The only really valid way to resolve this issue scientifically would be to carry out a prospective randomized trial, which we accept is not a realistic possibility. Faced with this problem, the challenge is to compare outcomes in two matched identical groups. Mrs Tew tries to do this, but we believe that she has seriously underestimated the difficulties.

By controlling for age and parity, Mrs Tew claims that there are two comparable groups, that is, any 24-year-old in her third pregnancy is the same as any other. Every doctor and midwife knows this is not true. A further important source of bias arises from the failure to exclude pre-term intrauterine deaths. For example, an intrauterine death at 30 weeks would always be delivered in hospital, never at home, and would therefore be included in the hospital statistics.

We do not accept that analysing perinatal mortality rates by 'labour prediction score' makes it 'possible to compare like with like'. As Newcombe and Chalmers[1] have pointed out, the problem with such scores is their low predictive value, that is, most mothers designated as being 'at-risk' by them will deliver a normal child whatever happens during pregnancy and delivery. Also the scores are based on crude risk factors which do not explain the actual mechanism of perinatal death. By themselves maternal age and social class have never killed anybody. Because of this we strongly dispute Mrs Tew's statement that 'also unlikely is the suggestion that a greater number of hospital births are at high risk on account of factors additional to those included in the labour prediction score, but totally independent of them'. We think this is highly likely and underestimates the clinical acumen of general practitioners, midwives and obstetricians.

The data on which her case is based are almost ancient history, but we also disagree with the analysis of subsequent events in the 1970s and 1980s. In view of the increasingly small number of home deliveries and perinatal deaths, is the use of correlation techniques here really valid? Is there not too much emphasis on the '*P*' value, mistaking 'significance' for 'importance'? Also, 'correlation' is mistaken for 'cause'. Even if it were true (which we do not accept, since no data is put forward to sustain it) that perinatal mortality decreased faster in years when hospitalization increased more slowly, it does not follow that 'if hospitalization had increased less, the perinatal mortality rate would have decreased more'. The fact is that the period since 1978, especially 1979 and 1980, has seen the biggest falls in perinatal mortality nationally since records began in 1928.[2] In the Nottingham Health District, the perinatal mortality rate fell from 18.5 per 1000 in 1975 (home delivery rate 6.5%) to 9.1 per 1000 in 1983 (home delivery rate 1.05%).

Although there are several reasons for this, an improvement in hospital services cannot be denied. A recently published epidemiological study of infants requiring neonatal care whose parents lived in the Nottingham Health District,[3] showed that the risk of death to infants of 29–32 weeks gestation reduced four-fold over the period from 1977 to 1983/84. This will certainly explain some of the fall in the rate locally. Mrs Tew does not deny that the transfer of such small babies is a bad idea.

We are also concerned that while referring to Holland, Mrs Tew did not mention that the percentage of hospital births there is increasing each year, and no reference was made to Sweden, with the world's lowest perinatal mortality rate and 100% hospital deliveries.

Finally, we wonder if there really is such a clamour for home births as is claimed? A postal survey of all 192 women resident in Mrs Tew's own health district who delivered at home in 1980 and 1981 was carried out by a medical student who was in favour of home deliveries and is the daughter of a general practitioner. The survey compared their opinions with those of a second group consisting of a random sample of women who had delivered in Nottingham hospitals during the same period.[4] The main finding was the very high level of satisfaction expressed by both groups. The percentages of women who had a hospital delivery and who wanted a home delivery next time, and vice versa, were almost identical — 12% versus 10%.

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A classification of drugs used in general practice

Sir,

I support Dr Carney's recognition of the need for a code for drugs used in general practice (April *Journal,* p.198). However, I believe that a classification (a systematic grouping of like with like) is more important than a code (a numbering system), although there are obvious advantages if the two go together.[1,2]

I hope that any developments encouraged by Dr Carney's initiative will be directed towards an internationally recognized classification rather than a national one (in Britain or elsewhere). Since an international classification of drugs for primary care, based on the Scandinavian anatomic therapeutic chemical (ATC) system approved by the World Health Organization, already exists as part of the International Classification of Primary Care: Process (ICPC-P), recently developed by the WONCA Classification Committee, it would be best if this were used as a basis and further refined in the course of its use.

Development of criteria for any classification prior to embarking upon its establishment is an important issue which is often neglected, but which is emphasized by Dr Carney. I agree with many of the criteria suggested by him but some refer to detail which can and should be left flexible for users. More fundamental is the primary axis of subdivision (I agree with his suggestion of therapeutic class) and the way in which this is interpreted. For example, are diuretics cardiovascular or renal drugs?

Any international or national classification should not go into great detail, but should provide a basic framework on which groups of users can expand or contract groupings to meet their own needs while at the same time maintaining compatability with the classifications of others. This is a very difficult task, since the frameworks which national health authorities in different countries already use, and with which general practitioners are to some extent familiar, are diverse. It may not yet be feasible to adopt all the features of the ICPC-P drug classification in any country (there are certainly great difficulties in Australia), but the more we all work towards common ground the better for the advancement of our discipline throughout the world.

