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Correspondence

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First episode schizophrenia: predicting outcome

SIR: Prediction of outcome following first schizophrenic episode is an area of great importance since clinical practice is likely to be influenced by positive findings, especially if it is suggested that interventions such as drug-treatment can disadvantage occupational functioning in new patients suffering from schizophrenia with a pre-treatment duration of illness of less than a year. This finding has face validity but unfortunately the paper by Johnstone *et al* (*Journal,* August 1990, **157,** 182–189) indicates the strength of this and all other associations only by providing a *P* value. I find this a great pity, and surprising, in a paper which addresses associations and has performed a logistic regression analysis (which yields measures of association). All a *P* value shows is the probability that an association could have been found by chance; it does not offer information about the strength of the association. If numbers are large, relatively low associations will be significant. The relevant statistical indicators are the correlations, and their confidence intervals between reported predictors and outcome, informing us of the amount of variation in outcome that can be explained by these predictors. I hope that these figures will be forthcoming, strengthening the importance of this study.

I was also unclear about the reason for the classification of all the unemployed as poor outcome, especially since the unemployed category contributed 88% of patients in the poor-outcome group, without any clarification as to whether these patients had always been unemployed, had become unemployed as a consequence of the illness before admission, or became unemployed after admission. An earlier paper reporting on the same patients (Johnstone *et al*, 1986) shows that 67 of the 253 patients (26%) were unemployed on admission. Assuming this group remained unemployed, they would either form 67% of good outcome patients (being unchanged) or 50% of the poor outcome group (being unemployed). Either decision is debatable and requires a justification. Possibly a refinement of outcome criteria is indicated, since such a homogeneous classification of unemployed patients may explain some of the significant correlations, and may have led to important but potentially misleading conclusions. I am looking forward to a clarification on this issue.

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Reference

JOHNSTONE, E. C., CROW, T. J., JOHNSON, A. L., *et al* (1986) The Northwick Park study of first episodes of schizophrenia: 1. presentation of the illness and problems relating to admission. *British Journal of Psychiatry*, **148,** 115–120.

AUTHORS' REPLY: We are grateful to Dr Muijen for his interest in our work and are sorry that he has found some aspects less than clear.

We are aware of the difficulties of statistical assessment in data like this, where a large number of variables are being assessed. We took this into account in our methods of calculation and mention the need for caution in the interpretation of data of this kind in the text. There are different views about the best means of dealing with this kind of data set, and it may help Dr Muijen to know that the analysis in the paper is a compromise between our own initial ideas and those of two referees whose views were to some extent conflicting.

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In the manuscript of the paper we initially included more information, but we were advised that the reader could be overwhelmed by the mass of data and that it would be better to highlight significant associations. From the point of view of most readers this is clearly good advice, although for those who are particularly interested in the data set it may be less than satisfactory. We would be pleased to provide interested colleagues with details of any particular aspect if they write to us directly.

We regret that we have expressed ourselves less than clearly with regard to the classification of patients' occupational outcome. Patients who were unemployed at follow-up were classed as unemployed even if they had also been unemployed at the time of the index admission. As may be seen from Table 5 of the paper, we did initially attempt a more refined classification of employment status, but the numbers who were employed in a higher status job were small and the numbers still in work but in a lower status not much greater. It is on the data in Table 5 that the result whereby good occupational outcome in patients with a relatively short pre-treatment duration was associated with the prescription of placebo medication during the follow-up period depends. All seven patients whose occupational status improved had a relatively short pre-treatment duration of illness. Six of them were prescribed placebo medication.

Dr Muijen indicates that he thinks we should not class patients as having a poor outcome because they are unemployed. There are many ways of looking at outcome in patients with illnesses which may be associated with long-term disability. For example, outcome in these patients has been considered in terms of relapse, death, in-patient days, judicial contact, ability to give child care and the rather non-specific concept of achievement. None of these measures, including employment status, is ideal and we understand from discussions with colleagues at the Maudsley Hospital that patients from their catchment areas are frequently unemployed and, even if they are reasonably well, are not likely to find work. This is not true of the areas in North-West London from which most of the patients in this study come. Many jobs are available, particularly for younger age groups. It is clear from the paper that many of the patients do work and it seems reasonable to say that at least in one sense the outcome in these patients has been better than that in the unemployed. Measures of outcome do not necessarily correlate. Perhaps the most disquieting finding of the study is the demonstration of better occupational outcome in patients with a short pre-treatment duration of illness who had been given placebo rather than active maintenance medication. In terms of relapse these patients did not do well but achieved their significantly better occupational outcome in the face of more relapses than similar patients prescribed active medication.

We hope that this letter provides the clarification that Dr Muijen seeks, but if there are any specific points he would like to discuss, he is most welcome to contact us.

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Gender-identity clinic patients

SIR: At a recent Journal Club meeting we discussed the paper by Burns *et al* (*Journal*, August 1990, **157**, 265–268), which proved to be interesting but contained some surprising incongruities in the tables of figures used. We note the following unexplained discrepancies.

The "DSM-positive" column of Table 1, the whole of Table 2 and the whole of Table 3 refer to the same population (DSM-positive transsexuals included in this study; $n=77$). However, the breakdown of this group of people differed depending on which table you look at:

(a) In Tables 1 and 2 there are 20 members of the sample with a mean age of onset over 13, 54 under 13, and three whose age of onset is "unknown". In Table 3 there are now 20 with mean age of onset over 13, 53 under 13, and four "unknowns".

(b) In Tables 1 and 2 there are 27 members of the sample listed as heterosexual and 12 as "sexual orientation unknown". In Table 3, 26 are listed as heterosexual and 13 are "unknown".

(c) In Table 1 only one DSM-positive transsexual displayed sexual arousal with cross-dressing and 69 did not. In Table 2, seven DSM-positive transsexuals displayed sexual arousal with cross-dressing and 67 did not. In Table 3, 10 DSM-positive transsexuals displayed sexual arousal with cross-dressing and 63 did not. The remainder of the 77 people (7, 3 and 4 respectively) were listed as "unknown" in each category.

(d) In Tables 1 and 3, 24 DSM-positive transsexuals were referred for surgery and 53 were not. In Table 2, 24 were referred for surgery, 49 were not and four were "unknown".

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