Personality and Experimental Psychology: The Unification of Psychology and the Possibility of a Paradigm

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It is suggested that the scientific status of psychology is put in danger by the lack of paradigms in many of its fields, and by the failure to achieve unification, psychology is breaking up into many different disciplines. One important cause was suggested by Lee Cronbach in his 1957 presidential address to the American Psychological Association: the continuing failure of the two scientific disciplines of psychology—the experimental and the correlational—to come together and mutually support each other. Personality study in particular has suffered from this disunity, and the debates about the number of major dimensions of personality illustrate the absurdity of the situation. Examples are given to show that by combining methods and theories typical of these two disciplines, one can put forward paradigms that would be impossible without such unification. Such a paradigm is suggested for personality and intelligence.

There is a good deal of agreement among philosophers of science that a mature science is characterized by the existence of paradigms, indicated by the presence of exemplars and disciplinary matrices (Kuhn, 1974), that is, a scientific theory together with examples of successful and striking applications, used for teaching purposes (Putnam, 1974; Suppe, 1974). This view is in good agreement with that of modern researchers such as Laudon (1977) and Lakatos (1970), and in its present form it seems to overcome many of the criticisms made of Kuhn's (1970) original theory. The fact that paradigms are largely absent in the social sciences, and the consequences of this absence, has been discussed in detail by Barnes (1982).

It would not be correct to say that all of psychology is lacking in paradigms. What is true of the correlational side (i.e., the study of individual differences) is not necessarily true of the experimental side (i.e., the study of perception, learning, conditioning, and so on). But here one finds a different problem, namely, the fact that the independent variable often contributes relatively little to the variance of the dependent variable in experiments, leading to a large error variance, and that experimental results are often difficult or impossible to replicate. I have suggested that this is so because much of the contribution of individual differences to the independent variable is neglected by experimental psychologists, and thus it becomes part of the error variance (Eysenck, 1964, 1965). I have also suggested that the failure of personality psychology to achieve paradigmatic status may be due to its failure to link findings and theories to the knowledge acquired by experimental psychology (Eysenck, 1994; Eysenck & Eysenck, 1985). In making these suggestions, I have in essence followed the arguments presented by Cronbach (1957) in his American Psychological Association presidential address on "the two disciplines of scientific psychology," namely, experimental and correlational studies. He advocated a coming together of these two strands as a necessary precondition for the unification of psychology; I hope to show that there is now much evidence to support his argument.

In addition to the correlational and experimental disciplines, I would like to add the psychophysiological as a third vital part of any complete and meaningful psychological framework. There will be little argument concerning the fact that man is a biosocial animal (Eysenck, 1980a, 1980b, 1983) and that researchers are always dealing with an organism having both mental and physical dimensions inseparably entwined. As physicists had to learn that they were dealing with a space–time continuum, psychologists will have to learn that they have to deal with a mind–body continuum—not with Cartesian entities entirely separate from each other. As I try to show in this article, personality cannot be understood with the biological side remaining a Skinnerian black box!

I have always argued that paradigms are impossible to achieve in the correlational disciplines alone, because the causal nexus is missing; hence, different ways of distributing the variance (as in factor analysis) cannot be discriminated as being better or worse. This is a common feature of taxonomic work in science, as Sokal and Sneath (1963) have pointed out; Eysenck and Eysenck (1969) argued the case in some detail. I have

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also attempted to suggest criteria for recognizing a paradigm (Eysenck, 1991)—unless psychologists are in agreement about just what constitutes a paradigm, they are unlikely to agree on whether a given theory is indeed to be regarded as a paradigm. I go into this thorny problem in the next section; here I want merely to discuss briefly the reasons why no claim for the existence of a paradigm in the field of personality can be made, and why claims such as those made for conceptions such as the Big Five are inadmissible.

