time, eight patients scored the maximum of 11, only two had any conversant language, eight were chair-bound and 12 (67%) needed help with feeding. Although not documented, our nurses were convinced that disturbance as well as dependence had increased since 1985.

These changes were not merely the effects of a new unit opening with 'easy' patients who subsequently all grew frail together, since only two patients survived from the 1985 cohort. Like Dr Hilton *et al*, our colleagues also believe that their local authority homes have been taking ever more dependent clients. It may be then that this trend, if it is indeed a widespread phenomenon, has led to all but the most behaviourally difficult demented individuals being placed in residential homes. The hospital services then have to cope with an ever more disturbed and disturbing group that cannot (and perhaps should not, given the skilled interventions required) be placed elsewhere.

Secular trends are clearly important for planning, and it would be interesting to know whether or not our experience is purely local. If not, then it must add to the concerns raised by Dr Hilton *et al* concerning the closure of hospital beds for demented patients. The White Paper *Caring for People* (HMSO, 1989) states that "there will be others, in particular elderly and seriously mentally-ill people ... whose combination of health and social care needs is best met by care in a hospital setting. There will be a continuing need for this form of care". Doctors with a proper training in old-age psychiatry develop skills in managing these patients and in supporting those who care for them, both nurses and involved relatives. Wholesale re-location into the 'community', away from specialist supervision, for reasons of financial expediency could lead to deteriorating standards of care for these vulnerable patients.

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HMSO (1989) *Caring for People: Community Care in the Next Decade and Beyond*. London: HMSO.

SIR: We were very interested to read the report by Hilton *et al* (*Journal*, December 1989, **155**, 782–786) since it is indeed the case, as the authors state, that there is a paucity of literature in this area.

We are able to confirm Dr Hilton *et al*'s results since we have recently completed a detailed observational study of two long-stay psychogeriatric wards which included the Clifton Assessment Procedures for the Elderly Behaviour Rating Scale (CAPEBRS) (Pattie & Gilleard, 1979). We also obtained scores typically higher than those reported by Pattie & Gilleard for a sample of long-stay psychogeriatric patients. Since the long-term aim of our research is facilitating collaborative ventures with neuroscientists, we also collected in-depth observational and reliability data. From this we make the following points.

We found the social disturbance scale of the CAPEBRS to be unreliable when rated by nursing staff, in contrast to Pattie & Gilleard. Dr Hilton *et al* report that the CAPEBRS was completed by the researcher, which may make this subscale more suspect. We feel this is an important point since it is this subscale which details objectionable behaviours so frequently reported in the literature. This unreliability may be due to the low frequency with which such behaviours occur in this setting, which is supported by Dr Hilton *et al*'s finding of an average score of 2.42 from a possible 10 on this subscale, and further supported by our own observational data.

We would suggest that the picture emerging of long-stay psychogeriatric patients is one of gross physical incapacity and dependency and agree with Dr Hilton *et al* on the need for appropriate staff support. However, the role of disturbed behaviour, typically wandering and aggression (e.g. Mann *et al*, 1984) is in our opinion less important in this setting than in earlier stages of the disease process.

Finally, we take issue with Dr Hilton *et al*'s last assertion, that the problems they have identified may describe any London borough. This may or may not be true, but is likely to depend on local availability of resources and, in the light of our comments above, we fail to see why this should make the authors wonder about the appropriateness of general plans for provision in the community.

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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