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Editorial

Culture and Schizophrenia

Criticisms of WHO studies are answered

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We welcome Edgerton & Cohen's (1994) highlight of the cross-cultural studies on schizophrenia by the World Health Organization (WHO). However, we are concerned that what purports to be a methodological critique of the reports of the two WHO studies on schizophrenia (the International Pilot Study on Schizophrenia – IPSS – and the Determinants of Outcome of Severe Mental Disorders – DOSMD) is replete with misinterpretations, misquotes, and elementary statistical errors. Edgerton & Cohen question the validity of the finding of a more favourable course of schizophrenic illnesses in the developing as opposed to the developed countries. Having attributed to the WHO monograph (Jablensky *et al*, 1992) "the conclusion that the putatively more favourable course is a product of culture", they go on to "conclude that the more favourable course in developing centres may be a product of artefacts unrelated to culture as such". We must state that a simplistic explanation of the differences in course and outcome as a "product" or "result" of culture is nowhere to be found in the WHO monograph. Its concluding chapter contains a single reference to 'culture' (in inverted commas) which Edgerton & Cohen quote out of context and only in part. Since the relevance of the methodological comments which Edgerton & Cohen advance (pp. 228–229) as a counterargument to our position hinges on this attribution, we reproduce the paragraph in question in full:

> "Having excluded, for lack of support by the data described in this report, the explanation of the observed difference between the prognosis of schizophrenia in developing and in developed countries as an artefact, a strong case can be made for a real pervasive influence of a powerful factor which can be referred to as 'culture'. Unfortunately, neither the IPSS nor the Outcome study could penetrate in sufficient depth below the surface on which the impact of this unknown factor was established – tentatively in the IPSS and definitively in the present study. . . . The contribution of the present study, therefore, is not in providing the answer but in clearly demonstrating the existence of the question" (Jablensky *et al*, 1992, pp. 88–89).

Since Edgerton & Cohen's paper could be seriously misleading to readers who are not familiar with the WHO report, we feel obliged to comment on each of their arguments in turn. Their claim that the DOSMD finding of a more favourable course of schizophrenia in the developing countries may be a product of artefacts is based on seven conjectures.

(1) *The more favourable course in the developing countries could have resulted in part from prior treatment or from continued but unreported treatment during the follow-up.*

In seeking support for this hypothesis, Edgerton & Cohen refer to the traditional remedies or unspecified 'drugs' received by 22–46% of the patients in Agra, Chandigarh and Ibadan, and to the one or more applications of electroconvulsive therapy received by 17% of the Agra patients before the first interview, as "pre-inclusion therapy". What they omit to mention is that the events in question took place within the three months preceding the inclusion of these patients into the study, and that by definition no subject who had had treatment contacts before that three-month period was eligible for inclusion. In fact, 66% of the patients in the developing countries and 70% of the patients in the developed countries were included in the study within one month of their first contact with any helping agency. The possibility that the outcome of schizophrenia at two-year follow-up could be biased by a brief pre-inclusion exposure to herbal medicines or other unspecified traditional treatments is too remote to be considered seriously.

(2) *The combined cohort attrition rate over the two-year follow-up was higher in the developed countries than in the developing countries. Since patients who drop out tend to be healthier than those who remain in contact, the reason for a more favourable course in the developing countries could have been the different attrition rates.*

While it is true that the two US centres had the highest attrition rates, while Chandigarh (rural), Agra and Cali had the lowest attrition rates, it is also true that Chandigarh (urban) and Ibadan had higher attrition rates than most of the non-US centres, while Dublin had a lower rate. Generally, the difference in the attrition rates between the two types of centre failed to reach the 0.05 level of statistical significance. Moreover, the comparison on eight variables between the patients lost to the follow-up and the patients who remained in the study (Table 4.1 of the WHO

report) does not support the notion that those who drop out tend to have a better outcome. There were only two significant differences between the patients lost and patients remaining: the former included a greater proportion using street drugs (a predictor of poor outcome) and the latter included a greater percentage of 'CATEGO S+ cases' (no prognostic significance).

(3) *Eighty included patients did not undergo an initial Present State Examination (PSE); 33 of them were in Chandigarh. If the majority of the rest of the non-PSE cases were in the developing countries it would mean that about 10% of all the cases in the developing countries had passed different inclusion criteria from the rest of the sample.*

Edgerton & Cohen claim no knowledge of the distribution of the patients who had no initial PSE. But they could easily have found this information in Table 2.9 of our report, which indicates that 35 patients (6.0%) in the developing countries and 56 patients (7.1%) in the developed countries (i.e. a total of 91) had for various reasons no PSE on the first assessment. Therefore, the imputation of different inclusion criteria to patients in the two types of setting is unfounded.

(4) *A higher percentage of patients in the developing countries reported having had medical problems in the year preceding inclusion. Schizophrenia-like symptoms may be the result of diseases or toxins common to developing countries.*

We presume the implication is that some of the cases in developing countries might be schizophreniform illnesses caused by a physical disease. Organic cerebral involvement (including alcohol- or drug-related brain damage) was an exclusion criterion. Nevertheless, the possibility of significant physical morbidity underlying the acute schizophrenic illnesses was examined in the WHO study and rejected. The detailed distribution is not reported in the WHO monograph (Edgerton & Cohen are right in pointing out this), but is available on request. With the exception of malaria, which is endemic in the catchment areas of the developing countries (13% of the patients in these areas reported malaria in the last six months), the data are unremarkable and do not suggest an 'organic' explanation of the findings. Malaria, in its cerebral form, is known to induce acute confusional states and, rarely, a chronic encephalopathy. Neither of these conditions would have passed the inclusion criteria.

