space. Indeed, he admits this himself when he states 'The somewhat bimodal distributions of the Newcastle workers may be interpreted as supporting, though rather weakly, a categorical model . . .' Of course, the argument about distributions is weak, and he is quite right in drawing attention to the effects of selection of cases, which can profoundly affect the correlations, factors and distributions. The problem of selection has been sadly ignored in the literature.

He blames the confusion which exists in the controversy on to the paper by Dr. White and myself, but the confusion does not lie where he suggests it is. In our paper, we extracted four factors and we suggested that the first could be named 'endogenous depression' and the second 'reactive depression'. This was a mistake, for careful examination of the factor loadings indicates that the first would be better regarded as a general factor of severity, and the second regarded as a bipolar factor of endogenous versus reactive (to use his terms, which I dislike because we are considering only symptoms, not aetiology). These two factors are (more or less) the two sloping axes in his Fig. I. I pointed out this mistake in *naming* in my paper (Hamilton 1967). The 'confusion' would not have arisen if the data in the Hamilton and White paper had been examined carefully. What was said in that paper was therefore appropriate, even if the terminology dealt with the factors as if they had been rotated factors (Eysenck's ordinate and abscissa), which they were not.

Professor Eysenck is correct in pointing out that in a two-dimensional surface each patient requires to be identified by two scores. Indeed, in my 1967 paper I pointed out that he should be identified by as many scores as there are significant factors in the matrix of correlations, and I found six. May I add here that I have examined the distribution of scores (using a much larger number of cases than in my 1959 paper) of the cases reported on in my 1967 paper, and have found, in both rotated and unrotated factors, that these distributions did not differ significantly from normal.

Again, Professor Eysenck is correct when he says 'Factor-space and person space are two different conceptions, and should not be used interchangeably', but the difference between them is not all that great. If we return to the original data plotted in multidimensional space, then a simple transformation will convert one into the other, as Godfrey Thomson pointed out (Thomson, 1940). This point is relevant to Dr. Kendell's Fig. II, in which is plotted the vectors representing the items in a space determined by the two factors of endogenous and reactive depression. An attempt is made here to demonstrate that the items fall into two clusters, and it would appear that 'the fundamental fallacy in Kendell's thinking' is simply that he is following the example of Thurstone (1947, pp. 126 and 185). I am not convinced by the diagram that the items do fall into two clusters, but had they done so it would have been legitimate to conclude that there are two factors, because such clusters do define factors. They are the rotated correlated factors which are so popular with the American workers in factor analysis.

To sum up, it is always worth while to look at the distribution of scores on an appropriate dimension to see if there is evidence of bimodality. If none is found then the case is 'not proven'; if it is found then it is necessary to consider the problems raised by selection. The argument concerning distributions is therefore a weak one, but in the absence of a better it is worth considering. This applies to all conclusions based on factor analysis.

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DEAR SIR,

Kendell and Gourlay in their article of this issue 'The Clinical Distinction between Psychotic and Neurotic Depression', pp. 257–66, found no distinction between depressive neurosis and depressive psychosis, as defined by the British Glossary, when they applied discriminate function analysis to data collected by several psychiatrists using a standardized technique.

Using a slightly different approach as the preliminary stage to another study, I have been able to confirm their findings. A consecutive series of 94 depressed in-patients was interviewed personally with the same standardized technique. Unlike Kendell and Gourlay, only mental state items were used (36 in all); historical items were omitted. The criteria of the British Glossary were not used in reaching a diagnosis because these descriptions presuppose certain points under investigation in the main study. Instead, descriptions were based on mental state items traditionally believed to distinguish between the two types of depression. The British Glossary description of depressive neurosis is anyway vague and unsatisfactory.

Surprisingly, only three patients (3·1 per cent) were classified on the discriminate function analysis, compared with 14 per cent in Kendell and Gourlay's group. This improvement shows that a single investigator using consistent criteria can achieve good clinical separation between the two types of depression. However, in spite of this improved clinical distinction, the analysis itself, like that performed by Kendell and Gourlay, produced a unimodal curve which did not differ significantly from a normal distribution (see accompanying table).

Score on the Discriminate Function Analysis	Total N = 94	Psychotics N = 55	Neurotics N = 39
−8.99 — −8.00	7	7	
−7.99 — −7.00	10	10	
−6.99 — −6.00	15	15	
−5.99 — −5.00	12	12	
−4.99 — −4.00	12	10 minimum	2
−3.99 — −3.00	8	overlap	8
−2.99 — −2.00	12		12
−1.99 — −1.00	7	1	6
− .99 — −.00	4		4
.01 — .99	6		6
1.00 — 1.99	1		1

Distribution of Weighted Scores on the Discriminate Function Analysis.

That one may distinguish two groups of patients clinically does not necessarily imply that they represent separate disease entities. By analogy it should be possible, using suitably refined criteria, to distinguish clinically between the characteristics of persons aged, say, under 40 years and over 40 years, but on placing the two groups together they would still be found to lie on a continuum.

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PSYCHOTHERAPY WITH FAILURES OF PSYCHOANALYSIS

Dear Sir,

In the May, 1970, issue of the *Journal* (p. 574) Dr. Hilda Abraham was outspokenly disparaging about Dr. Melitta Schmideberg's article, 'Psychotherapy with Failures of Psychoanalysis' (*Journal*, February 1970, pp. 195–200). She says of Dr. Schmideberg that 'it is very obvious that she has no knowledge of developments during' presumably the last 20 years.

I should like to ask Dr. Abraham to tell us just how the majority of medical analysts and analytically trained psychologists in the Health Service were provided with the medical and other schooling which enabled them to become 'skilled in choosing the method of treatment most likely to benefit a specific case'. Further, as Dr. Abraham contends that it is no longer true that little research has been carried out by analysts, will she give the extract reference(s) to such psychoanalytic research work, and for a rigorous assessment of the quality of those studies.

So far as Dr. Schmideberg's article is concerned I am in steadfast agreement with her. Negative suggestions put forth authoritatively by the analyst discourage the patient. He must be helped to face reality and learn how to tolerate or cope with true-to-fact anxieties.

Any therapy that isolates the patient from ordinary life and over-protects him against it produces undesirable consequences. The psychoanalytic relationship will tend to be self-perpetuating when realistic anxiety is attributed to irrational factors which are interpreted as deep-seated abnormalities that can be cured only by further analysis. It appears to me that the analytic schools gloss over generally accepted methods of handling difficult situations, and give inordinate emphasis to irrational material. Direction is avoided, positive suggestions are not given, reassurance is denied and encouragement withheld. No efforts are made to build up self-esteem or to encourage step-by-step improvement or to induce praiseworthy undertakings.

I too have long since discarded the training I received at the Boston Psychoanalytic Institute. As a clinical neurophysiologist who is also a Director of Research and Program Development, I have found it much more rewarding to myself, and much more gratifying to my patients, to upgrade the quality of the results by adopting the lines advocated, and avoiding the snares counselled against, by Dr. Melitta Schmideberg in her very fine, practical, realistic, sensible and rational paper.

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TREATMENT OF PHOBIC PATIENTS WITH ANTIDEPRESSANTS

Dear Sir,

Dr. Mawson's letter (July, 1970, *Journal*, page 117) illustrates the intellectual arrogance, coupled with

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