

Anthropology and Psychiatry The Role of Culture in Cross-Cultural Research on Illness*

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To illustrate the contribution anthropology can make to cross-cultural and international research in psychiatry, four questions have been put to the cross-cultural research literature and discussed from an anthropological point of view: 'To what extent do psychiatric disorders differ in different societies?' 'Does the tacit model of pathogenicity/pathoplasticity exaggerate the biological aspects of cross-cultural findings and blur their cultural dimensions?' 'What is the place of translation in cross-cultural studies?' and 'Does the standard format for conducting cross-cultural studies in psychiatry create a category fallacy?' Anthropology contributes to each of these concerns an insistence that the problem of cross-cultural validity be given the same attention as the question of reliability, that the concept of culture be operationalised as a research variable, and that cultural analysis be applied to psychiatry's own taxonomies and methods rather than just to indigenous illness beliefs of native populations.

Anthropology and medicine enjoy a long relationship. Among the late nineteenth and early twentieth century founders of anthropology were physicians who possessed social psychological and psychosomatic orientations (e.g., W.H.R. Rivers in England, Paul Broca in France, Rudolph Virchow in Germany). Key figures in the development of psychiatry maintained a cross-cultural interest and read the relevant anthropological literature (e.g., Emil Kraepelin, Sigmund Freud, Adolph Meyer, Aubrey Lewis, to mention only a few). Similarly, leading figures in twentieth century anthropology (most notably Alfred Kroeber and Edward Sapir in the US, Bronislaw Malinowski and Meyer Fortes in England, and Claude Lévi-Strauss in France) kept up with relevant developments in psychiatry and particularly psychoanalysis. In the 1940s and 1950s, the 'Culture and Personality' school of North American cultural anthropology brought together anthropologists and psychoanalysts in research seminars and collaborative projects. Even at present, although the two fields are moving in very different directions, many anthropologists maintain strong interest in psychiatric questions and, at least in North America, a small number are members of psychiatric research units. On the other side, psychiatrists conducting international, cross-cultural and cross-ethnic studies frequently cite the writings of anthropologists, and there is even a small cadre

of psychiatrists who, like myself, are trained in anthropology.

Yet, for all this cross-disciplinary interest (at least in North America), there is little evidence that anthropology exerts any significant influence in psychiatry, or vice versa. Even in cross-cultural and social psychiatry – the two subdisciplines that come the closest in problem framework to the concerns of the social sciences – anthropology is marginal: more of informal, heuristic interest than of central, substantive importance. Rarely are psychiatric trainees systematically taught about relevant anthropological concepts and findings, even where those trainees spend significant time practising in cross-cultural or cross-ethnic settings. Just as rare is the psychiatrist engaging in cross-cultural research who raises anthropological questions and findings pertinent to his enquiry. This is a harsh judgement, but I believe the facts support it: psychiatry proceeds, even or especially in centres that are non-Western or ethnic, as if anthropology did not exist, or was irrelevant to illness and care (Kleinman, 1985).

Conversely, anthropology graduate students most often go to the field to study the relationship of culture to illness or healing with hardly any systematic preparation in relevant diagnostic, epidemiological or treatment issues. Indeed, there are not a few anthropologists studying illness who seem remarkably unaware of key changes in psychiatry's data base and conceptual orientations. It is embarrassing to read anthropological accounts that provide sensitive discriminations of the heterogeneity

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of folk healing systems but caricature biomedicine and psychiatry.

This situation is, to my mind, deplorable. There are, however, heartening indications that it may improve. The argument I will advance is that much is to be gained by a robust, two-way relationship between psychiatry and anthropology; ambitious and fundamental exchange across the two disciplines could contribute in quite practical ways to the strengthening of both. In this paper I will illustrate what an anthropological orientation can contribute to psychiatry by examining four key questions that anthropology raises concerning cross-cultural and international research in psychiatry. While almost all psychiatrists are impressed by the revolutions in biochemistry and neuroscience and their transforming effects on the profession, few realise that medical and psychological anthropology have also undergone a transformation which has made anthropological questions more pertinent to epidemiological, psychopathological, and treatment concerns.

