the female is more tied to the home, it may depend more on territorial factors. For these reasons the two types of depression are likely to be found in purer culture in the male. On similar reasoning, one should avoid metropolitan populations where the lone or vagrant male is common, and select an area where the males are integrated into a reasonably cohesive social structure. Stable military or naval units in peacetime might be suitable.

Concerning the overall form of the illness, the main difference should be in the degree of change which has occurred in the patient's behaviour. No doubt even in the case of neurotic depression there will have been an aggravation before referral, but we would not expect the drastic reversal of attitude which was manifested by Schjelderup-Ebbe's birds. The change should be most noticeable to those just below the individual in the hierarchy. A scale ranging from "not much different from what he's always been" to "he's been a completely different person, you wouldn't have known it's the same man" would be appropriate.

Then, in the case of endogenous depression, there must be a rejection of behaviour patterns which used to be habitual for the patient. Therefore we would expect in the mental state a predominance of guilt about the past, or perhaps an incomprehension of how he could have behaved in his previous dominant manner. In either case he should be out of sympathy with his previous way of life. This should not, however, apply to neurotic depression.

In the premorbid personality, the picture should be normal for endogenous depression, but in the case of neurotic depression we would expect traits of sensitivity to aggressive signals, social anxiety, and a history of subordinate roles in relationships with peers. In neurotic depression, we would expect a family history of chronic mental disorder; in endogenous depression, a family history of phasic disorder.

These suggestions may not be very illuminating, and certainly those relating to the clinical picture are already accepted as promising features for making the distinction. However, perhaps the idea that the two types of depression may have different biological functions may spur on further attempts at dichotomy, if zeal is now flagging through disappointment. And if it is possible to produce in baboons or macaques the sort of state that Schjelderup-Ebbe noted in his defeated birds, we may be able to compare the condition directly with that of the chronically low-ranking and 'henpecked' monkey, and thus provide further promising variables for study in human patients.

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REFERENCES

1. BARNETT, S. A. (1967). "Dominance hierarchy and mental illness." *Lancet, ii*, 415.
2. PRICE, J. S. "The genetics of depressive behaviour." In: *Recent Developments in Affective Disorders* (eds. A. J. Coppen and A. Walk). In press.
3. SCHJELDERUP-EBBE, T. (1935). "Social behaviour of birds." In: *Handbook of Social Psychology* (ed. C. Murchison). Clark University Press.

NEUROTIC AND ENDOGENOUS DEPRESSION

DEAR SIR,

Dr. Garside in his letter (*Journal*, August 1967, p. 924) shows an understandable enthusiasm to maintain the hypothesis that depressed patients tend to show either the symptoms considered to characterize neurotic or those considered to characterize endogenous depression. He points out that three different groups of workers have found a similar clinical pattern of symptoms in depressed patients, and considers that this therefore excludes the possibility that such findings were due to bias. One's dedication to democratic principles and Carrollean logic (what I tell you three times is true) would tempt one to agree with him almost as much as the fact that this would allow us to abandon the use of the double-blind trial and merely accept the results of a series of uncontrolled trials.

In fact the results of the work that he quotes do not agree with those of his study. At least as Rosenthal and Gudeman (*Journal*, May 1967) interpret their data, the neurotic constellation of symptoms and the endogenous one are represented by two separate factors, indicating that these patterns of symptoms are distributed independently. That is to say, in their study patients showing one group of symptoms are just as likely as not to show the other.

In the other study quoted by Garside, that of Hamilton and White (*Journal*, October 1959), the trend for the scores of patients diagnosed as endogenously or reactively depressed to be bimodally distributed was not statistically significant.

Garside suggests that, as in our study the first and second factors did not reveal a bipolar distribution of symptoms, utilizing the third factor might do so. However, it was pointed out in our study that the first factor accounted for only 15 per cent. of the total variance, and the second for 7 per cent. This would mean that any single remaining factor is going to account for less than 7 per cent. of the total variance, and, as one would expect, the next five factors each account for from 4 to 7 per cent. of the variance. If Garside is right, there is no statistical reason why the

third factor rather than any one of these other five might not be the one to look at. This is in any case most unlikely, as in the second part of our study we looked at the four clinical features considered most commonly to be associated with endogenous depression and the four most commonly associated with neurotic depression. These showed no tendency to be distributed bimodally. However, as soon as convenient we will carry out the analysis suggested by Garside with at least the next three factors.

