made in the endoscopy clinic and investigations instituted there.

Although this is more time consuming, we have found that the full assessment and endoscopy can be easily completed in 40 minutes, and feel that the increased diagnostic yield makes this worthwhile.

ALAN K BEAVIS
SUSAN LA BROOY
J J MISIEWICZ

Department of Gastroenterology,
Central Middlesex Hospital,
London NW10

¹ Beavis, A K, La Brooy, S, Misiewicz, J J, *Gut*, 1978, **19**, 447.

SIR,—With reference to the paper on open-access endoscopy service for general practitioners (17 February, p 457), I would like to comment on the surprising finding of not picking up more early "gastric carcinomas"