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Is There a Paradigm in Personality Research?[1]

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It has often been suggested (e.g., Barnes, 1982; Kuhn, 1974) that the major difference between the hard sciences and the social sciences is the absence of paradigms in the latter. In his original work Kuhn (1970) was by no means as clear or consistent as might be wished in his definition and use of the term (cf Masterman, 1970), but later on Kuhn (1974) has recognized the problem and tried to eliminate it. Our use of the term here will be similar to that of Barnes (1982) who employed it to denote "an accepted problem-solution in science, a particular concrete scientific achievement." This is the sense of the term which, in his most recent work, Kuhn conveys by the term "exemplar."

The lack of a paradigm in psychology is particularly obvious and instructive in the personality field. Following the example of Hall and Lindzey (1970), most textbooks now simply give a set of chapters organized around one particular author, explaining his theories, quoting a few examples of work more or less relevant to it, but eschewing the scientifically important and indeed essential job of judging the *adequacy* of the theory in terms of the empirical work devoted to it and comparing the adequacy of one theory along these lines with that of all the others. Thus what we have is not the evolution of a paradigm, but a Dutch auction in ideas, alien to the spirit of science, and conducive to arbitrary choice in terms of existing prejudices on the part of the student. Not along these lines will we ever arrive at a paradigm, or a scientific resolution of the problems of personality research (Eysenck, in press-a).

Yet I think it might be claimed that we do in fact have the beginnings at least of a paradigm in the personality field, in terms of a descriptive and causal system of concepts centered around the three major dimensions of personality which I have called psychoticism (P), extraversion–introversion (E), and neuroticism–stability (N). The results of literally

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hundreds of factor analytic studies, starting with very different premises and hypotheses, carried out by psychologists of quite different theoretical orientation, located in many different countries, and using different methods of analysis and rotation, have practically always found major dimensions corresponding to E and N, and often P as well (Eysenck & Eysenck, in press). Royce and Powell (1983), after a thorough review of all the available evidence, come to a similar conclusion, although they use a slightly different nomenclature to identify these three major dimensions. Evidence shows that these dimensions can be identified in animals as well as humans (e.g., Broadhurst, 1975; Chamove, Eysenck, & Harlow, 1972), and that they can be identified in many different countries and cultures, from Hong Kong to Uganda, and from Japan to India (Eysenck & Eysenck, 1983). Furthermore, at least two of these three factors were already known and described by the ancient Greeks, in the form of the "four temperaments," and trace their history through men like Immanuel Kant and Wilhelm Wundt to modern times. We thus seem to have here at least the beginnings of a paradigm, particularly now that theories exist regarding the causal (biological) factors determining a person's position on these three factors, largely along genetic lines (Fulker, 1981).

One possible objection to the easy acceptance of this paradigm might be that there are anomalies and empirical failures of the theories in question to generate verified predictions. We shall deal with some of these objections presently, but note an answer given by Barnes (1982) to the question: "How does acceptance of a paradigm indicate problems for research; and how does the paradigm itself actually serve as a resource for the scientist?" (p. 46). His reply is that:

> The answer 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, which may in some cases amount to but a single application. In agreeing upon a paradigm scientists do not accept a finished product, rather they agree to accept the basis for future work, and to treat as illusory or eliminable all its apparent inadequacies and defects. Paradigms are refined and elaborated in normal science. They are used in the development of further problem-solutions, thus extending the scope of scientific competences and procedures. (p. 46.)

In other words, a paradigm is not expected to be perfect in the verification of its predictions, but to be fallible; the work of what Kuhn calls "ordinary science" of the puzzle-solving kind is precisely that of looking at anomalies and trying to reconcile them with the theory, through parametric studies and in other ways.

As Kuhn himself has pointed out (1970, pp. 79–80):

> Every problem that normal science sees as a puzzle can be seen from another viewpoint, as a counter-instance and thus as a source of crisis. Copernicus saw as counter-instances what most of Ptolemy's other successors had seen as puzzles

> in the match between observation and theory. Lavoisier saw as a counter-instance what Priestly had seen as a successfully solved puzzle in the articulation of the phlogiston theory. And Einstein saw as counter-instances what Lorentz, Fitzgerald and others had seen as puzzles in the articulation of Newton's and Maxwell's theories . . . either no scientific theory ever confronts a counter-instance, or all such theories confront counter-instances at all times. (Kuhn, 1970, pp. 79–80)

Barnes comments that "What one scientist sees as an anomaly another sees as a puzzle for the same paradigm—even as a succesfully solved puzzle" (p. 100). Thus the existence of anomalies should be no bar to the acceptance of the paradigm; the existence of such anomalies should merely act as a spur for the puzzle-solving capacities of ordinary science. It is only when all such efforts have failed in a number of different instances that what Kuhn calls a "crisis" is reached, and a "revolution" may be in order. But while anomalies certainly exist in the field here discussed, they equally certainly do not amount to crisis proportions; ordinary science has hardly had time to get to grips with these anomalies!

It may be interesting in this context to look at some of the people who have taken cognizance of anomalies in Eysenck's (1967, 1981) theory of personality, and to see to what extent these anomalies do in fact present a threat to the theory and suggest major modifications of one kind or another. We will then return to a consideration of the implications of our findings for the acceptance of the theory as a paradigm in personality research.

Eysenck's descriptive and causal theory of personality in terms of three major dimensions (psychoticism, extraversion, and neuroticism), in its latest form (Eysenck, 1967, 1981; Eysenck and Eysenck, 1976), has received critical attention from several authors (Gray, 1964, 1970, 1979, 1981; Brebner and Cooper, 1974, 1978; Claridge, 1967, 1972, 1973, 1981, 1983). These criticisms drew attention to certain empirical anomalies which had arisen in the attempt to put the theory to the test, and have in some cases led to suggestions for improvement. There is no doubt about the existence of such anomalies, and indeed the fact that the theory has proved so amenable to the testing of deductions on an experimental basis, supporting the theory in most cases, but also producing anomalies in others, suggests that the theory (unlike the majority of personality theories) is falsifiable and hence "scientific" in the Popperian sense.

Comments in the literature suggest that some readers have come away with the view that some of these critiques, particularly that of Gray, have amounted to an *alternative* theory to Eysenck's original postulation, but close reading of Gray's contribution (1981) does not bear this out. Occasionally, Gray's phrasing can be read to support such a view; thus in his 1981 chapter he speaks of "Gray's (1970) model" (p. 261). Elsewhere (Gray, 1981, p. 267) he speaks of "the difference between the two theories." And again, on page 270 he speaks about "the alternative to Eysenck's theory that has been sketched in the foregoing pages." And on page 260 he speaks of "the construction of a new theory."

