

## SOME RECENT CRITICISMS OF THE DIMENSIONAL ANALYSIS OF PERSONALITY

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IN recent issues of THE JOURNAL OF MENTAL SCIENCE there have appeared two critical papers (5, 7) dealing with some aspects of the dimensional analysis of personality which the writer has put forward. As replies to these papers separately would seem to require a good deal of repetition, it seemed better to frame a joint reply. This has been kept rather short on purpose, primarily because the writer does not believe that arguments are very helpful to the advancement of science, unless they are accompanied by new data of an experimental kind. In the main, therefore, this reply has restricted itself to simply pointing out that many of the points raised are factually incorrect, or, although they might be correct in themselves, are not relevant to the theory they are criticizing.

To begin with we may deal briefly with a paper by Hamilton (5), who reports obtaining 15 sets of results from 11 tests, which for some unstated reason he appears to consider as measures of one and the same personality trait; he does not give any correlational data to support this view. Apparently 12 of these 15 scores differentiate between his neurotic and his normal control subjects; in addition significant differences were obtained between various neurotic groups. Most numerous are the differences between anxiety states and hysterics (seven); least numerous those between obsessionals and hysterics (three); the number of differences between anxiety states and obsessionals (six) is intermediate. Hamilton considered these results to be "at variance with the conceptual experimental and statistical framework that is usually associated with the work of Eysenck". He seems to base his conclusion on two implicit hypotheses: 1. the tests used by him are measures of extraversion-introversion, and 2. the measures used are relevant to the theoretical analysis made by me of this concept. If this were so then indeed we might be mildly surprised that the number of significant differences between obsessionals and hysterics is not larger than it is, obsessionals usually being grouped with anxiety states as part of the dysthymic group. However, neither hypothesis is tenable. The theory of extraversion-introversion in terms of the excitation-inhibition balance, which I have advanced (2), does not permit of any predictions with respect to the majority of the tests used by Hamilton, and indeed he makes no effort to show that any such deductions can be made. It follows that the results can only have the most tangential relevance to the theory I have suggested. In the second place, Hamilton's own results show quite clearly that his tests are tests of *neuroticism* rather than of extraversion-introversion; it will be remembered that nearly all his tests differentiate significantly between normals and neurotics. This suggests the possibility that his various neurotic groups may have differed with respect to degree of neuroticism. The very perfunctory analysis of the data given by Hamilton makes it impossible to discuss his results any further; it

would have been necessary for him to have carried out a factor analysis and a canonical variate analysis before any conclusions at all could have been drawn from his experiment; even then, of course, it would have been better if he had chosen for his experiment tests relevant to the theory of extraversion-introversion. It is always difficult to disprove a theory when the tests used are irrelevant to that theory!

In addition to his experimental study, Hamilton criticizes my theoretical framework. His criticism here appears to be based on a fallacy, namely that the system of classification which I have put forward is one involving categorical groups. Thus Hamilton talks about "Eysenck's scheme of classifying nosological groups into dysthymics, hysterics and psychopaths", and states that my investigations "would appear to represent the substitution of one system of mutually exclusive classification of clinical types for another". This, of course, bears no relation to my actual theory as put forward in "Dimensions of Personality" and later publications. I propose there two main continua or dimensions, neuroticism and extraversion—introversion, to account for a large proportion of the behaviour characteristics of non-psychotic human beings. I have shown that as a matter of fact individuals high on neuroticism and extraversion tend to be labelled *psychopaths* by psychiatrists, while subjects high on neuroticism and on introversion tend to be labelled *anxiety states*. There are several other resemblances between the categorical method of classification of the psychiatrist, and the continuous dimensional method suggested by myself. These correlations are of some practical and theoretical interest but they do not convert my system into one of categorical classification, a notion against which I have argued on many occasions. Hamilton seems to be under the mistaken impression, as illustrated in his figures 3 and 4, that arbitrary changes in the metric of a dimensional analysis are relevant to this issue; this is too obviously incorrect to deserve a lengthy refutation. His argument is formally equivalent to stating that a change in the system of labelling longitudes, which would shift their origin from Greenwich to San Francisco, would alter our weather! As most of the remainder of Hamilton's criticism appears to be based on this fundamental error, there appears to be little point in continuing this reply.

