

DIMENSIONS OF PERSONALITY: 16, 5 OR 3?—CRITERIA FOR A TAXONOMIC PARADIGM

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Summary—It is suggested that what is needed in personality research is a paradigm which would give rise to coordinated research work ("normal science") directed in part at the elimination or clarification of anomalies. Several such paradigms have in fact been suggested, so that clearly what is needed is a set of criteria to decide between them. Several such criteria are put forward, and an attempt is made to apply them to three major systems, namely Cattell's 16 PF, the Norman "big 5", and the Eysenck PEN system. It is suggested that if agreement could be reached on these criteria, we might be able to approach agreement on the substantive issue of what are the major dimensions of personality.

INTRODUCTION

The nature of the paradigm

It is widely agreed that, as Kuhn (1970) has suggested, paradigms are largely absent in the social sciences (Barnes, 1982). It is true that, as Masterman (1970) has stated, the term "paradigm" has too many meanings and connotations to be unambiguous, but Kuhn (1974) has made the term more precise and sufficiently intelligible to be potentially useful. He distinguishes two major components of a paradigm: exemplars and disciplinary matrices. The former are conceptualized as concrete problem solutions, accepted by the disciplinary group as symbolic generalizations to its problems. A disciplinary matrix is "the common possession of a professional discipline", containing symbolic generalizations, models and exemplars (Kuhn, 1974, p. 463). As Suppe (1974) rephrases this, "The disciplinary matrix contains all those shared elements which make for relative fullness of professional communication and unanimity of professional judgement" (p. 495).

In less philosophic language, and possibly omitting important elements of Kuhn's concept, I would for the purpose of this article define a paradigm as a theoretical model shared by most workers in a given field, encompassing agreed methods of investigation, standards of proof and disproof, and procedures of experimental research. Such models and methods are outlined in current textbooks, are taught to students at universities, and constitute the basis for empirical investigations. Paradigms always contain anomalies, and a major part of scientific endeavour ("normal science") is to solve the problems constituted by anomalies in terms of the model itself. Where it proves finally impossible, and where anomalies accumulate, revolutions occur which advance a different paradigm (Krige, 1980).

Barnes (1982) asks how the acceptance of a paradigm can indicate problems for research, and how the paradigm itself can actually serve as a resource for a scientist. The answer, he replies, lies in the perceived inadequacy of a paradigm as it is initially formulated and accepted, in its crudity, its unsatisfactory predictive power, and its limited scope. In agreeing upon a paradigm, he points out, scientists do not accept a finished product; rather, they agree to accept it as a basis for future work, and to treat as illusory or eliminable all its apparent inadequacies and defects. Paradigms require to be refined and elaborated in normal science, and they are used in the development of further problem-solutions, thus extending the scope of scientific competences and procedures.

It hardly needs explicit and detailed argument to show that in personality research such a paradigm is absent. Consider as a typical textbook Hall and Lindzey's (1985) "Introduction to Theories of Personality". It lists 22 theorists putting forth 20 major theories and faithfully chronicles their extremely divergent views. There is no agreement on definitions, models, methods, results or indeed anything whatever; all is confusion, with no effort used to sort the chaff from the grain. The result, as far as the student is concerned, is simply a Dutch auction. No criteria are offered for judging the truth-value of given statements, no attempt is made to eliminate theories

which definitely contradict established facts, and there is no effort at integration; what we are given is a gigantic table setting out in diagrammatic form the major differences between theorists on nine major dimensions. No wonder that "hard" scientists do not take psychology seriously as a science, and refuse to accept its "findings" as being worthy of serious consideration.

In spite of all these considerations, I ventured to ask the question: "Is there a paradigm in personality research?", and to answer it in the positive (Eysenck, 1983a). This positive answer is based on the assumption that we are attempting to develop a natural science theory of personality, and that this assumption automatically eliminates many of the "theories" listed by Hall and Lindzey because they cannot by any stretch of the imagination be classified as falling within the class of natural science theories. As an example I would class among the inadmissible theories those of Freud, Adler, Jung, Binswanger, Horney, Sullivan, Fromm, Erikson and Maslow. They fail essentially because for the most part they do not generate testable deductions; because where they do so the deductions have most frequently been falsified; and because they fail to include practically all the experimental and empirical studies which have been done over the past 50 years. Historically these theorists have had some influence, but their theorizing and their mode of working has not been in the tradition of natural science, and they have not been found responsive to adverse criticism or empirical disproof. One does not have to share Popper's belief in falsifiability being the criterion of science, as opposed to pseudoscience (Popper, 1935) to realize that most of these theories are of a nontestable kind, or at least form part of a degenerating programme shift (Lakatos and Musgrave, 1970). For a more detailed critique of the type of approach characteristic of the writers named above, the reader is referred to Gruenbaum (1984), Zwang (1985), Eysenck (1985b), Eysenck and Wilson (1973).

It is not being suggested that hermeneutic, existentialist or idiographic approaches may not be useful and valuable, merely that they do not fit the modern model of natural science approach, in that they do not present a logically coherent, testable theory which has experimental or at least empirical backing. In this connection it is important to note that acceptable scientific theories in the early stages require positive support from such theory-testing procedures; it is only in its later stages that Popperian falsification is appropriate (Root-Bernstein, 1989). No such early empirical support is forthcoming for these essentially subjective approaches. Thus our first criteria for a proper theory of personality must be: (1) The theory covers a wide field, and is not restricted to a narrow segment of personality research. If it is so restricted (e.g. Maslow, Adler) it may become part of a larger, more inclusive theory in due course, but it cannot count as a theory of personality. (2) The theory is formulated in a logically consistent fashion, so that testable deductions can be made from it. Such deductions should (a) explain known phenomena as well as, or better than other theories, (b) predict as yet unknown phenomena, and (c) it should not be contradicted by well-established data.

It is of course realized that facts are more difficult to define and pin down than used to be thought; that facts are always linked with theory; and that prediction and postdiction are crucially dependent on relevant and possibly uncontrolled conditions affecting the experiment (Suppe, 1974; Gholson, Shadish, Neameyer & Houts, 1989). Nevertheless there is a very clear boundary between natural science, on the one hand, and hermeneutic speculations on the other, and what is being suggested is simply that debates concerning their relative worth are philosophical in nature, and do not admit of scientific resolution. It seems best to recognize their incompatibility and attempt to build up a natural science of personality, as far as that is in fact possible. Our concern here will be entirely with attempts to achieve this aim.

Personality: the problem of taxonomy

In any science, taxonomy precedes causal analysis; we must analyse and classify the entities in our field of study before we can frame meaningful theories concerning their behaviour. The astronomer classifies stars, the chemist elements, the zoologist animals, the botanist plants; the student of individual differences must do likewise. Taxonomy may of course be helped enormously by the results of empirical study, themselves based on a less-than-perfect taxonomy; there is a mutual interaction between these two sides of the scientific enterprise, to the advantage of both (Sokal and Sneath, 1963). They define classification as the ordering of organisms into groups or sets on the basis of their relationships, that is, of their association by contiguity, similarity, or both.

