reliability between observers for positive and easily defined symptoms is low. While standardised interviews do improve inter-rater reliability, the vast majority of ordinary psychiatric assessment is done on a more *ad hoc* basis and clearly insight is open to misinterpretation in this setting.

Secondly, the concept of pseudo-insight seems an important one. The hermeneutic value of an intellectual explanation of mental illness is important, but the form of an individual's appraisal of his mental disorder seems more significant than the content. Accepting treatment is one aspect of this, but perhaps an allowance needs to be made for the manner in which acknowledgement of medical illness comes about. There is a world of difference between the patient who says "I must be mad because you say so" and the resigned statement "you're right doctor, I'm breaking down again".

A third aspect relates to the psychiatrist's knowledge of mental disorder. His or her knowledge is generally by description (as opposed to by acquaintance). As insight is ultimately a clinical judgement of a patient by a doctor, what happens is for descriptive knowledge to be used to assess an experience that is classified as knowledge by acquaintance. There may not be a problem in this regard, but if knowledge by acquaintance is the route to insight, there seem no grounds on which to contradict a patient who tells his doctor "I have insight" when in reality he does not.

ALAN WEAR

Department of Clinical Neurology
Radcliffe Infirmary
Woodstock Road
Oxford OX2 6HE

Reference

KREITMAN, N., SAINSBURY, P., MORISSEY, J., *et al* (1961) The reliability of psychiatric assessment: an analysis. *Journal of Mental Science*, **107**, 887–908.

SIR: We agree with David's contention (*Journal*, June 1990, **156**, 798–808) that insight is best regarded as a multi-dimensional phenomenon. We would support Dr David's view that one such dimension is the ability of the patient to 'relabel' unusual mental events as pathological. However, we take the view that compliance with treatment should be seen not as a dimension of insight, but rather as a related phenomenon – as Lin *et al* (1979) have demonstrated, the correlation between insight and compliance is limited. This suggests that the schedule proposed by Dr David, which allows compliance itself to carry considerable weight, overemphasises the contribution of this variable to the core phenomenon.

The mechanisms underlying diminution of insight remain obscure, but are receiving increased attention. Insightlessness may be regarded as: (a) a normal phenomenon, insofar as many people demonstrate limited awareness of certain characteristics of their personality and behaviour; (b) a defence mechanism (denial); (c) a delusional phenomenon; (d) a feature of the schizophrenic defect state; and (e) a specific defect of cognition.

We have been attempting to operationalise the concept of insightlessness in schizophrenic patients. Given that direct measurement of the components of insightlessness is not possible, our proposed scale attempts to derive an overall measure, based on a semistructured interview. The scale distinguishes between attitudes to overall management and those to compliance with physical methods of treatment. Additionally, the scale permits measurement of behaviour in response to changes in psychopathology, perhaps the most important indicator of insight. An assessment of attitude to previous episodes of illness is included, an element which varies considerably between patients. Allowance is made for those subjects who reject the philosophical concept of mental illness, since it would be inappropriate to necessarily regard those as insightless. The scale measures insightlessness rather than insight, since the former has greater and more relevant clinical utility. (The schedule and score sheet are available from the authors.)

A pilot study of 13 patients fulfilling diagnostic criteria for schizophrenia indicates that scores derived from use of the schedule correlate well with global clinical impressions of insightlessness. In many of these patients, the degree of insight was not obviously correlated with the extent of delusional conviction.

TIMOTHY LAMBERT
DAVID S. BALDWIN

The Three Bridges Regional Secure Unit
St Bernard's Wing, Ealing Hospital
Uxbridge Road, Southall
Middlesex UB1 3EU

Reference

LIN, I. F., SPIGA, R. & FORTSCH, W. (1979) Insight and adherence to medication in chronic schizophrenia. *Journal of Clinical Psychiatry*, **40**, 430–432.

