Dr. Parnas Replies

Ms. Shinkwin's letter concerning my report on assortative mating in schizophrenia raises a simple but important issue. However, her alternative explanation of assortative mating is not consistent with my data. The schizophrenic mothers met their mates on average 7.7 years (s.d. 8.9) *prior* to their first hospitalisation for schizophrenia. Only in 17 cases was a schizophrenic women first admitted prior to meeting her mate. Four of these women's mates were hospitalised for psychiatric reasons. In none of these four instances did a mating occur because of shared contact within the same treatment facility. In addition, there was no difference between the hospitalisation rate among mates of schizophrenic women and their controls.

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DST, Endocrines and Loss of Weight

DEAR SIR,

Fichter & Pirke's report (*Journal*, July 1985, **147**, 94–95) that starvation reproduces some of the neuroendocrine changes of depression is of itself not evidence that the neuroendocrine changes in depression are due to a smaller reduction of calorie intake. To make that point Fichter & Pirke should reproduce the neuroendocrine changes of depression in normal subjects whose diets correspond more closely to those of a depressed patient.

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Schizophrenia with Good and Poor Outcome

DEAR SIR,

Kolakowska *et al* studied the relationship of prognosis of schizophrenic patients and putative measures of organicity (computed tomographic brain scan, neuropyschological assessment of cognitive function and neurologic soft signs examination). They concluded that "organicity" is not necessarily associated with poor long-term prognosis and does not identify a clinically distinct subtype of schizophrenia (*Journal*, March 1985, **146**, 229–246 and April, 1985, **146**, 348–357).

Their data may warrant another interpretation. They report that normal ventricular-brain ratio (VBR) and relatively unimpaired cognitive function were characteristic of patients with good outcome. However, there was no difference in soft signs prevalence in patients with good, intermediate or poor prognosis. Thus the only "organic" parameter which was not correlated with prognosis as predicted, was the presence of neurological soft signs. Kolakowska *et al* performed the examination while 90% of their patients were receiving neuroleptics. They suggest that there was no relationship between current dose of neuroleptic and soft signs. However, it is possible that for some patients there is a low threshold for neuroleptic induction of soft signs. This may have undermined the utility of the soft signs examination.

Our data suggest that neuroleptics may obscure the difference in number of soft signs observed in sub-types of schizophrenia. We examined 86 schizophrenic patients who were off medication for a minimum of 10 days. The 27 process schizophrenics characterised by premorbid asociality had more soft signs than a group of non-process schizophrenic patients (1). There was a correlation between presence of soft signs, IQ, and the Hain's score on the Bender-Gestalt Test (1,2). The association of soft signs and diagnosis was not observed when the examination was done on patients receiving neuroleptics (1,2). This was attributed to the fact that non-process schizophrenics who were receiving medication had a mean of 2 soft signs (42 signs in 21 patients), whereas non-process schizophrenics off medication had a mean of .89 soft signs each (53 signs observed in 59 patients). Since early prescription of neuroleptics was determined by clinical state, an alternate interpretation is that more severely ill patients, who were closer to the process end of the spectrum, required early intervention with medicine. We view this interpretation as unlikely for the following reasons. Diagnosis was determined by a rater blind to drug treatment status. The ratio of process to non-process schizophrenic patients examined on and off medicine was roughly equal (1:2). If process schizophrenics were more likely to have early prescription of medication, it is unlikely that the ratio of process to non-process schizophrenics in the treated sample would be the same as the untreated sample.

The fact that the neuroleptics may have "caused" the soft signs observed by Kolakowska *et al* is not surprising if we consider that dysarthria, impaired hopping and foot tapping were among the most commonly observed signs in their study. These were relatively rare in our untreated patient sample. In fact, of the 5 most common soft signs observed by

Kolakowska *et al*, dysarthria, right-left confusion, hopping, foot taps and astereognosis, only right-left confusion was among the 5 most common signs observed in our unmedicated patients. Further evidence of a difference in the two samples is suggested by the fact that 76% (45/59) of drug free non-process schizophrenics in our sample showed 0–1 soft signs whereas in the sample studied by Kolakowska, only 38% (19/50) exhibited 0–1 soft signs. We think that the soft signs examination done by Kolakowska *et al* was compromised but the rest of their evidence does support a relationship between chronicity and "organic" impairment in schizophrenia.

Furthermore, Kolakowska *et al* suggest that the fact that not all those with chronic illness showed abnormalities on the measures of organic impairment undermines the utility of these signs in helping identify a distinct sub-type of chronic schizophrenia. Schizophrenia is a heterogeneous syndrome and it is unlikely that there is one form of the illness or a single pathophysiologic process which leads to chronicity. Thus we would not anticipate that all chronic patients would exhibit a particular cluster of signs, symptoms or course pattern.

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MRC Fluphenazine Trial

DEAR SIR,

In this generally instructive set of papers (*Journal*, May 1985, **146**, 464–480) it is stated that: "Very high inter-rater agreement was reached, and this was checked during the course of the study by a number of joint interviews (r always greater than 0.8)."

Joint interviews are no guarantee of independence (Robinson *et al*, 1982). A screen placed between the raters prevents each from seeing the other's pen move to paper during a verbal interchange, so that justice is seen to be done. Was this strategy used herein?

Does the use of the coefficient r imply that the authors have measured association rather than agreement? Have they corrected for chance agreement? Did they distinguish between agreement about symptoms and that about signs and if so, what were the figures?

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Reference

ROBINSON, M. L., COOKSON, I. B. & WHITE, K. (1982) The "Consentiam" effect: Are your joint ratings really independent? *British Journal of Medical Psychology*, **55**, 285–286.

Dr Curson and Colleagues Reply

Our awareness of problems such as reliability, agreement and bias should be evident. We attempted to maximise and measure inter-rater agreement in the three of us who conducted the assessments by extensive formal training and practice before the study commenced and by conducting some joint interviews during it to avoid phenomena such as "drift back". The method adopted was exactly that used in PSE training courses held at the Institute of Psychiatry and Guys Hospital. One of us (DAC) has now been a recognised teacher of the PSE for eight years. This accounts for the use of the Pearson correlation coefficient as found in the PSE Manual. We recognise that association can be very high while agreement is poor. We feel confident however, that the level of agreement was as good as that achieved in the independent assessment of social measures (p. 476–477). Differences in ratings were discussed at length after each joint interview; the score of the main interviewer was used for analysis, and the statistical check was done later to ensure that association was always greater than $r = 0.8$.

The clinical context of the study precluded the use of sophisticated techniques such as screens. Patients and their relatives were interviewed in a variety of settings ranging from dingy bedsitters and the kitchens of council flats to day centres.

The greater reliabiltiy of eliciting symptoms, especially on the PSE, is well established. In reality the threshold for rating signs on this instrument is so high that few patients scored at all.

We agree with Dr. Robinson that such issues are very important in psychiatric research. While perfection is difficult to attain, we feel that within the limits imposed upon us we did the best we could.

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