

# Personality as a Fundamental Concept in Scientific Psychology

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It is argued that the concepts of individual differences and personality are an indispensable part of any scientific psychology, by virtue of the fact that stimulus-response sequences are inevitably mediated by an organism the structure of which, partly determined genetically and partly by its reinforcement history, critically affects the sequence. Theories in this field, to be useful, must be linked with the theories and findings of experimental psychology, and reflect advances in conceptualization and thinking there. Personality theories are both descriptive and causal, and it is argued that without the causal element descriptive patterns, usually arrived at by correlational and factor analytic methods, possess an element of subjectivity which is so strong as to make any definitive statements impossible. It is further argued that such a paradigm, embracing both descriptive and causal factors, is already in existence, and is capable of integrating well with many different areas of experimental psychology.

Cronbach (1957), in his Presidential Address to the APA, made the important point that there are two disciplines of scientific psychology, which he identified as the experimental tradition and the individual differences tradition. Deploing the fact that practitioners in either camp tended to disregard the achievements of the other, he maintained that a proper scientific discipline could only arise by uniting these two disparate disciplines. My own work in the experimental and individual differences fields has always been directed towards such a unification, and I believe that by keeping the two aspects wilfully apart psychologists are delaying the time when psychology will become a proper paradigmatic science. There is already sufficient evidence available to show that in experimental psychology, social psychology, industrial psychology, abnormal psychology, educational psychology and indeed any aspect of psychology the search for main effects has to be supplemented by the study of individual differences, in order to make meaningful, testable and replicable predictions.

## *Rationale for the Study of Individual Differences*

Examples abound. In experimental psychology, consider the hypothesis that sensory thresholds can be lowered by heteromodal stimulation. Although this hypothesis fits in with theoretical models of cortical arousal, there are as many experiments disproving it as there are experiments supporting it (Shigehisa & Symons, 1973), and this apparent lack of replicability of experimental studies is not atypical of psychological research in general. In part the difficulty is produced by the so-called "law of inversion", which, following Pavlov, predicts

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that increase in intensity of a heteromodal stimulus may lead to effects that are the reverse of those induced by weaker intensities. Shigehisa and Symons predicted, following Eysenck's theory of higher cortical arousal being characteristic of introverts, that the "inversion" would take place at lower intensities for introverts than for ambiverts, and for ambiverts than for extraverts, and demonstrated in a whole series of well-conceived studies, that by taking personality into account it became possible to make testable and replicable deductions from the theory which were borne out in every detail by the experimental results. Many other similar demonstrations have been collected in Eysenck's (1976a) book, *The Measurement of Personality*.

In the educational field, McCord and Wakefield (1981) hypothesized that in educational practice introverted children would show better learning responses to *blame*, extraverted children to *praise*. In an extensive experimental investigation, they demonstrated the high degree of statistical significance that the two methods of treatment did indeed have inverse effects on children depending on personality. Many other interactions between educational methods and personality have been listed by Eysenck (1978).

In the field of clinical psychology, DiLoreto (1971) compared the effects of different types of treatment (e.g. client-centred therapy and rational-emotive psychotherapy) on extraverted and introverted patients. Also using placebo and no-treatment groups, he demonstrated that for extraverts, client-centred therapy was effective, whereas for introverts, rational-emotive psychotherapy was effective. In each case introverts treated by client-centred therapy, and extraverts treated by rational-emotive psychotherapy did not do any better than the placebo or no-treatment groups.

In social psychology, Tiggemann, Winefield, and Brebner (1982) investigated a typical learned-helplessness induction procedure, and demonstrated that introverts displayed a much more marked helplessness effect than extraverts, in accordance with predictions made from the Brebner-Cooper (1978) model of extraversion in terms of inhibition and excitation deriving from stimulus analysis and response organization. The relevance of this finding for such clinical applications as the development of depressive illness will be obvious.

In industrial psychology the concept of accident-proneness has been linked quite closely with personality traits of neuroticism and extraversion (Shaw & Sichel, 1971), and a survey of a fairly large literature indicates that there is good evidence for linking accident-proneness and personality in this manner (Eysenck, 1982).

These are isolated examples from a very large literature emphasizing the point that there are considerable dangers in disregarding individual differences in studies of experimental, social, clinical, educational or industrial psychology. Such neglect often leads to an unnecessary enlargement of the error term, and a disregard of important interaction effects involving main effects and personality variables. Such a disregard may also lead to lack of replicability and lack of predictability of experimental effects. The literature strongly suggests that inclusion of personality variables is not only permissible but *mandatory* in psychological studies, and their absence constitutes a serious criticism of any study not incorporating such variables in its design.

