PATTIE, A. H. & GILLEARD, C. S. (1979) *Manual of the Clifton Assessment Procedures for the Elderly* (CAPEBRS). Sevenoaks: Hodder & Stoughton.

Asian patients and the HAD scale

SIR: Surely Chaturvedi is rather too severe in his criticism (*Journal*, January 1990, **156**, 133) of Nayani's translation of the HAD scale into Urdu (*Journal*, October 1989, **155**, 545–547)? He may be right that some researchers: "... have the impression that mere translation of an instrument is sufficient to make it applicable for use in populations of different ethnic or linguistic backgrounds", but a careful reading of Dr Nayani's report shows that he is not one of them. His research intention was not to *use* the scale to measure an Asian population but to *determine its usefulness* (i.e. to find out to what extent it could be applicable and what changes it might need), addressing, in fact, those very issues which Dr Chaturvedi thought were being ignored. Moreover, it is clear that Dr Nayani understands the limitations of word-for-word translation. Various authorities, he says, "... have emphasised the importance of translation of the concept rather than the literal translation of sentences. The HAD was translated on this principle...".

Dr Chaturvedi has opened a can of worms. Probably everyone would agree that if we take rating scales that are validated in one culture only, and use them in other cultures without modification, we can obtain nice neat columns of figures which don't mean anything. On the other hand, if we use different measuring scales, each one culturally appropriate and valid in its place of origin, the results will be more ethnographically satisfying and probably more clinically useful. The snag is that we won't be able to use those results for inter-group or international comparisons; and epidemiology is important.

How can we escape from this dilemma? The usual compromise seems to be to start with a well-known rating scale and translate it, then twist and bend it a bit, knocking off a few apparent irrelevances and substituting one or two 'cultural' features, and hope for the best. Is this right? Is there a better way? If compromises are in order, are there some general rules or principles? How many changes can be made to a rating scale before it becomes a different scale? Any? Of course, a scale taken out of its context should be revalidated; but what does that mean – recalibration against a local clinically-selected reference sample, or something more than that? Are there differences (in this respect) between instruments which identify diagnostic categories, and instruments used only within an agreed category to quantify severity or measure change over time?

If those who are wise in such matters could offer some guidance, I am sure the rest of us would be grateful.

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HAD and ROC

SIR: Razavi *et al* (*Journal*, January 1990, **156**, 78–93) investigate the characteristics of HAD scale in cancer patients. We have some observations concerning the reporting of such research findings.

Firstly, the HAD scale was devised in order to provide clinicians and researchers with estimates of the presence and severity of two separate emotional disorders: anxiety and depression. It was not devised in order to provide a 'global' concept of the presence of psychiatric disorder as does the General Health Questionnaire. There have been several instances of research reports based upon summation of the two subscale scores of the HAD, but this should not be done. Dr Razavi *et al* later present validation for the two subscales separately, and find the performance of the anxiety scale to be relatively poor; this is to be expected when the gold-standard for HAD *anxiety* is the presence or absence of *depression* (with or without adjustment disorder).

Secondly, the purpose of a receiver operating characteristic (ROC) analysis is to illustrate the relationship between false positives and false negatives at different cut-off points on the scale. ROC analyses are analagous to bar-charts – they should convey information more succinctly than the equivalent table. It is the scale points themselves, not the smoothed-out curve, which the reader wishes to examine, in order to judge relative merits of different cut-offs.

The authors state that "the optimal cut-off for the screening of major depressive disorders seems to be 19". This is incorrect. The purpose of displaying the relationship between true positives and false positives is to allow a *choice* of cut-off. The decision will depend on: (a) the prevalence of the target disorder in the study population; (b) the value and feasibility of intervention with cases identified; and (c) the fate which befalls those patients assigned to the wrong category.