Four Anthropological Questions for Psychiatry

To what extent do psychiatric disorders differ in different societies?

An anthropological reading of the literature in cross-cultural and international psychiatry reveals a strong bias of psychiatrists toward 'discovering' cross-cultural similarities and 'universals' in mental disorder. First, this bias should not come as any surprise. Much of cross-cultural psychiatric research has been initiated from a wish to demonstrate that psychiatric disorder is like other disorders: it occurs in all societies, and it can be detected if standardised diagnostic techniques are applied. Clearly this was an interest of the WHO's International Pilot Study of Schizophrenia (1973), which expected that core symptoms of schizophrenia would cluster together in more or less the same way in Western and non-Western, industrialised and non-industrialised, societies. It should come as no surprise that this is what the IPSS found, in a non-epidemiological, clinic-based comparison that applied a template of symptoms to psychotic patients in a range of societies to identify groups of patients who seemed similar. The problem with this study is that it leaves out those patients who fail to fit the template, the very patients of greatest interest from a cultural perspective, because they could be expected to reveal the greatest amount of cultural diversity. The IPSS also found that outcome varies inversely with the social development of the society – a striking finding

attesting to cultural difference, but one that has taken back seat to the emphasis on cross-cultural similarity (World Health Organization, 1979).

Following IPSS came the Determinants of Outcome Study, a similar study but one which began in an epidemiologically more rigorous manner with first-contact incidence sample of patients with psychotic disorder. Let us look in detail at a report of the findings of this study of more than 1300 cases in twelve centres in ten countries (Sartorius *et al*, 1986). It is a very important project that has been conducted with more rigour than most in the cross-cultural field, and it illustrates the divergent perspectives of psychiatry and anthropology. The authors note:

The frequency of the use of individual ICD subtype rubrics varied from 0 to 65% of the cases in the different centers. Overall, paranoid schizophrenia was the most commonly diagnosed subtype followed by that of 'other' (undifferentiated) and acute schizophrenic episodes. However, in the developing countries the acute subtype diagnosis was used almost twice as often (in 40% of the cases) as the diagnosis of the paranoid subtype (in 23% of the cases). Catatonic schizophrenia was diagnosed in 10% of the cases in developing countries but in only a handful of cases in the developed countries. In contrast, the hebephrenic subtype was diagnosed in 13% of the patients in the developed countries and in only 4% of the patients in developing countries.

Despite this impressive evidence of cross-cultural differences, the authors conclude:

Patients with diagnosis of schizophrenia in the different populations and cultures share many features at the level of symptomatology . . .

and

Once the existence of broad similarities or manifestations of schizophrenia across the centers was established . . .

The authors are of course correct that they do have evidence of 'broad similarities', evidence they choose to highlight. But they also have evidence of substantial differences that they choose to de-emphasise.

Or take, as another example, the data on annual incidence of schizophrenia. Two calculations are made – one for a 'broad' diagnostic definition of schizophrenia that includes virtually all cases sampled, and another for a restrictive definition based on the CATEGO computer program class S+. For the former, the rates per 10 000 population range from 1.5 in Aarhus to 4.2 in Chandigarh's rural