#### *Need for a Counsel Theory*

Parallel to this criticism of experimentalists who fail to use personality variables

in the design of their studies must be a criticism of personality theorists who fail to use the concepts and theories of general psychology in structuring their own field of study. The investigation of personality has posed a *descriptive* and a *causal* aspect, and personality theorists have been rather wilful in using a purely subjective approach to the descriptive study of their discipline, and have often failed entirely to even suggest the possibility of a causal analysis. Typically, textbooks of psychology, following the bad example of the original Hall and Lindzey (1965) text, have presented the reader with a series of chapters of an eponymous kind, each dealing with a different author, and without any attempt to judge his or her contribution in the light of experimental support or disproof. Thus the reader is not offered a proper paradigm, but a subjective choice of approaches to suit his or her own individual preferences. This is hardly a scientific approach to an important psychological question.

Where there have been serious attempts to solve the descriptive problem (e.g. in the work of Cattell, Guilford, Comrey, etc.), through the use of correlational and factor analytic methods, the authors have explicitly or implicitly disregarded the task of articulating a causal theory, and have equally disregarded the task of relating their findings to the concepts emerging from studies in experimental and physiological psychology. In this way they have aided and abetted the bifurcation of psychological research, and have made more difficult a rapprochement between the two sides.

There have, of course, been many reasons for the disregard of personality concepts on the part of scientists in other psychological disciplines, mainly (1) the argument that there is little agreement on the major dimensions of personality between experts, and (2) that there is little consistency of conduct which would justify the postulation of personality traits and types. Both these objections deserve at least a brief answer. As regards the former point, there is now considerable agreement among leading investigators following the correlational and factor analytic trait approach that there are three major dimensions of personality (or superfactors), which have been variously named, but which correspond to the extraversion-introversion, the neuroticism or anxiety vs. stability, and the super-ego or impulse control vs. psychoticism nomenclature (Eysenck & Eysenck, 1984, Royce & Powell, 1983). Not only are these dimensions found fairly universally in large-scale research efforts within the European-American tradition, representing work in the U.S.A., the U.K., Canada, Australia, Germany and Scandinavia, but they also extend to a great variety of different cultures, including such countries as Japan, Nigeria, Uganda, Hong Kong, India, etc. (Eysenck & Eysenck, 1983). Even in the animal field dimensions similar to these three have been found in the analysis of social behaviour in rhesus monkeys (Chamove, Eysenck, & Harlow 1972). While this unanimity does not necessarily extend to lower-order factors, it is impressive as far as the most inclusive, important and fundamental factors are concerned.

### *Consistency of Human Personality*

Personality theory is essentially linked with the hypothesis that human behaviour is consistent over situations. This consistency has been doubted ever since Thorndike (1903) wrote that "There are no broad, general traits of personality, no general and consistent forms of conduct which, if they existed would make for consistency of behaviour and stability of personality, but only independent and specific stimulus-response bonds or habits". This critique has been associated

in recent years most strongly with Mischel (1968) although the most recent publication by Mischel and Peake (1982) seems to argue a much weaker case. Eysenck & Eysenck (1980) have exhaustively answered Mischel's criticisms and in particular Eysenck (1970) and Rushton Brainerd, and Pressley (1983) have demonstrated that Mischel's criticisms leave out of account entirely the principle of *aggregation*, i.e. the fact that while single assessments, being unreliable, do not have high validity, aggregating groups of instances or predictors gives very much higher reliabilities to the aggregate, and increases very considerably the validity of the combined estimate. Thus the Thorndike/Mischel doctrine rests essentially on a misunderstanding of the appropriate empirical and statistical approach to the problem, and can be demonstrated to be factually erroneous. *There is considerable consistency in human conduct across situations*, and this fact alone necessitates conceptualization in terms of traits and types, or factors and superfactors if that terminology be preferred.

It will be obvious that the question of cross-situational consistency broadens out into a question of longitudinal consistency when the time dimension is introduced. Here too the correct answer requires use of the principle of aggregation (Rushton et al., 1983); when studies measure personality at different ages by aggregating over many different assessments, longitudinal stability is usually found. However, single measurements or other less reliable techniques result in longitudinal stability indices which are much lower. The work of Block (1971, 1981), Conley (in press-a & -b), Costa & McCrae (1980), and Costa, McCrae, and Arenberg (1980), all illustrate, on the basis of large-scale and long-continued follow-up studies, that consistency of human personality and behaviour can be documented over time as well as over situations. A survey of the literature is given by Eysenck & Eysenck (1984).