Eysenck and Eysenck (1969) have discussed at some length the application of the principles of numerical taxonomy to the study of personality, and they point out that the procedures ordinarily employed by the biologist are precisely those employed by psychologists attempting to lay the foundations of a scientific taxonomy.

How in fact does a biologist proceed? Sneath (1964) has set the procedures out according to the following four steps. (1) The organisms are chosen, and their characters are recorded in a table. (2) Each organism is compared with every other and their overall resemblance is estimated as indicated by all the characters. This yields a new table, a table of similarities. (3) The organisms are now sorted into groups on the basis of their mutual similarities. Like organisms are brought next to like, and separated from unlike, and these groups or phenons are taken to represent the 'natural' taxonomic groups whose relationships can be represented in numerical form. (4) The characters can now be re-examined to find those that are most constant within the groups that have emerged from the analysis. These can be used as diagnostic characters in keys for identifying specimens. Sokal and Sneath (1963) discuss in great detail the many theoretical problems that arise as well as the mathematical formulae useful in the estimation of taxonomic resemblances. Much of what they have to say is of great value and importance for psychology as well as for botany and zoology, although of course a number of problems are specific to each of these different sciences.

In the case of personality study the organisms concerned are human beings, preferably randomly chosen, or with sex, age and other restrictions; the characters are traits, measured by experiment, by rating, by self-rating, or in some other way (e.g. projective test, miniature situations, etc.). We can correlate traits over subjects, or subjects over traits, giving us groups of people showing similarity over traits, or groups of traits, cohering as factors over people. We can then look at the traits (or people) having the highest factor loadings in order to better identify the trait clusters.

The use of traits to characterize individuals has of course come under criticism (e.g. Mischel, 1968), but these are not well conceived and of doubtful relevance to actual research findings (Eysenck & Eysenck, 1980). Buss (1989) has put the situation very clearly: "If there is to be a specialty called personality, its unique and therefore defining characteristic is traits" (p. 1378). This agrees perfectly with the position I have consistently adopted (Eysenck, 1947; Eysenck & Eysenck, 1985), and I will not here argue the case beyond Buss's excellent advocacy. Let us merely note as our next criterion for a paradigmatic theory of personality: (3) Any theory of personality must deal with the definition, origin, and interrelation of traits of human personality.

The definition and measurement of traits in terms of consistency of behaviour across carefully limited and defined areas presents no difficulties in principle, although in practice difficulties do arise, as we shall see. Here let us merely note that traits often correlate with each other; as Vernon (1938) has pointed out, in the early days of measurement, inventories labelled Introversion correlated as highly with inventories labelled Neuroticism as with other inventories labelled Introversion, and vice versa (Eysenck, 1970a), Factor analysis has improved the situation, as has clearer theorizing, but the problem of naming factors is of course still with us.

However that may be, clearly the correlations between traits so frequently observed in empirical studies demand a more complex model, and it seems clear (Criterion 4) that only some form of a hierarchical model would fit the facts (Eysenck, 1947). The theory there developed was an early forerunner of Buss's (1989) act frequency approach. At the lowest level (level 1) we have isolated acts, such as initiating talking behaviour with a stranger. At level 2 we find that this behaviour is habitual, i.e. it is repeated over time (high reliability). If this habitual behaviour is found to correlate with other habitual behaviours, such as liking to be with other people, going to parties frequently, etc., we have the basis for postulating a trait of sociability; this constitutes level 3. And if we find that sociability correlates with other traits like physical activity, outgoingness, changeableness, responsiveness, liveliness, etc., we have the basis for a dimension of personality, a level 4 concept, a "type", or a higher-order factor (Eysenck, 1947). Some such hierarchical concept seems to underlie most if not all traits systems of personality description, and hence we may perhaps state as our fourth criterion for a paradigmatic model of personality; (4) Person-significant behaviours are organized in a hierarchical fashion, from the most specific (level 1) to the most general (level 4).

In the next section we will turn to the problem of deciding between different models of this hierarchy; here let us merely note some advantages and some disadvantages of the model. Among

the advantages are the following. (1) The model incorporates the distinction between states and traits, originally developed by Cicero over 2000 years ago (Eysenck, 1983b). The lower the level, the more it is a measure of state; the higher the level, the more it is a measure of trait. (2) The model incorporates the principles of aggregation (Rushton, Brainerd & Pressley, 1983). Level 2 concepts are aggregation-based on concepts from level 1. Level 3 concepts are aggregates of common elements from many level 2 concepts. Level 4 concepts are aggregates of common elements from many level 3 concepts. Hence level 4 concepts are the most far-reaching, aggregating behaviours characteristic of a given person over many years. (3) The model is empirically testable, and has indeed withstood a large number of such tests (Royce & Powell, 1983); it would be difficult to think of another model which as closely resembles the natural state of affairs.

Now for the problems. While it is easy to distinguish level 3 (trait) concepts from level 4 (type concepts, or dimensions of personality), there are in fact intermediate levels which pose a difficult question. Consider sensation seeking behaviour (Zuckerman, 1979, 1983). Is sensation seeking a trait, located at level 3? But factor analysis clearly demonstrates that it is not unitary, but made up of 4 traits only correlating around 0.3 with each other—TAS (thrill and adventure seeking), ES (experience seeking), Dis (disinhibition) and BS (boredom susceptibility). Is sensation seeking at the 4 level? Hardly; it correlates quite well with Psychoticism and Extraversion, with the 4 subfactors showing rather different patterns. Should we classify these 4 subfactors in terms of their position in the 3-dimensional system created by the 3 dimensions of the Eysenck system (Eysenck & Eysenck, 1985)?

Much of the same problem arises with respect to the trait of impulsiveness (Eysenck & Eysenck, 1977). This too breaks down into 4 factors: risk-taking, nonplanning, liveliness and narrow impulsivity. These factors are replicable from sample to sample, correlate only about 0.3 together, and correlate somewhat differently with P and E, as well as N. Thus, like sensation seeking, impulsiveness is a hybrid, neither clearly a trait nor clearly a type concept, lying in the hierarchical model rather uneasily between level 3 and level 4. Cattell has frequently drawn attention to the problem in factor analysis when different hierarchical levels are present in the same analysis, i.e. when 1st and 2nd order concepts are being intercorrelated (Cattell & Scheier, 1961). The problem becomes even less manageable when we have scales properly situated between levels! The problems is discussed more extensively in Eysenck and Eysenck (1985); it is typical of the kind of problem which must be left to normal science to solve.