An increasing number of reports of psychometric test data are being presented in terms of ROC analysis. As noted above, one purpose of the ROC chart is

to enable a suitable cut-off point to be discerned; presenting a series of straight lines joining the points rather than an artificially produced 'curve' is more helpful in this case. A second purpose is to judge the relative merits of a series of tests and, in this case, the judgment rests upon which of the series of 'curves' lies nearest to the top left hand corner or alternatively, has the largest area under the curve. In this case, therefore, smooth curves may suitably illustrate the findings.

Readers requiring further information concerning ROC analysis will find the study by Murphy *et al* (1987) to be useful.

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Reference

MURPHY, J., BERWICK, D. M., WEINSTEIN, M. C. *et al* (1987) Performance of screening and diagnostic tests: application of receiver operating characteristic analysis. *Archives of General Psychiatry*, **44**, 550–555.

CT findings in schizophrenia

SIR: I would like to reply to comments by Ingraham and Crichton & Hughes (*Journal*, March 1990, **156**, 444–453) on our recent paper (*Journal*, October 1989, **155**, 444–450).

In our study, multivariate analyses were used in order to correct for age and gender because enough matched controls were not available. In these multiple discriminant analyses, the enlargement of lateral ventricle: brain ratio (VBR_1), although present, was not significant to distinguish schizophrenic patients from normal controls, nor familial patients from non-familial ones. We recently reproduced these computerised tomography (CT) findings in a magnetic resonance imaging (MRI) study (Uematsu & Kaiya, 1988, 1989) of 40 schizophrenic patients and 17 normal controls. The subjects were all males, aged under 50, and there was little overlap with those in the CT study. Student *t*-tests showed significantly higher VBR_1 in schizophrenic patients than normal controls, and in schizophrenics with horizontal transmission than non-familial patients (unpublished data). However, multiple discriminant analyses again showed that, here again, VBR_1 was not a central finding for diagnosis and heredity although VBR_3 was significant both for the diagnosis of schizophrenia and for the differentiation of familial and non-familial patients.

The idea to divide schizophrenics into three subgroups stemmed from the contagion hypothesis (Crow & Done, 1986) related to the retrovirus/transposon hypothesis (Crow, 1984). Crow & Done (1986) found a correlation of age of onset between siblings and a tendency for the disease to occur at an earlier age in the younger sibling. One of their explanations for this finding was that the disease is transmitted from those who already have it to relatives who possess the genetic predisposition. We performed our study based on this hypothesis and showed a possibility that schizophrenia with horizontal transmission is a distinctive disease.

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References

CROW, T. J. (1984) A re-evaluation of the viral hypothesis: is psychosis the result of retroviral integration at a site close to the cerebral dominance gene? *British Journal of Psychiatry*, **145**, 243–253.

—— & DONE, D. J. (1986) Age of onset of schizophrenia in siblings: a test of the contagion hypothesis. *Psychiatry Research*, **18**, 107–117.

UEMATSU, M. & KAIYA, H. (1988) The morphology of the corpus callosum in schizophrenia. An MRI study. *Schizophrenia Research*, 391–398.

— & — (1989) Midsagittal cortical pathomorphology of schizophrenia: a magnetic resonance imaging study. *Psychiatry Research*, **30**, 11–20.

Chronic psychoses in Turner's syndrome

SIR: Bamrah & MacKay (*Journal*, December 1989, **155**, 857–859) presented a case of chronic psychosis in Turner's syndrome and reviewed the literature. I would like to add the following information to their report.

Firstly, the 45, XO sex chromosome karyotype makes up 51% of Turner's syndrome (TS) while mosaics of the 45 XO/46 XX sex chromosome karyotype make up 18% and 25% carry an X chromosome abnormal in structure (Fishbain & Vilasuso, 1981). Buccal smear analysis will not necessarily identify a mosaic for which karyotypic analysis in leucocyte culture is required (Akesson & Olanders, 1969). Drs Bamrah & MacKay did not specify how their patient's karyotypic pattern was determined. If only buccal smear analysis was utilised, it is possible that their patient was a TS mosaic.