Elsewhere, however, Gray (1981) speaks of outlining "a few ground-clearing operations which may smooth the way for an eventual new theory" (p. 260). And on page 270, he asks "about the alternative to Eysenck's theory that has been sketched in the foregoing page: assuming that it can indeed deal with the anomalies faced by Eysenck (as has been argued), can it nonetheless also account for the data that *fit* the arousal level theory of E-I? . . . I think it is clear that the answer to this question must be 'no.'" Thus there seems to be an ambiguity in Gray's attempt (which also, in a less extreme form, appears in the contributions by Brebner and Claridge). At times it would appear as if they were suggesting new theories to take the part of the original set of hypotheses, but these alternative theories deal only with a very small part of the experimental evidence, and they clearly fail to account for the great majority of findings that have supported the original theory (Eysenck, 1967, 1976, 1981). As an indication of areas where anomalies exist, and where new thinking may be necessary, these contributions have to be welcomed. However, if they are presented as alternative theories, then we must take into account, not only the few anomalous findings, but also the large number of supportive findings, and see to what extent the new suggestions fit in with, and can explain, the majority of the findings. I venture to suggest that Gray, Claridge, and Brebner have all made important suggestions and contributions, but that these can be incorporated into the original theory, and do not necessitate the formulation of a new and alternative theory.

Let us first consider the essential feature of Gray's "alternative theory." This is presented in Fig. 1. As will be seen, he has rotated through 45° the two major axes respresenting E (extraversion–introversion) and N (neuroticism–stability), so that now we have two major dimensions of *anxiety* and *impulsivity,* with the former N being a mixture of anxiety and impulsivity; stability, a state lacking both anxiety and impulsivity; extraversion a mixture of impulsivity and lack of anxiety; and introversion a mixture of anxiety and lack of impulsivity.

In this scheme, increasing levels of anxiety reflect increasing levels of sensitivity to signals of punishment, signals of nonreward and novelty. There is an underlying physiological system, called by Gray (1982) the "behavioral inhibition system, or BIS," activity in which controls the level of anxiety, and which consists of an interacting set of structures comprising the septo-hippocampal system, its monoaminergic efferents from the brain stem and its neocortical projection in the frontal lobe.

Increasing levels of impulsivity reflect increasing levels of sensitivity to signals of reward and signals of nonpunishment.

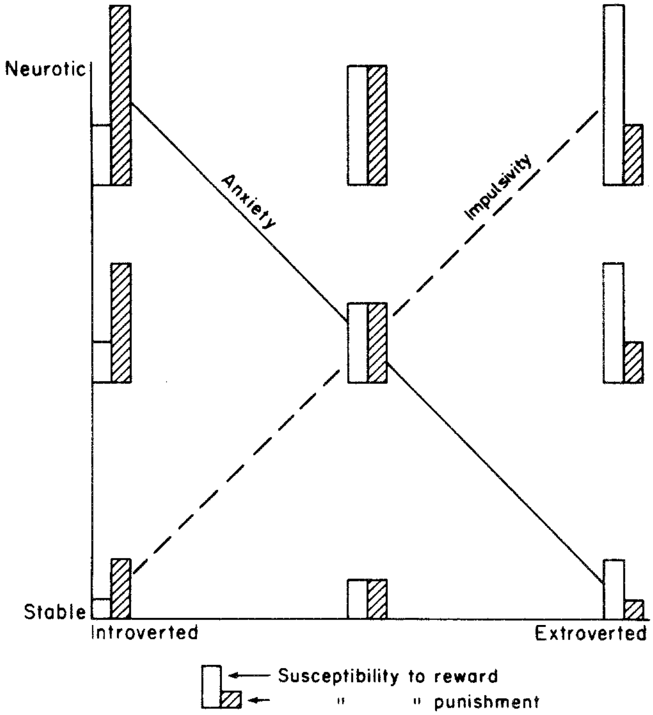

FIGURE 1

> "There is an underlying physiological system, independent of that which underlies anxiety, activity in which controls the level of impulsivity. Little progress has been made in describing the structures that go to make up this system" (p. 261).

In this system, as Gray emphasizes:

> E-I and N are secondary consequences of the interaction between the anxiety and impulsivity systems as defined above. Individuals in whom the BIS is relatively more powerful than the impulsivity system (i.e., individuals who are more sensitive to signals of punishment, signals of nonreward and novelty than they are to signals of reward and nonpunishment) are introverted; those in whom the reverse relationship holds are extraverted. Thus E-I reflects the *relative* strength of the two systems. N, in contrast, reflects their *joint* strength: increments in the sensitivity of either system provide increments to N. (p. 261)

This suggestion raises acutely the possibility of "rotating fators" in what appears to be a rather arbitrary manner. Mathematically, of course, the two solutions are equivalent, as Gray points out, and hence on purely mathematical grounds there is nothing to choose between them from the point of view of *description;* a transformation equation can be written to generate identical predictions. However, there are powerful reasons for opposing the rotation, and for believing that the original position of the axes is psychologically much more meaningful, as well as psychometrically preferable. The first and major point to be noted is that literally hundreds of large-scale investigations by many different people, from many different countries, adopting many different theoretical points of view, choosing many different types of items, have universally found factors identical with, or at least similar to, E and N (and usually P). A search through the factor analytic literature relating to intercorrelations between items on the major personality inventories in use (MMPI, 16PF, CPI, etc., Eysenck & Eysenck (in press)) demonstrates that in every case the P, E and N variables emerge, usually as second-order factors; in no single investigation is there any trace of the rotated factors Gray is suggesting. In other words, it is universally found that traits relating to neuroticism–stability cluster together, and that traits relating to extraversion–introversion cluster together, thus defining two orthogonal factors; there is a dearth of items or traits corresponding to the positions given by Gray to his anxiety and impulsivity factors.

It is of course possible that a diligent search might discover items or traits in these quadrants which might generate axes as demanded by Gray's hypothesis, but this certainly has not been done, and the overwhelming evidence suggests that in fact nothing of the kind would be found. Add to this the fact that the P, E, and N dimensions have been found not only in European countries, and in the United States, Canada, and Australia, but also in Third World and Communist countries (Eysenck & Eysenck, 1983), with indices of factor comparisons usually in excess of .95, often in excess of .98, and it will be seen that the factorial evidence for the originally postulated factors is so strong that the demanded rotation seems arbitrary, unconvincing, and psychometrically unacceptable.

There are many other difficulties with Gray's system. There are several measures of anxiety, from the original Taylor Anxiety Scale to late ones by Cattell, Spielberger, and others, but when these are correlated with E and N, none of them fall into the space suggested by Gray (as indeed he himself recognizes). For the great majority there is a very high correlation with N, and a rather small one with introversion; this would correspond to rotation not of 45°, but more like 10°, or at most 15°. That is one great difficulty about Gray's "anxiety" dimension; but there is another one. As Eysenck (1960) has pointed out, there are two "anxiety" factors, one cognitive, the other dealing with physiological expressions of anxiety (sweating, dizziness, etc.) (see also Buss, 1962; de Bonis, 1968; Hamilton, 1969; Schalling, Cronkolm, Asberg, & Espmark, 1973). Most of the anxiety scales in commercial use measure the cognitive rather than the physiological aspects of anxiety, but it is the former which correlates with introversion, the latter which correlate with extraversion. In other words, anxiety is almost collinear with neuroticism when both aspects are taken into account;

Gray's theory would seem to demand that it is the physiological rather than the cognitive aspects of anxiety which would lie on the axis shown in Fig. 1. The evidence simply does not support any such view, and hence even if we agree to a rotation of 45°, the term "anxiety" would certainly not characterise the resulting axis. But it is the use of the very popular term "anxiety" in relation to this axis which has made it acceptable to many people. When it is realized that the axis really has no name and very little relationship to "anxiety" as normally understood and measured, then it will be clear that the rotation has little to recommend it.

