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SIR: Dr Waring *et al* raise a number of questions about our attempts to help the families of schizophrenic patients. We have not followed up our two trials beyond two years. Although we recognise the importance of a longer perspective, it is difficult to maintain control over the interventions received beyond two years. For the same reason, it is rare for trials of maintenance drugs to exceed this period. Uncontrolled follow-ups are of doubtful value; however, we did maintain contact with a number of families for three years or more and did not learn of any further suicides. Moreover, there were no suicides in our second trial, which may reflect improved intervention techniques, or merely better luck. With regard to the demand on professional resources, this is maximal during the first year of intervention for most families. Beyond that, families appreciate having access to professional help from time to time, but in many cases a phone call or letter once a month or less is sufficient.

Our difficulty in accumulating sufficient families was not due to their reluctance to accept the help offered, but to our stringent entry criteria for the trials. We focused on the highest risk families; those in which patients were in high face-to-face contact (more that 35 hours per week in the *same room* as the relative) with high EE relatives. These constitute no more than 1 in 6 of all schizophrenic admissions. We recognize the limitation this imposes on generalising our findings to "the vast majority of outpatient schizophrenics seen in day-to-day clinical practice", and have never claimed that our recommendations extend to all patients living with families. Indeed, the study of the Scottish Schizophrenia Research Group (1987) reveals that for patients living with relatives in a rural community, the prognosis for schizophrenia is remarkably good, only 19% being readmitted in the course of a year.

Our recommendations apply, not to these fortunate individuals, but to the high-profile patients who are frequently readmitted despite receiving drug therapy. We are therefore less concerned with the first onset patients that Dr Waring *et al* are attempting to help, although we do attempt to engage their families if they are high EE, high contact. We have not experienced any insuperable obstacles to this, perhaps because we begin the process of engagement by offering knowledge about the illness, for which relatives are eager, and follow on by attempting "to help the family to help the patient". We never target the family as the object of "therapy". An additional factor which facilitates engagement is our readiness to make home visits to those families who are unwilling or unable to come to us.

It is our view that reduction in high-EE attitudes does represent an increase in tolerance and understanding for the sufferer. However, this is also accompanied by significant changes in the coping behaviour employed by relatives. We are in the process of studying these in more detail.

Finally, we share the concern of Dr Waring *et al* that our intervention techniques may be perceived as beyond the capability of ordinary clinical teams. This has impelled us to study whether the techniques can be learned and effectively applied by psychiatric nurses in a routine clinical service. We shall report on this research in the fullness of time.

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Better to be depressed in Australia . . .

SIR: Kiloh *et al* (*Journal*, December 1988, **153**, 752–757) describe better outcome after 18 years among New South Wales depressives than among our Maudsley patients (Lee & Murray, 1988). They suggest that the poorer outcome in London may have been due to the mis-inclusion of schizophrenic and schizoaffective cases, and are inclined to dismiss our findings concerning the predictive power of the neurotic-psychotic continuum on similar grounds.

We have re-analysed our data after excluding all those patients who developed schizophrenia and schizoaffective disorder: 25% of the remainder still fell into the operationally defined poor-outcome group. We further examined whether the presence of delusion, hallucinations, or mood incongruent features during the index admission predicted poor outcome. None of these indicators of possible "mis-inclusion" were correlated with poor outcome. Finally, and most surprisingly, we found that the development of schizophrenia or schizoaffective

disorder was not associated with higher ('psychotic') scores on the neurotic-psychotic continuum at the time of the index admission. Thus position on the continuum did predict poor outcome, but not by identifying individuals who went on to chronic psychosis (Duggan *et al*, 1989). Perhaps it was the Australian group's lack of such a continuum score that led to their failure to replicate our findings of poorer outcome among the more endogenous/psychotic depressives.

The absence of any patients in the Australian series who developed schizophrenia-like psychoses is suprising, as these have been a consistent feature of other follow-up studies of depression (Bebbington, 1982). Perhaps their cases were less severe. Alternatively, any patients who developed schizophrenia may have remained hidden among their 12 patients who refused interview or could not be traced. In our series, in which 99% of patients were traced, some of those who were most difficult to trace turned out to have chronic psychosis.

We are puzzled by the fact that our data appears to have undergone an optimistic transformation during its journey to the antipodes. We found that between 25% and 30% of patients remained always incapacitated or died by suicide; Dr Kiloh *et al* (Table IV, p. 755) report our figure as 19%. We found that one-third of our patients fell into our very poor outcome group; they report our figure as 25%. The detail of their own analysis is confusing, for they report 17% always incapacitated or dead from suicide, but then say that only 11% fell into the very poor outcome group, despite using our definition in which the former is a subset of the latter. We also wonder whether the figures in Table III (p. 754) are strictly comparable, as the Australian group describe their own as based on "proportions readmitted" rather than on survival analysis.

These differences apart, we are intrigued by the possibility that it is better to have a severe depressive illness in New South Wales than in London, particularly since the Australian endogenous patients were much older and therefore one might have expected them to fare worse. Dr Kiloh *et al* suggest that more effective treatment may be the key and point out that 10% of their whole series received lithium. We have re-examined our own casenotes and found that 20% of the London series received lithium. Perhaps we in London should recommend emigration to our patients!

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. . . Or not?

SIR: We read with interest the papers by Lee & Murray (*Journal*, December 1988, **153**, 741–751) from London and Kiloh *et al* (*Journal*, December 1988, **153**, 752–757) from Sydney. The London paper focuses on shortcomings in previous studies on the outcome of depression and attempts to redress them. The Sydney group use a longitudinal study in a continuing attempt to validate the neurotic-endogenous classification.

A major problem with the London study is the selection of the initial cohort. We feel that the cohort is more likely to have included atypical cases. Firstly, only 17% of the subjects were from the catchment area. A significant proportion of the remainder were undoubtedly referrals for second opinion, and therefore more likely to have been atypical or severe cases. The authors attempt to counter this criticism using anecdotal evidence, but do not provide any data to rule out sample bias. The better outcome in the referred group may have been related to more aggressive treatment and follow-up. Secondly, at follow-up the sample had only four unipolar depressive patients. We are not questioning the fact that 'complications' may be an intrinsic part of depression. However, a cohort of 89 in which 3 patients have leucotomy for bulimia nervosa and 18 go on to develop other psychotic disorders is likely to have been biased towards severe or atypical cases. It would be useful to compare the rate of 'complications' between patients whose index admission was their first with those whose index admission was not their first. It would have been better to exclude patients over the age of 65 altogether, rather than include only two.

The Sydney paper makes no comment about admission patterns and possible sampling bias. Their cohort was not made up of consecutive admissions. Their follow-up was incomplete, in that of the 145 patients only 92% were traced and 72% of the survivors interviewed. Twelve refused to co-operate or could not be traced. In London more than half of the severely disabled group had lost contact with the psychiatric services. This suggests that the poor outcome