### *Genetic Basis of Personality*

We thus have consistency among investigators as regards the identification of the major dimensions of personality, consistency of these dimensions over many different cultures, consistency of personality over situations, consistency of personality over time, and even some degree of cross-species consistency; it seems unlikely that such consistency could be achieved without some firm biological basis suggesting hereditary determination. There is now a large body of empirical data, summarized by Fulker (1981), demonstrating that indeed personality differences are determined to a high degree by genetic factors. This is equally true for the three major dimensions of personality as it is for single traits of various kinds which have been studied by recent elaborations of the twin method. When corrected for attenuation, estimates of heritability amount to about two thirds of the total variance, and thus approach the degree of heritability characteristic of intelligence tests. While there are thus important similarities between personality and intelligence, there are also important differences. Thus there is little evidence of assortative mating as far as personality is concerned, although this factor is very important in relation to intelligence. Equally, directional dominance plays no part in the inheritance of personality, although it does enter very forcibly into the inheritance of intelligence.

On the environmental side, between-family environmental factors play an important part in the genesis of differences in intelligence, but not in the genesis of differences in personality. Within-family environmental factors play little part as far as intelligence is concerned, but account for practically all the environ-

mental variance as far as personality is concerned. This has important consequences for any theory of personality, disproving for instance such theories as the Freudian which rely for their working almost entirely on between-family environmental variance factors. This important approach to the study of causal theories in personality development has received astonishingly little use or attention by personality theorists (Eysenck, 1977b).

If we agree that there is a strong genetic basis for the consistency to be found in human personality and conduct, then it would seem to follow that any causal type of theory which attempts to take account of individual differences in personality would have to look for anatomical, neurological, physiological and hormonal factors as mediating factors between the DNA-gene-chromosome assembly, on the one hand, and behaviour, on the other. Behaviour as such cannot of course be inherited in any direct manner; we must postulate as an intermediary anatomical structures, neurological, physiological or hormonal functioning, or other similar biological phenomena in order to account for the observed phenotypic differences in behaviour between people. It was along the lines of arguments of this kind that I was led to propose a causal theory for extraversion-introversion and neuroticism-stability (Eysenck, 1967), and more recently for psychoticism-impulse control (Eysenck & Eysenck, 1976). The theories regarding E and N are probably too well known to require lengthy restatement, but a little more may perhaps be said about psychoticism (P).

### *Causal Theory of Accountability*

As far as extraversion-introversion is concerned, my theory appeals to the concept of cortical arousal, modulated by the ascendant reticular formation, and states, in its most simple form, that low habitual levels of cortical arousal, or low cortical arousability, will lead to extraverted types of behaviour (sociable, sensation-seeking, active, etc.), while high levels of cortical arousal, or cortical arousability, will lead to introverted types of behaviour (unsociable, quiet, inactive). Ambiverted persons, of course, will be characterized by an average level of cortical arousal/arousability. The theory has found a great deal of empirical support, both along experimental and along physiological lines, and will not be reviewed here (see Eysenck, 1981).

As far as neuroticism is concerned, my theory links this concept with *autonomic activation*, considering this type of activation to be essentially different from *cortical arousal* (Routtenberg, 1966). Autonomic activation can be indexed along several different lines (strength of activation, duration of activation, speed of activation), and its measurement presents many quite considerable problems, involving for instance such doubtful generalizations as the law of initial value. The very careful work of Fahrenberg (1967) and Myrtek (1980) has indicated some of the difficulties of testing this theory of autonomic activation as underlying neuroticism, and although perhaps more widely accepted than the arousal theory of extraversion, its empirical support is probably much less. For a general survey of the physiological evidence regarding both E and N, Stelmack (1981) may be recommended.

When we turn to the P dimension, the biological variable that seems most closely involved is a hormonal one, namely androgens. It has become very clear that the behaviour-complexes associated with P tend to occur much more frequently in males than in females, and altogether males score about twice as high on P as do females, not only in Europe and the U.S.A. but also in a great variety

of different cultures. Equally, behaviour patterns associated with P, such as criminality, tend to be also associated with the male sex. There are some studies which support this general view, but it cannot of course be said that at the moment it is as firmly established as is the relationship between extraversion and low arousal.