The problem of choice

Given that our paradigm requires a hierarchical model of empirically measured traits, we have to consider the problem of choice, given that there are many greatly overlapping models which offer themselves. What has been said so far may suffice to eliminate a number of purely heuristic constructs, such as the MMPI (Friedman, Webb & Lewark, 1989), the CPI (Gough, 1957), or the Comrey Personality Scales (Comrey, 1980). These contain ad hoc scales for arbitrarily chosen traits, without any personality theory in mind; at best they have reasonable psychometric parentage (Gough and Comrey), at worst their psychometric properties are known to be very poor (MMPI—see discussion in Eysenck & Eysenck, 1985). Thus when factor analysed the scales of the MMPI fail to appear as hypothesized, items correlate better with scales they do not belong to than with their proper scales, etc. It is perhaps significant that the personality questionnaire more widely used than any other should violate all the rules laid down by psychometrists for the construction of such instruments; that it should be based on no recognizable or clearly stated theory of personality; and that the resulting scales should be interpreted in terms of highly subjective and scientifically meaningless psychiatric categories. (These categories are meaningless, because the evidence suggests strongly that psychiatric disorders form a dimensional, not a categorical system—Eysenck, 1950, 1970b.) In addition, of course, these alleged psychiatric categories are diagnosed with extreme unreliability!

It is not necessarily suggested that inventories like those criticized should not be used in research. As Eysenck & Eysenck (1985) have shown, most if not all widely used inventories share enough item content to cover much the same area. Thus, Gentry, Wakefield and Friedman (1985) have shown that by suitable selection of items from the MMPI, good measures of the 3 major dimensions of personality (P, E and N) may be obtained. What is suggested is simply that these inventories

are not based on any theory of personality, and that while their intercorrelations may be relevant to such a theory, they need not be considered in our attempt to find a paradigmatic theory.

It would appear there are three major candidates for such a position, namely Cattell's 16 PF theory (Cattell, Eber & Tatsuoka, 1970), the "Norman Five" (Norman, 1963), and the Eysenck PEN system (Eysenck & Eysenck, 1985). There are of course other claimants (e.g. Cloninger, 1986), but there is far too little empirical support to make their claims properly testable; any assessment must await further data.

Cattell's system is the oldest, and he deserves much credit for his pioneering effort in constructing a "personality system", in an attempt to cover the whole ground of personality—collecting relevant linguistic terms, and then proceeding to use the most advanced methods of statistical analysis available to construct his list of 16 major personality factors. He also analysed their intercorrelations, coming up with higher order factors such as anxiety, and *exvia-invia*. These closely resemble Eysenck's neuroticism and Extraversion concepts, and overall there is much similarity at the highest-order level. But as it stands, Cattell's system will not do; there are too many criticisms, too many failures to replicate, too many psychometric faults to continue to use the system (Eysenck, 1972), or regard the 16 personality factors suggested as constituting the long-sought paradigm of personality psychology.

Clearly replicability of outcome (Criterion 5) is an absolutely essential requirement of any suggested system of personality description, and failure to replicate on any large scale must rule out that system from consideration. Among clear-cut failures to find anything like Cattell's 16 factors in analyses of item intercorrelations, we may note the following: Becker (1961), Sells, Demares and Will (1970), Levonian (1962a, b), Peterson (1966), Timm (1968), Eysenck, White and Soueif (1969), Rican, Heinzl and Janouskova (1970), Greif (1970), Howarth and Browne (1971), Howarth, Browne and Marceau (1971), Howarth (1976), and Digman and Takemoto-Chock (1981). Several of these authors had been trained by Cattell, and were thoroughly familiar with his methods and procedures, yet they too failed to find a solution in any way similar to his. It may be argued that until fairly recently methods of assessment for number of factors and their oblique relation were somewhat primitive; hence it may be useful to concentrate on more recent and technically superior studies. Three of these will be examined.

The first study to be reviewed is by Barrett and Kline (1982b). They carried out an extensive series of analyses on a sample of data from 491 undergraduate students who had completed Form A of Cattell's 16 PF questionnaire. The data were factor analysed using both principal component and image analyses, and radial parcelling. They conclude that their extensive factorial study "has not yielded 16 factor scales. Rather, the results have suggested that between seven and nine factors will have both satisfactory coefficient alphas and factor validities" (p. 269). They also conclude that "all the 16 PF Form A scales are of little or no practical value" (p. 364). Having also tested Saville and Blinkhorn's (1976, 1981) alternate forms, they conclude that their reliabilities were no better.

The second study to be reviewed is by McKenzie (1988b). The initial data consisted of responses to the 16 PF and EPQ, obtained at the same testing, from 239 Independent Study Students. The responses to the 16 PF were subjected to a principal components analysis. A Scree test applied to the original solution indicated that between 8 and 10 factors should be retained for rotation. As this was at variance with Cattell's postulated factor structure supplementary analyses involving the retention and rotation of from 16 to 22 factors and item-factor analyses of groups of scales were carried out. All of these analyses failed to reproduce anything like a clear 16-scale structure. In order to accommodate the hypothesis that a 3-factor solution, in line with Eysenck's postulated dimensions of P, E and N, underpinned the Cattell scales and to test the validity of the intermediate solutions, 8, 7, 6, 5, 4 and 3 factors were retained and subjected to varimax and promax rotations. These solutions were compared with solutions obtained from analyses of the responses to the 16 PF of 200 students at the Independent Assessment and Research Centre. Only the 3-factor solutions showed a degree of replicability across samples. The samples were combined to form a total sample of 439 cases and this was tested for factor replicability using factor comparability coefficients based on factor scores. This revealed a clear 3-factor structure, of anxiety, *superego* and *exvia*, which was replicable across sexes. An analysis of the responses to the combined questionnaires indicated that the neuroticism and anxiety factors and the extraversion and *exvia* factors were coincident. A 3-step

analysis, in which pairs and triads of scales were tested for factor replicability, confirmed the equivalence of the neuroticism and anxiety factors and the extraversion and exvia factors.

McKenzie found that the Superego factor largely overlapped with Eysenck's Psychoticism, as expected, but lacked the cruelty or sadism elements of the latter. This may be due to one of two possible causes. It is possible that Superego in fact does not relate negatively to acts of cruelty and sadism, or it may be that these are simply missing from Cattell's scale, and hence fail to manifest themselves. The former hypothesis seems counter-intuitive; how can socially objectionable acts not be opposed by a person's superego? Clearly the matter requires a separate study. What is clear is that "only the 3-factor solutions provide an obvious degree of replicability across samples" (p. 848). There was no way in which the Cattell factors could be recovered, in spite of many trials.

