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Discussion

Depressive Illness: Clinically Diverse?

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The letter of Professor Roth and his colleagues (Correspondence, *Journal*, July 1979, **135**, 94–96) is extensively inaccurate and fails to address the true points at issue. Space restrictions preclude a full reply. Among other misattributions are the following:

(a) I did not suggest that psychiatric classification should be confined entirely to clinical items, only that this restriction is a *logically necessary first step* in validating any given classification (see Ní Bhrolcháin, *Journal*, January 1979, **134**, passim but especially pp 89–90).

(b) In the second paper (Ní Bhrolcháin *et al*, *Journal*, **134**, 94–107) we neither referred to nor commented on a 'bimodal anxiety-depression dimension' and certainly did not interpret any such 'dimension' as a severity scale. We did, however, conclude partly from the score distributions of our own discriminant analyses that, *clinically*, 'psychotic' and 'neurotic' depressives differ primarily in severity.

I agree that it is through the 'definition of clinically uniform groups of disorders that advances in knowledge of aetiology' etc. have been made in the past: what I have suggested is that contemporary attempts to classify should proceed in exactly this way. No such clinically uniform groups will emerge from the type of analysis advocated by Roth *et al* in which "all features that sharpen description and discrimination must be allowed to qualify for inclusion" *unless* models of type 1 or 4 of Fig 1 of my paper are actually true. To assume, as the Newcastle type of study implicitly does, that the choice is only between these two types of model is to engage in unscientific apriorism.

Roth *et al* fail to see that the separation claimed between depressive groups in Carney *et al* (*Journal*, 1965, **111**, 659–74) and between depressive and anxiety states in Gurney *et al* (*Journal*, 1972, **121**, 162–66) is quite inconclusive. The question is: can the groups in each case be separated using (a) symptoms alone? (b) aetiological variables alone? and (c) other background factors alone? A successful separation using symptoms alone implies a clinical difference: is this then (i) due only to severity? or (ii) because *each* group has a greater frequency of *some* symptoms than the other? The outcome of these distinct tests can be represented by simple models of the type given in Fig 1 of my paper or by more complex models. A coherent classification system may be built in this way but not from the procedures advocated by Roth *et al* which allow of no such dissection. Besides this failing, neither the Carney *et al* nor the Gurney *et al* study has ever been corroborated by further samples. The forthcoming Roth and Mountjoy paper does not, on the account given by Roth *et al*, replicate anything published to date by the Newcastle group, to the best of my knowledge, and certainly not the Carney *et al* or Gurney *et al* findings.

When factor analysis and related techniques are applied to a homogeneous group the factors may be interpreted through their loadings to represent the structure of individual differences within the group but, regardless of the number of factors emerging, the plotted factor *scores* cannot produce bunching of patients and hence neither the 'factors' nor their scores can separate patients into distinct groups. Used with a heterogeneous group, the 'factors' in such analyses cannot be taken to represent clinical structure within any of the groups included but a plot of factor scores *may* reveal clustering of individuals. A less selective reading of Roth *et al's* cited authorities discloses that e.g. Cattell (1965, p 421) explicitly discusses the *in*appropriateness of applying ordinary factorial methods to data from diverse groups and recommends that '. . . factor analysis is best preceded . . . where there is any doubt about homogeneity of population, by an examination for the existence and nature of possible type-structure in the form of subspecies'. There could hardly be less support than this for the sort of statistical procedure adopted by Professor Roth and his colleagues.

We did not use factor analysis because we were not

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concerned with the structure of depression but simply with whether there was any evidence for clinical differences between 'psychotic' and 'neurotic' depressives. Not only did we fail to find anything more than a severity difference between them but there is no reliable evidence in the literature that 'neurotic' depressives have *any* symptoms distinguishing them from 'psychotic' depressives: they appear simply to have fewer of the same set of symptoms. However, many studies do not provide the data that are basic for establishing group differences—viz. a tabulation of the frequencies of individual symptoms in each of the groups in question. Were the Newcastle group to publish this information there would be a firmer base for discussion and further inquiry.

Finally, the notion that a bimodal score distribution (and it seems that any scores will do) necessarily implies the existence of 'two (or more) conditions' at least one of which is 'categorical' is mistaken. We have shown that bimodal and unimodal distributions of discriminant scores can be obtained at will from the same groups of patients by choosing variables appropriately. As an absolute criterion bimodality is clearly useless. The many mechanisms which may generate spurious bimodal distributions are discussed and amply illustrated by Murphy (1964).

References

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Dr Ní Bhrolcháin appears to believe that useful data relating to the classification of affective disorders cannot be placed on record unless investigators allow themselves to be guided by the six models set out in her paper. She states that the Newcastle studies of depressive and anxiety states made the unvalidated assumption that either Models 1 or 4 are "actually true". The significance of these findings has nothing to do with the truth or otherwise of 1, 4 or any of the other models she lays down as a necessary starting point for all investigators. Their validity has been substantiated by the results of the independent follow-up studies to which we referred in our previous letter (July, 1979). It is in the light of such findings rather than the inferences Dr Ní Bhrolcháin deduces from her special models that the validity of the tentative classification which we proposed has to be assessed.

Neither in the studies of depressive illness (Carney *et al*, 1965) nor those relating to depressive and anxiety states was it concluded that the syndromes differentiated were homogeneous. Indeed, there is good evidence that there are two genetically different varieties of endogenous depression, unipolar and bipolar; whilst Kiloh *et al* (1972) have drawn attention to the heterogeneity of the group of patients diagnosed as suffering from 'neurotic depression'.

Dr Ní Bhrolcháin is in error in stating that the results of Carney *et al* (1965) have not been corroborated. Findings recorded by Sandifer *et al* (1966) Garside *et al* (1971) and by Kiloh and Garside (1977), derived from the original data of Lewis, all provided supporting evidence.

The conclusion reached by Ní Bhrolcháin *et al* (*Journal*, **134**, 94–107) that psychotic and neurotic depressives differ primarily in severity is refuted by three main lines of evidence. To regard an item such as 'retardation' as primarily a measure of severity is debatable. Secondly, 'psychotic-endogenous' disorders, ostensibly the more severely ill group, enjoy a more favourable course and outcome than neurotic cases. Thirdly, recent biochemical evidence strongly supports the qualitative nature of the difference between the two groups of disorders (Carroll *et al*, 1976 (a) and (b); Checkley, 1979). Finally, even if the bimodal distributions Ní Bhrolcháin *et al* derived from their maximizing analysis could be regarded as a 'severity' dimension (and this view is open to question), their conclusion that the result was compatible with the continuity of the psychotic and neurotic patient groups in their material would be erroneous.

Ní Bhrolcháin is incorrect in claiming that Cattell (1965) regards it as inappropriate to apply factor analysis to data from diverse groups. What he does say is that in applying factor analysis "one is forced to begin with an actual group of obscurely intermixed subspecies". Also (p 421) "factor analysis is best preceded, *or accompanied as suggested below*, where there is any doubt about homogeneity of population, by an examination for the existence and nature of

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