

Statistical design, analysis and correspondence

SIR: A statistician should know better than to become involved in disagreements between psychiatrists, particularly when these involve aspects of statistical design and analysis. But having read some of the statements in recent letters by Welch & Lewis and Marks *et al* (*BJP*, January 1995, 166, 120–122), I thought I should have the courage to take the role of the fool rather than that of the angel and point out some of the more obvious nonsense both in their remarks, and in the analyses reported by Marks *et al* (1994) that prompted their correspondence.

It is often hard to persuade psychiatrists (and others) that statistics is a thriving, constantly evolving body of knowledge. Techniques taught to today's consultant psychiatrist in the dim and distant past are very likely to have been superseded by recent developments. One example of an area that has altered dramatically in the past five years is the analysis of longitudinal studies, particularly when missing data occur. This is not the place to go into detail (two recent references are Everitt, 1995 and Diggle *et al*, 1994), but briefly, powerful and sophisticated modelling techniques are now available (with suitable accompanying software in most cases) that can undertake informative and appropriate analyses of longitudinal data, including dealing with missing observations in the correct fashion. Employing such methods would avoid both the suggestion of Welch & Lewis that using the 'last observation carried forward' approach to replacing missing values is sensible (it is not!), and the claim by Marks *et al* that 'repeated tests of significance . . . had to be done' (they did not and the results from them are extremely likely to mislead!).

Why is it, I often wonder, that psychiatrists are so ready to pontificate on topics statistical, whereas few statisticians write to psychiatric journals claiming that they know the best treatment for depression? I suggest readers regard this question as rhetorical.

DIGGLE, P. J., LIANG, K. & ZEGER, S. L. (1994) *Analysis of Longitudinal Data*. Oxford: Oxford Science Publications.

EVERITT, D. S. (1995) The analysis of repeated measurements: a practical review with examples. *The Statistician*, in press.

MARKS, I. M., CONNOLLY, J., MIYEN, M., *et al* (1994) Home-based versus hospital-based care for people with serious mental illness. *British Journal of Psychiatry*, 165, 179–194.

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Predictors of outcome in the behavioural treatment of OCD

SIR: The attempt by Keijsers *et al* (*BJP*, December 1994, 165, 781–786) to determine which factors predict outcome in the behavioural treatment of obsessive-compulsive disorder (OCD) is welcome, given the paucity of studies in this area. The authors provide a critique of extant work in this field, correctly pointing out the major flaws in many published studies. However, they themselves fail to address two fundamental methodological difficulties.

First, the number of patients in the study ($n=40$) is simply too low to allow robust statistical analysis of such issues as the possible effect of medication on the results. For example, the authors boldly state that, given no significant antidepressant drug \times treatment interaction effect, "there is no reason to suspect that unmedicated patients improved less than medicated ones". Reference to Table 1 of their study, however, shows that only 11 patients were taking antidepressants, resulting in very low statistical power to assess any such interaction effect. Second, we are told that 51 patients were approached to participate in the study, but that only 40 completed the study. No attempt is made to control for bias which might have been introduced by the attrition of 22% of the patient sample.

Despite these limitations, Keijsers *et al* do delineate a number of variables which appear to have some predictive value. Most of the variables considered were "complaint related" items such as duration of symptoms and severity of complaints, and not amenable to alteration. Of the "non-specific treatment variables", only "quality of the therapeutic relationship" (a rather vague construct) would potentially be amenable to change by the therapist. It is thus important to assess which elements of the therapeutic relationship are important in terms of predictive value, and this will be very difficult to do.

It seems reasonable to suggest that research such as this should consider variables which can potentially be altered. In this regard, in a study conducted by my colleagues and me at the Institute of Psychiatry, London (Castle *et al*, 1994), having a co-therapist was (surprisingly to us) the most powerful predictor of outcome in 178 OCD patients treated with behavioural psychotherapy; interestingly, the effect was robust only for women. Again, it will be important to ascertain what it is about a co-therapist that is of benefit. Much further work needs to be done in this area, so that more