The article by Storms (7) reanalyses certain data collected by Hildebrand (6) and uses in doing so the very methods which Hamilton ought to have used in the analysis of his own data. The result is an interesting comparison of different multivariate analyses tending to show, as one might have expected, that discriminant function analysis gives results which are not in all points identical with the results of factor analysis. Storms' arguments and mathematical developments are quite correct, and I would fully agree with his conclusion "that adherence to dimensions derived from factor analysis when analysing differences among groups can lead to serious loss of information, and may lead to inefficiency in practical applications or oversimplification in theoretical interpretations". Interesting as this demonstration may be, it is difficult to see why Storms should imagine that these results in any way contradict my theory. Indeed, Storms seems to realize this when he admits that "Eysenck will not find anything inconsistent in the finding of several principal components in each analysis, since he does not claim that his psychoticism, neuroticism, and extraversion-introversion dimensions cover the whole range of human variation". Storms appears to devote the rest of this paragraph to saying that if I had made such a claim his results would have disproved it; a point which would hardly seem even of academic interest, since few people would ever have

imagined that all the variabilities of human behaviour could be accounted for by just three dimensions!

Storms states that I imply "that the important differences among neurotic groups are accounted for by different degrees of extraversion". This statement is correct or incorrect, depending on the interpretation one chooses to put upon the word "important". I believe that a consistent theory of extraversion-introversion in terms of inherited differences in the excitation-inhibition balance can account for certain important symptom patterns, behaviour patterns, and test score patterns characterizing the main nosological groups recognized by classical psychiatry in the field of the neurotic disorders. The demonstration that other factors discriminate between the groups, and that the correspondence between clinical diagnosis and position on the extraversion-introversion dimension is far from perfect, is implicit in my theory. I would be willing to argue that from the scientific point of view the systematic differences corresponding to my theory and predictable from it are more important than the purely empirical findings having no psychological rationale which Storms reports. I am quite willing to agree that this use of the word "important" implies a subjective value judgment based on my reading of the history of science, which suggests the importance of theoretical considerations and the hypothetico-deductive method.

A similar dubiety attaches to the use of the word "practical" in Storms' belief that "adherence to dimensions derived from factor analysis . . . may lead to inefficiency in practical applications". By this he presumably means that if we wanted to discriminate between the diagnostic groups used by Hildebrand on the basis of the tests administered by him, factor scores would be less useful than scores based on discriminant function analysis. This is quite likely true, although one would have liked to have seen a cross-validation study applying both sets of formulae to sets of groups other than those from which they were derived. However that may be, I can see little practical point in using a long and complex battery of tests for the simple purpose of giving a diagnostic judgment which more or less efficiently approximates that given by the psychiatrist in charge of the patient, and obtainable with much less trouble. As I have pointed out before, I think there is some interest in showing that groups of psychiatrically diagnosed patients have certain predictable positions on the dimensions into which I have analysed certain aspects of personality; this, however, does not mean that the purpose of this analysis lies in the simple duplication of the psychiatrists' efforts. I believe that categorical segregation of patients into diagnostic groups is erroneous theoretically, and historically explainable in terms of a faulty analogy with the qualitatively different disease processes usually found in physical disorders (3). My purpose is to substitute for this a dimensional analysis describing each individual patient in terms of his position on a number of relevant continua, rather than in terms of separate categories. I would not regard the "practical" purposes which Storms is referring to as having any real practical value at all, and I would, therefore, regard his mathematical solution as being of relatively little use to the practising psychiatrist even from the practical point of view. From the theoretical point of view, his results lack significance because at no point do they make contact with psychological or psychiatric theory.

In all this I do not wish to deny the great value which discriminant function analysis may have when used in the right context. There are certain theoretical problems which can probably be solved by the use of these techniques, and indeed several such uses have been reported from our laboratory (1, 2, 4). What

does seem to be relatively pointless, however, is the use of complex statistical techniques for their own sake and without any recognizable theoretical context which could be clarified by their use. However perfect such applications may be technically, they do not advance their subject matter.

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