center. For the latter, the range narrows impressively from 0.7 in Aarhus to 1.4 in Nottingham. Sartorius *et al* discuss this change by arguing that the application of the restrictive definition does not result in loss of statistical significance owing to decreased sample size. They conclude later in the paper that there is a relatively uniform rate of incidence for schizophrenia across the ten societies. From the perspective of psychiatric epidemiology and biostatistics this may be a valid conclusion, but from an anthropological perspective it is tautological. The 'broad' sample, from the anthropological perspective, is the valid one, since it includes all first-contact cases of psychoses meeting the inclusion and exclusion criteria. The 'restricted' sample is a 'constructed' sample, since it places a template on the heterogeneous population sample and stamps out a homogeneous group of clinic cases. The restricted sample demonstrates to be sure that a core schizophrenic syndrome can be discovered among first-contact cases in widely different cultures. This is an important finding, but it is not evidence of a uniform pattern of incidence. Indeed, the restrictive sample leaves out precisely those cases that show the greatest cross-centre and cross cultural differences and therefore the ones that disclose that the pattern of incidence is not uniform. This sample is of most interest to anthropologists since it demonstrates a wide range of cultural differences. Ironically, the finding of a wider range of incidence, which is in keeping with the finding in other studies of significant variation in the epidemiology of schizophrenia world-wide, should be of most interest to those concerned with the 'biological base', because this is what would be expected of a genetically based disorder. Several other key instances could be cited in which the evidence reviewed by the WHO group discloses both important similarities and important differences, but the authors elect to focus principally on the former, namely the 'universals'.

When this outstanding group of investigators review the data on the differential course of schizophrenia across centres (with better outcome in the developing societies), they report that this finding holds up even when mode of onset (acute versus chronic) is controlled. For over 10 years this finding has been the most provocative to emerge in cross-cultural psychiatry. Readers will be disappointed, however, if they hope to learn more about its sources or implications. The authors are silent on these points, which apparently have not received detailed investigation. That is to say, the most important finding of cultural difference – arguably the single most important finding in the study – receives scant

attention compared to that devoted to the findings of cross-cultural similarity. In the summary of the paper, the other findings of cultural difference in mode of onset, symptomatology, and help-seeking are de-emphasised as well.

If this were a single instance of interpretive bias in cross-cultural psychiatric research, it would be one thing: in fact, it is representative of the dominant interpretive paradigm in cross-cultural psychiatry. There is a tacit professional ideology which functions to exaggerate what is universal in psychiatric disorder and de-emphasises what is culturally particular. This view fails to acknowledge, moreover, what the evolutionary biologist knows: biology is the great source of diversity, not just of uniformity (Mayr, 1982). Had I reviewed the anthropological literature, I could have demonstrated the obverse.

A more valid interpretation of the data base emerges when the two viewpoints, both biased but in opposite directions, are reviewed together. The findings for schizophrenia, major depressive disorder, anxiety disorders, and alcoholism disclose both important similarities *and* equally important differences. Hence the first anthropological question (How do psychiatric disorders differ across cultures?) is a necessary complement to the regnant psychiatric question (How are psychiatric disorders similar across cultures?).

Indeed, for all the talk about ethnological comparisons, anthropology is essentially a field of detailed, intensive, single-culture studies in which comparisons are made by a scholarly review of the literature on other cultures (Kleinman & Good, 1985) rather than by 'controlled' cross-cultural comparisons. Hence anthropological research points to the importance for psychiatry of scholarly analysis of studies of mental illness in one culture to deepen our understanding of cultural differences: in turn it benefits from psychiatry's example of undertaking multicultural projects in which mental illness is studied simultaneously in the same methodological framework in different societies – something anthropology has not done.

I have taken the liberty to be critical of the WHO research program because of its importance and the fact that the WHO group's studies are among the most rigorous and sophisticated in the cross-cultural field. I have every reason to believe that following this initial stage of demonstrating cross-cultural universals in mental illness the WHO group will begin to correct the balance by turning to the differences, which is already beginning to happen (c.f. Marsella *et al*, 1985).

Is there a tacit model in cross-cultural psychiatric research which exaggerates the biological dimensions of disease and de-emphasises the cultural dimensions of illness?