Other difficulties attend the "impulsivity" axis. There is some reason to believe that items in anxiety scales cohere closely together and define what might be considered a single second-order factor (although even that is doubtful; many studies have found separate factors within that field). But recent research has made it clear that "impulsivity" is not a unitary dimension, but breaks down into at least four separate and not highly correlated factors (Eysenck, 1983a). Even if we arbitrarily chose one of these factors to occupy the position in Gray's diagram orthogonal to "anxiety," we would have to bear in mind the fact that most of the impulsivity factors correlate more highly with P, than with E or with N; in other words, impulsivity does not lie in the position assigned to it by Gray, but would be very largely removed from the plane of the paper altogether. When it is added that Gray himself, as already shown, admits that little is really known about the hypothetical physiological system, independent of that which underlies anxiety, and activity in which controls the level of impulsivity, it will be seen, as he also admits, that the dimension, and its name, have been placed in the diagram only because something was needed that was at right angles to the anxiety dimension. Given that the position of the anxiety dimension is very doubtful in the first instance, to posit something as insubstantial as "impulsivity" because it might lie at right angles to anxiety does not seem a persuasive reason for abandoning the original E–N system.

Gray himself, of course, is well aware of some of these difficulties. As he points out, "it is much harder to argue that the particular biological correlates of Imp that have been found are of a kind that Gray's (1970) model can predict. So the rotation may be correct, but the postulated psychophysiological mechanisms that prompted this rotation wrong" (p. 261). But if the major reason for the rotation was a postulation of certain psychophysiological systems and mechanisms, then we seem to be involved in a vicious circle, in which the wrong system prompts the wrong rotation.

Having thus concluded that the support in favor of Gray's rotational system is decidedly weak, we must now take issue with his postulation of a "behavioral inhibition system," and the evidence showing that susceptibility to reward, and to punishment, may have powerful correlations with personality. Gray quotes some studies to support his view that introverts should show strong conditioning to aversive stimuli, extraverts stronger conditioning to appetitive stimuli. He quotes some evidence in favor of this view, but the Nicholson and Gray (1972) experiment is only indirect (stimulus generalization of responding for reward was wider in extraverts) and the three studies he quotes in addition are far from conclusive. Seunath (1975) showed that on a pursuit rotor task, introverts learn better when punishment is used and extraverts when reward (money) is used. However, pursuit rotor learning is hardly to be equated with Pavlovian conditioning, and may hence be quite irrelevant. Kantorowitz (1978) showed conditioning superiority of extraverts in one context (conditioning of sexual arousal) and the superiority of introverts in a second context (the conditioning of detumescence) (p. 121). (Gray (1981) erroneously states that this study "demonstrates the superior sexual conditioning . . . in introverts than extraverts" (p. 167). This is the opposite of the actual findings, and may be confusing to readers of his chapter.) This study is certainly in agreement with Gray's theory. However, Barr and McConaghy (1972) found a positive correlation between appetitive and aversive conditioning. They conclude that "the evidence of this study supports the hypothesis that a relationship in conditioning performance exists across response systems both in a given conditioning procedure and between quite different conditioning procedures" (p. 226). They considered this evidence for the existence of a general factor of conditionability, and the results are certainly not in agreement with Gray's theory.

Another study supporting Gray's theory is one by Gupta and Nagpal (1978), using Taffel's (1955) verbal conditioning task and finding that introverts learn better when punished for wrong responses, but extraverts when rewarded (with social approval) for correct responses. Also supportive is the study by McCord and Wakefield (1981), showing that praise and blame in a school situation worked better (in the sense of producing greater achievement), with extraverted and introverted children, respectively.

In addition to the study by Gupta and Nagpal (1978), mentioned by Gray, there are several other interesting studies from the same department (Gupta, 1976; Nagpal & Gupta, 1979; Gupta, in press). Using social approval (saying "good") and punishment (shock) as positive and negative reinforcers, these various studies show quite clearly that with positive reinforcers extraverts condition better, with negative reinforcers introverts. Note, however, that in the Gupta and Nagpal (1978) paper, where impulsivity and sociability were independently measured and related to the effects of positive and negative reinforcement, both showed roughly equal effects, in the same direction; this is unexpected in Gray's hypothesis, and suggests that the major factor here is extraversion, rather than impulsivity.

Also in agreement with the hypothesis that the differential effects of positive and negative reinforcement are related to extraversion and introversion, respectively, is the fact that studies using stimulant and depressant drugs have on the whole strongly supported Eysenck's drug postulate (Eysenck, 1983c). This postulate is illustrated in Fig. 2 as showing that various types of drugs have actions displacing a given person from its position on one of the P, E, and N axes, in one direction or the other, dependent on the type of drug involved. The work of Gupta and his colleagues on verbal conditioning has been extended to take into account drug action (Gupta, 1970, 1973; Gupta & Gupta, in press; Gupta, in press) with results which support both the Eysenck and Gray hypotheses. Drug studies are important because they might hold the key to a possible crucial experiment to decide between these two hypotheses.

In general we may say that these studies produce some mild support for a view linking extraversion with susceptibility to reward, and introversion with susceptibility to punishment. There is nothing in these studies to suggest a *rotation* of the E–N system; in all of them it is the extraversion–introversion axis which has been found to be related to these different kinds of susceptibility. If Gray's hypothesis suggesting rotation were correct, then similar and equal correlations should have been found with N and E, respectively. There certainly is no evidence for such an hypothesis, and the existing data could be incorporated in Eysenck's system by stating that the "behavioral inhibition system" is *directly* related to introversion. This indeed makes perfectly good sense in terms of Eysenck's original hypothesis of greater *cortical* arousal being

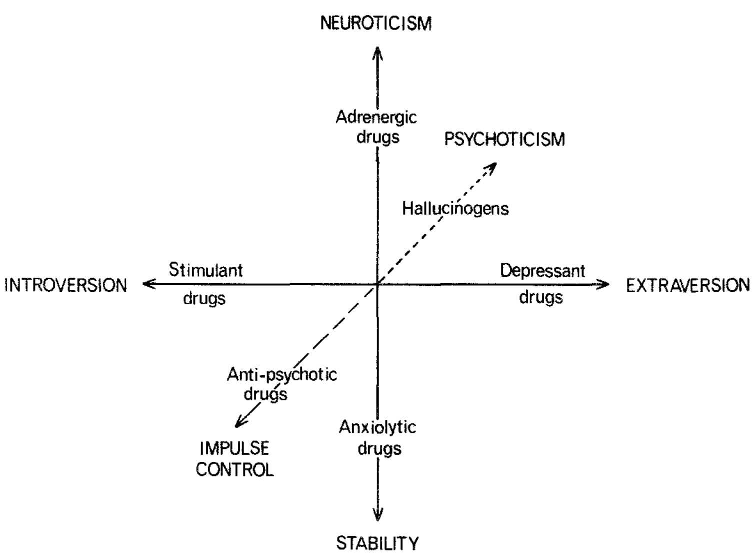

FIG. 2. Psychotropic drugs and personality.

characteristic of introverts, and producing *behavioral* inhibition. What might be said of Gray's theory is that it has given much physiological support, and greater refinement to this hypothesis, and has suggested an amplification of Eysenck's original theory relating conditioning to extraversion–introversion. Such an extension of the original theory is certainly a valuable and important development, and if further support could be found for it, both along physiological and behavioral lines, then it should be accepted and incorporated in the original theory. There is no reason, however, to suggest that acceptance of this extension would necessitate a rotation of the axes, as suggested by Gray.