The third study to be reviewed (Matthews, 1989) came to a similar conclusion. Cattell's 16 PF questionnaire was factor analysed in a sample of 410 British students. The number of factors to be extracted was determined by methods previously validated against artificial data sets: the MAP, PA and VSS procedures. Twelve primary factors were extracted from the inter-item correlation matrix. These factors appeared to be related to dimensions of personality identified in other research, but did not resemble the Cattell primary factors. Scales corresponding to the factors gave higher internal consistency and factor validity values than those reported for the Cattell primaries. Three factors were extracted from the inter-scale correlation matrix, which were labelled anxiety/neuroticism, extraversion and socialisation, a result consistent with previous research.

Here again we find that the 16 PF has been overfactored by Cattell (1973), and that the factors that do emerge bear little relation to the 16 PF, all efforts to the contrary notwithstanding. Three factors emerge at the higher order level, clearly representing E and N, and partly P. Again the third factor is one of socialization-superego, with the cruelty-sadistic aspect missing because it is not contained in Cattell's trait selection. Clearly, the most competent and recent, as well as the great majority of earlier analyses, agree that Cattell's factors cannot be recovered from analysis of intercorrelations between his items; that a smaller number of factors differing considerably in content and meaning from his emerges; and that at the second order level there are 3 clear-cut factors, replicable across samples, and identifiable as E, N and (largely) P.

Boyle (1986) has contributed to this argument in an analysis of the 16 PF, the Eight State Battery and the Objective Motivation Analysis Test of Cattell; he discovered 6 superfactors, of which 2 clearly identified E and N, and one (tentatively) superego-P. His wide-ranging analysis thus agree well with those listed above.

Of particular interest is one further study of the 16 PF, not carried out on the item level, but at the scale level. Krug and Johns (1986), using a very large and representative sample of 17,381 normal males and females, factored the 16 PF, and found essentially 5 major factors (Factor 6 had just one loading on Shrewdness, and Factor 7 was identified with general ability). Extraversion and Neuroticism were the two most clearly marked factors, with Tough Poise, Independence and (lack of) control quite highly intercorrelated to form a combination suggesting P, but again a P lacking the aggressive-sadistic elements. Krug and Johns argue that "the 'big five'—Extraversion, Anxiety, Tough Poise, Independence and Control are clearly identifiable in the present data" (p. 691). The use of the term "the big five" for these factors is clearly a challenge to adherents of the Norman "big five"—it would need a lot of interpretive ingenuity to discover any sort of identity between "Tough Poise, Independence and Control", on the one hand, and Agreeableness, Conscientiousness and Openness to Experience, on the other. The most likely overall solution to the 16 PF superstructure, in the light of all these studies, is a higher order 3-factor configuration of E, N and a partial P.

This summary of studies both current and recent is in strong contrast to a claim by Krug and Johns (1986) that there are 34 separate studies concerned with the reliability and validity of the primary factor model, involving approximately 25,000 subjects sampled across age, education, socioeconomic levels and cultures. (See also Krug & Cattell, 1986.) In none of these studies did the number of factors indicated on the nature of the resulting simple-structure solutions depart significantly from the personality model which forms the theoretical basis for the 16 PF" (p. 684). A detailed discussion of these studies cannot be undertaken here, but they all show a rather unusual and primitive choice of factoring method, and very subjective criteria for rotations. Their outcome stands in stark contrast to the best and most modern studies available at the present time.

One additional question remains to be asked, namely the degree to which the content of the lower order factor adds to the contribution made by E, N and P (or superego-socialization). Saville and Blinkhorn (1976, 1981) have answered this question in the affirmative, and in principle this must surely be so, although whether this added variance predicts important social, theoretical or practical aspects of behaviour is still a moot question.

It is noteworthy that this question has been so seldom raised, although it is crucial to a rational discussion of the claims of Cattell and others for the primacy of first-order factors. Reynolds and Nichols (1977), having demonstrated that a factor analysis of the Gough (1957) California Psychological Inventory resulted in two major factors similar to N and E, formally asked: "How well (do) the two factor scales represent the information in the 18 scales CPI profile . . . is the useful information in the scales contained mainly in this common factor portion of the variance, or is it contained mainly in the unique portion of the variance?" (p. 908). Their interesting and important far-ranging study showed that for 178 variables predicted or postdicted, "for the most part, the factor scales do seem to capture the valid variance in the CPI scales. In many instances a common factor portion of the scale's variance was actually more predictive of relevant criteria than was the total scale variance" (p. 914). The "obvious" answer that many primaries must be better in predicting important social consequences than few secondaries may be mistaken; the problem certainly deserves more study than it has received hitherto.

Curiously enough, even Cattell, who had always preached the supremacy for predictions of primary factor scales, has in practice abandoned this approach in favour of using combinations of 4 secondary factors curiously reminiscent of P, E, N and L. Thus Krug (1981), officially representing the views of Cattell's Institute for Personality and Ability Testing, argued that "second-order factors represent a condensation of information conveyed by the 16 primaries. This loss need not be critical for certain purposes; specifically, for developing a practical framework for analysing profile patterns. Operationally, the analysis restricted itself to just the first four second-order factors—extraversion, anxiety, tough poise, and independence—each of which corresponds to important, well-established constructs in personality theory, extending far beyond the 16 PF literature" (p. 13). Thus for all practical purposes the 16 primaries are ignored, and use is made only of the 4 second-order factors as being sufficient to predict independent social variables.

When we put the same question of replicability to the Eysenck P, E and N system, we received quite a different answer. Some early and rather inept attempts to recover these 3 factors were only partly successful (e.g. Lor, 1979; Helmes, 1980; Goh, King & King, 1982), but their psychometric faults have been pointed out by Barrett and Kline (1982a) and O'Gorman and Hattie (1986). The same authors, in a study of their own, found complete replicability for E, N and L in all their samples, but for P only in the large Gallup sample of 600 English male and 598 female adults; smaller samples of male and female students only allowed partial replication. The replication of E, N and L in this study follows earlier replication of these factors in the EPI (Eysenck & Eysenck, 1965), which showed a great deal of agreement (e.g. Green & Walkey, 1980; Walkey & Green, 1981, 1990; Walkey, Green & McCormick, 1986; McCormick, Green & Walkey, 1987; Walkey, Stumpf & Green, 1988) after earlier, psychometrically less satisfactory solutions (Howarth, 1972, 1976; Howarth & Browne, 1972; Oswald & Velicer, 1980). The absence of P in the EPI, as compared with the EPPQ, should make the factorial solution easier, but Walkey and his colleagues have shown that psychometric thinking of some complexity is required to produce acceptable results.

To return to the analysis of the EPQ, O'Gorman and Hattie (1986) on a sample of 426 undergraduate volunteers found that their results were "in close agreement with those of Eysenck *et al.* (1980) and Barrett and Kline (1982a), using different techniques of analysis" (p. 897). Note that these confirmatory results were obtained in a different country and culture to that in which the original work was done. Another replication study, using both the EPQ and the 16 PF on an Egyptian sample Abdel-Khalek, Ibrahim and Budek (1986) "showed three factors, i.e. N and E in addition to psychoticism (P)" (p. 65). The question of replicability in many different cultures will be dealt with in more detail presently.