Beginning with Hall and Lindsey's (1957) *Theories of Personality* and going on to Pervin's (1990) *Handbook of Personality*, textbooks nearly always consist of chapter after chapter offering the thoughts of selected writers, differing totally in their conceptions of personality, the problems encountered, the methods of study to be used, the theories underlying these studies, and the final products. There is no attempt to judge the value of the products, compare them on an evidential basis, or reject those obviously deficient in methodology, experimental support, or demonstrated validity. Nor is there any discussion of just what would be required of an acceptable theory that might form the basis of a suitable paradigm in this field. Most such books still begin with Freudian theories, in spite of their almost complete failure to generate positive empirical support (Eysenck & Wilson, 1973; Kline, 1981). This, surely, is preparadigmatic science at its worst—no agreement on facts, no agreement on methods, no agreement even on criteria for judging facts and methods.

In recent years, attempts have been made to present a special version of trait theory as an acceptable descriptive paradigm. This is the Big Five concept, based on the lexical hypothesis (Digman, 1990; Goldberg, 1993; John, 1990). But, as already said, a purely descriptive paradigm is a scientific impossibility, although description is an inevitable first step. Without causal implication and embedment in a proper nomological network (Garber & Strassberg, 1991), no proper construct validity can be claimed. Inevitably, even on their chosen ground, advocates of the Big Five have been criticized very harshly by a large number of experts, for example, Ben-Porath and Waller (1992a, 1992b); Block (1995); Brand (1994); Cattell (1995); Church and Burke (1994); Coolidge, Becker, Di Rito, Durham, Kinlaw, and Philbrick (1994); Draycott and Kline (1995); Eysenck (1991, 1992b); Hough (1992); Jackson, Paunonen, Fraboni, and Goffin (1996); McAdams (1992); Matthews and Oddy (1993); Mershon and Gorsuch (1988); Tellegen (1993); Van Heck, Perugini, Caprara, and Froger (1994); Zuckerman, Kuhlman, and Camac (1988); and Zuckerman, Kuhlman, Joireman, Tefa, & Kraft (1993). Not all of the criticisms leveled at the Big Five are justified, but no model that has been so widely criticized can be regarded as generally acceptable. It can be noted that many of the criticisms received are related to the fundamental weakness of the model as a scientific model of personality, namely, the absence of causal relations and a proper nomological network that would prove the legitimacy of the factors resulting from factor analysis. The purely inductive nature of correlational methods makes properly scientific conclusions impossible; competent scholars are still unconvinced that five is the correct number of fundamental dimensions of personality, or that the Big Five are the major contestants. Indeed, is it meaningful to postulate a correct number in the present state of ignorance?

The inadmissibility of any claim that the Big Five constitute some sort of paradigm is made clear when one considers the many alternatives still in the market. There are claims for three major factors (Cloninger, 1986; Eysenck, 1991, 1994); an S-factor set quite different from the Big Five (Zuckerman et al., 1988); a set of six factors similar to the Big Five (Brand, 1994), or quite dissimilar (Jackson et al., 1996); a bigger set of seven factors (Benet & Waller, 1995); going all the way to Cattell's (1950) famous 16PF. Not only is the number of major dimensions still up in the air, but so is the nature of the factors involved. The existence of a paradigm implies the existence of (fairly) universal agreement on its essentials; clearly, such agreement among experts is completely missing.

I want to mention here one particular argument (Eysenck, 1992d) because it illustrates very well the difference between a purely inductive and a hypothetico-deductive approach. The Big Five model posits two major factors labeled Agreeableness (A) and Conscientiousness (C); I have argued that both are primary rather than higher order factors, and both form part of my Psychoticism (P) factor (Eysenck, 1992c). There is a high negative correlation between A and C, on the one side, and P, on the other. Goldberg (1993) admitted that "a convincing solution to this seemingly intractable controversy merits a Nobel prize" (p. 31). This admission indicates the intractable problems facing any purely inductive procedure; I indicate in a later section how the problem can be resolved by adopting a hypothetico-deductive model allowing the experimental testing of deductions from a general theory.