The work of Brebner and Cooper (1974, 1978) and Brebner and Flavel (1978) also presents an extension of Eysenck's original theory. They propose that one should distinguish between central mechanisms involved in stimulus analysis (S) and in response organization (R). Both mechanisms can be in a state of inhibition or excitation, generating four variables to describe the state of the organism, with S excitation corresponding to arousal level, and R inhibition to reactive inhibition. It follows that introverts are thought to be normally higher on S excitation and extraverts on R inhibition. It is also suggested that extraverts are higher on R excitation and S inhibition.

The evidence quoted by these authors is certainly suggestive, and it is obviously not impossible that arousal of one system may carry with it inhibition of another. At the moment this cannot be termed more than an interesting hypothesis, and it is to be hoped that further evidence will be collected regarding it, very much as one would like to see further evidence collected to support or refute Gray's hypothesis of the differential susceptibility to reward and punishment of extraverts and introverts. The evidence in neither case is sufficiently strong to make the acceptance of these emendations to the original theory mandatory, but, if shown to make further predictions borne out by facts, they would certainly add importantly to the bare bones of the original theory.

Much the same must be said about the work of Gordon Claridge, particularly in relation to psychoticism. The theory he suggested originally (Claridge, 1967) was based on the idea that psychosis may involve, not a simple shift in, say, emotional arousal, but represents instead a much more complex dissociation of CNS activity. He suggested that in the schizophrenic, physiological mechanisms which are normally congruent in their activity, and thereby maintain integrated CNS function, become uncoupled and dissociated. He concentrated on two aspects of central nervous functioning which he considered to be particularly involved in this uncoupling process; these were *emotional arousal,* on the one hand, and, on the other, a mechanism concerned with the *regulation of sensory input,* including variations in perceptual sensitivity and in the broadening and narrowing of attention. He labeled this the "phenomenon of reversed covariation'' (Claridge, 1981) and the many studies reviewed in this last reference (e.g., Claridge, 1972; Claridge & Birchall, 1973; Claridge & Chappa, 1973; Robinson & Zahn, 1979; Venables, 1963) give ample support for the hypothesis.

What is notable is that this is not just a theory of schizophrenia, but also of psychoticism; normal high P scorers behave like schizophrenics, low P scorers like normals in these various tests, thus supporting very strongly the identification of the factor as one of ''psychoticism.'' At the same time Claridge's work supports the drug postulates which form part of the general P–E–N paradigm and which are illustrated in Fig. 2. According to these postulates, psychotropic drug actions are collinear with the personality dimensions, and act in such a way as to temporarily shift a person's position on these axes in predictable directions. Thus LSD-25 would be classed as a hallucinogen, and should in normal subjects produce psychoticising effects on the Claridge type of test. Claridge (1972) and Claridge and Clark, (1982) have provided good evidence for this. Thus Claridge has given a most impressive theoretical account linking the descriptive dimension P with a causal physiological hypothesis.

These studies constitute an interesting use of a variant of the method of criterion analysis (Eysenck, 1950, 1952) which I originally introduced to solve problems such as those related to the interpretation of the factor of ''psychoticism.'' Many people, e.g., Davis (1974), Bishop (1977), Block (1970a, 1970b) and even Claridge himself (1983), have doubted whether the scale actually measures the diathesis related to psychosis, and have suggested alternative interpretations, e.g., psychopathy, paranoia, etc. The particular variant of criterion analysis adopted by Claridge might be referred to as the ''proportionality criterion.'' This may be put in the form of an equation, which states that if the P scale actually measures psychoticism, then any objective test which discriminates between psychotics and normals should also discriminate between high and low P scorers. In other words, psychosis : normality = high P : low P scorers. The work of Claridge's just referred is one example; another is the work of Gattaz (Gattaz, 1981; Gattaz and Seitz, in press). This is concerned with a human leukocyte antigen (HLA B-27), which is found significantly more frequently in schizophrenic patients than in healthy controls (Gattaz et al., 1980; McCuffin, Farmer, & Yonace, 1981). If P does indeed measure a diathesis relevant to psychosis, and particularly schizophrenia, then we would expect that HLA B-27 would be found significantly more frequently in schizophrenics with high P scores, as compared with schizophrenics with low P scores, and in normals with high P scores as compared with normals with low P scores. Both these deductions have been verified by Gattaz, and thus constitute strong support for the hypothesis. The work of Gattaz and Claridge does not constitute the only evidence available to strengthen the hypothesis of truly dealing with ''psychoticism'' rather than psychopathy or paranoia; this is not the place to go into this question. I merely want to draw attention to the need for some such use of criterion analysis in order to establish the interpretation of factors in an objective manner, and to suggest that the proportionality criterion may be a form of criterion analysis particularly useful in this connection.

Of equal interest are deductions from the genetic hypothesis of the psychotic ''Erbkreis.'' If the P scale does indeed measure psychotic diathesis, then we should be able to look at first-degree relatives of schizophrenics compared say to a control sample of first-degree relatives of neurotics, and predict that the former group should show elevated P scores, as compared to the latter. One would also expect that measures of Claridge's ''phenomenon of reversed covariation'' would show similar differences between relatives of psychotic and neurotic probands. Results in support of these hypotheses have been reported by Claridge, Robinson, and Birchall (in press), and it is particularly interesting that the rather unusual pattern of psychophysiological responses ''was especially evident in a small subgroup of schizophrenics' relatives whose personality profiles tend to differ in the predictable direction, towards greater psychoticism.'' Findings such as these powerfully reinforce the interpretation of psychoticism as being truly related to psychosis.

The work of Gray, Brebner, and Claridge may therefore be looked upon as mainly concerned with an elaboration and extension of the original paradigm, and as in no way incompatible with it. The theoretical conceptions contained in these extensions of the original theory are potentially very important, and may help to make the theory more readily aligned with experimental facts. The degree to which this is true is entirely a matter of experimental demonstration, of course, and I do not intend here to predict the degree to which these emendations will be supported by future research.

We must now turn to the question of anomalies which threaten the original paradigm, and might form the basis of suggestions for alternative theories. It is of course a commonplace in the history and philosophy of science that all theories give rise to anomalies, Newton's theory of gravitation being no exception, and indeed illustrating very well the fact that such apparent anomalies can often be reduced to actual illustrations of the workings of the theory by further research. Eventually, of course, all theories encounter anomalies with which they cannot deal, such as the precession of the perihelion of Mercury in the case of Newton's theory, and a Kuhnian revolution takes place. However, such revolutions usually occur after a long time of ''ordinary science,'' during which the puzzle-solving abilities of scientists have had ample scope in trying to reduce the apparent anomalies to actual instances of the theory in question.