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Dear Sir,

Two letters, one from Dr. Rosenthal (*Journal*, October, 1967) and one from Dr. McConaghy (see above), have attributed to one of us opinions which he does not hold. Since misunderstandings may continue to arise, we welcome the opportunity of stating our position regarding the following:

1. *All depressions are either "endogenous" or "neurotic".* This is not our view. We believe that other types of depression may, and probably do, exist.

2. *Patients with different types of depression fall into discrete groups, with mutually exclusive symptoms.* Again, this is not our view. On the contrary, many patients show both neurotic and endogenous symptoms. This does not mean, however, that depression must be a unitary disorder, that is a disorder which cannot be subdivided on the basis of aetiology or of symptoms. There are four lines of evidence that suggest that depression is not unitary:

(*i*) The neurotic—supposedly milder—form tends to have lasted longer and to respond less well to E.C.T.

(*ii*) The difficulty of including the cyclic and manic disorders in a unitary concept that embraces the neurotic varieties of depression, without sacrificing important pragmatic distinctions (treatment, prognosis). Of course, if similarities, and not differences, are being looked for, the concept could be made broad enough to include all diseases known to man.

(*iii*) Factor analyses have shown that the relationships between depressive symptoms cannot be explained by one general factor only. Kiloh and Garside (1963) and Carney *et al.* (1965) found a bipolar factor ("endogenous-neurotic") which accounted for more of the total variance than the general factor. Rosenthal and Gudeman (1967a and b) found two factors corresponding to "endogenous pattern" and "self-pitying constellation". These two factors were orthogonal, suggesting, as McConaghy rightly points out, that the corresponding syndromes are independent, that is, uncorrelated. But this independence is consistent with the conclusions of Kiloh and Garside (1963) in their paper entitled "The independence of neurotic depression and endogenous depression". There is, therefore, no conflict between the two sets of data if "self-pitying constellation" and "neurotic" are regarded as descriptive of the same syndrome. Independence necessarily involves some overlap, and Kiloh and Garside's data should not be regarded as indicating that both symptoms and patients form two mutually exclusive groups.

(*iv*) The demonstration of bimodality of patients' scores. As Professor Moran (1966, p. 1168) has pointed out "the evidence for such heterogeneity rests on the bimodality of the frequency distribution of the score and not on the manner in which the latter has been found". Carney *et al.* (1965), for example, used scores calculated by the multiple regression technique, and Sandifer *et al.* (1966) used a simple summation of items chosen because of their differentiating power. In both cases unequivocally bimodal distributions were obtained, thus supporting the view that some, at least, of depressed patients can usefully be diagnosed as either endogenous or neurotic. The fact that Rosenthal (1967) did not obtain a bimodal distribution of factor scores when he rotated his factors, does not necessarily indicate that the population distribution is normal. His distribution is consistent with two hypotheses: one that the population distribution is normal and the other that it is bimodal.

3. *Factors describing covarying clusters of symptoms do not define patients' groups.* We agree with this point of view, as far as it goes. Yet *factor scores* do differentiate patients, and, moreover, if the distribution of these scores is bimodal, then different groups of patients are indicated. Such bimodal distributions of factor scores have been obtained, and thus factor analysis can in practice define patient groups, though it is not suggested that factor analysis necessarily provides the best means of so doing. But in any case factor scores provide a useful and valid means of describing patients. Rosenthal, for example, has himself shown (Rosenthal and Gudeman, 1967b) that patients who differed in respect of the independently derived characteristics "apparent precipitants", premorbid personality patterns" and certain historical data also differed in their mean scores on a factor describing an "endogenous depressive pattern". Moreover, groups of patients tentatively defined as "endogenous" or "neurotic" on the basis of their factor scores have been found to differ in long-term prognosis (Kay *et al.*, unpublished), and it would seem to be useful, if these findings are confirmed—and for practical purposes at