different cultures on opposite sides of the world; (*b*) it is already well established that in the United States public and private psychiatric patients differ radically both in the diagnosis awarded and the method of disposal (Hollingshead and Redlich, 1954). In the Blackpool and Fylde area a survey of representative samples of all psychiatric patients, private and N.H.S., is under way, and although final results have yet to be extracted impressions of certain differences between private and N.H.S. patients are beginning to appear. These are as follows:

(*i*) The private patients tend to have higher social status.

(*ii*) Private patients tend to be more "neurotic", hysterical and importunate. They are much less co-operative in the Survey despite tactful interviewing and repeated assurances that their confidence will be preserved, perhaps because they wish to "buy" a privileged position for their illness.

(*iii*) The local General Practitioners have a very low opinion of private treatment as compared with other channels of psychiatric disposal. They ranked it in usefulness below the three N.H.S. Hospitals, the N.H.S. Out-Patient Department and the N.H.S. Day Hospital locally available. It seems reasonable to suppose that this bias would be reflected in the sort of patient referred for private treatment.

2. *Interviewer Bias*

The authors rightly point out that symptom clusters elicited by factor analysis may to a large extent reflect the nosological preconceptions of the interviewers. However, all clinicians suffer from this defect and the only possible way of finally eliminating it would be to eliminate clinicians from these studies!

3. *Response to Amitriptyline*

The authors considered that a significant response to a tricyclic antidepressant confirmed that the patients had been depressed. This does not necessarily follow, as tricyclic compounds have been shown to have anti-anxiety effects (Carney and Maxwell, 1967).

4. *Double-Blind Studies*

The authors criticize two recent follow-up studies of E.C.T. response, including that of Carney, Roth and Garside, 1965, because they were not double-blind. It must be emphasized that in the latter study the investigators did not make the diagnosis; this was done by obtaining a consensus of opinion from the clinicians in charge of the case; further, because this objection was foreseen, the criteria selected for assessing degrees of recovery were made as objective as possible and depended upon the presence or absence of social recovery as judged by return to work, normal social relationships and premorbid hobbies and interests.

This is apart from any criticism of the statistical methods used by the authors.

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NEUROTIC AND ENDOGENOUS DEPRESSIONS: A SCEPTICAL VIEW

Dear Sir,

In his letter (*Journal*, August, 1967, p. 924) Mr. R. F. Garside makes reference to two recent papers by Dr. Gudeman and myself (Rosenthal and Gudeman, 1967a, 1967b), and to the autonomous pattern and the self-pitying pattern represented by our first two factors. I should like to make clear, however, that we do not feel, as Mr. Garside does, that the entire depressive population can be divided into neurotic and endogenous depression as represented by these patterns. As we stated, the distribution of patients' factor scores indicates normal rather than bimodal distributions, blended pictures are the rule rather than the exception, and patients with high factor scores should be considered not as clear-cut patient groups but as examples of common symptom patterns. Indeed I am afraid that if the population did have the type of simple structure referred to, that is if it was made up of two independent and mutually exclusive entities which could be separated by the study of their symptoms alone, we would not now be using a complicated device such as factor analysis to discover this. The entities would have been considered self-evident, and long since have been accepted.

The difficulty lies both in the attempt to group patients by symptoms alone and in the nature of

factor analysis. Factor analysis selects covariant patterns of symptoms, but does not define groups of patients. Moreover, it is based on correlation rather than description, and does not correspond to the diagnosis of medical illness. Medical illnesses usually have largely overlapping pictures, with a relatively few discriminating symptoms which tend to be lost in an investigation based on correlations alone.

I suspect that, rather than representing a disease entity, factor scores on our autonomous factor (Factor I) in some sense represent the degree to which a patient has developed somatic concomitants to her illness. This somatic component may be related to serotonin or norepinephrine depletion or some other unknown mechanism and may or may not have a psychological precipitant. Factor scores on our self-pitying pattern (Factor II), on the other hand, may represent modification of the symptom picture by pre-existing neurotic personality features. It is apparent that these conditions may coexist and that most depressed patients will show some varying degree of each. It is also evident from clinical experience that the same patient will show changes during the course of her illness. For instance, she may appear more "endogenous" with the development of more somatic symptoms as the illness progresses.

Mr. Garside emphasizes bipolarity of factor loadings as indicating a demonstration, in some sense, of a separation of the population. However, the mere presence of positive and negative values going into the score does not imply a bimodal distribution of patient factor scores. Moreover, a bimodal distribution might be achieved with a unipolar scoring system as well as with a bipolar one. Patient distribution depends on the characteristics of the patient population rather than on the scoring system. Consider that any item with a negative loading can be reworded to produce the positive loading (i.e., "guilt" loading $-\cdot 40$ can be changed to "lack of guilt" loading $+\cdot 40$). Thus the polarity of any factor can be manipulated by simply rewording the questions, while the shape of the patient distribution curve will of course remain entirely identical.

It is asked in Mr. Garside's letter that we rotate the axes of our first and second factors by 31 degrees and publish the distribution of patient factor scores on the rotated second factor, to ascertain whether the distribution is bimodal. I have done so, using the item loadings he provided for me. The number of patients in each cell, running from 2·0 standard deviations from the mean to minus 2·0 standard deviations from the mean by intervals of 0·5 standard deviations is: 2, 4, 7, 23, 19, 16, 11, 11, 3, 4. Thus, using the rotated factor, I do not find evidence of bimodality in our population. (Indeed if a discrimination by any factor were to have clinical meaning it should be testable by any investigator in his own patient population.)

I feel that attempts to definitively divide the depressive population by symptom correlations will only lead to disappointments. They can, however, aid other investigators to choose relatively homogeneous groups of patients for selective studies, which we may hope will eventually lead to a greater understanding of depression.

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DRUG TREATMENT OF DEPRESSION

DEAR SIR,

The conclusion of the trial by Dr. Hunter and his colleagues (*Journal*, June, 1967, p. 667) that trimipramine and amylobarbitone are no more effective than a placebo in the relief of depression and neurotic illnesses should not dissuade clinicians from continuing to use these drugs.

The result of a cross-over study in which one patient receives two active drugs and a placebo, i.e. in which the patient acts as his own control, is only likely to be valid:

(*a*) if the clinical condition of the patient remains static;

(*b*) if the drug is given for a length of time which is likely to achieve a clinical result;

(*c*) if the effect of a preparation is limited to the period of its administration.

None of these conditions are satisfied in this trial.

1. The patients chosen, being new admissions suffering from neurotic and depressive illnesses, would be likely to have fairly short-term breakdowns which could well be influenced by the hospital environment.

2. As each drug was only given for a period of two weeks, the anti-depressive medication in particular had not sufficient time to effect a measurable improvement.