

Personality and Factor Analysis: A Reply to Guilford

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Consideration is given to the problem of how we are to arrive at personality factors that possess some demonstrable degree of representativeness. It is suggested that psychometric considerations play an important part and that factor analysis in particular can be of great value in this connection. It is also suggested that there are other considerations of at least equal importance. Factors emerging from such analyses must be replicable and reliable, and they must fulfill certain basic psychometric requirements. In addition, there should be evidence of their heritability, they should have theoretical backing leading to objective laboratory verification of deductions from such theories, and they should be socially relevant in the sense of correlating significantly with social parameters. The evidence suggests that there are three and only three such factors emerging from relevant research, that these are all higher order factors, and that primary factors lack some or all of the qualities required. It is suggested that the models presented by Guilford, Cattell, and Eysenck converge on some such model as is here presented.

A recent paper by Guilford (1975) expressed a point of view regarding the determination of personality dimensions by factor analysis that will probably be shared by most psychometrists. In this reply I do not wish to dwell on the many points of agreement (e.g., distrust of purely psychometric considerations in rotation, preference for orthogonality where possible, use of factor analysis in a hypothetico-deductive manner, disbelief in the meaningfulness of specifying factors as second-, third-, or higher order, selection of variables for analysis), but wish rather to discuss briefly two points of disagreement, one factual and dealing with matters largely internal to factor-analytic methodology and findings, the other external and dealing with the relation between factor analysis and other parts of psychology.

In his section on "Eysenck's Factors E and N [Extraversion and Neuroticism]," Guilford (1975) correctly stated that the early work on the Maudsley Personality Inventory (MPI) and the Eysenck Personality

Inventory (EPI) was profoundly influenced by his own work on personality factors, but he concluded his brief summary with a statement that is perhaps less than accurate: "As for his Factor E, I am forced to conclude that it is not a factor at all, but a kind of 'shotgun wedding' of R and S" (p. 809). R and S, of course, stand for Guilford's factors Rhathymia and Sociability, and it is true that in the early development of the E and N scales, many items from these two scales were included in the definition of E. However, this was no 'shotgun wedding,' a term that suggests that the union was unnatural and forced; the items were selected through a series of factorial studies that convincingly demonstrated that the two factors belonged together and defined a common higher order factor (H. J. Eysenck & S. B. G. Eysenck, 1969). Indeed, there are grounds for disputing Guilford's basic hypothesis of the unidimensionality of S, one of the components in this "shotgun wedding." In a paper that initiated the development of the E and N inventories, H. J. Eysenck (1970a) used Guilford's R scale as the prototype of future extraversion scales and Guilford's C (Cyclic Disposition, or stability of emotional reactions) scale as the prototype of future neu-

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roticism scales. He then asked the question of where, in this two-dimensional space created by these two orthogonal scales, S belonged, there being evidence suggesting that S was related to extraverted behavior (directly) and also to neurotic behavior (inversely).

Correlating individual S items with both R and C, Eysenck showed that some items correlated largely with R (positively), others with C (negatively). Hardly any items correlated with both these scales. It was concluded that the S scale had no internal unity, but was made up of extraverted sociability items (liking to be with people) and of neurotic unsociability items (being afraid of being with people). Items correlating with the R and C scales were then incorporated into the respective scales, as were other items; in this way the MPI scales were built up by accretion, followed by several factor analyses of complete scales. The items of the E scale seemed to fall naturally into two major groups (primary factors), called sociability and impulsiveness; these two subfactors were found to be strongly intercorrelated (S. B. G. Eysenck & H. J. Eysenck, 1963; Sparrow & Ross, 1964).

Eaves and H. J. Eysenck (1975) returned to this problem in a study of 837 pairs of monozygotic and dizygotic twins who had been administered inventories of sociability and impulsiveness items. Their conclusions may be worth quoting:

1. Genetical factors contribute both to the variation and covariation of sociability and impulsiveness.
2. Environmental factors also contribute to the covariation of sociability and impulsiveness.
3. The genetical correlation between the two factors is estimated to be .42, the environmental correlation to be .66 after correction for unreliability.
4. Combining sociability and impulsiveness scores by addition to provide a measure of extraversion provides the most powerful single means of discriminating between individuals with respect to the genetical and environmental determinants of their responses to the sociability and impulsiveness items of the questionnaire.
5. The interaction between subjects and tests has a significant genetical component, so there is some justification for regarding sociability and impulsiveness as distinguished genetically. (p. 110)

It was also found that "the genetical and environmental determinants of variation are

homogeneous over sexes, suggesting that the effects of sex linkage and sex limitation are negligible" (p. 111). All genetical effects were additive, thus eliminating dominance, epistasis, and assortative mating as important causal factors.