Let us be quite clear on this point. The fact that anomalies exist does not in any sense disprove the theory, nor is it fatal to its continued use. If this were otherwise, then no scientific theory of any kind would exist, not even in the hard sciences, and scientific endeavor would come to a full stop. The aim of normal science is to try to understand and if possible abolish the anolamous status of these apparently recalcitrant facts, by parametric and other investigations, by the addition of new laws, or by slight changes in the original theory. Psychologists have shown a tendency to shun the work of normal science and to reject theories without attempting to see to what extent it might be possible to rescue the theory by detailed experimental investigation of the facts in contention. This is one of the reasons why psychology (and the social sciences in general) have failed to produce any paradigms; paradigms require this puzzle-solving work of normal science in order to demonstrate their value.

Gray (1981) lists a number of apparent anomalies in the Eysenck system, and it may be interesting to see to what extent these represent real anomalies, and to what extent their status may be less antagonistic to the theory in question. In his account of the change from an inhibition to an arousal theory of extraversion–introversion, Gray cites the work of Spielman (1963), and Eysenck's (1964) replication, showing that extraverts show more pauses in a tapping task than do introverts, as an example of phenomena which, while they were predicted by the inhibition theory, could not be explained by arousal theory and would hence constitute an anomaly for the revised theory. This does not seem acceptable. We may look upon a continued tapping task as an example of *vigilance*. Gray cites the literature on vigilance as one of the strong supports for arousal theory, and it is difficult to see why routine tapping over a period of time should not be considered part of the general concept of "vigilance." Looked upon in the most general terms, vigilance simply means the maintenance of attention (usually in perceptual tasks) over a period of time; arousal is supposed to facilitate the maintenance of vigilance, and hence introverts do better than extraverts, who show increasing errors of omission (with introverts sometimes showing the opposite errors of commission). Tapping would seem to fall into this general pattern, and the work of Frith (1967a, 1967b) shows that this hypothesis can give rise to testable predictions, e.g., following the drug postulate that a stimulant drug can produce an increase in "vigilance," and prevent a decline in performance on the test. It is of course possible that this interpretation is erroneous, but it seems a reasonable one in terms of the definition of "vigilance" usually adopted, and the empirical evidence. This does not seem to be a good example of a true anomaly.

The second example given by Gray is the time-of-day effects, which suggest that introverts and extraverts have different diurnal rhythms in arousal level (Blake, 1967). Revelle, Humphreys, Simon, and Gilliland (1980) suggest on the basis of Blake's data that introverts and extraverts swap places between early morning and late evening. Using an academic-type test similar to the familiar American Graduate Record Examination they tested subjects under three conditions: baseline, i.e., no time pressure and no drug; time pressure; and time pressure plus caffeine. These three conditions represent ascending levels of arousing stimulation in the order given, according to arousal theory. Assuming the usual inverted U relating arousal level and performance, and assuming that introverts are chronically more highly aroused than extraverts, the prediction would be that introverts should outperform extraverts in the baseline condition, but that extraverts should be helped and introverts hindered as one goes from baseline progressively through the other two conditions. Revelle et al. (1980) did find this when testing was done in the morning, but found exactly opposite results in the evening. As Gray comments:

> So, if one assumes that body temperature is a reliable index of arousal level, the combination of Blake's data and those reported by Revelle et al. (1980) compose a striking testimony to the power of the general theory of arousal—but at the same time a dagger that goes to the heart of Eysenckian theory. (p. 258)

The reason for this dramatic mayhem is of course that the theory attempts to account for *stable* features of the personality, whereas here we would seem to have a change from one time of day to another in personality, and the physiological basis for personality.

There are three reasons for not taking these results too seriously. In the first place, as Eysenck and Folkard (1980) have pointed out, there are serious criticisms to be made of the experiment and its interpretation by Revelle et al. These criticisms reduce its evidential value very considerably and make it doubtful whether it can really assume the importance given to it by Gray. In the second place, it seems very arguable whether in fact one can "assume that body temperature is a reliable index of arousal level"; an experimental determination of this would be absolutely essential in order to clinch the matter, and no such investigation has yet been reported. And as a third point, note that the Revelle experiment deals with a task which has never been related to extraversion–introversion in any unambiguous manner. Extraverts and introverts do not differ in tests of the academic-type achievement kind used in the test, and hence the whole experiment is based on a mistaken choice of task. Had Revelle et al. used a vigilance, or a conditioning, or some other task theoretically and experimentally linked with extraversion–introversion, their demonstration would have had much greater relevance to the theory in question. As it stands, their work cannot be interpreted at all in terms of extraversion–introversion, and hence cannot constitute an anomaly.

Altogether, I would doubt whether there is really good evidence for time-of-day effects in relation to extraversion–introversion. The evidence is sparse, indirect, and difficult to interpret. The possibility certainly exists that time-of-day effects will be found, and will affect performance on tasks truly linked with the extraversion–introversion theory of arousal. When a number of such determinations have been carried out, then it will be time to evaluate their contribution to the theory, and their possible status as "anomalies." At the moment the evidence simply does not exist to come to any kind of decision on this point. Eysenck (1982) has reviewed the literature exhaustively and concludes that (1) "precise statements are difficult to make because various physiological and self-report measures of arousal place the time of peak arousal anywhere between approximately 1100 and 2100 hours," (2) "it is probably best to assume that there is more than one diurnal rhythm for arousal," and (3) "recent data suggest that the earlier view that peak performance on most tasks occurs in the middle of the evening is not correct" (p. 146). The whole field clearly is in a state where little is definitely known, and the data of the Revelle et al. (1980) experiment can therefore hardly be considered to constitute a serious anomaly for the arousal theory.

A third point raised by Gray is associated with a criticism voiced by Rocklin and Revelle (1981), namely, that the E scale in the EPQ is not truly identical with the E scale in the EPI, because some of the impulsivity items have emigrated to the P scale. Hence Revelle would regard the new extraversion scale as a simple measure of sociability. There are two errors in this thinking. In the first place, not all impulsivity items have been taken out of the new E scale. In the second place, it has to be recognized that there is a highly significant correlation between sociability and impulsivity, and hence the elimination of some impulsivity items would not mean that the new scale did not include impulsivity as one of its components. To the extent to which sociability correlates with impulsivity, to that extent would a pure sociability scale also measure impulsivity! But most important of all is of course the empirical question of the relationship between EPI and EPQ. Barrett and Kline (1982) have undertaken such a comparison and found a correlation of .83 for E and .91 for N; they conclude that "it would appear that the Eysencks' assumption of scale comparability for E and N is upheld by these results" (p. 78). This would seem to be the most decisive answer to Revelle's point.

Gray's criticism (what he calls "the third crack in the Eysenckian facade") derives from the fact that Eysenck and Levey (1972) found that the correlation between eye-blink conditioning and E was mediated more by the impulsivity than by the sociability items; as Gray points out, a shift of conditionability to the impulsivity dimension would rob the Eysenck theory, as far as its social implications for neurosis and criminality are concerned, of much of its attraction. Fortunately recent work by Frcka, Beyts, Levey, and Martin (in press) shows that eye-blink conditioning is positively correlated with introversion on the EPQ scale, although there is a reversal of this correlation in high P subjects (who of course constitute only a small minority in the population). This reversal certainly poses a problem, as do the results of studies by Beyts, Frcka, Martin, and Levey (1983) using paraorbital conditioning and showing lower levels of conditioning in subjects scoring high on psychoticism. Clearly there is a problem still of integrating the influence of psychoticism on conditioning with previous work using only E and N, but whether these future developments will support or contradict the original hypotheses, and their extension to social conditioning, is a matter on which the last word has certainly not been spoken. Lack of conditioning in high P scorers might even be taken as support of the theory linking lack of conditionability with antisocial behavior.