Using terms like *nomological networks* and *theory* requires at least a brief statement of the meaning of these terms. I have argued that psychologists never really deal with categorical differences (theory or no theory) but deal with a continuum ranging from hunch, through hypothesis and theory, to law. Nor do they deal with categorical differences in their methodology (Viennese school verification vs. Popperian falsification), but they do deal with methodologies appropriate to the stage of theorizing reached (Eysenck, 1985). Figure 1 illustrates my position. Science begins with a hunch, acquired through observation and induction, which is clearly a preparadigmatic position. If the hunch seems to work, psychologists construct small-

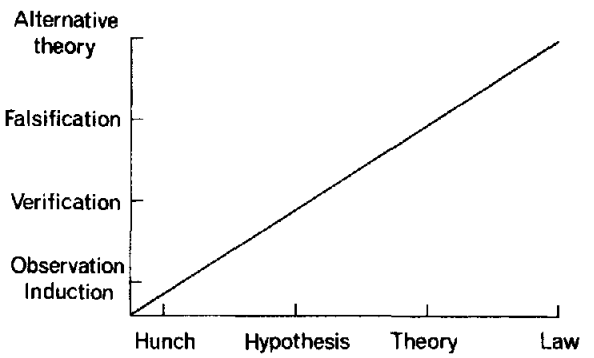

Figure 1. Types of nomological networks and appropriate scientific methodologies (Eysenck, 1984).

scale hypotheses, for which they seek verification. If such verification is forthcoming in sufficient quantity, the level of theory is reached, and one may then consider the demands of falsification. If there is no evidence of such falsification, the theory is enshrined as a law, such as Newton's law of gravitation. But a law can be overthrown in a scientific revolution, and an alternative theory can be enthroned as law, such as Einstein's theory of relativity. The point between hypothesis and theory would seem to mark the advent of a paradigm, but of course there is no precise point, merely a rough area, not clearly marked out, where the change occurs. A paradigm begins to be fashioned when the ordinary business of science takes over, that is, the large-scale testing of deductions from the theory, and the attempt to explain anomalies in terms of the theory's apparent failure. The notion of a nomological network, I would think, links roughly at this point, between hypothesis and theory, although of course it would not be sensible to try to be prescriptive in such matters.

Theories, in turn, can be subdivided into strong and weak, demanding differential methodological treatment (Eysenck, 1960). Testing any theory demands not only a precise deduction from the theory but also adequate knowledge of the general set of conditions in which the testing is carried out; failure of the experiment to support the theory may be due to inadequacies in the theory or erroneous assumptions concerning surrounding conditions. Such knowledge tends to be inadequate in weak theories; hence, failure may be due to lack of such knowledge rather than inadequacy of the theory. In weak theories (almost all psychological theories), verification is more important than falsification, because verification is likely only if both theory and knowledge of surrounding conditions are essentially correct; failure may be due to errors in either and is hence uncertain in its meaning (Cohen & Nagel, 1936). Psychologists often apply principles appropriate to strong theories inappropriately to weak ones, citing Popper's support, yet premature attempts and falsification may slay promising theories before they have had a chance to mature. Newton's theory of gravitation encountered anomalies from the beginning and continued to until overthrown by Einstein.

A Hypothetico-Deductive Model

I have outlined on several occasions what I believe a model of personality should look like (Eysenck, 1981b). Figure 2 illustrates the major components of this model. Central to it is a taxonomic theory based on the psychometric study of trait constellations. However, the three major dimensions of personality (psychoticism, extraversion, and neuroticism) form part of a much wider nomological network, beginning with the distal antecedents of these dimensions, that is, the genetic personality determinants (Eaves, Eysenck, & Martin, 1989). Of course, DNA cannot directly cause behavior; it is copied onto RNA by a complementation process, and the RNA participates with various intracellular structures to produce peptides, which compose proteins that include structural, transport, and catalytic proteins (enzymes). These in turn facilitate the chemical reactions of life. Clearly, biological intermediaries are needed to translate genetic potential and environmental pressures into behavior; these intermediaries constitute the proximal antecedents of psychoticism, extraversion, and neuroticism. I have tried to identify some of the major intermediaries in some detail (Eysenck, 1967; Eysenck & Eysenck, 1985; Zuckerman, 1991).

