PSYCHOTIC AND NEUROTIC DEPRESSION: A REPLY ON METHOD

Dear Sir,

In her paper 'Psychotic and Neurotic Depression' (*Journal*, January 1979, **134,** 87–93) Ms Ní Bhrolcháin criticizes the methodology employed in a number of previous studies of the classification of affective disorders, including those published in recent years by the Newcastle group. We should like to comment on her criticisms.

Ms Ní Bhrolcháin holds that in studies of classification the analysis must be restricted to the clinical symptomatology alone. Any other approach, she maintains, fails to take account of the possible heterogeneity of depressive disorders in respect of aetiology and treatment response. The logic is difficult to follow. A valid and orderly classification is an essential precondition for the discovery of new treatments and the refinement of old ones. As far as aetiology of disorders of affect is concerned the causes are unknown. Restriction of the enquiries to the mental state cannot therefore dispose of this source of variation.

Ms Ní Bhrolcháin has put the cart before the horse. If the causes of depressive illnesses were already known, we would be in a position to investigate their relationship to variations in clinical profile. The true state of affairs is quite different. It is through the definition of clinically uniform groups of disorders, also differentiated from others by their course and outcome, that advances in knowledge of aetiology and the discovery of new treatments have been achieved in the past. So it is likely to prove in the future. And in the quest for the most clear, simple and valid calssification, all features that sharpen description and discrimination must be allowed to qualify for inclusion.

Ms Ní Bhrolcháin appears to equate non-clinical items relating to pre-morbid personality with aetiological factors. This equation is employed to buttress the view that taxonomic investigations confined to the mental state will make it possible to determine the causal origins of psychiatric syndromes. But, as the manner in which personality features relate to clinical syndromes is unknown, it is inadmissible to treat the former as if they were causal agents. The assumption that groups of patients in respect of 'clinical items' are bound to be more homogeneous in regard to aetiology than those that draw upon a wider range of variables is invalidated and arbitrary. The investigator is justified in drawing upon all the features that characterize individuals who suffer from different forms of psychiatric disorder if by this means more clear delineation of a discrimination between clinical groups can be achieved.

For 'mental state' items considered alone will often defy interpretation until they have been brought into relationship with information regarding personality and pre-morbid behaviour patterns. Whether the patient is 'deluded', 'retarded' and even 'depressed' and whether hypochondriasis, or for that matter 'self-pity' or 'hysterical features', are features of illness or merely a colouring added by personality traits can only be judged by a juxtaposition of presenting features with past behaviour. The diagnoses of 'psychotic' or 'neurotic' depression used by Ms Ní Bhrolcháin cannot be reliably arrived at unless the examiner looks in this manner before and after. By including personality features in our investigations we have merely expressed in explicit form assessments made in any thorough-going examination of the mental state.

The accolade she awards to numerical taxonomy and related techniques as providing more 'systematic and elegant' solutions to the problems surveyed has ignored the ability of these methods to identify 'non-existent clusters'. And the critical final look she proceeds to take at our 1972 studies might have benefited from Cormack's warning that numerical taxonomy tends to be chosen as 'a satisfactory alternative to clear thinking' (Cormack, 1971).

To take Ms Ní Bhrolcháin's criticisms in turn:

(1) She returns to her objection to the use of a mixture of features. As we have indicated, the range of features drawn upon is immaterial. The value of maximizing analyses lies in their power to demonstrate a line of cleavage that may exist between separate entities that happen, as is common with biological phenomena, to overlap to some extent with one another. The optimal separation of such groups of disorders is not undertaken for the sake of tidy pigeon-holing. The purpose is to make possible more precise predictions of course and outcome, and ultimately more effective and specific methods of treatment. Any items that assist in this discrimination are therefore grist to the mill. Ms Ní Bhrolcháin's statement that "one cannot say whether the separation achieved is due to symptoms, background factors or both" is incorrect. One can. In our studies (Carney *et al*, 1965; Gurney *et al*, 1972) both contributed to the discrimination achieved.

(2) We accept the point made that the differentiation of neurotic depressions from anxiety states is of particular importance. But the unitary view of Lewis and Mapother, which views all disturbances of affect as merging insensibly with one another, continues to be influential partly because there are real difficulties in differential diagnosis in this entire area. Nor is there general agreement that

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endogenous and neurotic depressions can be differentiated. We therefore chose as a first step to investigate a material that spanned the entire range of conditions comprised within the unitary hypothesis. Having achieved separation between the depressive states and the anxiety states as a whole, further analyses have been undertaken.

A study of depressive and anxiety neuroses in a further group of patients deals with Ms Ní Bhrolcháin's point that results derived from discriminant function analysis should not be accepted until they have been replicated in a second sample (Roth and Mountjoy, 1979). The group comprised patients with neurotic depressions and anxiety states alone; endogenous depressions were excluded. A principal components analysis of the scores derived from seven rating scales for anxiety and depression yielded both a general and an anxiety-depression factor. A discriminant function analysis undertaken on patients' scores on this factor revealed a non-unimodal distribution with clear separation of depressive and anxiety states. Analysis of the clinical items yielded a similar finding. The fact that in this instance the discrimination was achieved with the aid of the clinical items alone complements the information derived from the first study.

