THE APPLICATION OF FACTOR ANALYSIS TO THE STUDY OF PERSONALITY: A REPLY

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Mr Albino's thoughtful and well-argued paper deserves a more detailed reply than I shall be able to give it here; many of the points which arise are discussed in my forthcoming book on *The Structure of Human Personality* (1953) and will therefore not be dealt with at any length in this brief note. I welcome Mr Albino's agreement with my stress on the importance of taxonomy in science generally, and in psychology in particular, as well as his recognition of the partly subjective nature of all dimensional systems in science; less sophisticated critics have often decried the importance of taxonomic problems as compared with so-called 'dynamic' ones, and have erroneously supposed that the subjectivity inherent in factorial solutions is not also found in the dimensional systems of physics.

Mr Albino's main criticism seems to be contained in the first paragraph of his *Discussion*. 'It is clear that Eysenck's application of factor analysis is only an example of scientific method in so far as he has made more precise descriptions of clinical categories. But those categories were isolated in the first place by the crude methods of the clinicians and were not originally the observations of the users of factorial methods. The question of whether the use of factorial methods was worth while depends entirely upon the theoretical importance of the original categories.... Eysenck is fortunate because he has chosen some classifications which are clinically useful and valuable. But this is not a result of his method; it is a result of the work of others, and nothing in his method will aid in establishing valid and useful forms of classification.' There are two main comments.

In the first place, history shows that in the early stages of any science common-sense observation and classification precede more exact scientific measurement. Heat and cold are distinguished before the thermometer is invented; bodies are observed to fall to the ground if unsupported, before the theory of gravitational force is enunciated. To compare large things with small, it would seem a little grudging to say that the inventor of the thermometer *only* made more precise descriptions of facts already known, or that Newton *only* succeeded in doing a little more accurately what everybody else was doing already! If indeed my work be judged to have succeeded in making more precise the description of personality, I would consider this praise indeed, rather than criticism. It is seldom realized to what extent *objectivity* and *measurement* aid in the development of science; in making common-sense or clinical observation more precise, they also furnish the means for correcting its errors and testing rigorously its underlying hypotheses.

There is one further advantage which objective measurement has over clinical diagnosis, an advantage which is so obvious, and appears to be at so elementary a level, that many psychologists fail to mention it at all. Scientists must obviously be able to designate their subject of study; the botanist must be able to recognize and identify trees, flowers and shrubs, and distinguish them from frogs, film stars and footballs. Much experimental work in clinical psychology is done in accordance with the simple schema of contrasting the performance of groups of schizophrenics and manic depressives, or hysterics and anxiety states, on some test or series of tests. This presupposes that we are able to identify our 'subjects of study', i.e. schizophrenics, hysterics, and the like, with sufficient certainty to make the results meaningful. The contradictory results too frequently found in the literature suggest that this supposition is not justified, and the studies of reliability of psychiatric diagnoses summarized in *The Scientific Study of Personality* support the view that sources of error in diagnosis are so powerful as to make correct (i.e. reliable) recognition of the subjects of study practically impossible. If we can make bases of judgement more precise and more objective, then we would at least be able to identify members of groups for further study. It is difficult to see along what other lines we could proceed in trying to eliminate the contradictory findings of equally competent experimenters.

A final advantage implicit in this method is that it enables us to link up the abnormal, psychiatric field and its numerous theories and insightful discussions, with the large body of experimental studies in the normal field. Where there has been any attempt previously to link up the theories of Jung, Kretschmer, Kraepelin, Bleuler, Freud, and other psychiatrists on the one hand, and the experimental contributions of Heymans, Wiersma, Webb, Guilford, Thurstone, Wenger, Cattell, Freeman and other psychologists on the other, it has always been on a purely *semantic* level, based on argument from analogy. By undertaking precise and objective measurements, we make possible a direct comparison and link-up between these two great but distinct contributions to the psychology of personality.

So much for my first comment. The second one is rather more important scientifically. All through Mr Albino's paper runs the idea that I have taken some clinical classification (more or less at random, apparently, because he seems to think I was 'fortunate' in hitting upon a useful and valuable one) and made it more precise, but without adding anything to it. This is indeed an odd view to take, because my actual procedure was quite different, and I can only conclude that I have failed completely in my book to explain clearly and intelligibly the development of my theory. Perhaps an autobiographical note may help. When I entered the psychiatric field, I went through a number of psychiatric and psychoanalytical text-books in order to construct for myself some sort of orderly, systematic picture of the set of theories, hypotheses, nomenclatures, symptomatologies and classifications to be found there. I was looking, not necessarily for completeness or even truth, but I was looking for what must characterize every scientific system, namely consistency. But this I did not find. Even within the covers of a single text-book, I found syndromes treated as constituting *qualitatively* different mental states on one page, but as merely *quantitatively* different positions on a continuum on another. I found neurosis and psychosis treated as lying on two different dimensions on one page, but as different stages along one and the same dimension on another. I found Kretschmer's schizothymia identified with Jung's introversion on one page, only to find on another that Kretschmer groups the hysteric with the schizothyme, while for Jung the hysteric is the prototype of the extravert. In fact, there was such a lack of consistency that any experimental finding could be explained after the event, but none could be predicted with certainty before the event. The same lack of consistency which characterized psychiatric writings was also found to an equal extent in psychological texts, which, far from contributing anything new, were merely pale replicas of the psychiatric ones.