This question is a corollary of the first question. An anthropological analysis of cross-cultural research in psychiatry shows that the model of pathogenicity/pathoplasticity comes fairly close to being a professional orthodoxy. In this revealed version of the psychiatric truth, biology is presumed to 'determine' the cause and structure of disorder, while cultural and social factors, at most, 'shape' or 'influence' the content of disorder. The classical example is paranoid delusions in schizophrenia or depressive psychosis: the biologically based disease causes the delusional thought process but the system of cultural beliefs organises the content of paranoid thinking – here as fear that the CIA is out to harm one, there as belief that the KGB is the culprit.

The widespread finding that somatic symptoms in depression and anxiety disorders play a more central role in the experience and expression of disorder in non-Western societies and among ethnic groups in the West is another informative case. Somatic amplification and hypochondriacal focusing are seen as distinctive manifestations of a similar underlying disorder. The latter is said to be 'masked' by the former. In this stratigraphic vision, biology is bedrock, whereas psychological, and in particular social and cultural, layers of reality are held to be epiphenomenal. They need to be stripped away to disclose the 'real' disease. As for medical anthropology's distinction between illness and disease – in which illness is the patient's perception, experience and communication of symptoms, while disease is the clinician's reformulation of the problem in terms of psychiatric models – disease is taken to be 'real' and hidden by the illness, which is a cultural artifact: catatonic or somatic or hysterical manifestations of the underlying causal process. Diagnosis then becomes reductionist, the semiotic interpretation of 'signs' of disease as an entity or object out of "the blooming, buzzing confusion" of illness symptoms.

The anthropological perspective suggests an alternative model. Depression experienced entirely as low back pain and depression experienced entirely as guilt-ridden existential despair are such substantially different forms of illness behaviour with different symptoms, patterns of help-seeking, course and treatment responses that though the disease in each instance may be the same, the illness rather than the disease is the determinant factor. There is overwhelming evidence in North American society

that the social and psychological components of the illness experience of chronic pain are more powerful determinants of disability and return to work than the biological abnormalities, which are usually 'real' enough (Yelin *et al*, 1980; Stone, 1984). Hence in the newer medical anthropological model, biological and cultural factors dialectically interact. At times one may become a more powerful determinant of outcome, at other times the other, but most of the time it is the interaction (the relationship) between the two which is more important than either alone as a source of amplification or dampening of disability in chronic disorder. Furthermore, that dialectic may transform the biology just as it alters social relationships (Lewontin *et al*, 1984).

The pathogenetic/pathoplastic model also does an injustice to the culture-bound syndromes. The tendency is to interpret these as illness manifestations of particular 'underlying' disease: e.g., *susto* is depressive disorder; semen loss syndromes are anxiety disorders; *amok* is brief reactive psychosis; etc. The picture is much more complex. Carr (1978, 1985) shows that *amok* is a final common pathway of behaviour along which are channelled various kinds of problem: acute and chronic psychoses to be sure, but also criminal behaviour without psychopathology, alcohol- and drug-induced states, and so forth. Guarnaccia *et al* (1987) and Low (1985), among others, demonstrate that *ataque de nervios* among Hispanics is not simply conversion disorder, but may be anything from a medical disease, to schizophrenia, to a culturally appropriate bereavement reaction. The anthropological model of an idiom of distress offers a more accurate mapping of the experience of culture-bound disorder, and its sources and consequences, than does the medical model. Alternatively, the demonstration of Rubel *et al* (1984) that *susto* is associated with higher rates of mortality indicates the necessity of combining biomedical and anthropological assessments.

What place does translation have in cross-cultural research?

Medical, including psychiatric, research often proceeds as if translation were a nuisance to be quickly handled in much the same way as one controls the demographics in matched samples. For psychologists, translation is rendered a technical problem, one that can be handled through a rigorous process of translation by one set of bilingual key informants, back-translation into the original language of the psychometric instrument by another set of bilingual informants, negotiation of the differences, and

testing of reliability of the instrument, when compared with other measures of the same phenomena that have already been used in the recipient society, and when used by different groups of investigators. But, for the anthropologist, translation is neither of nuisance value nor a strictly technical problem; it is the very essence of ethnographic research. For the anthropologist, translation of findings into terms and categories that allow for cross-cultural comparison is the final, rather than as in psychiatric research, the first step. Therefore, the anthropological contribution to psychiatry in this area is to model a much more rigorous, systematic and contextual approach to translation. Let us examine a few examples.