### *Alternative Theories*

There have been several attempts to formulate alternative theories to the ones outlined above, both from the point of view of description, and from the point of view of causality, the most noteworthy of which are those of Brebner and Cooper (1978), and Claridge (1967, 1972), and Gray (1981). Both the Gray and the Claridge theories involve a rotation of axes within what would still be a three dimensional space; my own feeling would be that such a rotation is not easily reconcilable with the fact that practically all factor analytic studies have come up with similar or identical factors in the position demanded by the Eysenck theory; there would have to be particularly strong reasons for departing from this universal agreement and postulating a forced rotation into a position not suggested by the empirical data.

Furthermore, it seems to me that most of the suggestions and novelties incorporated in the theories of Gray and Claridge can be incorporated in the original system without any rotation. Thus there is no difficulty in accommodating, should it prove to be correct, Gray's suggestion that introverts condition better to stimuli having a negative valence, whereas extraverts condition better to stimuli having a positive valence. If true, this might be regarded as a parametric extension of the Eysenck paradigm, rather in line with the inclusion in that paradigm of Pavlov's law of transmarginal inhibition (Levey & Martin, 1981). Further empirical work is required to see to what extent Gray's extension is in fact supported, but on the whole it must certainly be said that there is some strong evidence in its favour.

The Brebner and Cooper model assumes that dealing with sensory input and organizing appropriate responses generates either a relatively inhibitory or excitatory state. Inhibition in this system is defined operationally as a tendency to discontinue or decrease ongoing behaviour, excitation as a tendency to continue or increase it, where the opportunity to vary behaviour is constrained by experimental conditions. The model particularly emphasizes the distinction between stimulus (S) analysis, and response (R) organization, postulating that these processes may be independently producing inhibition or excitation.

On the theory, introversion is characterized by the generation of excitation by S-analysis, inhibition by R-organization, whereas extraversion is characterized by the generation of excitation by R-organization and inhibition by S-analysis. The prediction would seem to follow from this hypothesis that introverted individuals will continue in tasks which are higher in their demands for S-analysis than R-organization, and extraverts to continue in tasks which are higher in R-organization rather than S-analysis demands. Thus introverts would be geared to inspect, extraverts would be geared to respond.

As in the case of the Gray and Claridge hypotheses, there is evidence for the theory in the work of Brebner & Cooper (1974, 1978), Brebner & Flavel (1978), Katsikitis & Brebner (1981), and Tiggermann et al. (1982). The theory does not require re-rotation of factors, but does emphasize aspects of the theory which fit in very well with many of the features of the original theory. It is notable

that all these suggestions for improvement or alteration have been in relation to extraversion-introversion, rather than neuroticism, although Claridge's suggestion does involve the P factor. Indeed, Claridge is the only theorist who has been deeply involved with psychoticism (Claridge, 1981).

This is not the proper place to review in detail and discuss different theoretical issues of this kind, or to review extensively the literature. This has been done recently elsewhere (Eysenck, 1981), and a repetition would be tedious. Instead it may be useful to look at the most recent work done in relation to one of the three major dimensions of personality, namely psychoticism, and illustrate the experimental mode of argumentation which goes from the postulation of a factor to its measurement, and then to further investigations of its nature. The steps taken in this progression, and the respective influence of objectivity and subjectivity, are not always correctly appreciated, and it may be useful to illustrate the theoretical discussion by means of reference to empirical investigations. Some of the work mentioned is as yet unpublished, but this may give additional interest to the discussion.

### *Recent Research on Psychoticism*

The psychoticism dimension has been chosen to illustrate the complex ways in which empirical research, theoretical preconceptions and empirical facts are integrated to produce a given model which can then be researched more directly. The hypothesis of a dimension of psychoticism arose originally from a consideration of the diathesis-stress model of schizophrenia and manic depressive disorder; such a model postulates an underlying dimension of *predisposition to psychotic illness* which would run all the way from normality to abnormality. The model was based on observations, summarized elsewhere (Eysenck, 1972), which showed that the relatives of schizophrenics and manic depressives were more frequently schizophrenic or manic depressive than chance would permit, and that often among the relatives of schizophrenics there were manic depressive patients, and among the relatives of manic depressives there were schizophrenics, suggesting some kind of *general* psychotic diathesis. Furthermore, among the relatives of psychotics there were many individuals who, while not psychotic, were odd in various ways — alcoholics, criminals, psychopaths, schizoid personalities, and "odd balls" of various kinds; these would easily fit into the middle part of the diathesis continuum, i.e. being intermediate between the normal and the clearly psychotic groups.