(3) Ms Ní Bhrolcháin is in error in stating that the weights attached to the thirteen items were derived from the 58-item analysis. In fact the 13 items were run as a separate sub-set (as summation of the percentages in column 3 of Table I of our paper will confirm). It was the weights from this analysis that were used for computing the patients' scores that proved to be bimodally distributed. It is difficult to detect logic or purpose behind the 'minimizing analysis' she suggests. What kind of hypothesis does the minimizing analysis test? The answer is that no hypothesis can be so tested, because none would stand refuted by an analysis into which the greatest possible amount of statistical noise has been introduced.

(4) The objection to the post-factum derivation of the 13 items with the largest coefficients of individual determination is unjustified. How else is the discriminating value of individual items to be determined other than by the results of an experiment that sets out to determine the independent contribution of each to the variance between the two groups?

In the comments Ní Bhrolcháin *et al* (1979) make on the interpretation of the bimodal anxiety-depression dimension, the arguments are on occasion contradictory and erroneous. For example, it is restated that the dimension is a severity scale. They may have been led to this view by their judgement that 'retardation' among other features was a measure of severity—an arbitrary and unacceptable interpretation. And, in Table I, they have an item labelled 'loss of weight' and another 'severity of appetite disturbance', defined as "dichotomized at marked and moderate loss vs little or no loss or some gain in weight: weight gain was very rare". It seems that they have employed the same item twice, but with different weights, one positive and one negative.

It is, in our view, unfortunate that Ní Bhrolcháin *et al* did not carry out a component or factor analysis and examine the distribution of factor scores obtained. They based this omission on Maxwell's (1971) objection that such an analysis should not be carried out if there are 'two distinct homogeneous groups' in the sample to be analysed. This view is not shared by others (Cattell, 1965; Gorsuch, 1974; Rummel, 1971). Moreover, their failure to use factor analysis begs the question not only of whether there are two distinct syndromes but also two distinct patient groups.

Two of the papers reporting our study on anxiety states and depressive illnesses (Kerr *et al*, 1972; Schapira *et al*, 1972) provided further evidence that led us to conclude that there were two distinct (though overlapping) syndromes. Briefly (i) they differed significantly in outcome, (ii) the features that best predicted outcome in each group were quite different, (iii) despite variations in severity during the follow-up period the clinical profiles of the two groups changed very little, (iv) the two groups responded differently to the same physical treatments (Gurney *et al*, 1970), a strange result if they differ in severity alone.

As has been argued elsewhere (Garside and Roth, 1978), a bimodal distribution can only be obtained from patient data if:

(a) there are two (or more) conditions (such as psychotic and neurotic depression)
(b) at least one of these conditions is categorical rather than dimensional in nature.

Thus, even if Ms Ní Bhrolcháin were correct in her view that the anxiety-depression dimension measures no more than severity she would be in error in the conclusion she has drawn about the distribution of scores we found. For a bimodal distribution, even on a dimension such as this, would admit of only one interpretation: the patient population under investigation must contain more than one group.

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Some Books Received

COMMUNITY CARE

Community Psychiatric Nurses: An Abridged version of the Report of a Descriptive Study. By Joan W. Parnell. London: The Queen's Nursing Institute. £1.00 plus 25p p & p (paperback). Complete version £5.00 plus 75p p & p.

Therapeutic Communities: Reflections and Progress. Edited by R. D. Hinshelwood and Nick Manning. Henley-on-Thames: Routledge & Kegan Paul. £9.50, £5.50 (paperback).

EDUCATIONAL

The Teaching of Psychosomatic Medicine and Consultation-Liaison Psychiatry: Reactions to Illness. Edited by C. P. Kimball and A. J. Krakowski. Switzerland: S. Karger. Sw. Fr. 83.

A Resident's Guide to Psychiatric Education. Edited by Michael G. G. Thompson. New York: Plenum Publishing. £9.41.

PSYCHOLOGY

Language and Social Psychology. By Howard Giles and Robert St Clair. London: Basil Blackwell. £12.00, £3.95 (paperback).

SEXUALITY

Psychology of Human Sexuality. By Zella Luria and Mitchel D. Rose. Chichester: John Wiley. £9.50.

Female and Male Climacteric: Current Opinion. Edited by P. A. van Keep, D. M. Serr and R. B. Greenblatt. Lancaster, Lancs: MTP Press. £6.75.

CHILDREN AND ADOLESCENTS

The Psychoanalytic Study of the Child. Volume 33. Edited by Albert J. Solnit, Ruth S. Eissler, Anna Freud, Marianne Kris and Peter B. Neubauer. London: Yale University Press. £6.20.

MENTAL HANDICAP

Mental Retardation and Developmental Disabilities. Edited by Joseph Wortis. New York: Brunner/Mazel. $17.50.

Brain Damage, Behaviour and the Mind. By Moyra Williams. Chichester: John Wiley. £8.50.

PSYCHOPHARMACOLOGY

Psychopharmacology of Affective Disorders: A British Association for Psychopharmacology Monograph. Edited by E. S. Paykel and A. Coppen. Oxford University Press. £12.50.

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