Perhaps the technically most sophisticated analysis of the EPQ items is contained in a study by McKenzie (1988a). He concluded that "the analyses provide conclusive confirmation that

Eysenck's 4 factors, of P, E, N and L are real, reliable and replicable across populations and sexes, (and) that they can be located at the first order" (p. 809).

He also found, in an examination of the factor loadings, that "*the 4-factor solution meets the criterion of psychological interpretability as the items comprising the respective factors are totally consonant with the constructs of E, M, N, L and P*" (p. 808; italics in original).

Finally, a latent trait analysis of the EPQ is available (Grayson, 1986), which uses yet another psychometric type of analysis to uncover the dimensionality of the EPQ. Grayson concludes his analysis by saying that "the present data support the view that all four scales are predominantly unidimensional, with occasional pairs of items intercorrelating more than expected. Analysis of such item pairs clearly shows that this is due to surface similarity in their content beyond that determined by the basic dimension" (p. 233). Altogether, Grayson's approach presents an interesting extension to the more usual types of analyses.

Summarizing the factor analytic studies done on the 16 PF and the PEN system, we may say that earlier, psychometrically rather primitive work suggested, and later more sophisticated work confirmed, that (1) Cattell's 16 factors are not replicable; (2) Eysenck's P, E, N and L factors are replicable; (3) Cattell's scale generates 3 major factors closely resembling E and N, and in large part P. Thus on the criteria so far suggested, we would have to agree on the prominence of P, E and N (with L regarded either as a lie scale, or under circumstances not likely to evoke dissimulation, as a measure of conformity negatively correlated with P—Michaelis and Eysenck, 1971). This conclusion is strengthened by two further recent studies which extended the model by (1) the use of nonmetric multidimensional scaling techniques (Hammond, 1987), and (2) by extending factor analytic replication to the EPQ-R (Eysenck, Eysenck & Barrett, 1985).

The Hammond (1987) study involved the smallest space analysis (Lingoes, 1973), and a comparison of the results with factor analytic solutions. Figure 1 shows the results of the smallest space analysis in diagrammatic form. Only 3 items are misclassified, none badly, and Hammon argues for these positions being psychologically meaningful. (His 3-scale solution, leaving out L items, has only 1 misclassification.) The 4-scale solution agrees well (by alienation coefficient) with the factorial solution originally presented by Goh *et al.* (1982), and thought by them to disprove the Eysenck hypothesis. As Hammond points out, "the study . . . supports the contention of Barrett & Kline (1982a) that failure to replicate the Eysencks' factor structure may be largely explained by methodological weaknesses" (p. 548).

The Corulla (1987) study attempted to replicate the improved EPQ-R version of the scale (Eysenck, Eysenck & Barrett, 1985). His analysis also included items from the Impulsivity-

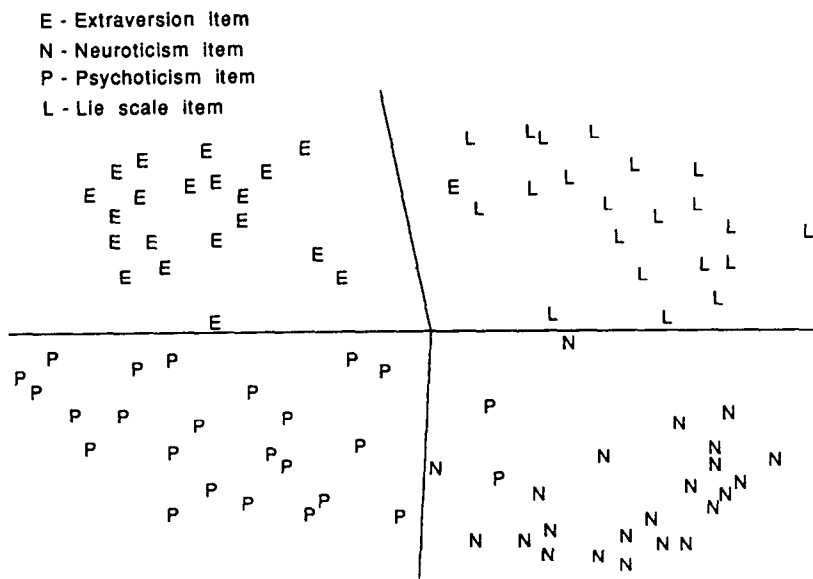


Fig. 1. Multi-dimensional scaling (smallest space analysis) of Irish replication of British studies of the PENL system (from Hammond, 1987).

Venturesomeness–Empathy scale (Eysenck & Eysenck, 1978). He concluded that the revised scale was psychometrically superior to the older scale, and that “item factor analysis reveals the presence of many factors which generally correspond to the primaries which make up the superfactors described by Eysenck and Eysenck (S.B.G.) (1976) and by Eysenck and Eysenck (M.W.) (1985)” (p. 658). He further states that “it seems worthwhile to point out that the Alpha factoring, Principal Axis factoring, Image factoring, Unweighted Least Squares, Generalized Least Squares and Maximum Likelihood methods of factor analysis have all extracted three factors that are identifiable in terms of P, E and N” (p. 657). Thus the meticulous analysis leaves our main conclusions unaltered.

We must now turn to the third model, namely the “big five” or the “Norman five” model (John, 1990; Wiggins & Trapnell, 1990). The history and present status of this model is well described, although uncritically, by these authors, and hence a brief recapitulation will suffice, with some bringing up-to-date. The approach adopted by proponents of this system has certain similarities with that of Cattell (1943), who started with a complete list of trait names, reduced the list by omitting synonyms, grouping together traits very similar, and finally factor analysing the remaining traits, correlated over persons. Goldberg (1982) also began with a reduced list from a previous dictionary search, reduced this list markedly, structured the taxonomy according to specified rules, and generated a large number of categories which mapped into higher-order dimensions. The taxonomy finally arrived at was thus evaluated by several different criteria.

The development of a 5-factor solution can be traced through early studies by Thurstone (1934), Fiske (1949), Tupes and Christal (1958, 1961) and Norman (1963, 1967) who is often cited as the father of the “Norman five”. In more recent years, advocacy has centred largely on the work of Goldberg (1981, 1982, 1986); Digman (1989), and Costa and McCrea (1988) and McCrae and Costa (1987, 1989), with a German contribution by Borkenau and Ostendorf (1989, 1990), and by Ostendorf (1990) in a recently published book which contains a good overview of the field.

Actually the original “big five” model, which contained the traits of extraversion, agreeableness, conscientiousness, neuroticism and culture, has been somewhat transformed by McCrea and Costa (1987) who substituted “openness to experience” for “culture”, a substitution calmly accepted by all concerned although the two concepts seem to have little in common. Interpretation of factors does of course tend to be subjective, as is their naming, but this substitution would seem to deserve more empirical justification than it has received; Wiggins and Trapnell (1990) in their summary chapter hardly mention it, and certainly don’t offer any justification.