Most assessment instruments are developed in a vernacular that is quite difficult to translate into other languages. North American diagnostic instruments, for example, employ the terms 'feeling blue' or 'feeling down' to assess dysphoria. A strictly lexical translation of these terms is meaningless in most non-Western languages. Manson *et al* (1985) translated the NIMH-sponsored Diagnostic Interview Schedule (DIS) into Hopi, an American Indian language. One DIS item combines the concepts of guilt, shame, and sinfulness. Twenty-three bilingual Hopi health care professionals clearly distinguished each of these concepts from the others and indicated that three separate questions were required to avoid confounding potentially different responses. Kinzie *et al* (1982) discovered much the same thing in developing a Vietnamese language depression scale. They found that 'shameful and dishonored' but not 'guilt' discriminated depressed from non-depressed Vietnamese Americans.

Gaviria *et al* (1984), working with a translation of the DIS in Peru, detected five kinds of problem in its validity that offer a broader sense of problems in translation. First, by content validity they meant that the content of instruments must be relevant in the culture into which the instrument is translated. Thus, many of the substances in the substance abuse section of DIS were unavailable in Peru; yet coca paste, a major drug in Peru, did not appear in DIS. Secondly, semantic validity requires that the words used in the original DIS and the new instrument have the same meaning, a problem we have already reviewed. Thirdly, technical validity refers to societies where languages are not written (or at most by an elite), schooling is limited and rates of illiteracy are high. In this context, the process of answering a questionnaire is foreign and one may elicit answers that represent a misunderstanding of intentions more than an accurate reflection of affect or thought. Fourthly, criterion validity measures whether

responses to similar items relate to the same normative concept in two cultures. The presence of culturally consonant hallucinations among American Indians undergoing uncomplicated bereavement is normative in these tribal societies but not so in the broader North American culture. Assessment of hallucinations among American Indians must take account of this alternative norm. Fifthly, conceptual validity requires that responses to an interview relate to a theoretical construct within the culture. Neurasthenia is a key popular concept of disorder in China but no longer forms a coherent category for most North Americans (Kleinman, 1986). For a research interview to be conceptually valid in China, it would need to operationalise this category, not simply list its symptoms. In their entirety, several symptom check-lists include items that appear in locally salient syndromes like neurasthenia. By not organising these into operationalised syndromal clusters, the symptom check-lists fail to elicit subjects' responses.

Intra-cultural diversity makes the process of culturally meaningful translation even more complex. Canino *et al* (1987) found that in using the Spanish language version of the DIS developed at the University of California for use with Mexican Americans in a study of Puerto Ricans, they had to alter 67% of the items to adapt the instrument to the colloquial Spanish spoken by Puerto Ricans.

To adequately assess cultural differences, it is essential to translate local idioms of distress and add them to standard questionnaires. This may seem obvious, but it is not routinely done. For example, Ebigo (1982) has demonstrated that Nigerian psychiatric patients have a unique set of somatic complaints which are not represented in standard symptom-screening scales. In a number of African cultures, anxiety is expressed as fears of failure in procreation, in dreams and complaints about witchcraft. To adequately evaluate psychopathology in these settings, those common idioms must be assessed. The most impressive attempt to take translation into account in a psychiatric epidemiological study is the study by Manson *et al* (1985) of depressive disorder among Hopi. They built an instrument with two components: a very carefully translated version of the DIS that met each of the issues reviewed above, and an operationalised list of symptoms and categories from Hopi indigenous nosology that had a *prima facie* resemblance to depression.