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Individual and group cognitive therapy

Sir,
As a non-statistician and somebody with an interest in seeing cognitive therapy evaluated, I was unsure of your rationale in publishing the paper by Ross and Scott (May *Journal,* pp. 239-242).

First, they state 'treatment gains are mainly aimed at follow-up at 12 months', yet out of their invited (small) group of 51 patients, only 20 had been followed-up for 12 months (39% of the initial group), 14 others having 'not yet reached 12 month follow-up', the remaining 17 presumably having dropped out. I am not sure that on this basis their statement is justified. Should not publication have waited for all of the 34 patients to have reached 12 month follow-up?

Secondly, they state 'There is no significant difference between patients treated with group or individual cognitive therapy.' This statement is based, I believe, on Table 2 and some statistical ramifications thereof — but where are the actual figures on which this statement is based so that the reader can verify this important statement?

Thirdly, how are the 20 patients in Table 5 made up? These 20 patients achieve mean scores of 9.4 (Beck) and 7.6 (Montgomery–Asberg) after cognitive therapy but the 'waiting list' group achieved mean scores of only 16.8 (Beck), not even achieving remission, and 12.7 (Montgomery–Asberg) — despite their roughly comparable pre-treatment values. Is this a reflection of the fact that group therapy is predominent for these patients (12 out of 21) or are the patients in Table 5 and the 'waiting list' group really quite distinct?

This paper ends by confusing me — or have I just got the reasoning wrong? Surely a paper should present as complete a set of results as possible so that the reader can verify the conclusions drawn.

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Sir,
To answer Dr Gunstone's queries:

1. The 51 patients who received cognitive therapy in our study represent the largest sample studied in general practice to date. (In fact our study is of a comparable size to the only published British hospital study of cognitive therapy involving 49 patients by Blackburn.[1]) Seventeen patients dropped out during treatment leaving 34. When we first submitted the paper we had intended only to present data on the completion of cognitive therapy. However, the referees enquired whether some preliminary data might be available for 12-month follow-up. This we provided. As stated, there was no systematic qualitative or psychometric difference between those who completed 12-month follow-up and those who have yet to do so, and so this was an eminently reasonable thing to do. We shall of course eventually publish definitive results for all of the group.
2. Dr Gunstone has concluded that the Beck scores of the waiting list group and those of the immediate cognitive therapy group differed after cognitive therapy treatment. He has concluded this by comparing figures quoted in Tables 3 and 5. These tables, however, are not comparable because Table 3 includes patients who dropped out in the waiting list period and Table 5 is presented for different reasons to look at the prognosis for completers. Because our 'intention to treat' analysis necessitated assuming no further progress since last point of contact, the results are over pessimistic. For the Table 3 patients excluding these drop-outs, the mean Beck score is 12.7 ± 8.2 — 13 out of 21 patients scored 16 or less.

   Furthermore, because some patients are present in both tables (as waiting list patients who subsequently had cognitive therapy treatment), and others are not (because they were in the immediate cognitive therapy group) it is not possible to perform a meaningful statistical test between the groups.

We are sorry that Dr Gunstone was confused and hope his points have now been answered. We are sure that the effectiveness and economy of cognitive therapy provision in primary care as demonstrated by the paper, underline the need to disseminate provision of this treatment method without delay. For this reason we fully defend the publication of our paper.

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Laughter and medicine

Sir,
I would like to challenge Dr C.P. Elliott-Binns (August *Journal,* pp. 364-365) in his assertion that laughter is not generally recognized as a psychiatric technique, and that it is rarely used in that specialty. If one adopts Dr Elliott-Binns' definition of laughter as a state where 'the corners of the mouth are raised and a series of guttural noises issue from the mouth', this indeed may be true. However, laughter, as more usually defined, is much in evidence as a means of communication between patients and staff in everyday contemporary psychiatry. There are, I feel, some good reasons for this, including the informal atmosphere prevalent in the psychiatric setting, the relatively long time available for talking with patients and the considerable intimacy which develops when problems are viewed in depth.

Many of Dr Elliott-Binns' comments hold true for traditional analytical psychotherapy, such as the emphasis on detachment rather than attachment in training and the view that laughter allows patients to escape from sensitive issues. However, analytical psychotherapy is but one small part of current psychiatric practice, being confined to large cities and executed predominantly in the private sector. Although psychotherapy generally has an important place in contemporary psychiatry, it is rarely of the dead-pan analytical variety and is more likely to be supportive in nature and characterized by less emotionally stilted interaction. Many of the new psychotherapies, which are of increasing important in the National Health Service owing to their cost effectiveness, emphasize humour as part of the genuineness and empathy established between therapist and patient. Cognitive therapy[1] provides a good example of this.

Finally, the author's proposal that doctors need training in humour and wit, with the aid of videotapes only seems to call into question the priorities exercised in medical student selection and training.

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1. Beck AT, Rush AJ, Shaw BF, *et al.* *Cognitive therapy of depression.* New York: John Wiley, 1979.

Sir,
I read with interest Dr Elliott-Binns' thoughtful leading article on laughter and medicine (August *Journal,* pp. 364-365). Dr Elliott-Binns draws our attention to the