Most of the past work in genetics has used the algorithms of behavioral genetics, but now there is the promise of molecular genetics (Aldhous, 1992; Cloninger, Adolfsson, & Svrakic, 1996; McGuffin, Owen, & Gill, 1992), which "will revolutionize behavioural genetics by identifying specific genes that contribute to genetic variance in behaviour" (Plomin, 1993, p. 473). An idea of the possibilities of these techniques is given by the study of novelty seeking by Ebstein et al. (1996), who analyzed D4DR exon III genotypes, and Cloninger's tridimensional personality questionnaire, which measures three major personality factors posited by Cloninger, Svrakic, & Przybeck (1993). In particular, the trait of novelty seeking (psychoticism?) was postulated to be related to one particular exonic polymorphism, the 7 repeat allele in the locus for the D4 dopamine receptor gene (D4DR). A highly significant relationship was observed, with harm avoidance and reward dependence failing to show any such relation.

A replication study, using the Extraversion scale of the Revised NEO Personality Inventory (NEO-PI-R) questionnaire (Costa & McCrae, 1991) found relations between the 7 repeat allele in the locus for the D4DR and three of the six facets of extraversion (warmth, excitement seeking, and positive emotions), as well as the single facet of (lack of) deliberation from the conscientiousness factor (Benjamin et al., 1996). None of the other NEO factors were associated with this polymorphism. D4DR accounted roughly for 10% of the genetic variance of novelty seeking. This is merely a suggestion of what may be done along these lines, both in mapping personality factors into specific polymorphism and in demonstrating specific biological associations, as with dopamine transmission. Thus, direct genetic relations can be traced between biological factors, such as hormones, neurotransmitters, and so on, and personality variables. It may be possible to decide between different personality models, such as those of Eysenck, Cloninger, Gray, Costa and McCrae, and Zuckerman and Tellegen, on the basis of studies in molecular genetics. This is an important task for the next 10 years, and it is a task for the future.

The nomological network so far delineated gives rise to large numbers of deductions, both psychophysiological and experimental, that constitute the next step in Figure 2, labeled *proximal consequences.* Some of these are reviewed later on in this section. These predictions, arising from the general theory, are to my mind the most important aspects of any theory of personality; if verified, they constitute solid evidence for the theory concerned—evidence of a kind not available to any purely taxonomic theory based simply on factor analysis. If apparently disconfirmed, they challenge the regular work of science to show whether the disconfirmation is real or is simply apparent and due to the neglect of important constraints. I give examples of the latter kind of disconfirmation later on.

Beyond the proximal consequences are the distal consequences, that is, macrobehaviors of a social kind. The theory accounting for extraversion in terms of low cortical arousability would predict greater changeability of extraverts as compared with introverts (Eysenck & Eysenck, 1985). This can be tested on the proximal consequences side with experimental studies

