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.

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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

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then has to be reintroduced through focusing, as Professor Leff puts it, on "the societies that are most culturally different from the west, and hence of greater interest". If all societies are becoming increasingly similar, as he argues, then patterns of, say, overdoses presumably become similar and one eventually concludes that overdoses in general are not a social phenomenon. Not surprisingly, two papers in that issue of the *Journal*, both concerned with the perceived consequences of parasuicide, were not in the 'cultural' section.

The newer critiques do not, as Leff suggests, necessarily fault psychiatry's *aspirations* to be a scientific discipline: that is one that seeks explanations independent of the observer's perspective. That at least should be clear from my review. Indeed I myself warn against the too casual neglect of evidence from the biological sciences. The problem is that the wish to be scientific, and the claim to be scientific, are often very different from actually being so. The difficulty with much of the older transcultural psychiatry, carried out by psychiatrists untrained in any social science, is that it mistakes the particular for the universal, the contingent for the necessary, the political for the biological.

To distinguish the two sets of categories is presumably the aim of any attempt at scientific truth. In certain particulars we find that the context of observation determines the observed events to the extent that locating patterns such as overdoses or agoraphobia solely in a person's individual characteristics does not help us to interpret the pattern at all.

These difficulties are not of cause restricted to psychiatry. My speculation as to the value of 'pathology' (or disease) is not, as Leff protests, some sort of Laingian romanticism but a concern as to whether any conception of 'pathology' is truly useful in scientific terms: we are, as the medical student joke has it, on the side of the human not the virus. Fair enough, as humans embedded in our illnesses and in our struggles we are compelled to act, but the prescriptive urge is not necessarily the appropriate ground for understanding. My suggestion in the paper which Leff refers to (Littlewood, 1984) that the experiences associated with what we psychiatrists conventionally refer to as cerebral pathology may at times be taken up by societies in certain situations as meaningful experiences is not an exhortation to consider this as the real meaning, merely that it can occur and that it may have interesting theoretical implications for us. I deal with this possibility at greater length in a forthcoming volume which uses field data including conventional Present State Examination assessments (Littlewood, 1990).

Professor Leff's editorial, its appeal to the demands of the 'practical difficulty' as justification for the validity of the findings, is an instance of the confusion between fact and value which many of us associated with the 'new cross-cultural psychiatry' argue is inevitable. We do not however pretend it can always be avoided, or allow value to masquerade as fact. A little closer examination of epistemology, of actually learning to distinguish the baby from the bathwater, will not come amiss.

Incidentally, in his put down of local knowledge, Professor Leff characterises the Yoruba masculine power Shopana (Shopona, Sopono) as a 'goddess'. I am not sure if he is thinking of the probably cognate Ewe/Fon power Shapata which is sometimes represented as a generic or androgynous emanation of the Mawu/Lisa principle. To characterise these concepts as free-standing deities rather than powers, principles, faculties or even mechanisms is, in any case, problematic. This is not trivial scholasticism but an example of the sort of problem we run into when we interpret others' meanings through our own frameworks. Our own categories of neurosis are hardly independent of assumptions about gender.

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Progesterone prophylaxis?

SIR: I was most surprised to read Meakin & Brockington's statement that "Progesterone is widely used in the treatment and prophylaxis of post-natal depression (*Journal*, June 1990, **156**, 910). To my knowledge, progesterone has never been used successfully as a treatment for post-natal depression. In fact, prolonged administration of progestogens may lead to depressive symptoms (Silverstone & Turner, 1982). Dalton (1985) claimed that progesterone prophylaxis was successful in reducing a recurrence rate of post-natal depression from 68% to 10%. However, the study was flawed in two ways. Firstly, it was not double-blind. Also, there was no