From an anthropological perspective, the problem of translation is that there may exist objective and universal referents for a term like headache which enables a process of searching for semantic equivalence. But, as Good *et al* (1986) note,

In general, however, the referents of symbols – i.e., their meaning – are aspects of a culture or a life world, not objects outside of language through which language obtains meaning. ‘Heart discomfort’ for Iranians is not the equivalent of ‘heart palpitations’ for Americans; it does not *mean* the same thing (c.f. Good, 1977). It is a symbol which condenses a distinctive set of meanings, a culture-specific semantic network . . . Complaints of feeling impure in India refer to a semantic domain of profound cultural significance, one which expresses distinctions in caste, sexuality and social hierarchy: there is no equivalent among North Americans.

The Chinese term *huo qui da* refers to a feeling of heat rising from abdomen into the chest. It is based on the traditional Chinese medical concept that *qi* (vital energy) can be excessively hot and thereby cause symptoms. But it also metaphorically conveys the idea of an irascible personality. This metaphoric meaning would not be tapped by standard research instruments or translating practices: yet it is important for psychiatric assessment. Fear of obesity and of houseboundness in the USA does not carry the same meaning in South Pacific, African and Asian societies. Ideally, clinical care should provide a kind of culturally sensitive mini-ethnography of a patient that encompasses these cultural and personal metaphors. How to achieve this in a research project is a major problem. But the presence of such information can make a great difference in the validity of cross-cultural research findings.

Does the standard approach to cross-cultural research in psychiatry commit a ‘category fallacy’?

A category fallacy (Kleinman, 1977) is the reification of a nosological category developed for a particular cultural group that is then applied to members of another culture for whom it lacks coherence and its validity has not been established. Shweder (1985), for example, suggests that among traditionally oriented rural populations, in many non-Western societies, the phenomenology of depressive disorder is better captured by local syndromes of ‘soul loss’ than by Western existential categories. A psychiatrist from such a society could operationalise the concept and symptoms of soul loss in his society, then organise them into a questionnaire, establish its reliability for use in his society, then translate its items into English, have them back-translated into the original language by another team of bilingual mental health workers, adjust the questionnaire for semantic equivalence, measure its reliability in the hands of native English-speaking clinicians, and apply the questionnaire to a stratified sample of an

urban, middle class North American population in an epidemiological survey. He would come up with prevalence data. But would such data be valid, inasmuch as the disorder soul loss has no coherence for middle class North Americans? Obeyesekere (1985) has illustrated the same problem by conjuring up a South Asian psychiatrist studying semen loss syndrome among North Americans. This is a category fallacy, and it occurs routinely in cross-cultural psychiatric research, only the other way around, by the imposition of Western categories in societies for which they lack coherence and validity.

Dysthymic Disorder in DSM-III (or neurotic depression in ICD-9) is a possible example. It may hold coherence in the more affluent West, but it represents the medicalisation of social problems in much of the rest of the world (and perhaps the West as well), where severe economic, political and health constraints create endemic feelings of hopelessness and helplessness, where demoralisation and despair are responses to real conditions of chronic deprivation and persistent loss, where powerlessness is not a cognitive distortion but an accurate mapping of one’s place in an oppressive social system, and where moral, religious and political configurations of such problems have coherence for the local population, but psychiatric categories do not. This state of chronic demoralisation, furthermore, is not infrequently associated with anaemia and other physiological effects of malnutrition and chronic tropical disorders that mirror the DSM-III symptoms of Dysthymic Disorder. In such a setting, is the psychiatrist who is armed with a local translation of DIS or other clinical instruments devised for Western populations in order to study the prevalence of Dysthymic Disorders any different from the Nuer psychiatrist studying soul loss in midtown Manhattan? Great care must be exercised in the application of this diagnostic category to assure that its use is valid.

