patients and controls may well be due to errors of measurement rather than true differences.

WILSON, A., & HENRY, A. D. (1992*a*) Meta-analysis part 1: an assessment of its aims, validity and reliability. *Medical Journal of Australia*, **156**, 31–38.
— & — (1992*b*) Meta-analysis part 2: assessing the quality of published meta-analyses. *Medical Journal of Australia*, **156**, 173–187.

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AUTHORS' REPLY: We thank Professor Daradkeh for his interest in our meta-analysis of the ventricle–brain ratio (VBR) in schizophrenia (*Journal*, May 1992, **160**, 687–697), and would like to respond to the issues he raises. Without citing particular reasons he questions the validity and reliability of our meta-analysis according to the ten criteria of Wilson & Henry (1992). Our study met all these criteria adequately except publication bias, and here we wish to note, using the formula of Rosenthal (1979) for assessing the 'file-drawer effect', that about 3900 non-significant studies would need to have remained unpublished to negate the effect found in these published studies.

Professor Daradkeh's conclusion that differences in VBR between schizophrenics and controls "may well be due to errors of measurement rather than true differences" is based on his own statistical analysis, which we believe is erroneous. He compares the mean VBRs reported for schizophrenics and controls in 11 studies which used DSM–III criteria and computerised planimetry, and had each reported a significant effect. Using an unpaired *t*-test to compare these means, Professor Daradkeh concludes that schizophrenics and controls do not show a significant difference in mean VBR. That conclusion is patently absurd, since it is clear that all eleven studies show a higher VBR in schizophrenics than controls ($P=0.5^{10}<0.001$ on a two-tailed exact binomial test). The error is in using an unpaired *t*-test, when the data are clearly related within studies (see our original Fig. 1). It is also statistically dubious to consider only studies which were originally significant. A paired *t*-test on the schizophrenic and control means for the 14 studies using DSM–III and computerised planimetry gives $t=6.20$, d.f. $=13$, $P<0.001$, a result consistent with schizophrenics having a higher mean VBR than controls. Taken overall, studies using computerised planimetry and hand planimetry showed no systematic difference. In passing, we also note some minor errors in our original Table 1, which should indicate that the studies of Dewan *et al* (1986) and Luchins & Meltzer (1986) used DSM–III criteria, that Nasrallah *et al* (1982) used hand planimetry, and that the studies of Nasrallah *et al* (1990) and Pearlson *et al* (1981) used computerised planimetry.

Professor Daradkeh suggests that our study finds a "negligible difference in VBR between patients with schizophrenia and control subjects"; that was not the conclusion of our study. Instead we began our discussion by saying, "it is clear ... that schizophrenics seem to have a higher VBR than do controls". We still believe, while accepting the comments of Birley (1992), that the *average* VBR is higher in schizophrenics than in controls.

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Cost–benefit analysis of the Daily Living Programme

SIR: The preliminary results from the Daily Living Programme reported by Muijen *et al* (*Journal*, March 1992, **160**, 379–384) have provoked a lively correspondence, most of which has focused on the adverse events in the experimental group. I wish to comment instead on the way in which the authors report their initial cost data. The paper is largely descriptive and the authors scrupulously avoid drawing misleading clinical conclusions from an incomplete data set. It is intriguing that they were prepared to present quantitative data at this stage comparing the relative costs incurred by the two treatment groups.

Cost-benefit and cost-effectiveness analyses are now accepted as integral to the evaluation of psychiatric services. Considerable progress has been made in overcoming the major conceptual and methodological difficulties inherent in this type of work (Knapp, 1991), and recent publications have reflected an increasing sophistication in the application of health economics to psychiatry (Beecham *et al*, 1991). Comparing very limited

treatment costs may appeal to hospital and health authority managers with responsibility for specific budgets, but the findings reported by Dr Muijen *et al* fail to reflect the true costs associated with the different types of treatment under investigation. Certain items of care are difficult to cost in purely financial terms, such as relatives' time and the capital element of service costs, and these may require special treatment, based on more than one model (McGuire, 1991). There can be no excuse, however, for neglecting relatively straightforward costs which are disproportionately distributed between the experimental and control groups, including the costs incurred by the hospital out-patient service, primary-care services, the criminal-justice system, community agencies and informal carers. In ignoring these, the authors fail to account for costs which are shifted away from the hospital in-patient service but which are borne by other agencies.

Although unacknowledged in the present paper, the cost–benefit analysis promised for future publication is being undertaken in collaboration with the Personal Social Services Research Unit (PSSRU) at the University of Kent, originators of some of the most innovative and compelling work on the economic evaluation of mental-health services (Knapp, 1991). The PSSRU values comprehensiveness above all other virtues in measuring costs (Knapp & Beecham, 1990), a feature which will certainly be reflected in later papers. It is especially disappointing therefore that the present authors saw fit to draw invalid conclusions based on incomplete cost data at this stage. It is interesting to note that such findings were omitted from a more comprehensive clinical evaluation which appeared simultaneously in another scientific journal.

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AUTHORS' REPLY: The title and text of our paper indicate that the findings are preliminary. Since then, intriguing additional results have emerged from the complete data set. The paper reporting the full clinical results will be ready soon; that on the full cost–benefit analysis will follow later. We are delighted that the definitive full cost-benefit analysis is indeed being done by such experts as Professor Martin Knapp and Jennifer Beecham. Professor Marks recently had the pleasure of publishing a book (Marks & Scott, 1991) which contained an excellent chapter by Professor Knapp lucidly analysing relevant issues. Dr Weich might wish to add it to the references he cited.

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Insight and illusion

SIR: In our analysis of the Hamilton and Hopkins scales, when comparing imipramine and placebo, we found the Rasch model more appropriate than factor analysis. Dr Evans *et al* (*Journal*, August 1992, **161**, 272) fear that our results can induce the illusion of the death of factor analysis.

Hamilton (1977) has made it clear that placebo-controlled trials refer to general dimensions of therapeutic activity. When comparing imipramine with a placebo control in panic disorder we found it meaningful to refer both to a dimension of depression and to a dimension of outcome of general nature in panic disorder. In a cross-national setting we showed that the Rasch model item analysis was superior to factor analysis in evaluating the transferability of these general dimensions (depression and discomfort) across such variables as cultures, age, and sex.

Hamilton (1977) also made it clear that if we wish to know not only whether a drug has any therapeutic effect at all (i.e. better than placebo) but also to find its place within the other established drugs we should include items relevant for describing the clinical profile. In this respect, factor analysis might give an important, multidimensional insight.

The factor-analytic method in our study was similar to that used by Hamilton. This method might be inferior to the method suggested by Dr Evans *et al*. It would, therefore, be of great importance to