Under these circumstances, two courses of action are open to the investigator. One is to start with a *tabula rasa*, and to disregard clinical description completely. This is the path followed, with important and suggestive results, by Cattell. The other is to take up hypotheses derived from clinical and psychiatric authors, state them in a form which makes possible an exact test, and carry out the necessary experiments and calculations. It is this path that I have followed, largely from a belief that it would be wasteful to throw out the baby with the bath-water, and to deprive oneself needlessly of the accumulated wisdom of centuries of sometimes brilliant, often acute, always relevant, clinical observation. I have therefore taken up such questions as the problem of quantitative or qualitative differences between clinical syndromes, or that of the relation between neuroticism and psychoticism, and submitted them to the best and most appropriate experimental test I could devise. The value of my contribution must stand or fall by the degree to which these tests are judged to be well designed, and, in particular, the degree to which the method of criterion analysis can be used to answer definitively questions of the kind I had in mind.

It is in dealing with Mr Albino's criticisms here that I have some difficulty. He makes two criticisms, the first of which applies to an argument I have never used, and the second of which misquotes completely an argument I do use. Mr Albino says that the fact that normals and neurotics show continuous distributions of test scores is trivial and is not relevant to the question of quantitative or qualitative differences; I agree, but having never used this argument at all am somewhat at a loss to know why it is adduced by Mr Albino.

The other argument, taken from the differences between normal subjects and typhoid patients, reveals a serious misunderstanding of the method of criterion analysis. Mr Albino says: 'Temperature, water loss and respiration rate are correlated in normal subjects taken alone and also in typhoid patients taken as a group and, also, the three variables are correlated between normal and typhoid groups. Nevertheless, normal and typhoid patients form a true dichotomy from the standpoint of origin.' Thus, apparently, the *qualitative* differences existing between normals and typhoid patients are not brought out by the statistical test, which gives the erroneous impression that the differences are merely quantitative. Extensive search having failed to reveal any trace of such correlations actually having been calculated, I must assume that Mr Albino has invented them in order to suit his argument, and has then based his argument on these hypothetical correlations which were invented precisely for the purpose of supporting it! Such circular reasoning does not appear to be a useful method of criticism. However, even if the facts should be as Mr Albino states them, they would be quite irrelevant to the methodological requirements of *criterion analysis*. What would be required would be two sets of correlations resulting in identical factors, and a set of criterion correlations *proportional* to one of these sets of factors. No doubt these could be invented as easily by Mr Albino as the original correlations, but I venture to predict that if the experiment were actually carried out (involving, of course, more than three items to be correlated), the results would not support the hypothesis of a quantitative continuum, but clearly indicate the essentially *qualitative* nature of the difference. In the absence of empirical data it is difficult to discuss this point any further.

Another point which I want to take up relates to Mr Albino's view that the work described in *The Scientific Study of Personality*, and presumably in *Dimensions of Personality* as well, is purely descriptive and not predictive or explanatory as is, for instance, that of Newton. I would gladly agree that Mr Albino is absolutely right in this judgement; I would, however, venture to add that before we can formulate an explanatory theory to account for more elementary facts we must know what facts to account for. Newton was able to formulate an hypothesis taking in the movements of the planets and the falling of apples because for thousands of years precise though perhaps somewhat pedestrian determinations had been made of these movements. No such body of ascertained, indisputable facts is available in the field of psychology, least of all in that part of it dealing with personality; it follows that theory-making must either proceed *in vacuo*, or else must remain at the rather modest level at which I have kept it. Now, however, that we have succeeded in unearthing a large amount of factual material, it may be possible to go on to the formulation of more ambitious theories of the kind called 'explanatory' by Mr Albino, and indeed much work is in progress at the moment in attempts to verify and possibly modify such theories as have been suggested to us by our previous work.

One last comment. I have no wish to follow Mr Albino and assume the role of prophet, as when he maintains that 'it is likely that the crude hypotheses of the Freudians are genuine hypotheses and explain many facts of behaviour. Attempts at rigorous confirmation of these hypotheses might be of greater value, both in producing a sound taxonomy of personality and establishing a theoretical foundation for psychology. The psycho-analytic hypotheses may be false; to show them to be so by experiment would be more scientific than to reject them out of hand and to put in their place a set of descriptions which explain nothing'. Mr Albino has the right to his opinion, of course, but he is wrong in assuming that I reject psycho-analytic hypotheses 'out of hand'. My main reasons for not accepting them are: (1) there is no acceptable proof of any of them after more than 50 years of work in this field; (2) it is difficult to see how any crucial test can be carried out to disprove hypotheses stated so vaguely and in such imprecise terms that any factual result can be rationalized *ex post facto*. This is realized by psycho-analysts such as J. D. Sutherland, who says in his recent Presidential Address to the Medical Section of the British Psychological Society 'our hypotheses are not stated in a form which can be tested in a scientifically valid way'. It is not clear, then, on what grounds Mr Albino asserts that it would be more 'scientific' to show psycho-analytic hypotheses to be false than to try and build up a set of facts, and of explanatory hypotheses to account for these facts. Could it be that our notions of what constitutes science and scientific advancement are not in complete agreement? It would take us too far afield to discuss this point.

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