Note also, as already mentioned, that as far as verbal conditioning is concerned Gupta and Nagpal (1978) found conditioning to correlate equally with sociability and impulsivity.

In now turning to the social applications of conditioning theory, we should note first of all that, as Gray admits, the difficulties he points for Eysenck's theory are not obviated by Gray's new model; as he suggests, the evidence he discusses "is as much a problem for Gray's (1970) modification of Eysenck's theory as it is for this theory in its unmodified form" (p. 263). He attempts to get over this difficulty by suggesting that phobic and other anxiety responses are not in fact acquired through a process of conditioning, but may be *innate*. Gray rejects Seligman's (1971) and Eysenck's (1979) suggestion of "preparedness" for conditioning as explaining some of the failures of Pavlov's doctrine of "equipotentiality," suggesting that this is merely a halfway house to a completely genetic theory. He proposes a view that phobic and other anxiety reactions are simply innate reactions to releasing stimuli (see also Gray, 1979).

This view apparently gains some support from results reported by Rose and Ditto (in press). Using a 51-item fear survey, they collected data from 222 MZ and 132 pairs of DZ like-sexed twins aged 14–34. Factor analysis disclosed seven fear factors and the intraclass correlations for MZ and DZ twins for these fear factors, as well as the heritabilities (h^2), are shown in Table 1. It will be seen that heritabilities vary from a low of .28 (loved one's misfortunes) to a high of .72 (personal death). These data would seem to support Gray's theory.

In looking at the results of Table 1, it is important not to overinterpret the findings. In the first place, the authors have used orthogonal rotation to establish their factors; these are consequently uncorrelated by fiat, and an oblique rotation might have shown (and indeed almost certainly would have shown) that the different factors are themselves correlated, so that at least part of the heritability is attributable to the general factor of neuroticism. This, indeed, is the second weakness of the study; there was no independent measure of neuroticism, and hence we do not know

TABLE 1
HERITABILITIES OF SEVEN FEAR FACTORS[a]

Fear factor	r MZ	r DZ	h^2 (Heritabilities)
(1) Negative social interaction	.50	.28	.44
(2) Social responsibility	.54	.24	.60
(3) Dangerous places	.43	.14	.58
(4) Small organism	.53	.20	.66
(5) Deep water	.52	.36	.32
(6) Loved one's misfortune	.52	.38	.28
(7) Personal death	.52	.16	.72

[a] From Rose and Ditto, in press.

to what extent the genetic causes of general emotional instability were responsible for these specific phobic anxieties investigated. There is no doubt about the strong heritability of neuroticism (Fulker, 1981), and hence the apparent genetic specificity of these seven phobic types of fear may be largely an artifact due to improper statistical analysis.

In rejecting Seligman's theory Gray (1982) also rejects the experimental evidence for it presented by the Upsala school, and while some of his criticisms are no doubt valid, it must be doubtful whether they dispose entirely of the confirmatory evidence presented by these workers. Here clearly is another area where the puzzle-solving capacities of scientists could be put to good use.

However that may be, Gray's theory is only appealing in relation to very specific phobias, and these, as is well known, are relatively rare. What is usually observed is a much more complex picture of phobic fears, anxieties, and depression, based no doubt on an inherited basis of neuroticism and possibly introversion, but going well beyond fears of quite specific and isolated objects or situations. If it would be difficult for Gray's theory to account for such complex phobias, it would seem even more difficult for him to develop a purely genetic theory of the much more frequent anxiety states which are not related to any specific phobic fears, and which are very much more frequently observed in psychiatric practice. It is here that theories of conditioning, and the notion of "preparedness" seem indispensible.

Gray is driven on by his theory to favor Watts's (1971) view of behavior therapy as habituation, rather than as extinction (Eysenck, 1982). "If much of the behavior of the dysthymic is an innate reaction to stimuli to which he is particularly sensitive, it follows naturally that the disappearance of such reactions is due to the habituation of the kind described by Sokolov (1960) and Horn and Hinde (1931)." It is in practice difficult to distinguish between habituation and extinction, but Gray would have to show why, as in the Napalkov (1963) experiment, the repeated presentation of the unconditioned stimulus leads to habituation, that of the unreinforced conditioned stimulus to incubation (Eysenck, 1979). This and similar studies would seem to speak powerfully against the adoption of Gray's hypothesis.

We must next turn to another interesting point raised by Gray (1981), namely, that some empirical findings cannot readily be explained at the moment by the original personality theory. He instances reminiscence (Eysenck and Frith, 1977), but of course many others could be mentioned also. What we find quite frequently is that highly significant and replicated correlations are found in the experimental literature between P, E, or N, on the one hand, and some experimental, social, educational, industrial, or medical finding on the other, which were not predicted, and are difficult to interpret in terms of the theory under discussion. This does not imply, of course, that no causal links will be found in the future, but simply that investigators have not succeeded in doing so up to the present. An example may illustrate such apparently capricious and explicable findings, and also the possibility of accounting for them along perfectly rational, deductive lines. The finding, amply replicated and confirmed in many studies summarized by Eysenck (1980), is that of a relationship between lung cancer (and other forms of cancer also), on the one hand, and N and E, on the other. Low N and high E are apparently correlated with cancer propensity, and this finding did not only seem inexplicable at first but also counter-intuitive. It is known that stress may cause cancerous growths, and it seems likely that emotional instability produces considerable stress for people with high N; one would therefore expect a *positive* rather than a *negative* correlation between N and cancer. As regards the correlation between E and cancer, this is neither intuitively plausible nor improbable; there just does not seem to be any obvious relationship.

In searching for an explanation, Eysenck (1983b, in press-c) made use of two findings which have only come into the public domain fairly recently. The first of these concerns the fact that while *acute* stress does seem to have causal relevance to the development of cancer, *chronic* stress appears to have the opposite effect, i.e., to protect the individual from cancer. Given that neuroticism is likely to be chronically stressful, we may have here an explanation of its negative correlation with the development of lung cancer and other types of cancer (Eysenck, 1983b).

As regards extraversion, it has recently been shown that the immune reaction can be conditioned along Pavlovian lines. Taking this together with the well-established fact that under certain conditions introverts can be shown to condition better and more strongly than extraverts, it may be that introverts are able to protect themselves by a Pavlovian conditioning of the immune reaction, thus preventing the spasmodic and quite frequent emergence of cancer cells from spreading. Admittedly this and the preceding explanation must be considered highly speculative, but both hypotheses are testable, and can hence be refuted or supported by appropriate research. The existence of unexplained correlations is therefore not necessarily an anomaly, nor can it be construed as a criticism of the theory. Such unexplained relationships are a rich and fertile soil for the puzzle-solving abilities of psychologists and may indeed constitute a particularly interesting and important source of inspiration for research (Eysenck, in press-c).