The hypothesis of such a continuum obviously requires experimental support, and in order to provide this Eysenck (1950) worked out the method of criterion analysis, which, when applied to psychotics (Eysenck, 1952), demonstrated clearly that a continuity theory was more appropriate than a theory postulating categorical distinctions between normals and psychotics. Both criterion analysis and factor analysis demonstrated furthermore that this continuum was separate and unrelated to that going from normality to neurosis, thus suggesting the existence of two separate normal continua, one of neuroticism, the other of psychoticism. It was on these grounds, aided by much further empirical evidence that Eysenck (1952) postulated the existence of a dimension of psychoticism, independent of extraversion-introversion and neuroticism.

The publication of a questionnaire attempting to measure degrees of psychoticism (Eysenck & Eysenck, 1975), and a book summarizing the evidence (Eysenck & Eysenck, 1976) provoked a good deal of criticism (e.g. Bishop, 1977;

and Block, 1977a & b) the main burden of which was that whatever the P scale was measuring, it was not psychoticism, but might be something else, such as psychopathy. It is interesting to consider for a moment the actual development of the scale, and the evidence to suggest that the scale does in fact measure psychoticism rather than psychopathy or some other variable. This discussion will also illuminate the question of the degree of objectivity and subjectivity adhering to taxonomic efforts of this kind.

Our first task was to write or collect a number of items which would be relevant to the various aspects of "psychoticism" as we saw it, on the basis of our knowledge of the literature, of psychotic individuals, etc. It was clearly necessary to avoid a simple listing of symptoms, such as occurs in the MMPI, because the scale was to be administered to normal people, and was supposed to measure psychoticism outside rather than inside the clearly psychotic range. The test was not supposed to be used for the diagnosis of psychotic individuals, as opposed to neurotics or normals; its purpose was to measure psychoticism in the normal range, and possibly also within the psychotic range, but only incidentally to discriminate between normals and psychotics. Furthermore, such a scale should not correlate with neuroticism or extraversion-introversion, but should be orthogonal to both these major dimensions of personality.

The first scale so constructed fulfilled some of these requirements, but rather less adequately than was required, and was subjected to a factor analytic study which ruled out various items, either as having no loadings on the new factor of P, or as having high loadings on other factors also. The results clarified some of our hypothetical ideas concerning P, and new items were written, older ones eliminated or altered, and a second round of factor analytic studies was undertaken. This produced improvements, but clearly the scale was still rather weak, and so a third, and a fourth, and a fifth replication of the procedure was needed; in fact, altogether there must have been about 20 factor analytic studies before the final, much improved though still imperfect questionnaire was produced.

Note that certain conditions were imposed on the construction beforehand, namely independence for the P scale of N and E. It might not have been possible at all to produce a scale embodying the essence of personality traits in our view related to P, and showing such independence; such an outcome would have disproved our hypothesis. In actual fact it did prove feasible to do so, but of course the resulting scale, while in line with our theories about psychoticism as a dimension of personality, might in fact be measuring something entirely different. Thus what was needed was a method for demonstrating that the P scale does in fact measure what it is supposed to measure, and for this purpose we used an adaptation of the criterion analysis method, which we called the proportionality criterion method. I will first briefly discuss the rationale of this method, and then illustrate by means of a few examples.

Consider a continuum going from A, complete normality, to B, severe psychosis, passing through a point X, which is situated in such a way that to the right of it are all patients diagnosed psychotic, while to the left of it are persons not so diagnosed, and according labelled normal. Let us also postulate the existence of a suggested measure of this P dimension, the question being whether or not measure and continuum are in fact colinear. One way of attacking this problem would be to discover an objective index which clearly differentiated the normal from the psychotic population; let us call this index alpha. We can now say that if the hypothetical continuum from A to B exists, and if P is a measure of this continuum, and if alpha clearly differentiates the two parts of



the continuum separated at point X, then and only then will alpha correlate with P both in the normal and in the psychotic population. This, in brief, is the rationale; now for an example.

