differences in outcome in the three studies quoted. There were some modest differences related to illness, such as severity, but probably more important are the roles of social adversity and health invalidity as maintaining factors for depression, with an ensuing poorer prognosis, and here the Australians seem to have a clear advantage. This is reflected in their earlier observation (Emmerson *et al*, 1989) that severe, enduring life difficulties were rarely encountered in this affluent part of Western Australia. One could infer that such considerations, alongside the striking absence of cases with symptoms which had endured for over a year before presentation, are likely to lead to an improved prognosis. Certainly their prognosis is more positive than that of Murphy's cohort (1983).

Yet even these factors must be set alongside the critical role of treatment adequacy and aftercare, which the Australian group aptly highlight, and which was the major weakness of Murphy's study (although she was not responsible for management of the patients she studied). An agreed treatment protocol should surely be as important a part of future research into the outcome of depression as is the use of agreed operational diagnostic criteria for selecting which patients are for study. This will be especially relevant if the comments of Drs Burvill *et al* are registered concerning the low statistical power of all the studies to date: the most expedient way forward might be multi-centre studies of outcome.

Drs Burvill *et al* devised two methods of (dichotomised) outcome, and used the second, more stringent, one to assess the relationship of predictor variables to outcome. Yet examination of these two methods of expressing outcome suggests that they are as much to do with the uncertainty which exists in trying to define what we mean by an outcome as they are to facilitate comparison with other studies.

One method expresses outcome cross-sectionally (ignoring intervening relapses, etc.) and the other incorporates some longitudinal component (for example, clinical course of illness). This too is an important aspect of future research. Tools, such as the longitudinal interval follow-up evaluation (LIFE; Keller *et al*, 1987) are available, which allow systematic recording of mental state, psychosocial functioning, life events, medication and physical health, or any other factor thought likely to be relevant to prognosis, over a defined follow-up. When combined with statistical methods, such as survival analysis, such instruments form the basis for prognostic statements which can take into account a wide range of factors and can then be applied to the individual patient.

BALDWIN, R. C. & JOLLEY, D. J. (1986) The prognosis of depression in old age. *British Journal of Psychiatry*, **149**, 574–583.
EMMERSON, J. P., BURVILL, P. W., FINLAY-JONES, R., *et al* (1989) Life events, life difficulties and confiding relationships in the depressed elderly. *British Journal of Psychiatry*, **155**, 787–792.
KELLER, M. B., LAVORI, P. W., FRIEDMAN, B., *et al* (1987) The longitudinal interval follow-up evaluation (LIFE). *Archives of General Psychiatry*, **44**, 540–548.
MURPHY, E. (1983) The prognosis of depression in old age. *British Journal of Psychiatry*, **142**, 111–119.

R. C. BALDWIN
York House
Manchester Royal Infirmary
Oxford Road
Manchester M13 9BX

AUTHOR'S REPLY: In reply to the letter of Dr R. C. Baldwin, I acknowledge and apologise for the misquote of the sex ratio, when referring to the paper of Baldwin & Jolley (1986). Even with this correction, our suggestion that the apparent unrepresentativeness of their 100 patients, compared with those presenting to most psychogeriatric services, as one possible explanation for the differences in the outcome of the three quoted studies, cannot be ignored. The causes of the differences are likely to be multifactorial, including the lower proportion of life difficulties encountered in the Western Australian study, as highlighted by Baldwin. We fully agree with his reservations about the methods of assessment of outcome. His comments coincide with our own views, which have been documented in detail elsewhere (Burvill *et al*, 1991).

BALDWIN, R. C. & JOLLEY, D. J. (1986) The prognosis of depression in old age. *British Journal of Psychiatry*, **149**, 574–583.
BURVILL, P. W., *et al* (1991) Issues in the assessment of outcome in depressive illness in the elderly. *International Journal of Geriatric Psychiatry* (in press).

P. W. BURVILL
Department of Psychiatry and Behavioural Science
The University of Western Australia
Perth
Western Australia

Lithium education programme

SIR: We read with great interest the article by Peet & Harvey "Lithium maintenance: standard education programme for patients" (*Journal*, February 1991, **158**, 197–200), which included several important issues about patients' compliance and knowledge about medication.

