

## THE CONTINUITY OF ABNORMAL AND NORMAL BEHAVIOR

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In a recent paper in this journal, Pearson and Kley (11) complain of

the proclivity of psychologists for assuming or demonstrating variables to be distributed in continuous fashion throughout the general population, but concentrating attention on the pathological extremes, which may, in fact, constitute discrete series. The familiar concept of a normal distribution for "emotional adjustment" ranging from "super-normal" and "normal" to "neurotic" and "psychotic" has led laymen and behavioral scientists alike to picture human emotions in various shades of gray. . . . While such conceptualizations may serve a useful purpose, they may also be misleading. The danger lies in the temptation to infer continuous distribution of underlying etiological factors from the fact that behavioral traits appear to be so distributed.

Pearson and Kley then quote a study by Eysenck and Prell (8) as an apparent example of this fallacy. They say of these authors that:

their assumption that neuroticism is on a continuum in the general population and the samples employed make (*sic*) it impossible to infer that clinical cases of neurosis arise at the extreme end of the continuum only because of the *degree* to which they inherit the neuroticism factor. Testwise or symptomwise, the diagnosed neurotics do constitute the extreme of the distribution, but the reason for their coming to this sorry end may be quite different from the reasons which cause individuals in the "borderline" or "normal" range of tests scores or clinical behavior to fall where they do.

The point that the distribution of scores on a single test cannot be safely interpreted to give a correct indication of the distribution of the underlying determinants in the absence of a proper metric and in view of the usual large error variance is well taken. It was made explicitly by the

writer in *The Structure of Human Personality* (5, p. 11), and having always argued against the tendency of psychiatrists and psychologists to *assume* either continuity or discontinuity of normal and abnormal behaviour in the absence of *proof*, the writer was not unnaturally surprised to find himself accused of this very crime. It is the purpose of this brief note to show that this very fundamental criticism of the genetic and other experimental work done in the field of abnormality by the writer and his colleagues is not in fact subject to this charge.

We have already agreed with Pearson and Kley that no faith can be put in the distribution of scores on any one test in arguing for or against the continuity hypothesis. The writer accordingly put forward a method for investigating this problem along hypothetico-deductive lines which was published under the name of "Criterion Analysis" (2). Having outlined his theory of the existence of the general factor of neuroticism, similar in mode of derivation and general interpretation on the orectic side to the general factor of *intelligence* on the cognitive side, the writer went on to say that what was at issue in this paper was "the hypothesis that this putative factor of 'neuroticism' forms a quantitative continuum, on one extreme of which are to be found hospitalized neurotics, while so-called normals are to be found all the way from the near neurotic and neurotic to the conspicuously non-neurotic, mature, stable,

and integrated type of personality" (2, p. 42). It was also pointed out that a similar problem arose in connection with Kretschmer's hypothesis of the existence of a normality-abnormality continuum ranging from the normal to the psychotic. Having thus indicated the problem, which of course also includes the identity or independence from each other of these two hypothetical continua, the writer went on to outline the method of criterion analysis, whose specific merit was claimed to be its ability to provide evidence relevant to this type of hypothesis. Empirical data have been given to demonstrate the truth of the continuity hypothesis, particularly with respect to neuroticism (2) and psychoticism (3). Later studies using the technique of canonical variate analysis (6, 9) have demonstrated the essential independence of these two continua. A detailed discussion of all this work is given in *The Dynamics of Anxiety and Hysteria* (7).

It would be open to Pearson and Kley to criticise this method along various lines. They might argue against the logic underlying criterion analysis which postulates that if, and only if, there exists a continuum between normal and abnormal mental states will there be found (a) corresponding factors in correlation matrices derived separately from tests administered to normal and abnormal groups and (b) significant correlations between these factor loadings and what the writer has called the "criterion column," i.e., the column of biserial correlations between normality-abnormality on the one hand, and the various tests used in the experiment on the other. Such attempts as have been made in the literature to impugn the logical validity

of criterion analysis (e.g., 1) appear to have rested on a misunderstanding of the method (4) and cannot be regarded as fatal to the postulation.

Secondly, it might be open to Pearson and Kley to argue that the specifications of the method are not sufficiently clearly expressed to make its use feasible. Thus a large and varied battery of tests is required, all of which must discriminate significantly between the normal and the abnormal group. It might be argued that "large and varied" is too indefinite a description, and that no particular standard of significance has been specified. These objections would be well taken, and it is to be hoped that in due course it will prove possible to give a more operational definition of "varied" than is possible at present. The writer doubts, however, if these difficulties are fatal to the method. Until better criteria of selection are available, it might be suggested that the number of tests should not be below 20, with the standard of significance to be taken as the  $p=.01$  level, and that the term "varied" should be interpreted as referring to the abilities involved in the tests used, as determined by factorial analysis; different muscle groups used in the execution of the tasks; different sense organs used in the mediation of the tasks; and so forth. It is doubtful whether in practice much doubt will arise on any of these points.

Lastly, it will be open to the critic to point to certain weaknesses in the mathematical treatment of criterion analysis, as was done for instance by Lubin (10). These difficulties are very real, and the writer has no wish to gloss over them. Until they are completely overcome it is obviously necessary to use the method with

considerable circumspection, and only with the fullest understanding of the assumptions underlying each step taken. Nor should the interpretation of the final results be made in any but the most tentative fashion. Nevertheless, and in spite of all these qualifications, the writer does not know of any other method available at present which tackles this particular problem, or which can offer us worthwhile information relating to it. Until a better method is available, therefore, criterion analysis will remain as a worthwhile addition to our methodological set of tools.

It is noteworthy, however, that Pearson and Kley do not criticise criterion analysis on any of these grounds. What they do instead is to neglect the whole body of work done by the writer in connection with this method and this problem and to present him as basing his views entirely on an invalid argument from the simple distribution of single test scores. This does not appear to the writer to be a reasonable form of criticism, and consequently it seems desirable to put the point in its proper perspective.

In spite of all that has been said in this note, the writer would not wish to dismiss the possibility or even the likelihood that in any random group of clinically diagnosed neurotics there would be found a small number of people who might "constitute a group apart, different not in degree, but in kind, by reason of some specified bio-

chemical error, which is highly predictable in terms of inheritance, and which operates in a manner quite different from anything observed" in the kinship relations of the remainder of that group. The evidence quoted makes it somewhat unlikely that the major part of any given neurotic or psychotic group would be made up of such individuals, but no one familiar with the heterogeneity of psychiatric groups would wish to suggest seriously that all the members of such groups were homogeneous with respect to hereditary processes and genetic determinants. Nevertheless, *as a first approximation*, we must be concerned with the major sources of variance affecting the majority of members of such groups, and it is in this connection that the writer cannot follow the criticism levelled by Pearson and Kley against the Eysenck and Prell study.

#### SUMMARY

Pearson and Kley (11) criticize the writer for basing his belief in the continuity of normal and abnormal states on the invalid consideration that test scores tended to be continuous between the groups. In answer, the writer has pointed out that he himself had discussed the lack of validity of this procedure in detail and had advocated a different method, namely, that of criterion analysis, specifically designed by him to deal with problems of this kind.

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