As a useful measure alpha, consider the human leukocyte antigen HLA-B27. The human leukocyte antigens are individual traits genetically determined by a locus on chromosome 6. Gattaz and Beckman (1981), Gattaz, Ewald, and Beckman (1980) and McGuffin, Farmer, and Yonace (1981) have given surveys of the literature, and there is strong evidence that HLA-B27 has a significantly increased incidence in schizophrenic patients as compared to healthy controls (Gattaz et al., 1980; Mendlewirz, Verbauch, Linkowski, & Govaerts, 1981; Rosler et al., 1982).

Using HLA-B27 as an alpha measure, Gattaz (1981) showed in a schizophrenic population that there was a significant relationship between high P scores and a presence of HLA-B27. In a later study, Gattaz & Seitz (1983) found a similar association between P score and HLA-B27 presence in a normal group of subjects. Thus insofar as this particular measure is concerned, we find complete validation of the proportionality hypothesis.

Even stronger is the evidence put forward by Claridge and his colleagues, using what he calls the "phenomenon of reversed co-variation" (Claridge, 1981.) When correlated with each other, measures of tonic emotional arousal and measures of perceptual responsiveness should, according to conventional psychophysiological principles, covary in a predictable way. Normally, perceptual response will improve with increasing arousal though it may subsequently decline again, forming the familiar inverted-U function. However, it seems that in psychosis the opposite relationship holds, with the result that at any given level of tonic arousal perceptual responsiveness is paradoxically greater rather than less than it should be (Claridge, 1972, Venables, 1963). Given this reversal of correlation between normals and psychotics, it should follow that normals with high P scores would also show this "reversed covariation", and Claridge & Birchall (1978), Claridge and Chappa (1973) and Robinson and Zahn (1979) have indeed shown results along these lines. The P scale certainly differentiates normals from psychotics, in the expected direction, and within the psychotic group, more seriously ill patients have higher P scores than less seriously ill patients (Verma & Eysenck, 1973). However, as Block and Bishop have pointed out, groups such as criminals and psychopaths tend to have higher P scores than do schizophrenics or manic depressives, and they conclude that this constitutes a strong argument against the interpretation of P as psychoticism. This argument is of doubtful value. Psychotics are differentiated from the other groups in many ways which may cause a lowering of their P scores. For one thing, psychotics are institutionalized, and our general finding has been that institutionalization strongly increases L (dissimulation) scores, regardless of the reasons; thus patients in hospitals who are there for physical reasons tend to have very much heightened L scores, and as the L score is negatively correlated with the P score this inevitably means a lowering of P scores. Again, psychotic patients in hospital are invariably being treated with various kinds of drugs, and the influence these may have on their scores remains indeterminate. What does characterize psychotics on the EPQ is a combination of moderately high P scores and inordinately high L scores; looking at P scores by themselves gives them lower values than prisoners and psychopaths. However, prisoners and psychopaths usually have low L scores, so that there is no artificial lowering of their P scores.

It is not suggested that these studies, and the many others that could have been mentioned, prove the correctness of the identification of P with psychoticism. They are quoted here in order to illustrate the possibility of attacking the problem in an experimental and psychometric manner, and generating data which may not be conclusive, but are certainly relevant to the problem in question. Opponents of factor analysis often argue that the naming of factors is arbitrary and subjective; this may often be true, but it is not necessarily so. Hypotheses about the nature of a factor can and should be tested, and the methods suggested above are only some of those which can and have been used in this context.

Altogether, my contention has always been that factor analysis is a necessary but not a sufficient method of determining the major dimensions of personality. There is too much subjectivity involved in the location of axes, the selection of tests, and the decisions regarding numbers of factors and other important matters to give results which can be regarded as the objective building stones of a proper taxonomic system. Factor analysis has to be based on carefully thought out theories, linked with a great deal of factual information, and has next to be subjected to experimental procedures designed to test these hypotheses, and their links with the results of factorial investigations. The development of the P scale may serve as an example of this integration of psychometric, theoretical and experimental approaches; it is certainly far removed from the overly simplistic factorial analyses that are so characteristic of the literature on personality.