When proper explanations can be found for such wildcat facts, they may in due course join the very large number of experimental findings predicted by the theory, and hence be supportive of them. Eysenck (1967, 1976, 1981) has published long lists of such findings, and it is clear that at the moment no alternative theory exists which could explain the great majority of these findings. I have already quoted Gray's admission that his own revised theory could not explain all these positive instances, and this alone would rule out the revision, insofar as it calls for a substantial change in the theory; new theories are only acceptable if in addition to explaining hitherto unexplained facts they can also succeed in giving an adequate explanation of the large body of information already accounted for by the theory to be supplanted. Where the new theory cannot do this, it is not usually found acceptable as a substitute, although of course it may suggest minor revisions in the original theory (Eysenck, in press b).

The paradigm which is constituted by the various personality theories here mentioned might thus be regarded as a Kuhnian "revolution" in that it produces a picture altogether different from the psychoanalytic-cum-projective alignment that passed as orthodoxy in the past 30 years or so. A characteristic of such revolutions, according to Kuhn, is that it involves a change in the *character of research*. Speculation becomes more acceptable, and novel and radically deviant procedures in interpretations are tolerated more readily. When a new paradigm is accepted, a large-scale reordering of practice and perception occurs, reflecting the requirements exemplified in the new paradigm. Eysenck's theory of extraversion–introversion, as compared with the "orthodox" Jungian theory, is clearly a case in point. No Jungian would have countenanced a theory of extraversion–introversion which involved measures of EEG and evoked potentials, conditioning and extinction, figural after effects and CFF, alternation behavior and vigilance, salivary responses to lemon juice and circadian rhythm changes, tolerance of pain and sensory deprivation, sensory stimulation modulation and sensory thresholds, drug effects and reminiscence, memory retrieval and consolidation of learning, and many, many others.

Another example of such a revolution is the recent work on evoked potential measures of intelligence, particularly the paradigms associated with Hendrickson, Schafer, and Robinson (Eysenck & Barrett, in press).

It has been shown that very high correlations can be obtained between IQ, as measured by standard tests like the Wechsler or the Progressive Matrices, and certain scores on the evoked potential. These correlations, in excess of .8, are impossible to reconcile with the traditional Binet-type paradigms which postulate the IQ as a rather artificial mixture of separate and independent abilities, individual differences in which are largely determined by environmental factors, education, socioeconomic status, the teaching of differential strategies, etc. None of these are very likely to play any part in the genesis of differential evoked potential patterns on the EEG, and hence a revolution in outlook is implicit in these findings, leading to altogether new and different expectations and experiments. What would now concern investigators would be parameter studies related to the positioning and types of electrodes used; the intensity and duration of the stimuli used, and the inter-trial intervals of these stimuli; the type of analysis undertaken of the records, looking at variability, as well as amplitude and latency; and many other variables which might help in distinguishing between the various paradigms suggested. At the same time, the factors usually discussed in connection with the topic of "intelligence" (Sternberg, 1983) are now seen to be relevant, not to intelligence as such but to the social application of intelligence ("intelligence B" as opposed to "intelligence A"—Eysenck & Barrett (in press)). Thus a single fact, clearly unassimilable to current paradigms, is sufficient to produce a revolutionary change in perspective, to lead to entirely new avenues of research, and to interpretation of existing data. This revolution in the field of intelligence is similar in many ways to the one already discussed in relation to temperament, i.e., the noncognitive aspects of personality; both are "reductionist" in that they seek to link social behavior and its consistencies (in the form of traits) with genetically determined biological factors in the organism. That there are such relations, and that they are very prominent, is now hardly in doubt; what is in doubt, of course, and will perhaps remain so for a long time, is the precise nature of the relationships involved. It is here that the puzzle-solving aspects of normal science will have to take over, and settle the issues remaining. The new paradigms do not do away with knowledge painfully acquired under the guidance of the old paradigms; they simply show the insufficiency of the old paradigms, and add an entirely new dimension of causality to them, which marks the essential revolution that has taken place.

Given the considerable amount of positive reinforcement which the general theory here considered has had from a large number of experimental and empirical investigations, it is interesting to ask why it has not in fact been generally accepted as a paradigm, and as a basis for research along the lines of "normal science." The main answer seems to be that psychologists (and other social scientists) are not on the whole aware of the demands for discipline which science exerts from all its followers, and prefer the free and easy atmosphere of arbitrary choice to the rigors of puzzle-solving within the well-defined context of a paradigm. Psychology grants every research worker the right to choose one from the numerous theories of personality, and then among the many measuring instruments those which he prefers, without asking for justification along the lines of known reliability, validity and experimental support. Theories and instruments alike are almost completely impervious to demonstrations of lack of reliability, lack of validity, and lack of such empirical support; hence the presentation of these theories in textbooks of personality in a personalized form, as if there were no scientific grounds for choosing between the many different offerings.

This refusal to lay down and accept rulės of decision between different theories has the disastrous consequence that ordinary science cannot function. Theories and paradigms demand rigorous testing, detailed investigation along parametric lines, and the experimental investigation of deductions; also required in many instances is the assessment of the ability of different theories to predict the experimental outcome actually determined. This means long continued investigations of detailed problems, a concentration on fundamental questions rather than easy application to more "relevant" social problems, and a decision to try to complete the investigation of one particular deduction rather than jump from one to another in the random fashion so much favored by modern researchers in this field.

Why are parametric studies so all important in the construction of a proper theory of personality? One of the major reasons is simply that very few of the regressions of experimental variables on personality are rectilinear. Many if not most follow the lines of Pavlovian *transmarginal inhibition* (alternately known as the Yerkes-Dodson Law, or the inverted U hypothesis). Large numbers of experiments, in many different fields including conditioning, perception, and motor behavior have shown that as the strength of stimulation increases, also does the strength of the reaction, but only up to a point; beyond that point, the strength of the reaction declines. The arousal theory of extraversion clearly demands that this turning point should occur at lower levels of stimulus strength for introverts and for ambiverts, and for ambiverts and extraverts, and many experiments have shown this to be true. Thus with unconditioned stimuli of weak intensity, introverts form conditioned responses more readily and strongly, while with unconditioned stimuli of strong intensity extraverts do so. Only detailed parameter studies can demonstrate the precise operation of this law in any particular instance, and without such knowledge proper testing of the theory becomes impossible.

As an example of the importance of such investigations, let us consider EEG differences between extraverts and introverts. These are important because a concept of arousal has been intimately linked with the frequency and amplitude of alpha waves on the EEG, high arousal being linked with fast, low amplitude waves and lack of arousal with low, high amplitude waves. The theory would therefore predict that under resting conditions introverts would show the former type of waves, and extraverts the latter. Gale (1981) has reviewed the fairly large literature on this topic and has shown that among the over 20 studies of this relationship, three classes of outcome have become apparent. Extraverts have been shown to be less aroused than introverts, more aroused than introverts, or equally aroused. To make sense of these findings, Gale suggests a generalization which takes into account the different *conditions* under which the EEG has been taken in the investigations summarized by him. As he states:

> My general proposition is that when extraverts are either too bored with the procedure or too interested with the task, they will be more aroused than introverts. That is to say, a moderate level of arousal is required to optimise on the personality differences in this context. Where the extravert is too bored (habituation task, or simply lying with eyes closed) boredom leads to self-arousal, possibly involving imagining, which in turn activates the EEG. Where tasks are interesting (performing arithmetic problems, watching the Archmides Spiral, talking to the experimenter) the extravert becomes aroused. With moderately arousing tasks (opening and shutting eyes upon instruction, or a simple eyes closed recording procedure in a laboratory which does not preclude sound of the experimenter's activity) the extraverted subject is more able to obey the instructions to relax and keep his mind clear). (Gale, 1973, p. 245)

Gale gives a table showing that in studies where the conditions are highly arousing, extraverts show greater arousal; in studies where the conditions are likely to produce very low arousal, again extraverts show greater arousal. In those studies in which conditions are moderately arousing, it is introverts who show greater arousal. This outcome thus fits in very well with the notion of transmarginal inhibition, high arousal in the testing situation producing "protective inhibition" in the introvert but not the extrovert.