These results, taken in conjunction with those mentioned earlier, seem to make the hypothesis of a "shotgun wedding" rather implausible. Guilford preferred two independent second-order factors, one of which, SA (Social Activity) is made up of his primary factors, G, A, and S (General Activity, Ascendance, and Sociability), whereas the other, which he called Introversion-Extraversion (IE), is made up of R and T, that is, Rhythymia and the reverse of reflectiveness (i.e., presumably impulsiveness in its widest sense). Our factor analyses (H. J. Eysenck & S. B. G. Eysenck, 1969) suggest that G, A, S, R, and T items all correlate together to define a single extraversion factor, the major components of which are Guilford's SA and IE, that is, Social Activity and his restricted notion of Introversion-Extraversion. Guilford gave no evidence that his SA and IE factors would be truly independent; it would be easy to sum the component scales and report the correlation. On the basis of our hypothesis this would be significant and positive, and uncorrelated with his E factor (Emotional Stability, similar to our Neuroticism). Guilford's last second-order factor, Pa (for Paranoid Disposition) bears some resemblance to our Psychoticism (P) factor (H. J. Eysenck & S. B. G. Eysenck, 1976), which we found to be independent of E and N. Guilford hypothesized that Pa would be correlated highly with E to form a super-factor EH (Emotional Health); our data do not support any such correlation, but again it should be easy to demonstrate the presence or absence of correlation between these groups of primaries by actual experiment. There are clear-cut differences in prediction here between Guilford's position and that taken by H. J. Eysenck and S. B. G. Eysenck (1976); no doubt these will be resolved by empirical test in due course. Such a test should not, of course, be restricted to college populations; the British work has mainly

been done with fairly random samples of the adult population.¹

Carrigan (1960) in her review of extraversion-introversion as a dimension of personality, left open the question of the unidimensionality of this trait: "The unidimensionality of extraversion-introversion has not been conclusively demonstrated" (p. 355). If by unidimensionality is meant something akin to Spearman's and Thurstone's notion of unit rank matrices, then clearly, E is not unidimensional in fact and was not conceived to be so in theory; E was conceived as a higher order factor emerging from the intercorrelations between primaries, very much as Spearman's *g* emerged as a second-order factor from the correlations between Thurstone's primaries (H. J. Eysenck, 1973). It appears that the forces holding together these primaries, in the case of extraversion, are both genetic and environmental and possess considerable strength; this is one essential argument in favor of regarding extraversion-introversion as a useful and biologically meaningful psychological construct.²

The mention of genetic factors leads us into the second point on which it is possible to disagree with Guilford, although in fact this disagreement is probably more with the implications of Guilford's omissions in his article than with his real attitudes toward the issue in question. Guilford dealt entirely with factor-analytic work in the paper; the position taken here is that this is scientifically undesirable and logically inadequate. To the present writer, the decision on whether a particular concept in the personality field is useful and compelling is based on four quite distinct criteria, only one of which is related to psychometrical argument in general and to factor analysis in particular. The first criterion, and admittedly a very important one, is, of course, that the concept in question should fulfill certain psychometric criteria; in other words, it should find support in adequate factor-analytic studies. The factors P, E, and N have found such support (H. J. Eysenck, 1970c; H. J. Eysenck & S. B. G. Eysenck, 1976), and it is noteworthy that a recent survey of all the replicable major factors in personality work came up with

precisely these three factors, variously named (Royce, 1973).³ This, however, I would regard as a necessary but not sufficient condition for acceptance. Further criteria are required.

The first of these is the genetic criterion. I feel that the major, most fundamental dimensions of personality are likely to be

¹ It should be specified that such analyses must include correlations between items, rather than simple correlations between scales. Guilford, Cattell, and Comrey all seem to prefer intercorrelating "parcels" of items, arriving at these parcels on the basis of some subjective judgment. This seems to be a dangerous practice; the virtue of factor analysis is precisely that it gets away from the old-fashioned reliance on face validity of items as measuring some preconceived trait or other. As a warning signal, we have already mentioned the Eysenck (1970a) paper, which demonstrated that the Guilford Factor S was not a unitary factor at all, but was split into two independent halves. It cannot be assumed that any factor that is not based on the intercorrelations between individual items has appropriate psychometric properties. Individual item responses are our basic data and all calculations must be based on objective combinations of such data, unadulterated by subjective judgments during the course of analysis.

