British Journal of Psychiatry (1991), **159**, 156–166

Correspondence

Editor: Ian Pullen

Contents: Prescribing practices/Outcome for elderly depressives/Lithium education programme/Computerised tomography in schizophrenia/'The disease concept of alcoholism'/Double blind acceptance/Ordering thoughts in thought disorder/Anabolic steroid use outside competition/Violence and sudden death/Treatment-resistant depression/Psychiatry and post-traumatic stress disorder/Access to Health Records Act, 1990/Chloroquine-induced mania/Catatonia and NMS/Amitryptyline in clozapine-induced sialorrhoea.

Prescribing practices

SIR: The following letter was written by my father John Dunne, Emeritus Professor of Psychiatry at University College, Dublin, shortly before his death at the age of 92, on 1 January 1991. Having qualified in 1922, he lived through many changes in psychiatry, such as the discovery of electroconvulsive therapy (ECT), antidepressants, and neuroleptics. However, in recent years he became concerned about certain prescribing practices; his views are stated below.

SIR: There is reason for concern about the prescribing of neuroleptics by some colleagues, who, firstly, are using very large doses, e.g. chlorpromazine 800 mg to 1000 mg, haloperidol 70 mg and over, etc., daily, and who, secondly, prescribe a number of neuroleptics, three, occasionally four, simultaneously, often in similar large dosage. This practice is most likely to occur if the patient's behaviour is difficult to manage, particularly in the setting of an acute in-patient unit.

Such practice is likely to get neuroleptics a bad name with the public at large, resulting as it does, in greater incidence of side-effects, which could lead to litigation by injured parties seeking compensation, and this, in turn, would make many individual psychiatrists reluctant to use them in adequate dosage. The discrediting of neuroleptics would also increase patients' resistance to taking them.

This would be a major disaster for psychiatry; I write as one still in active practice, who has been practising for over 65 years, and who recalls the treatment of psychotic patients before the introduction of neuroleptics.

It is my opinion that the prescribing patterns described are, firstly, due to pressure to get patients out of hospital in short periods of time, or to keep patients out of hospital, and secondly, due to failure to ensure that adequate numbers of trained nursing staff are available at all times, sufficient numbers being essential to nurse confidence. When nursing staff are confident that they can cope effectively with all behaviour, and that they themselves, and their patients, are safe, they can then promote the other aspects of a therapeutic environment; respect for, understanding of, and sensitivity towards each patient, accompanied by constructive communication and activity, thereby promoting recovery, and making untoward incidents unlikely. This has been my experience, and it is supported by extensive literature, dating from at least the 18th century to the present.

Pharmacology now makes a major contribution to psychiatric treatment, how major perhaps only individuals such as I fully appreciate, but this does not obviate the need for human therapeutic process. Attempts to replace human means of treatment by excessive reliance on pharmacology, for whatever reason, is not just a poorer form of treatment, it is likely to lead to the discrediting of very valuable drug therapies, with serious consequences for many who would benefit from them.

JOHN DUNNE

I believe that the views expressed in his letter are of great importance to current psychiatric practice.

DAVID F. DUNNE

St Stephen's Hospital
Sarsfieldscourt
Cork, Eire

Outcome for elderly depressives

SIR: The paper by Burvill *et al* (*Journal*, January 1991, **158**, 64–71) is a further helpful addition to the debate over what outcome can be expected nowadays for elderly depressives referred for specialist psychiatric treatment. Regarding the work which I carried out with David Jolley in 1986, there is one misquotation. It concerns the sex ratio of our sample, to which reference is made twice; the correct ratio is male:female = 1:4 (and not the other way round, as stated by Drs Burvill *et al*).

Having corrected this, and taking into account arguments from our original paper, the suggestion by Drs Burvill *et al* that our patients were not likely to be representative of those usually presenting to psychogeriatric services seems an unlikely explanation for

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