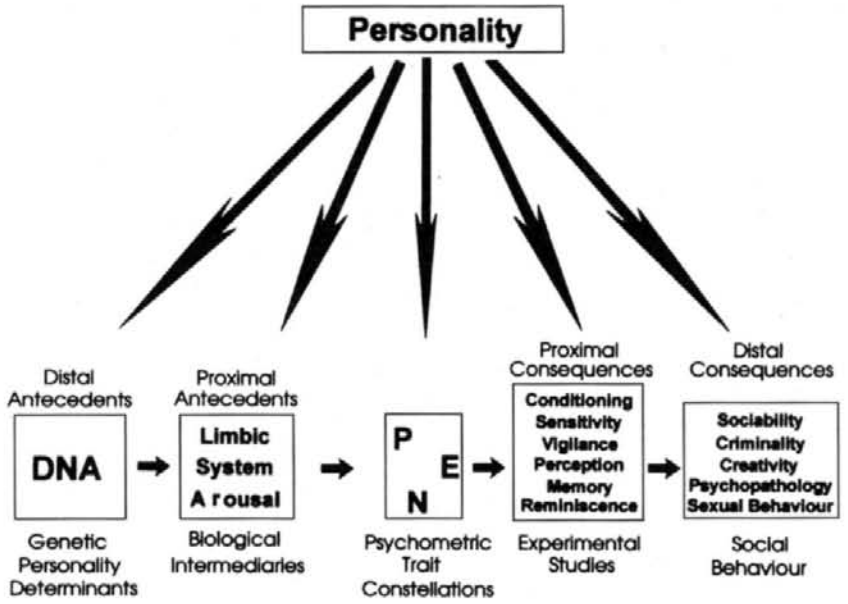

Figure 2. Diagrammatic representation of five steps in any causal theory of personality. P = psychoticism; E = extraversion; N = neuroticism.

(Eysenck & Levey, 1965) and on the distal consequences side with investigations of whether extraverts show sexual behavior characteristic of changeability (Eysenck, 1976). Predictions along both lines have given very positive results, and thus the theory has been confirmed. Note that the prediction does not derive from the psychometric test constellation descriptive of the extravert; such predictions are characteristic of the Big Five type of argumentation. The prediction is based on a causal theory, going back to the proximal antecedents of extraversion, in particular the hypothesized low arousability of the extravert. It is the extent of the evidence supporting such causal links that is crucial in deciding on the status of a given model of personality. In particular, I have always emphasized the study of proximal consequences, that is, the properly experimental study of personality, because this seems to me most characteristic of the experimental approach.

Equally important of course is the direct study of the psychophysiological theory in question. There is a large body of work concerned with this task (Eysenck, 1990), and I do not discuss it here.

Experimental Studies of Personality

Let us now consider a few experimental studies and the way they bring together correlational and experimental psychology. These are used as illustrations of a scientific methodology, not as proof of the underlying theories. They are taken from experiments testing my theory that extraversion is the product of low cortical arousability, due to sluggish functioning of the ascending reticular activating system (ARAS) (Eysenck, 1967). Introverts would be characterized by an ARAS that was functioning more powerfully than average, with ambiverts in between. The examples given below are chosen to illustrate the way deductions can be made from the theory and tested experimentally. Hundreds of such experiments have in fact been done and are discussed in detail elsewhere (Eysenck & Eysenck, 1985). It would not be sensible to give a scoreboard total; the very nature of scientific theories makes it likely that any new theory requires considerable refinement through the working of ordinary science before all of the anomalies are removed and all of the constants specified that favor outcome. Nor is it suggested that the findings are necessarily universally true. The theory suggests optimal conditions for testing that are not always followed, and they may themselves require extensive experimental work before one can be sure of what they are. What can be claimed is that there have been too many successful tests of a number of deductions to doubt that the theory is pointing in the right direction and deserves further testing and improving. My first example is the Urbantschitsch (1883) theory that thresholds for perception of stimuli in one modality can be lowered by heteromodal stimulation (i.e., by increased cortical arousal). Many studies have given divergent and contradictory answers, leading to no generally tenable conclusions (T. Shigehisa & Symons, 1973). These authors argued that the reason for the discrepancies might be a differential response to heteromodal stimulation for extraverts and introverts, together with the intervention of Pavlov's law of transmarginal inhibition (law of inversion), according to which strong stimulation leads to defensive inhibition (i.e., an inversion of the effects of weaker stimulation). The arousal theory predicts that the point of inversion is reached at a lower intensity of stimulation for introverts than for extraverts, due to the higher arousability of introverts. On the basis of this theoretical argument, T. Shigehisa and Symons predicted that visual stimulation would lower auditory thresholds at low intensity for extraverts and introverts but would increase auditory thresholds for introverts at high levels of inten-