Angleitner (1990) has reported on a German language study showing that the fifth factor (culture or openness) was in fact not in the temperament field but was concerned rather with ability; the separate factorization of the temperament and character terms revealed only a four-factor solution in which no culture, openness or intellect factor appeared. Thus looked at in terms of personality or temperament, i.e. excluding specifically cognitive abilities which of course make a separate appearance in the Cattell and Eysenck schemes, we have only a “big four” as compared with Eysenck’s 3-factor model. As suggested below, if we regard agreeableness and conscientiousness as primary factors related (negatively) to Psychoticism, agreement is complete.

While a number of studies converge on 5 factors resembling the “big five”, other studies aimed at replication have conspicuously failed to find such confirmation. Livneh and Livneh (1989), for instance, used the Gough and Heilbrun Adjective Check List (1965) to study the cross-measure validity of the five-factor model. “Results of the factorial ACL adjective scales . . . failed to confirm the structure of the five-factor model. Contrary to expectation, none of the hypothesized sets of ACL marker scales converged to form any of the suggested five personality factors” (p. 75).

A study by Brand and Egan (1987), also devoted to an attempt at replication, failed to support the model. “A new ipsative checklist was completed by 160 young adults of above-average educational attainment. When 47 principal components having eigenvalues greater than unity were rotated, the first six Varimax factors recovered from the inter-adjectival correlations were interpreted as variants of the “Big Five” personality dimensions (Extraversion, Autonomy, Neuroticism, Conscientiousness and Tender-mindedness) plus Intelligence. Visual scree test, however, suggested the extraction of 12 components, and Varimax rotation of these yielded possible equivalents of Eysenck’s Psychoticism, Extraversion and Neuroticism as the three chief factors beyond which factor-definition was markedly less feasible. Both the number and the nature of

major, recognizable personality dimensions depended in this study on how such specificity was retained for factorial rotation: retaining more specificity seemed to countenance dimensional differentiation such as that between 'fluid' and 'crystallized' intelligence" (p. 1165).

We see in this study very clearly a problem with the five-factor model which is not usually discussed by its proponents, but which has always been emphasized by Cattell, namely the danger of mixing up first-order and second-order factors. (Not doing so might be regarded as our 6th criterion.) Brand and Egan find 12 first-order factors, including the "Big Five"; however, at the second order level only P, E and N are found. This suggests that Agreeableness, Openness and Conscientiousness may be first-order factors or traits, mixed up with second-order factors like E and N, and possibly constituting partial aspects of P.

Support for this view is given in a paper by McCrae and Costa (1985), in which they found correlations between P and Agreeableness of -0.45 , and P and Conscientiousness of -0.31 . This does suggest that self-report measures of Agreeableness and Conscientiousness are in fact traits in part defining P, and constituting 2 of the many first-order factors making up that super-order factor concept. Both certainly fit into the hypothetical nature of psychoticism.

It is perhaps relevant to mention, as Borkenau and Ostendorf (1989) point out, that in their works on extending the NEO personality inventory to include agreeableness and conscientiousness, Costa and McCrae list 6 primaries for N and E, which are separately scored, but do not list any primaries for A and C, seemingly realizing that A and C are more properly regarded as primaries themselves.

It is difficult to know to what extent differences in outcome are due to differences between assessment by questionnaire, as mainly relied on by Eysenck and Cattell, and assessment by adjective checklist and rating scale, as mainly relied on by Goldberg, Costa and McCrae. There does not appear to be any theoretical reason for such differences, and it seems unlikely that personality structure would be different according to whether people rated themselves on questionnaires or adjective checklists. The problem has not received as much study as it deserves.

However that may be, one important criterion (No. 7) for a paradigm would surely be that a meta-analysis of large-scale studies should demonstrate convergence on a particular model. Such a meta-analysis has been published by Royce and Powell (1983) and their conclusion targets 3 major factors at the higher order level; these they denote as Introversion-Extraversion, Emotional Stability, and Emotional Independence (p. 123). The first two are clearly E and N, and the third, characterized by autonomy, lack of trust, lack of cooperation, realism, tough-mindedness, lack of affect, dominance, realism and contention, has many of the characteristics of P. This meta-analysis certainly does not support Cattell at the first-order level, or the five-factor model at the second order level; it clearly supports a 3-factor model very much like the PEN construct.

More recently, several authors have used large numbers of scales to capture what are widely believed to constitute major human traits, and discovered through factor analysis the major higher order factors. (This might be regarded as the 8th criterion.) Of particular value is the study reported by Zuckerman, Kuhlman and Camac (1988), because of its width of coverage. Using 46 scales from 8 tests, they rotated varying numbers of factors. The 5 factor solution clearly identified sociability and emotionality, which are now universally agreed as 2 basic dimensions of personality. Next came "Impulsive-Unsocialized-Sensation-Seeking (ImpUSS) and Aggressive-Sensation-Seeking (AggSS)"; these two coalesce in the 3 factor solution to form P. Activity is the fifth factor. The possibility is suggested that conscientiousness could be the reverse of the ImpUSS factor, and agreeableness the obverse of AggSS; if so this would strengthen the suggestion that agreeableness and conscientiousness (reversed) are traits contributing to P. The solution gives only very limited support to the five factor model. Zuckerman, Kuhlman and Camac go on to point out that "the three-factor solution approximates the model proposed by Eysenck, and his own scales provide excellent markers for the three factors" (p. 96). There is nothing in the data to elevate a five factor solution over solutions containing more or fewer factors; the most comprehensive is certainly the 3-factor solution, which also fits in best with the Royce and Powell meta-analysis.

A similar result is reported by Martorell, Petro, Dacer, Navarro and Silva (1990), who used a large battery of personality tests on a group of younger subjects. "The three factor solution appeared as the most suitable, both in terms of distribution of the explained variance and of interpretability". These three factors were interpreted as P, E and N, with EPQ-J scores on these

dimensions loading high on the extracted factors. The study was done in Spain, suggesting cross-cultural validity for these 3 factors.

A third multi-inventory study was reported by Schverger and Allen (1986), who used 5 different questionnaires. They decided on 5 major factors, which however bear little relation to the "Big Five". E and N of course appear prominently, with P suggested in their 2(?) "control" factors. The outcome is less clear than in the preceding two studies, perhaps because no attempt was made to rotate a smaller number of factors.

A final comprehensive factor analysis of personality questionnaire items by Browne and Howarth (1977) has been factor analysed by Eysenck (1978b); the outcome was a clear-cut 3 factor solution indicating very apparent P, E and N factors. There is nothing corresponding to the five factor model in these data, other than the overlap with the 3 factor model.

