premature and unjustified conclusion untenable on the basis of their research design and use of inappropriate statistical methods. More specifically:

1. The criterion against which the rating scales were compared was a set of ratings of severity of depression made by two psychiatrists and a nurse. The authors use a weak non-parametric test (rank-order correlation) to test their own scale, yet use a relatively stringent parametric test to evaluate the rating scales. And as the authors compare rating scale scores on adjacent severity levels based on their own 'scale', then they should have presented reliability figures for *each adjacent pair of severity levels*. For all the reader knows, the pairs of items on which the BI and the WI are 'failed' could be of very low reliability: certainly a crude measure such as the rank-order correlations tells one little about the reliability of discrimination between two items.

2. Related to this first point, the scales scrutinized by the authors are a result of extensive piloting and validation studies. This is not the case for the *ad-hoc* scale devised by the authors. Why is this weak instrument used as a criterion by which to judge properly designed and tested instruments?

3. Finally, in many cases, self-rating inventories as opposed to interviewer-rating scales are necessary in research. The two self-rating scales which 'pass the test' do no better in my opinion, than do the Wakefield and Beck Inventories by the criteria of the authors' flawed design. Taking the author's 6 grade scale for instance, the Leeds Scales fail to discriminate between two levels at the 0.05 level of significance, and the Irritability-Depression-Anxiety Scale fails to discriminate on three levels. The Beck Scale, on the other hand, fails to discriminate on three comparisons, and the Wakefield on two. Why do the authors conclude that the two latter instruments are unsatisfactory, and the former ones satisfactory?

In conclusion, the authors fall short on scientific caution and experimental design, and their recommendation to abandon two instruments should be rejected until more adequate studies are conducted.

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DEAR SIR,

I welcome the opportunity to reply, on behalf of my colleagues, to Dr Robertson's criticisms of our study. Dr Robertson seems to take great exception to our choice of criterion which he refers to as 'a weak instrument'. He does not say what criterion he would have chosen; clearly it would have been futile to compare all the scales with yet another scale. We could have chosen some weak measure of severity such as in-patient, day-patient or out-patient status or videotaped assessments of some characteristic such as retardation. However we considered that our choice of a criterion, derived from an interview by an independent psychiatrist and a nurse who knew the patient well, was the best that can be obtained; we still maintain this and believe it is one of the major strengths of our study.

I do not follow Dr Robertson's objection to our use of a non-parametric rank correlation which is generally accepted for ordinal data; we were not 'testing our own scale' but providing information that we were justified in combining the psychiatrist's and the nurses' ratings into a single measure. Nor have we stated anywhere that we used 'rank order correlations to test the reliability of discrimination between items'; we used the parametric Student's t-test to distinguish between scale scores at different degrees of severity and still consider that we are justified in doing so. The further objection is made that we failed to provide information concerning the differences between the various grades in our criterion; again, I do not understand this since the information is all supplied in our first figure and the critical reader can soon assure himself that the differences between all successive grades are in fact statistically significant.

Our advice to abandon the use of the Beck and the Wakefield scales rests not only on the number of non-significant differences between successive grades but also on the finding that, in those two scales, higher scores were achieved at a lower compared with a higher grade. The advice also has another source which is that of economy of time; the Beck Depression Inventory consists of 21 items and the user is advised to read all these aloud to the patients. The expenditure of this amount of time would be justified if it resulted in a more accurate assessment but our study has shown this not to be the case.

Finally Dr Robertson appears to have a rather naive belief that certain well-known scales are better than others because they have been subjected to 'extensive piloting and validation studies': What in fact happens is that a scale is frequently devised, often after a minimum of preliminary work, and is subsequently found to work in a number of situations; this need not cause much surprise or prove that the scale is particularly good. At another place (Snaith, 1981) I submitted that one of the major impediments to progress in psychiatry was the primitive state of measuring instruments. Progress will continue to be hindered if research workers continue, perhaps out of some form of misguided loyalty, to adhere to the use of the scales provided by the earlier pioneers in psychometrics. The lack of comparative studies of various

extant scales is a serious drawback to rational choice and we still claim to have provided some useful information on which to base this choice. We do not think our claims were over bold or our study unduly flawed.

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Reference

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FOLATE, AFFECTIVE MORBIDITY AND LITHIUM THERAPY

DEAR SIR,

In their paper (*Journal,* July 1982, **141,** 87–9) Coppen and Abou-Saleh report significantly lower plasma folate concentrations in the lithium-treated patients than in the control subjects. Unfortunately, however, the validity of their observation is impaired in the absence of the pre-lithium folate values. The baseline data are important particularly because there is some evidence to suggest an interaction between lithium and folate metabolism (Herbert and Colman, 1980; Prakash *et al,* 1981). Besides, the control group does not appear to have been matched with the sample. The authors have also not commented on their findings of folate concentrations in the unipolar patients (N = 81) who not only constituted a larger but also more important subgroup of the sample because folate deficiency has been reported more frequently in depression than mania (Shulman, 1979).

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SHULMAN, R. (1979) An overview of folic acid deficiency and psychiatric illness. In *Folic Acid in Neurology, Psychiatry, and Internal Medicine,* (eds. M. I. Botez and E. H. Reynolds). New York: Raven Press, pp. 463–74.

DEAR SIR,

Dr Prakash's comment that the validity of our observation is impaired in the absence of the pre-lithium folate values seems unjustified. The underlying assumption is that lithium therapy *per se* could have caused the relative reduction in plasma folate concentration in these patients, but evidence supporting such a contention can only be described as anecdotal. Nevertheless, we fail to see how such an assumption could explain the observed association between low folate concentrations and high affective morbidity in these patients.

As regards his second point, we agree that these patients were not perfectly matched with the control group. Age makes no contribution to low plasma folate levels observed in psychiatric patients (Carney, 1979). In our patients, low, medium and high plasma folate groups had similar sex distributions with proportions of males to total of 37 per cent, 33 per cent and 40 per cent respectively.

As regards the last point, our data suggest that plasma folate levels are also reduced in mania.

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Reference

CARNEY, M. W. P. (1979) Psychiatric aspects of folate deficiency. In *Folic Acid in Neurology, Psychiatry and Internal Medicine,* (eds. M. I. Botez and E. H. Reynolds). New York, Raven Press, pp. 475–82.

SCHIZOPHRENIA AND LATERALIZATION OF GALVANIC SKIN RESPONSE

DEAR SIR,

Perhaps I may be allowed to comment on the recent paper by Gruzelier and Manchanda (*Journal,* November 1982, **141,** 488–95) in which they report that the direction of lateralization of the galvanic skin response differentiates two forms of schizophrenia, a retarded, emotionally withdrawn form and a type characterized by florid delusional symptoms and emotional reactivity. In their discussion the authors comment that such a subdivision has rarely in the past produced 'decisive psychophysiological and behavioural differences'.

Fifteen years ago, in my book *Personality and Arousal,* I demonstrated an almost identical dichotomy of the schizophrenias, revealed in the clustering of certain psychophysiological and psychological test measures in drug-free patients. I draw attention to this not to detract from the results reported by Gruzelier and Manchanda—which are indeed impressive—but to illustrate that the clinical typology they describe can be arrived at without reference to the notion of hemisphere dysfunction which is currently capturing interest as a possible neurophysiological basis for the psychotic states. My own work, which was carried out