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Correspondence

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HAMILTON RATING SCALE FOR DEPRESSION

DEAR SIR,

The paper by Knesevich *et al* (*Journal*, July 1977, pp 49–52) comments upon a paper by Bech and his co-workers in Denmark (1). The Danish paper sought to show that the Hamilton Rating Scale for Depression (HRS) is not a valid measuring instrument for depressive illness throughout the full range of severity, since scores failed to distinguish the more severe degree of illness. The paper by Knesevich *et al*, however, purports to refute the Danish findings.

Unfortunately, both groups of workers have fallen into certain methodological errors, and it is important that these should be recognized lest future workers in this field repeat them. Both groups have used repeated measures from the same patients, and this really should not be allowed, even though ratings were included at different stages of the illness. Both groups purport to have tested the HRS throughout the full range of severity of depressive illness, but both have failed to include any patients with a severity greater than the ninth grade on an eleven-point scale. It is perhaps understandable that they have failed to do so, for severe retardation or agitation causes difficulty in making ratings. In this connection it is important to note that inspection of the Figure in the paper by Knesevich *et al* shows that the mean HRS score at the eighth grade of severity is about 24·7 whereas the mean score at the ninth grade is about 21·2 i.e. lower scores at the more severe degree as judged on the global rating.

The final and most severe criticism applies to the American paper, while the Danish workers appear to have avoided the pitfall. It concerns the fact that the same workers made both the global ratings and the Hamilton ratings (if this is not so, it is not stated in the paper). HRS scores are subject to influence by the rater's general impression of how ill he considers the patient to be and therefore work based upon validating one measure against the other demands that the global and the Hamilton rating are made by different workers and that they do nothing to betray their rating to each other until the final analysis. This error may well account for the finding in the American paper that the total HRS and the two sub-scales ('Bech' and 'non-Bech') derived from it, all correlate to about the same degree with global ratings.

The HRS is now a well-tried instrument. Like a good medicine, its staying power probably attests to its effectiveness, but this should not imply that further work on improvement and modification should cease. Both groups of workers have recognized this. However, there is a major point which the Danish workers appear to have overlooked. At the time of its inception, Hamilton was concerned that his scale should reflect not only the severity of a depressive illness but the total symptom profile. By reducing the scale to six items as suggested by Bech and his co-workers the scale may well be easier to use and so commend itself to some researchers and clinicians but this does not indicate that the abbreviated scale is always the more appropriate instrument. In a depressive illness, 'pure' depressive symptomology is intermingled with anxiety symptoms and somatic symptoms and this fact is recognized in Hamilton's scale. If in future the severity of the illness is to be determined by a measure of just one group of symptoms it will be equivalent to a measure of the severity of rheumatoid arthritis based solely on pain whilst ignoring limitation of movement and joint swelling. There may well be occasions on which an abbreviated scale may be more appropriate but this may not always be the case. (An occasion for the use of partial Hamilton scores is stated in one of our own papers (2).)

There can be no doubt that the most accurate measures of the severity of depressive (and anxiety) states are based upon composite scores, in which observer ratings such as those of Hamilton achieve an equal weight with the patients own view of the severity of his state, the self-rating, and a third measure based upon an independent global assessment of severity. Any two or even all of these measures when combined, will give a better overall assessment of severity than any one alone.

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THE IMPACT OF AN ABNORMAL CHILD UPON THE PARENTS

Dear Sir,

In the April 1977 issue of this *Journal* (130, pp 405–10), Dr Gath comments upon the impaired 'quality of the marital relationship' of the parents of mongols, as evidence by their admissions of 'severe tension, high hostility or marked lack of warmth' at interview.

May I suggest that Dr Gath has fallen into the same trap as did Singer and Wynne (1963) in their study of parents of schizophrenics? It will be recalled that the latter were found to give more pathological descriptions of ink-blots, but careful studies by Hirsh and Leff (1975) later revealed that the difference resulted simply from the increased willingness to talk at interview on the part of parents with problem children.

In Dr Gath's study, parents of mongols confessed more frequently to *all* the measures assessed, whether constructive or destructive to the marriage, while control parents related more moderate feelings. This may reflect the anxiety of the former to enlist help, rather than supporting Dr Gath's hypothesis.

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THE HIERARCHICAL MODEL OF DEPRESSION

Dear Sir,

It is comforting to have one's work replicated (Bagshaw, 1977) and to see this twice in one Journal issue suggests something more than 'sheer carelessness'. I wish to point out, however, that the author also comes close inadvertantly to replicating the error of Kendell (1976). Within the hierarchical model of personal illness the 'two types of depression' are not differentiated simply by the presence of delusions of contrition in one group. We most recently concluded (Foulds and Bedford, 1976a) that '(1) those with a preponderance of delusions of contrition over delusions of grandeur or of persecution may, in the absence of delusions of disintegration be regarded as virtually synonymous with Psychotic Depressives; (2) dCs (or Psychotic Depressives) almost invariably suffer from neurotic symptoms (with ruminations the commonest); (3) all dCs (or Psychotic Depressives) suffer from either a state of anxiety or of depression and usually both; (4) a state of depression, *as a diagnosis*, can only be identified in the absence of neurotic symptoms (phobic, hysterical, or obsessional) and of delusions.' In other words, dCs are members of Classes 3, 2, and 1, sDs are only members of Class 1. There are, therefore, differentiae at two class levels.

Bagshaw found that 42% of her 'psychiatrically diagnosed depressed sample obtained a DSSI diagnosis of psychotic (dC) or neurotic (sD) depression'. A further twenty-two cases (29%) fell into one of the neurotic symptoms groups. This is precisely the area of confusion we commented upon when we made a case for separating out the dysthymic states from the neurotic symptoms (Foulds and Bedford, 1976b). As 92% of her patients fitted the hierarchy model it would seem plausible that the agreement in her study between the clinical and DSSI 'diagnoses' could be as high as 71%.

Finally, unpublished data from a pilot study of self-reinforcement in depression has been made available to me by Mr R J Wycherley. 93% of his 40 in-patients have DSSI patterns which fit the hierarchy model. Of the 28 scoring sD 16 are also dC (57%); of the 19 scoring dC 16 are also sD (84%). Again the relationship between these two sets might reasonably be described as inclusive and non-reflexive.

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