² Guilford (1975) claimed that "the lack of cohesiveness of Eysenck's E . . . is serious because there have been so many studies using the score for Eysenck's E as a variable. When positive results are obtained in such experiments, the ambiguity of the E score leaves us with the feeling of uncertainty as to whether to attribute the effects to differences in R or in S, or in both" (p. 809). One could invert this criticism and state, in the most general form, that when a primary factor gives rise to positive correlations in experiments such as those mentioned by Guilford, then we are left with the feeling of uncertainty as to whether to attribute the effects to the variance specific to that primary, or to the variance it contributes to the higher order factor, that is, in this case, E. If we were to take Guilford's specific objection seriously, then we might ask whether, supposing we had followed his advice and used S scores to correlate with the particular dependent variable in the experiment, we would really be any better off, seeing that S itself has been shown to split up into two unrelated parts (Eysenck, 1970a). In the long run, this type of question cannot be answered along purely psychometric lines, but requires an answer in terms of general theoretical formulations about the nature of the factor in question and about experimental tests of deductions made from these formulations. This point is further discussed below.

³ Royce names the P factor by its opposite pole, "Superego."

those on which variation has had evolutionary significance, and that this evolutionary history is likely to manifest itself in strong genetic determination of individual differences along these dimensions. The new methods of biometrical genetics (Mather & Jinks, 1971) enable us to fit proper genetic models to the empirical data collected, rather than to manipulate monozygotic and dizygotic intraclass correlations in genetically meaningless ways, which was the practice even in the recent past (H. J. Eysenck, 1975); when this is done, there emerges very strong evidence for genetic models of P, E, and N, which emphasizes D_R and E_1 as the only major components, that is, additive genetic variance and within-family environmental variance emerge as the determinants of behavior differences along these dimensions (Eaves & H. J. Eysenck, 1975; H. J. Eysenck, 1975; H. J. Eysenck & S. B. G. Eysenck, 1976; Jinks and Fulker, 1970). Thus genetic criteria, in my view, must be added to psychometric criteria in order to make a dimension of personality acceptable as being fundamental in a biological sense.

A third requirement is theoretical understanding of the biological factors underlying individual differences along these dimensions, issuing in laboratory investigations designed to test specific predictions emanating from such theories. Such predictions may be direct or indirect. Thus the hypothesis linking introversion with high cortical arousal levels may be tested *directly* by means of electroencephalogram studies of arousal levels under a variety of experimental conditions (H. J. Eysenck, 1967; Gale, 1973; Strelau & Terelak, 1974), electrodermal reactivity (Coles, Gale, & Kline, 1971; Mangan & O'Gorman, 1969), or of pupil size and change of pupil size under stress (Stelmack & Mandelzys, 1975). *Indirect* deductions would lead, through the arousal postulate, to investigations of conditioning (H. J. Eysenck & Levey, 1972), verbal learning and remembering (M. W. Eysenck, 1974, 1975; Schwartz, 1975), the effects of "transmarginal inhibition" (P. M. J. Shigehisa, T. Shigehisa, & Symons, 1973; T. Shigehisa, 1974; T. Shigehisa & Symons, 1973; Zuckerman, Murtaugh, & Siegel, 1974),

and many others (H. J. Eysenck, 1967). Such theoretical and empirical links with general and experimental psychology are essential if a dimension of personality is to figure as part of our general psychological heritage.

A fourth requirement is some association between the dimensions of personality in question and important social events and groups. It is theoretically possible that no such association might exist, but if it does not, one might feel inclined to doubt the general importance of dimensions failing to show a connection of this kind. There is no need here to review the connections between P, E, and N, on the one hand, and neurotic and psychotic behavior, criminality, accident proneness, school and university performance, drug taking, sexual attitudes and behaviors, and many others; much of this material has been reviewed elsewhere (Eysenck, 1970b). These relations, in turn, require theoretical deduction from prior hypotheses relating to the nature and biological substructure of the factors in question; simple Baconian "shotgun" collection of correlational statistics is clearly unsatisfactory.

My answer to Guilford's (1975) rejection of extraversion as a meaningful dimension of personality is therefore fourfold. In the first place, the psychometric evidence for such a factor, conceived along the lines of the MPI and the EPI, is strong. In the second place, there is good genetic evidence for the existence of such a factor, combining elements of sociability, impulsiveness, and other primary traits. In the third place, there exists a clear-cut and explicit theory about the psychological and physiological nature of this factor, and deductions from this theory can be, and have been, experimentally verified in the laboratory. In the fourth place, this and the other major factors of personality may be said to have social implications and importance, in the sense that quite close associations are found between socially relevant behaviors and scores on these factors. I would rate psychometric considerations as equally important with the other three sets of considerations (genetic, hypothetico-deductive, social), but would not agree that any final decisions about

the scientific value of a theory could be made on the basis of psychometric considerations alone. Guilford would probably agree with this view; his article, in leaving out such nonpsychometric considerations, gives a wrong slant to arguments about the nature and meaning of factors and may prolong misunderstandings that have existed for a long time among experimental psychologists and personality theorists about the value of factor analysis and the admissibility of factor-analytic evidence in the general field of personality structure (Eysenck, 1970c).

