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Correspondence

Letters for publication in the Correspondence columns should be addressed to:
The Editor, British Journal of Psychiatry, 17 Belgrave Square, London SW1X 8PG

THE LEEDS SCALES

Dear Sir,

Snaith and his colleagues have published (*Journal*, February 1976, pp 156–65) an important study of the self-assessment of anxiety and depression. It may perhaps be worthwhile to correct the impression given by the statement on page 164: 'Zung (1967) found no evidence that the scores on his scale were affected significantly by age or sex.' Zung has since (1972) published an article showing a higher prevalence of elevated scores among two extreme age groups (19 and under, 65 and over). Using an SDS index of 50 as the 'morbidity cut-off score' he found that this misclassified only 12 per cent of normal subjects. Within the extreme age groups mentioned above, 48 per cent and 44 per cent respectively exceeded this cut-off point.

R. G. Priest

Department of Psychiatry,
St Mary's Hospital,
Harrow Road, London W9 3RL

Reference

Zung, W. W. K. (1972) How normal is depression? *Psychosomatics*, 13(3), 174–8.

THE CONCEPT OF DISEASE

Dear Sir,

In my essay on the concept of disease (*Journal*, October 1975, **127**, pp 305–15), which has provoked such a spate of correspondence in your columns, I had two main aims: to refute the 'there is no such thing as mental illness' argument, by showing that at least some of the conditions traditionally regarded as mental illnesses possessed as good a claim to be regarded as disease as tuberculosis or hypertension; and to stimulate people to think what they really meant by 'disease' and 'illness'. I was not trying to prove that any particular phenomena were or were not illness, though it is true that I do suspect we have been rather uncritical in accepting as 'illness' any problem we have been asked to deal with.

Several people have commented on the disparities between the set of conditions commonly regarded as illnesses and those embraced by Scadding's 'biological disadvantage' criterion, or rather my operational interpretation of it in terms of increased mortality or reduced fertility. As I said at the time, I realize that this definition is not ideal; we would all prefer our criterion of illness to include trigeminal neuralgia and psoriasis, but not rock climbers or Catholic priests (though I don't think the problems posed by essentially voluntary life styles such as these are insuperable). However, for the historical reasons I described, any definition will almost inevitably clash with contemporary usage in some respects, and it seems to me that Scadding's definition raises fewer serious problems that the traditional alternatives. If we reject it we must either find a more satisfactory alternative—and if any of your correspondents has one, he has not said so—or else accept that we can't define what we mean by disease and aren't going to.

This latter course has obvious attractions. It allows us, and society, to label as illness any phenomenon we regard as undesirable and which doctors seem better placed to deal with than other agencies. It also allows us to change our minds whenever we want to. But to do this is to accept Sedgwick's argument that the attribution of disease, mental or physical, is fundamentally a social value judgement, and that disease is really a socio-political concept rather than a biological or medical one. It may be that he and Jenner are right, but I think we should realize the full implications of this view before rushing to embrace it. It would mean that we could never maintain on medical grounds that x or y *were*, or were not, diseases. We could only argue on social grounds that they *ought*, or ought not, be *regarded* as diseases. And as the criteria would be social rather than medical such decisions would lie with society as a whole rather than with the medical profession, though doubtless they would be influenced by the effectiveness or otherwise of the treatments medicine had to offer. A further important implication is that we could not criticize Russian psychiatrists for incarcerating sane political dissidents in their beastly asylums: they would be perfectly entitled to regard political dissent as a mental illness if, as is probably the case, most of their fellow-citizens disapproved of political dissenters and it happened to be more convenient to deal with them as patients than as

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criminals. (We could still, as laymen, criticize them on humanitarian or political grounds, but not as doctors on medical grounds.) There would also be no answer to Szasz's thesis, other than the general social argument that madness is undesirable and that doctors are better equipped to deal with it than other people. Perhaps none of these things worry Professor Jenner; but they worry me.

R. E. Kendell

Edinburgh University Department of Psychiatry,
Morningside Park,
Edinburgh EH10 5HF

Reference

Sedgwick, P. (1973) Illness—mental and otherwise. *Hastings Center Studies*, **3**, 19–58.

PARENTS OF BATTERED CHILDREN

Dear Sir,

The well-disciplined study by Selwyn Smith and Ruth Hanson (*Journal*, December 1975, **127**, pp 513–25) shows some important statistical differences between the child-rearing behaviour of battering parents as compared with controls.

There are two unrecognized tendencies which both work towards submerging the observed differences between abusive parents and controls.

1. Battering parents *attenuate* accounts of accustomed rearing practices and battering incidents, whether or not they give direct admissions of guilt.

   Such parents have responded to subtle cues which betray the attitudes of others. Unlike the 'control' parents, they have had a lifelong experience of doing just this, having themselves usually been victims in childhood. Subsequent accounts either of the battering incidents or of rearing practices are modified accordingly. 'I couldn't stand his crying, and shook him until he went limp' may be the culmination of incompetent rearing, or using the baby as an emotional prop for an inadequate mother, rather than a single incident.

2. Battering parents have an inaccurate or no yardstick of normality. Thus, an item such as 'Severe in training methods', or 'obedience demanded', or 'allows to cry unless something obviously wrong', will mean something quite different to an abusive parent from what the same phrase would mean to a control parent. The same applies to the 'frequent use of smacking . . . withholds love . . . rarely deprives, rarely praises', etc.

Without these two tendencies Smith's and Hanson's findings would have been even more significant, and further items of marginal significance might have been shown to have been important.

No one will now be able to take refuge in anodyne beliefs such as, on the one hand, 'Any parent is a potential batterer', or on the other 'People who batter children must be mental'. The reality is more complex.

J. E. Oliver

Burderop Hospital,
Wroughton, Swindon SN4 0QA

DIURNAL VARIATION AND ENDOGENOUS COMPONENT OF DEPRESSION

Dear Sir,

We wish to report a research in which we examined the classical psychiatric opinion that endogenous depressives tend to improve towards evening. The limited research upon this concept has not established it as a fact (Kiloh and Garside, 1963; Rosenthal and Klerman, 1966; Stallone *et al*, 1973). In our research we employed well constructed scales for assessing both variables.

Subjects were 20 heterogenous depressives not suspected of being schizophrenic, mentally retarded, or organic. The Depression Category-Type Scale (DCTS) of Sandifer *et al* (1966) was used for determining the degree to which depression was endogenous. The Diurnal Variation Rating Scale (DVRS) was used for what its name implies.

The DCTS product-moment correlation for the 13 patients interviewed the day of admission by both H.K. and A.E. was ·87; that for the 17 interviewed by both H.K. and D.T. ·80; that for the 16 interviewed by both A.E. and D.T. ·87 (all $ps < \cdot 01$). The DCTS mean of the two or three interviewers was used for each of the 20 patients. The DVRS, for which clinical impression is praccally nil, was administered at 5 pm on the next three consecutive days. The correlation between first and second DVRS score is ·82; that between first and third ·72; that between second and third ·79 (all $ps < -\cdot 01$). Mean DVRS score for the three days was used.

The correlation between DCTS (upon which a higher score indicates a greater endogenous component) and DVRS (upon which a higher score indicates improvement towards evening) is −·01 (NS). However, this does not necessarily imply that a relationship between the two variables never exists. The period in the course of a depression could be relevant, as suggested by Waldman (1972), who maintained that diurnal variation ceases at the depth of endogenous depression and reappears as it improves. DVRS scores indicated improvement as the day progressed for 17 of our 20 patients. This is

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