### *Dimensions of Personality and Behaviour*

The various dimensions of personality discussed above are certainly related quite intimately with many aspects of social functioning, such as neurosis (Eysenck & Rachman, 1965), smoking (Eysenck, 1980), criminality (Eysenck, 1977a), sexual behaviour (Eysenck, 1976b), education (Eysenck, 1978), political ideology (Eysenck & Wilson, 1978) and many others (Wilson, 1981). These relationships are not only correlational, but also causal, at least in the sense that from the causal theories of personality briefly described above it is possible to make testable predictions in all these fields, and on the whole these predictions have been borne out. This is an important aspect of any wide-ranging theory of personality that should recommend it to the attention of social and other psychologists who are concerned with the general applicability of personality theories. A theory which makes predictions testable in the laboratory and also in everyday life situations is clearly more than a statistical artifact, and deserves to be regarded as an integral part of general psychology. This brings us back to Cronbach's (1957) thesis regarding the two disciplines of scientific psychology. The wide applicability of an integrated personality theory to experimental, social, educational, and industrial psychology, demonstrates the essential correctness of Cronbach's statement.

### *A Kuhnian Paradigm in Personality Research*

One powerful reason why this highly desirable change in the relationship between the experimental and psychometric approaches to psychology has not so far taken place is the unfortunate fact that, as Kuhn and other philosophers of science have observed, psychology is essentially pre-paradigmatic. In other words, psychology has not advanced to the point where agreed paradigms exist,

are universally taught, and are generally applied by practising psychologists (Barnes, 1982). Such a failure to develop paradigms may have two reasons, and it is interesting and relevant here to look at these in an effort to determine which is the more likely one to obtain in this field. The first possibility is that the field is so inclusive as to make it quite impossible to point to any set of variables as constituting a paradigm. Such a belief apparently underlies the custom of textbook writers in the field of personality, already mentioned earlier in this paper, of presenting the systematic views of many different authors in separate chapters, without presenting the reader with any way of deciding between the differential truth-values of these many approaches. It is clear from a reading of such texts that many of the theories involved, such as the Jungian, the Freudian, etc., have no experimental or empirical support whatsoever, and are presented rather as the views of different philosophers are presented in textbooks on the history of philosophy. Along these lines it is clear that no agreed paradigm will ever be reached.

If we decide, instead, to eliminate all theories which do not have strong empirical support, which do not specify testable predictions, and which do not in other ways obey the dictates of what is normally considered as representative of the scientific method, we can immediately discard most of these writings as being philosophical rather than scientific, and concentrate on those which do in fact have some pretensions to empirical support. In a careful survey of large bodies of empirical research, Eysenck & Eysenck (1984) demonstrated that while there no doubt are many additional traits and dimensions of personality, there is practically universal agreement on the importance of the three factors here designated as P, E and N. There is of course still argument regarding the precise nomenclatures to be adopted, the precise extent of the domain over which each of these three superfactors extends, and other questions which are soluble according to the methods of ordinary science, as Kuhn calls it. Nevertheless, there clearly does exist a paradigm based on many different kinds of empirical investigations, and specifying both experimental and psychometric methodologies to be used in testing and extending the scheme. In view of this fact, why is the scheme not more universally accepted and used? There are several reasons.

In the first place, many psychologists receive no proper training in the psychometric procedures involved (e.g. factor analysis, discriminant function analysis, etc.), or in the experimental and psychophysiological measures to be used. The position is similar to what it would be in physics if the majority of physicists refused to learn the principles of the calculus; clearly until such knowledge is universal, it is practically impossible even to argue the case for and against a particular theory employing the calculus, or factor analysis, or discriminant function analysis. Psychology will not cease to be a pre-paradigmatic science until each practitioner receives the proper training in the mathematical and statistical bases underlying any kind of model building (Eysenck, 1981).

Another powerful reason is the tendency of psychologists to prefer originality to what Kuhn calls "puzzle solving" of ordinary science. New personality tests are constantly put on the market, although it is very doubtful whether in actual fact they measure anything other than odd combinations of P, E and N. Eysenck & Eysenck (1984) have demonstrated that many of the more widely used measuring instruments in the field of personality do in fact contribute very little if anything above measures of P, E and N, including the MMPI, the 16PF, the CPI, and many others. The fact that the variance is distributed oddly in these

many instruments makes it impossible or at least difficult to interpret findings of studies in which they are used in such a way as to make them truly additive, in the way that scientific findings should be additive. Thus inter-test comparisons are rare and depend to a great extent on subjective judgements and interpretations. It would be most desirable for psychologists to get together, decide on standard measures of P, E and N, and require every new test to show to what extent it is merely another measure of P, E and N, and to what extent it genuinely measures something lying outside these three superfactors. In that way it should be possible to achieve a much greater degree of uniformity, and hence the preconditions for the development of a paradigm, than is possible at the present time. It is not necessary for all psychologists to agree on the theories relating to or underlying these three major dimensions; what is required is merely a recognition of the fact that practically all the large-scale studies of personality that have been done in the past 50 years end up with these three major factors (as well of course as with many other, minor and much less replicable ones). A paradigm is already available in this field were psychologists only ready to embrace it!