Also relevant here may be a monograph presenting a "survey of factors in some of the best known personality questionnaires" (Kline & Barrett, 1983). This survey is technically the most sophisticated, and its conclusions deserved to be taken seriously. They argue as follows:

(1) "The failure of the Cattell primaries, despite the enormous statistical and psychological expertise behind their research and development, to emerge with any clarity and the equal failure of any of the other systems of primary factors to stand scrutiny, would appear to show that primary factors are to be eschewed. They are not stable enough to form a basis for theory or model".

(2) "Three factors, higher-orders, neuroticism, extraversion and psychoticism do seem to have been properly and reliably identified, although there is less clarity concerning psychoticism. Their psychological nature has been extensively investigated and it makes good sense to regard neuroticism and extraversion as major dimensions of temperament, largely heritable dimensions reflecting psychophysiological mechanisms . . .".

There seems no doubt that primary factors, more specific than these higher-orders, are necessary for an adequate description of personality. Present work, although it has revealed a number of factors, as we have shown, is confused. Apparently well established factors are failing: new factors are emerging. However, these need external validation. Without this, these factors may be of little importance" (p. 197).

Note that in this wide-ranging survey there is no mention of "agreeableness", "conscientiousness" or "openness"; these variables only emerge in studies directly targeted at the "big five", and not in the many large-scale studies aimed at comprehensive coverage of the whole field.

Our discussion of the taxonomic criteria for a paradigm of personality suggests that the nonfactorial models (MMPI; CPI) cannot be seriously considered, and indeed make no claims to be so considered; that the "big five" and the PEN models have good psychometric backing for the most part; but that 2 of the "big five" scales may be of a different level (first order rather than second order) from the remaining 3; and that 2 of the "big five" may be merely traits contributing to the second order factor P. The studies reviewed here and elsewhere (Eysenck & Eysenck, 1969, 1985) have used the polythetic method (Sokal & Sneath, 1963), which is now generally preferred to the monothetic method, i.e. they have relied on similarities based on shared features, rather than categorical characterization based on a unique set of features. Can we conclude that the criteria mentioned thus far enable us to give a confident answer to the question which motivated our search?

The answer must be in the negative because taxonomy is inevitably subjective to some extent as long as there is no causal factor which can be used to test alternative hypotheses. For animal taxonomy, for instance, the theory of evolution provides such a causal link; alternative sets of classificatory hypotheses can in principle (though at present not always in reality) be tested by reference to development from ancestry. Where not available from direct analysis through phylogeny, phylogenetic trees may be constructed by biochemists and microbiological geneticists from quantitative estimates of variance between species as regards substances such as DNA or cytochrome *c* (e.g. Fitch & Margoliah, 1967). Can we find evidence of such phylogenetic factors in human personality?

I have argued elsewhere that we must regard man as a biosocial animal (Eysenck, 1980a), and that only by accepting such a biosocial model can we achieve any degree of unification of psychology (Eysenck, 1980b). Such an approach would lead us to a search for biological

(evolutionary) roots of personality traits and types (Eysenck, 1967, 1981), and would help to unify the two disciplines of scientific psychology (experimental and correlational—Cronbach, 1957) into a coherent body of knowledge (Eysenck, 1984). Such an approach leads us into a consideration of genetic causes (Criterion 9), and through them to a search for underlying anatomical, physiological and hormonal factors (Criterion 10) in individual differences (Williams, 1956). In order to consider a factor to be more than a statistical artefact, and to regard it as a fundamental building stone of human personality, one would demand some such causal reference as has been suggested above.

There is now a large body of evidence supporting the view that P, E and N are strongly determined by genetic factors, and indeed the whole genetic architecture involved has been clarified to a considerable degree (Eysenck, 1990b; Eaves, Eysenck & Martin, 1988). Little is known about openness, agreeableness and conscientiousness, and until this gap is closed it may be suggested that they do not deserve the title of paradigmatic dimensions of personality; at best we might say that they are on probation. Possibly genetic research will tell us whether we should indeed regard them as independent major factors, or as subfactors of more inclusive dimensional concepts, like P. Eaves, Eysenck & Martin have given an analysis of the kind required for anxiety and depression, showing how these two factors emerge clearly as contributors to the super-factor N.

A high degree of heritability, as present in P, E and N, suggests a search for biological factors underlying these dimensions of personality, and much advance has occurred since the early suggestion of such a biological model (Eysenck, 1967). The most recent summary (Eysenck, 1990a) lists many contributors who have classified the theories originally formulated to account for individual differences along these dimensions, and it seems clear that while many anomalies remain, in essence the field is becoming clearer (Gale & Edwards, 1983; Mangan, 1982; Stelmack, 1990; Strelau & Eysenck, 1987; Thayer, 1989). There is no such biological underpinning for openness, agreeableness and conscientiousness, and their absence again suggests that until this gap is filled they cannot lay claim to parity with P, E and N.

Angleitner (1990) agrees that the four temperament factors of the "big five" relate to behavioural tendencies based on the constitutional or biologically determined make-up of individuals, and he sees "the first four factors of the Big Five as primarily temperamental dimensions" (p. 10). Thus this criterion is clearly accepted by both sides. (Angleitner also agrees on the importance of cross-cultural replicability of factors, stability of factor through life, and predictive power for behaviour in and outside the laboratory. On all of these the evidence for agreeableness and conscientiousness is minimal.)

The strong biological determination of differences in P, E and N immediately suggest two further criteria (Nos. 11 and 12) for a valid dimension of personality. The first is universality. A truly fundamental concept in personality research should be largely independent of national, racial and cultural differences. Such independence has been demonstrated in a large-scale set of studies, using the EPQ in now 36 countries including Western countries (Germany, France, Italy, Scandinavian countries; Canada, U.S.A., Australia); Socialist countries (U.S.S.R., Poland, Hungary, Bulgaria, Yugoslavia); Eastern countries (Japan, Mainland China, Hong Kong); African countries (Egypt, Nigeria, Uganda); Indian countries (India, Sri Lanka), etc. Papers by Eysenck and Eysenck (1983) and Barrett and Eysenck (1984) have surveyed the first 25 studies done, and Eysenck, Barrett & Eysenck (1985) have answered some of the criticisms of the indices of factor comparison used, showing that homologous indices for comparing countries average 0.98 while nonhomologous indices only average 0.13. There is thus surprisingly high agreement on the nature of factors extracted in a large and very variegated number of countries and many different cultures. More recent studies extending the range of countries still further have shown similar agreement. There is no similar evidence for the "big five" factors other than with E and N.

The second criterion suggested is consistency of personality over time. If the major dimensions of personality have a strong genetic foundation, and are mediated by physical structures in the CNS and the ANS, then it seems essential that under ordinary conditions of upbringing and life circumstances (i.e. life histories not including catastrophic events like torture in a concentration camp), a person should retain his relative standing on the major dimensions of personality. The evidence that this is indeed so has been reviewed elsewhere (Eysenck & Eysenck, 1985), and strongly suggests that this expectation is indeed fulfilled for E and N; there is less evidence for P (Conley,

1984a, b). Again the evidence for openness, agreeableness and conscientiousness is largely missing.