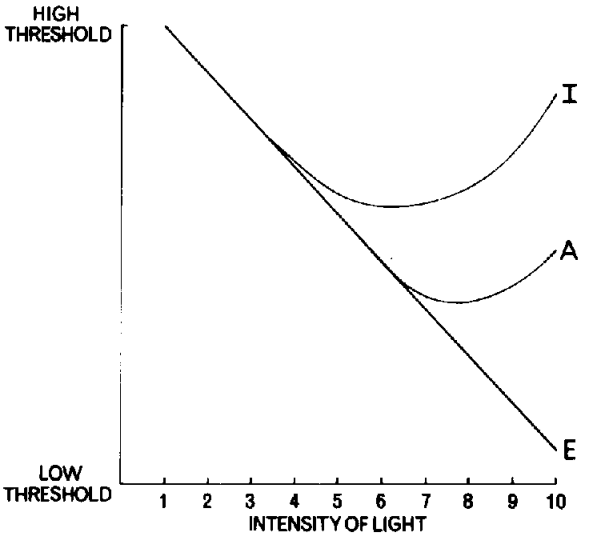

Figure 3. Predicted and found relation between personality (I = introvert, A = ambivert, E = extravert) and heteromodal effect of varying light intensity on hearing thresholds.

sity, while continuing to lower them for extraverts. Ambiverts would be in between. Figure 3 shows the nature of the prediction.

The results supported this prediction in detail. P. Shigehisa, Shigehisa, and Symons (1973) then repeated the experiment in reverse, measuring visual thresholds under heteromodal scintillation by different intensities of sound. Again they found the prediction of personality modulation confirmed. Thus, these experiments not only support the personality theory of extraversion but also show why purely experimental studies of the Urbantschitsch (1883) effect have failed to give any clear results. Where extraverts and introverts react differentially, and even in opposite directions, to stimuli of different intensity, differences between studies in the range of intensities and in the mixture of extraverts and introverts used clearly will lead to different outcomes. Only a combination of experimental and correlational methods is likely to give meaningful results.

Another example relates to Kleinsmith and Kaplan's (1963, 1964) action decrement theory, according to which high arousal produces an active memory trace of longer duration; this in turn leads to enhanced consolidation and long-term memory. However, during the time the process of consolidation is continuing, there is a transient inhibition of retrieval (referred to as action decrement), which protects the active memory trace from disruption. As a consequence, whereas high arousal is beneficial for long-term retention, it impairs short-term retrieval for periods of time up to several minutes after learning. If one were to accept this general law, then one could predict that introverts would show short-term memory impairment compared with extraverts, but would show long-term memory improvement. Howarth and Eysenck (1968) tested this prediction, with very positive results, as shown in Figure 4. Note again that if extraverts and introverts differ so profoundly in their reactions, mean effects will be rather meaningless. By ignoring the interaction, powerful individual differences effects will be consigned to the error term, and the importance of the main effects will be reduced. This may account for the difficulties found in establishing the action decrement as a law. Eysenck and Eysenck (1985) discussed this and related studies in detail.

As a third example, consider the prediction that high arousal would facilitate Pavlovian conditioning and that, consequently, introverts would condition better than extraverts. Using eyeblink conditioning, we have several times shown that the prediction is indeed borne out (e.g., Eysenck & Levey, 1972). Figure 5 shows the results from one such experiment; the difference is clear.

The inclusion of personality variables in experimental designs extends way beyond the laboratory, to distal consequences. Consider education. The past 30 years have seen great interest in different teaching methods; *discovery learning*, as contrasted with the traditional *reception learning*, has been used on a large scale. Yet the evidence from many studies has not demonstrated any superiority of one over the other. In other words, changing the independent variable produced no consistent change in the dependent variable. Leith (1974) suggested that this might be due to differences in personality, with extraverted children preferring and doing better with discovery learning and introverted children preferring and doing better with reception learning.