Clear-cut demonstrations of transmarginal inhibition in psychophysiological experiments, and their relationship with personality, have been demonstrated by O'Connor (1980), using the contingent negative variation. It should never be concluded from a single study, using only a one-parameter value, that the results favor or oppose the theory in question. Either specific predictions should be made concerning the selected parameter value or else several different parameter values should be used, so that the shape of the function could be characterized in relation to the personality variable selected. It is interesting that in all the hard sciences parameter studies of this kind constitute perhaps the largest part of the experimental literature, but in psychology such studies are almost nonexistent. This thus is another area in which a lack of discipline, and a failure to adopt the puzzle-solving behavior of the orthodox scientist prevents psychology from obtaining and utilizing those paradigms necessary for the development of a truly scientific discipline.

In actual fact the personality system discussed here, together with associated systems such as the neo-Pavlovian one of Nebylitsyn, Strelau, and others (Mangan, 1982), has given rise to a good deal of the kind of "puzzle-solving" activity that I have in mind, probably more than any other theory in the field (Eysenck, 1981). However, such activity will obviously have to be increased manyfold before we can hope to find an answer to many of the questions that are raised by anomalies and other complications, such as the interaction of different dimensions of personality with each other.

One particular point should be made here, however, because it concerns the optimal use of different measures of personality dimensions. If it be agreed that the three major dimensions of P, E, and N are relatively all-pervasive in the personality sphere, and can be shown to be fundamental in terms of genetic research, animal work, cross-cultural studies, etc., certain consequences follow for the creation and use of other measures of personality. Almost every week sees the arrival of a new type of test, and the total number of tests, published and unpublished, runs into the hundreds, and possibly the thousands. It is a solemn thought that results achieved by the use of one of these tests can in no way be transferred to the personality space created by other tests, so that we do not have a general psychology of personality, but individual psychologies created in terms of different testing procedures. That this is an absurdity will hardly need documentation or discussion; the question arises what can be done to obviate it.

The first step, or so I would suggest, would be to determine for each particular test (or score within a given test) the degree to which this is determined by and correlated with the major dimensions of P, E, and N. Once this has been done, the question arises of whether any specific variance is still associated with the test or score, or whether it merely measures, to varying degrees, the fundamental personality traits of P, E, and N. It would then be possible to determine the position of the test or score within the three dimensional space generated by P, E, and N, and in addition to say what proportion of the total variance of the test or score was specific to that test, and outside the three dimensional space in question. In this way different tests and scores would become comparable, in a meaningful kind of fashion, and many would indeed be shown to measuring nothing but combinations of P, E, and N (Eysenck & Eysenck, in press). Such tests, clearly adding nothing to the fundamental dimensions, should be rejected outright for further use, and direct measures of P, E and N substituted.

We can thus think of what Cattell would call primary traits of personality as clusters of item points lying partly within and partly without the three-dimensional space generated by P, E, and N. Such clusters can be relatively tight and homogeneous, or larger and less homogeneous; decisions on such points are of course purely artibrary (recalling the battles among instinct psychologists who were either "splitters" or "slumpers," i.e., preferred to subdivide instincts and end up with a large number, or else simply retain a very small number of relatively heterogeneous instincts, like those of self, sex, and society). The putative trait of "impulsivity" would thus be constituted of a cluster of items points lying in the P+, E+, and N+ octant; this fairly heterogeneous cluster can be shown to be divisible into four more homogeneous clusters (and these no doubt could be split again into more homogeneous clusters still). There is no "true" impulsivity, and the best way of looking at "impulsive" behavior in a causal manner would be by referring it to P, E, and N, in combination.

It may be doubted whether anything survives of "impulsivity" after the contributions to its variance by P, E, and N have been summed. In other cases the portion of the cluster lying outside the three-dimensional space defined by P, E, and N, may of course be much larger; this is an empirical problem which ought to be resolved in each case before any new trait is proposed and admitted to the science of individual differences. Basic to this approach is the belief that there is something more fundamental and special to P, E, and N than to the other suggested personality dimensions and variables; the evidence strongly suggests that this is indeed so, and if we do in fact have here the beginnings of a paradigm, then it seems logical that we must follow some such procedure as that outlined above.

The same procedure might with advantage be used in making predictions, or calculating correlations between personality and various experimental or social variables. The first step should be to see to what extent these variables can be predicted, or are correlated with P, E, and N; it should then be established whether the particular trait or score to be added did in fact add a significant amount of variance to the interaction between personality test and criterion behavior. Again, only in this way can investigations using different instruments be brought down to a common denominator and hence compared in a meaningful manner. The importance of agreeing on such procedures cannot be exaggerated; in no other way can we achieve a unification of the field which has hitherto been so sadly lacking and change the belief that paradigms do not, and possibly cannot, exist in the social sciences.

It might of course be replied that the existing measures of P, E, and N are far from perfect, and that their use in this manner would therefore be contraindicated. This does not seem to be a reasonable objection. Once it is agreed that the model here advocated is a fundamental paradigm in personality, research using the puzzle-solving propensities of scientists should not find it difficult to improve on existing scales, and finally arrive at a set of scales both highly reliable and highly valid which could form the foundation stone for experimental work along the lines suggested.

I believe that the pursuit of some such methodology is vitally important for psychology and social science as a whole, because I believe that a solution to the problem of personality research and measurement is fundamental to the development of a truly scientific psychology, whether in the experimental, social, industrial, educational, or clinical field (Eysenck, in press, a, b). Practically every main effect to be investigated in these various disciplines is moderated by personality factors, or correlated with them, and shows important interaction effects which may be much larger than the main effects normally studied. But to use such procedures adequately requires the use of proper theories of personality and measuring instruments derived from these theories. The arbitrary use of multiphasic instruments of doubtful validity or unknown psychological meaningfulness does not encourage the proper formulation of theories regarding interaction effects, and the likely failure of arbitrary selection of such multiphasic tests bids fair to destroy the belief in the necessity for amalgamating what Cronbach (1957) called the two disciplines of scientific psychology, namely, the experimental and the correlational or personology side. Again we must reject the evil of arbitrariness and demand a much more disciplined approach, necessitating the justification of instruments used and theories employed. This is taken for granted in the hard sciences, and the absence of such discipline is one of the major reasons why psychology has not achieved scientific respect and reputability in spite of its now quite lengthy history, extending over more than 100 years. It would not be meaningful or sensible to carry on along lines which have proved to be barren and unsuccessful. The creation and use of proper paradigm is imperative if we want to make the study of personality a truly scientific discipline; nothing else will do.

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