sorrow—which did develop into "depression"—of Ahab, who "laid him down upon his bed and turned away his face and would eat no bread" because he was refused the coveted Naboth's vineyard.

The *New English Bible* version fully bears out this interpretation. It reads: "For the wound that is borne in God's way brings a change of heart too salutary to regret; but the hurt which is borne in the world's way brings death"—though I would suggest that the nature of the hurt or wound is relevant here as well as the way in which it is borne.

Although my argument is concerned with St. Paul's meaning, and not with the classification which mediaeval theologians may have based on his words, it does seem to me surprising that Cassian and others should have so confidently identified "beneficent" with "rational", and "malignant" with "irrational" depression. No one could say that Ahab's depression was other than "rationally" caused, yet nothing could have been more "malignant", leading as it did to crime and eventually to downfall and death.

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NEUROTIC AND ENDOGENOUS DEPRESSIONS

DEAR SIR,

McConaghy *et al.* report (*Journal*, May 1967) that they failed to replicate the findings of Kiloh and myself (1963). They ascribe this failure to two possible reasons: interviewer bias and patient selection.

In the same number of the *Journal*, however, Rosenthal and Gudeman state "Several recent factor-analytic studies rating symptoms in depressed patients have had results which portray a common clinical pattern (Hamilton and White, 1959; Kiloh and Garside, 1963; Rosenthal and Klerman, 1966; Rosenthal and Gudeman, 1967b). In each of these studies the first or primary factor has suggested the endogenous depressive pattern. In the most recent of these papers we presented the first factor in our study of 100 depressed women (Rosenthal and Gudeman, 1967b). This factor was shown to be similar to the principal factors of the other studies, and to suggest the 'endogenous' or 'autonomous' pattern. This replication has been an encouraging indication that studies carried out in different patient populations may indeed give reproducible symptom patterns."

It therefore seems that the findings of Kiloh and myself, and those of Carney *et al.* (1965), were not merely due to bias of one sort or another (see below). Thus one is led to search for other reasons why McConaghy *et al.* failed to replicate our results. Their largest first factor loadings are associated with the items of 'hysterical features present' (−0·745) and of 'previous psychological adjustment good' (+0·761). Moreover, their loading of the personality feature of 'anxiety' was −0·390. Thus, their first factor (reversing the signs of their loadings) seems perhaps to be over-contaminated with the personality dimension of 'neuroticism' and thus not to be a pure factor of depressive *illness* as such. In this connection it may be worth while drawing attention to the fact that their material consisted entirely of private patients. They do not attempt to interpret their factors, but the hypothesis that their first factor is not one of depressive illness as such is supported by the fact that the correlation between their first factor loadings and ours is only 0·21. Our first factor did seem to Kiloh and myself to be one of depressive illness; our highest loadings were associated with 'failure of concentration' (0·572) and 'agitation' (0·485) and the loading of the personality feature of 'anxiety' was only 0·073.

If it is true that their first factor is tilted towards neuroticism, then one would expect their second factor to be a mixture of depressive illness in general and of the bipolar dimension of endogenous against neurotic depression. Again this is supported by the correlations of their second factor loadings with those of our first factor (0·33) and of our second factor (0·22). I have attempted to increase the correlation between the two second factors by rotating their factors, but without success. They also carried out varimax rotation, but "this did not improve their ability to differentiate the clinical features of the two forms of depression". The reason for this state of affairs may well be that their third factor, which they do not mention, is perhaps a mixture of the differentiating bipolar dimension and some other factor, as their second factor seems to be. If this is the case, then it is the second and third factors which should be rotated to arrive at a differentiating factor, not the first and second factors.

It is hoped that McConaghy *et al.* will publish their third factor loadings and carry out a suitable rotation. If this is done, however, the varimax method of rotation, which they mention, should not be used. The aim of this method of rotation is to achieve simple structure, that is, *descriptive* factors. Such factors are often quite distinct from *differentiating* ones.

The distinction between descriptive and differentiating factors is well illustrated by the two recent papers of Rosenthal and Gudeman (1967a, 1967b). In these papers they discuss the self-pitying constellation and the endogenous depressive pattern respectively, as indicated by their first two factors. If these two factors are rotated through 31°, the first factor

becomes more general in that the negative loadings are reduced from four to two, and the second factor becomes more bipolar, or differentiating, the negative loadings being increased from 10 to 18 out of 30. The four highest positive loadings on the rotated second factor are associated with the following features in order: 'self-pity, reactivity of depression, hypochondriasis, demanding'. The four highest negative loadings on the same factor in order are: 'retardation, guilt, worthlessness, suicidal symptoms'. Thus, by a suitable rotation, their two descriptive factors produce a factor clearly differentiating neurotic from endogenous depression. Perhaps Rosenthal and Gudeman could be persuaded to rotate their factors, as I have done, and publish the distribution of patients' scores on the rotated second differentiating factor. Is this distribution bimodal?

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"NEUROTIC STYLES"

DEAR SIR,

Dr. Brenda Grant's review of my book, *Neurotic Styles*, has recently come to my attention (*Journal*, August, 1966, Vol. 112, p. 849). May I reply briefly to this extraordinary piece?

Dr. Grant complained of my book, in the first place, that its whole aim or thesis was "difficult to grasp", meaning, of course, that it had none. It has, but Dr. Grant did, indeed, fail to grasp it. The book's thesis is that the nature or form of the specific traits, symptoms, and defence mechanisms that comprise the well-known neurotic syndromes derives from, and can be understood in terms of, more general modes of function (or what I call "styles"), such as mode of thinking, characteristic of the various conditions. I attempt to explain this thesis, to relate it to the stream of psychoanalytic theory, and to indicate something of its practical significance in an Introduction of 32 pages out of the book's total of 200 pages. What Dr. Grant made of that Introduction I cannot imagine; she simply does not refer to it. The major part of the book is then devoted to a close examination of the form of well-known traits and symptoms of a number of syndromes in order to show the general formal principles, i.e. the characteristic modes of thinking, of action, and the like, manifest in them. Since she has missed the point, however, to Dr. Grant all of this apparently remains aimless and therefore peculiarly "minute" description. In a grand sweep, she asserts "Like many of the writers on psychoanalytic theory" (who?) I have "fallen into the trap" of confusing description with explanation.

To this Dr. Grant adds charges of unsubstantiated speculation and vagueness or meaninglessness of formulation, but she supports these charges with remarkably selective editing of what I actually said. She charges me with arbitrary assertions while ignoring my clinical evidence, with overgeneralization while ignoring my qualifications, and with vagueness of formulation by quoting out of context. Thus, she quotes the following in order to ask "what this really means": "a consequence of any neurotic style is the exclusion from consciousness of certain classes of subjective experience and mental content." She omits the preceding words, "If we say that . . ." as well as a following clause. In so doing she avoids indicating to the reader that this is a summary statement referring to an immediately preceding argument of some length, in which I develop the thesis that in neurosis it is not a single or a few specific mental contents that are excluded from consciousness, as is sometimes assumed in psychoanalysis, but whole kinds, or classes, of subjective experience and mental contents.

In short, I believe Dr. Grant has yielded to the temptation to make a speech of one full page in length, but has not really reviewed my book at all.

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DEAR SIR,

It is apparent that Dr. Shapiro believes I have reviewed his book with neither understanding nor