It will be clear that so-called primary factors, such as those of Cattell, do not fulfill my four criteria, particularly the second and third. But even with respect to the psychometric criterion, as Guilford (1975) points out, there must be doubt. This doubt is based on two major considerations:

1. Cattell's factors are difficult or impossible to replicate. There are at least half a dozen large-scale, well-executed studies that have failed to reveal a factorial structure anything like that posited by Cattell (H. J. Eysenck, 1971, 1972); these studies usually give second-order factors closely resembling E and N, but not the primaries on which Cattell's system is mainly based. Cattell would probably argue that the methods of rotation used by these authors were sub-optimal, but this does not seem plausible; Promax, the method used in this respect by H. J. Eysenck and S. B. G. Eysenck (1969), was developed by former students of Cattell with the intention of giving an analytic rotation as closely related as possible to Cattell's principles (Hendrickson & White, 1964), and there is no obvious sign of arbitrariness or lack of competence in the other studies in question.

2. Cattell's primaries do not seem to measure anything specific over and above the second-order factors E and N. H. J. Eysenck (1971, 1972) has reanalyzed correlations between Cattell's primaries, correcting for attenuation, using Cattell's own data as presented in his handbook (Cattell, Eber, & Tatsuoka, 1970); Eysenck was able to show that these correlations, both for the five "anxiety" (neuroticism) scales and for the

five "exvia" (extraversion) scales, are very near unity. This leaves hardly any specificity for these scales, apart from their E and N variance. This is, of course, largely due to the low reliabilities of the scales, to which Guilford also draws attention. Cattell and Tsujioka (1964) have argued that low reliabilities may not only not be a defect in scale construction, but may actually be an advantage; their highly original argument has not been considered by Guilford, although it does appear to have merit. For this reason, I would not list low reliability as a special criticism of the Cattell scales; I agree with him that validity is a more important consideration.

My disagreement with Cattell does not extend to his superfactors. As H. J. Eysenck and S. B. G. Eysenck (1969) have pointed out, higher order factors show considerable agreement between the two models. As the same reference explains, I believe that the distinction between primaries and higher order or superfactors is useful, although the allocation of a factor to a particular order (first, second, third, etc.) is not. This distinction between primary and higher order factors is closely tied up with my distinction between T and C factors, that is, tautological factors (primary), which essentially resemble a Guttman scale, and complex factors (higher order), which combine several different primaries on the basis of their intercorrelations. Psychologically, tautological factors are of less interest than are complex factors, if only because logically the items in T factors are connected through similarity; the discovery of such a factor is hardly surprising, and any number of such factors could be artificially created by simply rewriting a particular questionnaire item in several slightly different ways. C factors are not as predictable on commonsense grounds and are therefore much more interesting psychologically. Cattell has tried to get away from the simple cluster-analysis method of deriving primaries by stressing rotation based on hyperplanes; hence his primaries are less T and more C. It is possible that it is because of this feature of his analyses that they are so difficult to replicate.

It may be concluded that psychometric considerations are not the only ones that determine the judgment of the scientific merit of a personality factor or scale; that genetic, theoretical-experimental, and social criteria are, at least, equally important; and that the only factors that have hitherto satisfied to any reasonable degree these four requirements are the higher order factors, which in our system have been labeled P, E, and N, although they also go in many other guises in the writings of others. It is unlikely that these will turn out to be the only factors in the personality field to fulfill these criteria, but as matters stand, it is difficult to think of any others for which an equally impressive body of evidence is available.

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Erratum to Doleys

In the article, "Behavioral Treatments for Nocturnal Enuresis in Children: A Review of the Literature," by Daniel M. Doleys (*Psychological Bulletin*, 1977, Vol. 84, No. 1, pp. 30-54), it was noted on page 38 that Schwartz, Colligan, and O'Connell (1972) reported a 46% failure rate in their use of the urine alarm. This percentage figure represents a misinterpretation of the figures in the original article. In fact, only 1 of the 14 patients who completed the treatment program failed to achieve the dryness criteria.