For the psychiatric epidemiologist, determining a ‘case’ of a disorder from a person with distress but no disorder is the crucial dilemma. In making that distinction, taxonomy becomes entangled in its own decision rules. For patients with loss of energy due to malaria, appetite disturbance owing to the anaemia of hookworm, sleeplessness associated with chronic diarrhoeal disease, and dysphoria owing to poverty and powerlessness, the difference between 3 and 4 ‘vegetative’ complaints is the difference between becoming a case of depression as a disease or being an instance where depression is a socially caused type of human misery. DSM-III was not created with such problems in mind, but it is applied in such settings. The result is as distorted a view

of pathology as it is an inappropriate use of diagnostic categories.

Conclusion

Again, we confront the problem of validity. In a recent major statement on psychiatry, McHugh & Slavney (1986) define reliability as the "verification of observations" and validity as the "verification of presumptions". These influential American psychiatrists further define reliability as "the consistency with which one can make an observation". They claim that

the reliability of some psychiatric observations is high. This indicates that psychiatrists can determine whether a patient does or does not have abnormal mental states such as delusions and hallucinations.

Reliability, following their own useful definition, indicates no such thing. It indicates only that the measurement of the observations is consistent. It does not tell us if the observations are valid: that is, whether a patient does or does not have an abnormal mental state. Here is an example. Ten psychiatrists trained in the same assessment technique and diagnostic criteria who are asked to examine 100 American Indians shortly after the latter have experienced the death of a spouse, a parent or a child may determine with close to 100% consistency that those individuals report hearing, in the first month of grieving, the voice of the dead person calling to them as the spirit ascends to the afterworld. That is reliability of observation. But the determination of whether such reports are a sign of an abnormal mental state is an interpretation based on knowledge of this group's behavioural norms and range of normal experiences of bereavement. Hearing the voice of the dead is an expected experience in bereavement among a number of American Indian tribal groups; this experience does not portend psychosis or other abnormal complications of bereavement. Thus, to interpret these normal experiences as 'hallucinations' with all the significance of pathology that term connotes is reliable but *not valid*.

For McHugh & Slavney, and many psychiatrists who are less theoretically sophisticated, observations are direct representations of reality. A word, hallucination, points to an empirical reality - i.e., abnormal mental state - in the real world. For the anthropologist, the word is a sign that signifies a meaningful phenomenon in a practical, day-to-day world mediated by a cultural apparatus of language, categories, taxonomies, and different hierarchies of

relevance. In such a local world of culture, interpretations are always judgements whose reliability may be determined by consistency of measurements but whose validity needs to be established through *understanding* that particular cultural context. Validation is not simply verification of concepts used to explain observations. Rather it is as well verification of the meaning of the observations in a particular social system (a particular village, town, research laboratory, or clinic). Perception is a process of observing *and* interpreting in a practical situation guided by concerns of relevance influenced by human interests. Anthropologists would not say that clinical assessment cannot be validated, but that such validation is at heart an ethnographic enterprise. Thus, to avoid a category fallacy, cross-cultural research must be grounded in the local ethnographic context. This simply does not take place in most psychiatric studies.

I have tried to illustrate through these four questions some of the practical contributions of an anthropologically informed viewpoint to cross-cultural psychiatric research. Perhaps the most availing role for anthropology in relation to psychiatry is to continually remind us of these dilemmas, to challenge the hubris in our attempts to medicalise the human condition, to encourage humility in the face of alternative cultural formulations of human problems, which for anthropologists are different visions of the world rather than reflections of 'ignorance', and to make us uncomfortable with our taken-for-granted professional categories and the tacit 'interests' they represent.