Figure 6 shows the outcome of an experiment to test this hypothesis; clearly, there are very large differences in the predicted direction between extraverted and introverted children, suggesting that neglect of personality factors has produced the erroneous impression of no effect and that analysis of personality teaching style interaction could recover the true effects. Many other examples of such interaction were given by Eysenck (1978). The possibilities of using such information for the improvement of teaching effectiveness have been very much improved through the advent of the computer; more individual teaching is now a distinct possibility (Reed, Ayersman, & Liu, 1995).

Or consider the social effects associated with overly strong or overly weak Pavlovian conditioning. I have based my conditioning theory of neurosis on the well-known facts of Pavlovian

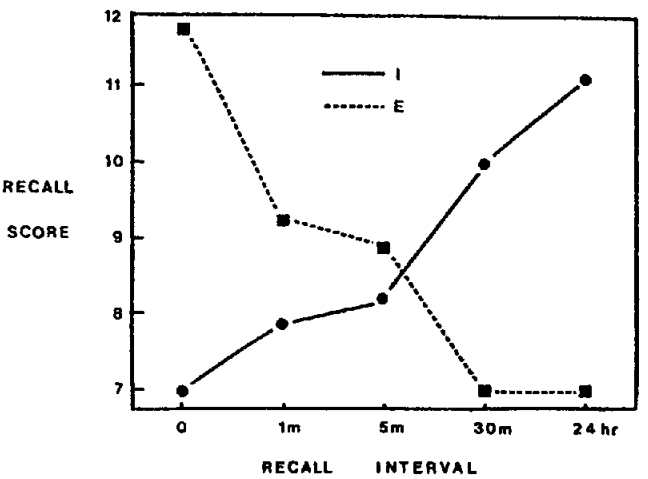

Figure 4. Recall as a function of personality and recall interval (Howarth & Eysenck, 1968). I = introverts; E = extraverts.

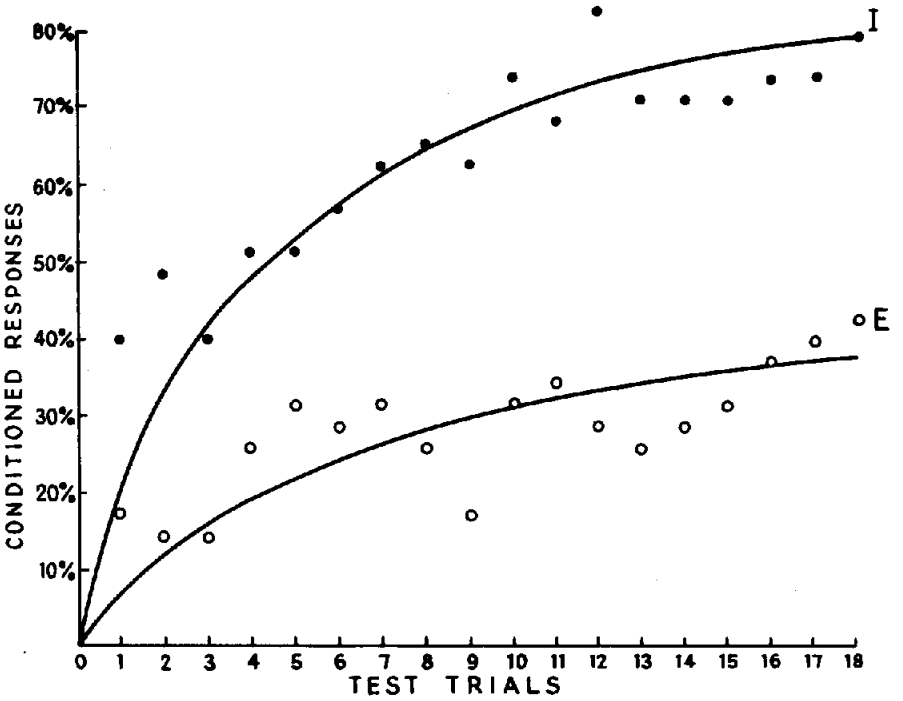

Figure 5. Eye-blink conditioning of extraverts and introverts (Eysenck & Levey, 1972).