There is one other criterion for the admission of a suggested dimension of personality into a paradigm of personality description, namely the social importance of that dimension (Criterion 13). It would be difficult to take seriously the claims of a factor to be of fundamental importance if that factor could not be shown to interact powerfully with social activities widely regarded as important. Wilson (1981) has listed many of the social implications of personality differences in P, E and N, ranging through criminality (Eysenck & Gudjonsson, 1990), sexual behaviour (Eysenck, 1976), mental illness (Eysenck, 1970b, 1973), occupational choice and industrial performance (Wilson, 1981), academic aptitude and achievement (Wakefield, 1979; Eysenck, 1978a), accident proneness (Shaw and Sichel, 1971), sport (Eysenck, Nias & Cox, 1982) and many others. Little is known about the social relevance and importance of openness, agreeableness and conscientiousness, although in a small way all may be related to some areas of social behaviour, perhaps through their relation with P. What is lacking is a series of large-scale studies which would flesh out such possibilities, and indicate in more detail their effectiveness.

The theory of the biosocial nature of man is too strongly supported to admit of serious doubt (Eibl-Eibesfeldt, 1986; Lumsden & Wilson, 1981), and should be taken seriously in deciding on criteria for a model of personality. It is not suggested that we should embark on a completely reductionist programme of theorizing and research; the model postulates an interaction of biological and social influences, and a reciprocal influence of mind and body. It suggests that just as physicists had to accept a space-time continuum, so psychologists will have to learn to accept a mind-body continuum. However, acceptance of such major theoretical considerations is not necessary for an acceptance of the criteria here suggested; these follow directly from a consideration of the factual material referred to. The wider implications have merely been mentioned to indicate the general importance of the issues raised.

What is most relevant, of course, is the demonstration that on all the points raised there is very little if any evidence that the factors of the "big five" model, other than E and N, fit the criteria suggested. It is also clear that E and N, and for the most part P also, do fit these criteria, and may hence have some justification to be regarded as constituting a model which has some justification for being regarded as a paradigm for personality research. Clearly such a formulation does not exclude the possibility that additional dimensions may in the future be added to the "gigantic three"; what is advocated is a positive identification of an existing paradigm, not a prescriptive exclusion of possible additions to the model. For reasons already given we may doubt whether openness, agreeableness and conscientiousness will achieve that status, or will end up as primaries contributing to higher order factors; there is no reason to prophesy. All that needs to be said is that there is little evidence at present to justify large claims for these 3 additional factors, and some impressive evidence that 2 of the 3 are subsidiaries of P. Thus it may be concluded that at present, and relying exclusively on available experimental studies, the PEN system has greater claims to paradigmatic status than any other system examined.

If the argument advanced so far be acceptable, it might nevertheless be asked what if any consequences follow from it. It may be useful to spell out some of these consequences, if only briefly; they follow logically from an acceptance of the whole notion of the place of a paradigm in science (Barnes, 1982; Eysenck, 1985b). In the first place, then, we would have a unified model to present to students, not a great variety of utterly dissimilar and contradictory bits and pieces made up of assumptions, axioms, speculations, values, preferences, and occasionally facts. Such a model would not necessarily be correct in any absolute sense; indeed, its only claim would be that it constituted a progressive rather than a degenerating problem shift, to use Lakatos' term, that it accounted for more of the variance than competing models, and that it fulfilled the criteria for a paradigm more adequately than any alternative conception. It goes without saying that the model requires extension (through deductions not yet tested), improvement (through removal of anomalies), and amendment (through changes enforced by contrary findings).

All this, of course, is the task of "normal science", and hence the second major advantage of the paradigm is that it enables research falling under that heading to take place, and be accorded the importance it deserves (Criterion 14). At present personality psychologists work along so many divergent lines that little progress is possible. Instead of concentrating on improving an existing

model of considerable descriptive and exploratory power, psychologists insist on rediscovering the wheel, and produce ever new models and concepts overlapping with existing ones and not advancing our understanding in any way. This overlap has been documented elsewhere (Eysenck & Eysenck, 1985), and is one of the main arguments for agreement on a single paradigm. Without it, research fails to accord with the most elementary criterion of scientific advance, namely additivity. With a paradigm, one study builds on others and helps in developing and improving the model; without it, the great majority of studies fail to be relevant to the model, and are incapable of being directly translated into the parameters of the paradigm. Where we have literally hundreds of inventories, incorporating thousands of traits, largely overlapping but also containing specific variance, each empirical finding is strictly speaking only relevant to a specific trait, measured by a specific inventory; extension beyond that is by assumption, speculation, and argument. This is not the way to build a unified scientific discipline.

Assessment of the empirical literature through citations already suggests that the PEN system has given rise to far more empirical tests (psychometric, experimental, genetic, pharmacological, social, psychophysiological) than any other; in that sense it is already approaching the position of a paradigm. It would seem only a small step to recognize the situation, and accept it in that spirit.

One great advantage of taking such a step would seem to be the likely improvement in the meaningfulness of empirical research. At present many students seem to make their choice of personality inventory almost on a random basis, and similarly do not choose the hypothesis to be tested on the basis of a consideration for the elimination of anomalies, or extending the deductive properties of a model; hence the failure of most published articles to be represented in the Citation Index. The existence of an agreed paradigm, with known empirical support as well as known anomalies, points directly to worthwhile research problems which ensure the value of the resulting research.

However, this may be, prior to agreement on a paradigm or model must come agreement on the criteria for judging the value of different models. A list of the criteria suggested here is given below. It is the purpose of this paper to introduce a discussion of this problem, and to suggest certain criteria. If we cannot agree on the criteria, then clearly we are unlikely to agree on the most promising paradigm. This obvious fact illustrates the importance of the problem; it deserves more discussion and debate than it has received in the past.

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APPENDIX

Criteria for a Paradigm of Personality

Taxonomic criteria

- (1) Theory-based, and covering a wide field.
- (2) Theory formulated to permit testable deductions.
- (3) Theory deals with description and interaction of traits.
- (4) Theory has a hierarchical form.
- (5) Outcome of psychometric tests must be replicable.
- (6) First- and second-order factors must be clearly distinguished.
- (7) Meta-analysis of results must show convergence.
- (8) Outcome of studies using great variety of scales must show convergence.

Causal criteria

- (9) Genetic architecture of model must be well understood.
- (10) Biological basis of model must have a theoretical foundation, and be testable.
- (11) Model must be universal across cultures.
- (12) There must be consistency over time.
- (13) Model must generate socially important deductions.
- (14) Model must suggest role for “normal science”.