(5) *The finding that the more favourable course in the developing countries is not restricted to the cases of acute onset but is also found in cases of gradual onset may be artefactual because of the frequent unreliability of self-report data on type of onset, especially when a patient was included after a lengthy period of illness.*

Type of onset was a strong predictor of the course, but did not explain fully the differences between the two types of centre. One of the key findings was that within the subset of cases with insidious onset of schizophrenic symptoms, patients in the developing countries still had better course and outcome than patients in developed countries. Conceding that the assessment of mode of onset presents difficulties, the research protocol was designed with special attention to this variable. Notably, type of onset was *not* rated from patients' self-reports only, as implied, but was ascertained after structured questioning and cross-examination of a key informant. For most patients (61%), psychotic symptoms appeared within three months of the date of inclusion, and there was no difference between the patients in developing and developed countries as regards the length of previous illness.

(6) *Data from Nigeria are omitted from the most crucial analyses of course and outcome in the DOSMD study.*

This is an incorrect statement. Chapter 4 of the WHO report contains all the data on Ibadan patients' course and outcome: pattern of course (Table 4.2); percentage of follow-up time with psychotic symptoms (Table 4.3); percentage of follow-up time in remission (Table 4.4); percentage of follow-up time on antipsychotic medication (Table 4.5); percentage of follow-up time in hospital (Table 4.6); and percentage of follow-up time with unimpaired social functioning (Table 4.7). The only table excluding Ibadan (because of differences in the rating scale for mode of onset) is 5.7 in Chapter 5.

(7) *Female schizophrenic patients in the developing country centres (Agra excepted) had a dramatically better course than male patients. This gender difference may account fully for the 10% differential in better outcomes among those with schizophrenia in the developing countries.*

Edgerton & Cohen confuse two issues. First, the "10% differential" (also referred to as the "approximately 10% greater likelihood") is *their* arbitrary inference from the reported data, and it does not make statistical sense. Apparently, this inference derives from an out-of-context reading of two lines in Table 5.7, which compares the distributions of the variable 'pattern of course'. However, our conclusion about better outcomes in the developing countries is based on six different measures of course and outcome (Table 4.10 of the report), on five of which the percentage differences between the developing and developed countries are far in excess of 10%. Secondly, female patients do tend to have a better outcome than males in both developing and developed countries, and gender is a

significant predictor of five out of the six measures of course and outcome. However, we have shown that this effect is independent of centre, since the log-linear model used in this analysis removes the interaction effect between centre and gender. We are puzzled by Edgerton & Cohen's inference (p. 227), apparently based on their own interpretation of Table 4.8 of our report, that "while a higher percentage of female patients seem to have a mild course in all of the centres, in Cali, Ibadan, and North India the average difference between men and women comes to +16.2%", and that "among the developed countries it is only +5.1%". It seems to us that they have simply averaged horizontally the percentages in the top rows of Table 4.8 A and B ignoring the fact that these percentages are based on different numbers of subjects. Had they taken the care to calculate from the data in the table the mean female/male percentage difference for pattern of course 1 (single psychotic episode, complete remission), they would have come up with +2.9% for Cali, Ibadan, and Chandigarh, and with +17.8% for the developed countries, exactly the opposite of their claim.

Finally, we wish to correct Edgerton & Cohen's misinterpretation of the IPSS results. On page 223 of their article we read:

> "Reliability of diagnosis, too, was problematic. Re-diagnosis at follow-up found that substantial numbers of patients originally diagnosed as schizophrenic in developing centres had been misdiagnosed (28% in Agra, 18% in Ibadan and 28% in Cali), a phenomenon that occurred with similar frequency in only one of the 'developed' centres (Moscow)."

Although this statement is not referenced by the authors, we identified the source for the above percentages in Table 6.17 of the IPSS follow-up report (World Health Organization, 1979). In fact, Edgerton & Cohen quote out of context the percentages of IPSS patients with an initial diagnosis of schizophrenia who in the course of the two-year follow-up developed affective or other psychotic episodes. For completeness, they should have added the percentage for Washington (20%), which is higher than that for Ibadan. However, the point is that none of these percentages actually refers to either re-diagnosis or misdiagnosis but to an observed change in symptoms with which the patients presented on follow-up. It is a commonplace clinical observation that, over time, the symptoms of a proportion of patients can be expected to change. The authors seem to confuse here the reliability of psychiatric diagnosis with its capacity to predict symptoms.

Conclusion

We believe that "the DOSMD challenge" is real and cannot be explained away as an artefact of data collection and analysis. Without claiming that our analysis and conclusions are either final or unassailable, we think that the weight of the evidence points to consistent differences in the course and outcome of schizophrenia between developing and developed countries. While no single variable has so far been identified that would explain the course and outcome variance across the two types of setting, further analyses of the data, as well as the long-term follow-up of the DOMSD patients currently in progress, may reveal effects of multiple factors on the observed differences in prognosis.

We agree that culture should not be used as a synonym for unexplained variance and that "culturally sensitive" research designs are needed. It is doubtful, however, that either intracultural or new cross-cultural studies will advance our understanding of the effects of the environment on schizophrenia unless we have testable hypotheses about specific pathogenetic mechanisms that lead from 'culture' to symptoms of schizophrenia.

Discussion of these issues is much needed, especially as the enthusiastic embrace of new biological techniques in psychiatry tends to obscure the importance of the cultural context of psychiatric illness. However, the debate about a complex interdisciplinary issue such as culture and schizophrenia can be discredited in the eyes of readers unless we apply to it standards of precision and rigour that should be custom and practice in every scientific discourse.

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