However, we would like to raise some points that we feel weaken their results. The first point concerns their sample. Looking at the characteristics of the

sample quoted, it is obvious that the patients tested had a longstanding illness, and that lithium had substantially reduced their relapses. This, by itself, is a sign that they were good compliance subjects, and brings into question the generalisability of the findings to poorly compliant patients: in particular that lithium leads to better compliance and therefore fewer relapses.

Secondly, as regards the method, it seems that the authors did not control for input of time. The education group had an educational-video presentation while the control group had nothing. This weakens the results of the study further, as it could be argued that any video presentation on a subject of mental health could have an effect on the patients' attitudes in itself.

We would like to draw attention to the fact that, while using the lithium knowledge test (LKT) repeatedly during this study, no account seems to have been taken of practice effect. I would have been interested to have seen the questionnaires themselves as well as their reliability and validation data.

Finally, we felt it would be of great use to see if the long-term effects of the study improved compliance and safety, and reduced relapse. This would require a longer follow-up than the 24 weeks of this study.

ATHANASIOS DOUZENIS
NEIL BRENER

Charing Cross Hospital
Fulham Palace Road
London W6 8RF

AUTHORS' REPLY: We thank Drs Douzenis & Brener for their interest in our work, concerning which they raise a number of points of method and interpretation.

The primary finding of our study was that the lithium information package led to a substantial improvement in patient knowledge about their treatment, which was sustained over 24 week follow-up. It is difficult to understand what Drs Douzenis & Brener mean by their statement that our study took no account of practice effects with the lithium knowledge test (LKT), when a control group was used to take such effects into account.

With regard to the effect of the educational programme on attitudes towards lithium, we state quite clearly in our article that improved attitude was not mainly due to the information programme, but that other factors in the study probably contributed. The point we make is simply that attitude to lithium was not harmed by a research programme in which information was presented about its adverse effects and, in fact, there was a modest improvement in attitude.

The patients attending our lithium clinic were all at different stages of treatment, not always of prolonged duration. In our experience, the mix of patients is typical of that found in lithium clinics elsewhere in the country. It is, therefore, likely that our findings are generalisable to other lithium clinic populations. We plan, with the help of sponsorship, to make the full educational programme widely available so that others can see for themselves.

The patients were not selected by us as 'good compliance' subjects, and we have examined self-selection in the two years up to the study. Eighteen patients left the clinic in that time. They were of comparable age and sex, and the mean durations of their illnesses and remissions were similar to those for patients still attending. Their reasons for leaving appear to relate to social mobility, physical illness or old age. Those remaining, on entering the education programme, showed attitudes on the lithium attitudes questionnaire (LAQ) suggesting doubtful compliance. As many as 45% expressed opposition to continuing with their lithium treatment.

Finally, the question of whether compliance would be improved, and relapse rate reduced, over the long term, as a result of proper education, remains to be answered. Our study was not designed to address this issue. Unfortunately, it would not be possible to perform such a study using a parallel control group, because of the ethical problems of purposely leaving a large group of patients relatively uninformed about their treatment. In our own case, long-term follow-up of patients has been handicapped by the dispersal of patients from the Lithium Clinic, due to the introduction of sector-related services in Sheffield.

NORMAN S. HARVEY
MALCOLM PEET

University of Sheffield Department of Psychiatry
Royal Hallamshire Hospital
Glossop Road
Sheffield S10 2JF

Computerised tomography in schizophrenia

SIR: Thank you for asking me to reply to Dr Miller's letter (*Journal*, June 1991, **158**, 863). "Unnecessarily dogmatic" I happily accept: "probably wrong" I think is unlikely. Nonetheless, Dr Miller raises an important issue. The realisation that the minor structural brain abnormalities seen in some schizophrenic patients might be non-progressive has been important in the conceptual shift from schizophrenia being a neurodegenerative disorder, to a neurodevelopmental view. To my eyes, the weight of evidence does not allow us to reject the null hypothesis that there is no progressive enlargement of

https://doi.org/10.1192/S000712500002448X Published online by Cambridge University Press