AUTHOR'S REPLY: I am delighted that Dr Culliford finds my discussion on insight in accord with his clinical practice. The point about one patient's insight into another's delusions is a fascinating one as it suggests that the basis mechanisms of logical inference leading to a delusional misinterpretation may be

domain-specific rather than generally impaired. This is analogous to agnosias confined to certain classes of things seen in the neuropsychological literature (e.g. Warrington & Shallice, 1984). Dr Wear chooses to tackle the vexed issue of assessing insight. He is a little unfair in invoking Kreitman's classic 1961 paper, as our profession has moved on since then. The World Health Organization in the *International Pilot Study of Schizophrenia* (WHO, 1973) found that insight, defined operationally, achieved respectable inter-rater reliability coefficients of around 0.77. However, the deeper question is how we interpret the attitude behind the words of our patients. Here we begin to lose on the swings of reliability hopefully to gain on the roundabouts of validity. When it comes to treatment compliance it seems most safe to consider both what the patient says and what he or she does.

The schedule proposed by Drs Lambert & Baldwin has much in common with my own. However, they are mistaken in their attempts to pin down the 'core phenomenon' since I propose that there is no single core but at least three separate but overlapping constructs. It is not surprising that 'insightlessness' as measured by their scale does not correlate with delusional conviction since this aspect of insight is ignored. Patients may have convictions, perhaps of delusional intensity, as to whether they are ill or not but this is separate from other delusions concerning their bodies, minds or the state of the world. Exploring these relationships will teach us much about the nature of psychosis.

ANTHONY DAVID

Institute of Psychiatry
De Crespigny Park
Denmark Hill
London SE5 8AF

References

WARRINGTON, E. K. & SHALLICE, T. (1984) Category specific semantic impairments. *Brain*, **107**, 829–854.

WORLD HEALTH ORGANIZATION (1973) *Report of the International Pilot Study of Schizophrenia (vol. 1)*. Geneva: WHO.

The 'new cross-cultural psychiatry'

SIR: The invitation to contribute a review article on recent developments in psychiatry and anthropology was welcome. When my article was published (*Journal*, March 1990, **156**, 308–327) I was somewhat disconcerted to find it appearing in an issue specially devoted to 'cross-cultural psychiatry'. Disconcerted, because I had suggested that current work in the two disciplines argued that contemporary psychiatry could be faulted for ignoring the context of its own assumptions and methods through relegating the cultural domain into something called 'transcultural psychiatry', whose subject matter was that of ethnic minorities and non-British and non-American communities. Precisely what the other papers were about.

My surprise was compounded by the unprecedented editorial by one of the associate editors (Leff, *Journal*, March 1990, **156**, 308–307) which did not attempt to introduce the papers, or indeed 'cross-cultural psychiatry' however defined, but instead was concerned solely with my paper. While one should doubtless be flattered at being singled out for the sort of 'health warning' it offered, this novel procedure does raise certain questions about the editorial impartiality of the *Journal*. Surely the place for scholarly debate and criticism is the space devoted to your correspondence columns, not a preceding editorial?

In this editorial, Professor Leff makes certain flippant assertions about the newer approaches, which he derives from my review but which remain by and large mistaken. His proposals for further work are generally unexceptional and recapitulate sections of my paper, but he simplifies the notion of a biological-sociological explanatory continuum which I point out is a conventional representation, not the basis for seriously considering the relative contribution between the biological and the social, a basis which is impossible when we are concerned with a dialectical relationship in which each responds to the other in a complex manner (Simons, 1985).

In one respect Professor Leff reiterates a conventional error. Culture is not cultural distance even if the latter is more easily measured. In attempting to ascertain the cultural contribution to psychopathology he suggests the problem is simplified if we hold culture constant, as in his comparison of patients from Salford and London. The baby has however followed the bath water (to employ his aphorism) for, if culture is held constant in a study of difference, then the only observable differences which remain are these of individual, biological and psychological variation. To take a rather simpler instance, if access to nutrition is in part determined by class status we find associations between cultural position and physical height: in an egalitarian society culture determines nutrition rather less and thus differences in height are determined especially by hereditary factors. That does not make 'height' *per se* a genetic phenomenon. Similarly, if we reduce cultural psychiatry to comparative epidemiological studies in which we attempt to control for culture, culture vanishes to make the phenomena of interest apparently biological or psychological in nature. It