Given that the dim outlines of a paradigm are now recognizable, and that future progress along scientific lines is almost certainly dependent to a large extent on the recognition of this paradigm by psychologists working in this field, we may emphasize the way in which ordinary science, devoted to problem solving of the Kuhnian kind, may now advance and help establish the paradigm on a firmer basis.

The most important aspect in my view is work on the parametric aspects of the many different kinds of deductions that can be made from the general theory. To take but one example, let us consider the relationship between personality and Pavlovian conditioning. Two alternative hypotheses were advanced by Spence (1964) who believed that Pavlovian conditioning was correlated with anxiety, considered as a drive, and by Eysenck, who believed that Pavlovian conditioning was correlated with introversion, because of the higher cortical arousal of introverts. Both sides produced experimental studies supporting their view and disapproving that of the opposition (Eysenck, 1981). In the end, it was found that the reason for this apparent opposition lay in the divergent nature of the way in which experimental subjects were treated in the different laboratories. Spence used a methodology producing the strongest degree of anxiety in his subjects, failing to reassure them about the possibility of receiving electric shocks, having all the apparatus openly visible, etc., whereas Eysenck reassured subjects about the purpose of the experiment and the complete absence of electric shocks, had no apparatus visible to the experimental subjects, and generally reduced their anxiety to a minimum. Under these circumstances it is small wonder that differential degrees of anxiety played an important part in the conditioning of Spence's subjects, but none in the conditioning of Eysenck's subjects. Equally, the very large differences in conditionability of extraverts and introverts noted by Eysenck were wiped out by the overwhelmingly strong cortical arousal produced by the autonomic activation of Spence's subjects.

Taken together with the application of Pavlov's principle of transmarginal inhibition, which reverses the correlation between strength of UCS and speed of conditioning after a given point on the intensity scale has been reached (Eysenck & Levey, 1972), these findings tell us a lot about the importance of looking at the parameters involved in any given type of experimental tests. Many other examples are given by Eysenck (1967, 1981) of the importance of the study

of parameters; a proper understanding of the theory, and any improvement must await a detailed study of such questions as these. Curiously enough, problem solving studies of this kind, which are the bread and butter of scientific research in the hard sciences, are not usually regarded with favour in psychology, where subjectivity, putative originality, and general freedom from discipline are regarded with much favour. Even studies believed to be replications of earlier ones usually deviate from these in important ways, making it impossible to tell whether or not the replication has been successful. Altogether this mixing up of the notion of replication and the notion of parameter investigation is one of the banes of research in psychology. Only by recognizing clearly whether we are dealing with a replication or an investigation of different parameters can we resolve this problem, and begin to arrive at this paradigmatic stage which Kuhn so clearly considers essential for the recognition of a particular discipline as scientific.

### Conclusion

To summarize briefly what has been said, we would argue the following.

1. The study of individual differences forms an important area of psychology which is complementary to experimental psychology.
2. Personality differences enter into practically all studies of experimental, social, clinical, educational, or industrial psychology, and their inclusion in research designs is mandatory.
3. There is considerable evidence for the consistency of behaviour and conduct which underlies personality variables, both across situations and across time (longitudinal consistency).
4. The three major dimensions of personality (P, E and N) emerge from practically all large-scale studies of personality that have been carried out in the European and Anglo-American context.
5. The same dimensions of personality appear in many different cultures, and are therefore truly universal.
6. The major dimensions of personality have a strong genetic basis, and much is already known about the genetic architecture of P, E and N.
7. The consistency, universality and genetic basis of personality all argue that there must be psychophysiological, anatomical, neurological and hormonal factors involved underlying the phenotypic behaviour, and much evidence exists to show that theories regarding these biological components of personality are probably along the right lines.
8. The identification of the major dimensions of personality is not as subjective a procedure as is sometimes suggested, and the involvement of experimental methods along the lines of criterion analysis or proportionality analysis methods can test specific hypotheses in this line also.
9. It is suggested that a Kuhnian paradigm exists now in the field of personality research, and that the best strategy for a scientific advance would be recognition of the existence of the paradigm, and the adoption of "ordinary science" problem solving methods to expand and consolidate this paradigm.

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