References

1. Bridges-Webb C. Standard classifications and terminology for general practice. *Med J Aust* 1984; **140**: 8.
2. Bridges-Webb C. Codes and classification in general practice computing. *Practice Computing* 1984; **1**: 16-17.

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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MICHAEL SCOTT

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Reference

1. Blackburn IM, Bishop S, Glen AIM, *et al.* The efficacy of cognitive therapy in depression: a treatment trial using cognitive therapy and pharmacotherapy, each alone and in combination. *Br J Psychiatry* 1981; **139**: 181-189.

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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Reference

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