References

- CANINO, G. J., BIRD, H. R., SHROUT, P. E., RUBIO, M., BRAVO, M., MARTINEZ, R., SESMAN, M. & GUEVARA, L. M. (1987) The Spanish DIS: reliability and concordance with clinical diagnoses in Puerto Rico. *Archives of General Psychiatry* (in press).
- CARR, J. (1978) Ethno-behaviourism and the culture-bound syndromes. The case of amok. *Culture, Medicine and Society*, 2, 269-293.
- (1985) Depression and the culture bound syndromes. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- EBIGBO, P. (1982) Development of a culture specific (Nigeria) screening scale of somatic complaints. *Culture, Medicine and Psychiatry*, 6, 29-44.
- GAVIRIA, M., PATHAK, D., FLAHERTY, J., GARCIA-PACHECO, C., MARTINEZ, H., WINTROB, R. & MITCHELL, T. (1984) Designing and adapting instruments for a cross-cultural study on immigration and mental health in Peru. Paper presented at the American Psychiatric Association Meeting.
- GOOD, B. (1977) The heart of what's the matter. *Culture, Medicine and Psychiatry*, 1, 25-38.
- & DEL VECCHIO GOOD, M. J. (1986) The cultural context of diagnosis and therapy: a view from medical anthropology. In *Medical Health Research and Practice in Minority*

- Communities: Development of Culturally Sensitive Training Programs* (eds M. Miranda & H. Kitano). Washington, D.C.: United States Government Printing Office for NIMH, 1-27.
- GUARNACCIA, P. J., DELA CANCELA, V. & CARRILLO, E. (1987) *Nervios* in Puerto Ricans. *Culture, Medicine and Psychiatry* (in press).
- KINZIE, J. D., MANSON, S. M., VINH, D. T., TOLAN, N. T., ANH, B. & PHO, T. N. (1982) Development and validation of a Vietnamese language depression rating scale. *American Journal of Psychiatry*, **139**, 1276-1281.
- KLEINMAN, A. (1977) Culture, depression and the 'new' cross-cultural psychiatry. *Social Science and Medicine*, **11**, 3-11.
- (1985) Some uses and abuses of social science in medicine. In *Metatheory in Social Science* (eds D. Fiske & R. Shweder). Chicago: University of Chicago Press.
- (1986) *Social Origins of Distress and Disease: Depression, Neurasthenia and Pain in Modern China*. New Haven: Yale University Press.
- & GOOD, B. (1985) Culture and depression. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- LEWONTIN, R. C. *et al* (1984) *Not in Our Genes: Biology, Ideology and Human Nature*. New York: Pantheon.
- LOW, S. (1985) *Nervios in Costa Rica*. Philadelphia: University of Pennsylvania Press.
- McHUGH, P. & Slavney, A. (1986) *The Perspectives of Psychiatry*. Baltimore: John Hopkins University Press.
- MANSON, S. M., SHORE, J. H. & BLOOM, J. D. (1985) The depressive experience in American Indian communities: a challenge for psychiatric theory and diagnosis. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- MARSELLA, A. J., SARTORIUS, N., JABLENSKY, A. & FENTON, F. R. (1985) Cross-cultural studies of depression. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- MAYR, E. (1982) *The Growth of Biological Thought*. Cambridge: Harvard University Press.
- OBEYESEKERE, G. (1985) Depression, Buddhism and the work of culture. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- RUBEL, A., O'NEILL, C. W. & COLLADO-ARDON, R. (1984) *Susto*. Berkeley: University of California Press.
- SARTORIUS, N., JABLENSKY, A., KORTEN, A., ERNBERG, G., ANKER, M., COOPER, J. E. & DAY, R. (1986) Early manifestations and first contact incidence of schizophrenia in different cultures. *Psychological Medicine*, **16**, 909-928.
- SHWEDER, R. (1985) Cross-cultural study of emotions. In *Culture and Depression* (eds A. Kleinman & B. Good). Berkeley: University of California Press.
- STONE, D. (1984) *The Disabled State*. Philadelphia: Temple University Press.
- WORLD HEALTH ORGANIZATION (1973) *International Pilot Study of Schizophrenia*. Geneva: WHO.
- (1979) *Schizophrenia: An International Follow-up Study*. New York: John Wiley.
- YELIN, E., NEVITT, M. & EPSTEIN, W. (1980) Toward an epidemiology of work disability. *Milbank Memorial Foundation Quarterly*, **58**, 386-414.

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