conditioning (Eysenck, 1976, 1979), and there is much support for the theory in the literature, as well as for the application of behavior therapy based on it (Grawe, Donati, & Bernauer, 1994). Equally important, I suggested that Pavlovian conditioning was at the basis of developing a conscience, which in turn would make antisocial (criminal) conduct less likely (Eysenck, 1977). The deduction that criminals would be difficult to condition experimentally has received powerful support in 20 studies,

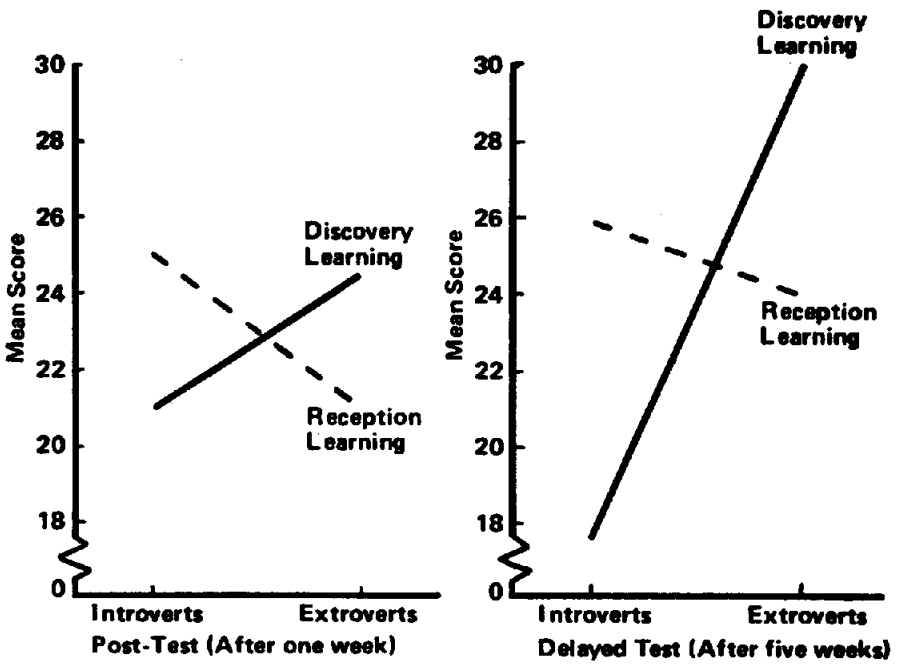

Figure 6. Interaction between personality and teaching method (Leith, 1974).

with only 2 failing to give positive results (Raine, Venables, & Williams, 1996). Raine et al. even demonstrated highly significant differences in conditionability between criminals and matched "desistors" (i.e., youths who had misbehaved to the same extent as future criminals but did not actually turn to crime). This type of extension of laboratory experiment to the prediction and explanation of social behavior is one of the main advantages of the bringing together of the experimental and correlational approach suggested here.

This general principle of analyzing interactions rather than main effects in experimental situations extends to many other fields besides education; it applies with equal force to social psychology, clinical psychology, occupational psychology, and indeed to all types of psychological research. In learning theory, for instance, great disputes between Hullians and Tolmanians were caused by apparently irreconcilable differences in experimental outcomes. But the two groups used quite different strains of rats; Spence, the main proponent of Hull's view, used a nonemotional strain, whereas Tolman used a strain much nearer the emotional strain originally bred by C. H. Hull (Eysenck, 1967, p. 19). Jones and Fennell (1965) have shown experimentally how the differential behavior of these two strains, which parallels human neuroticism, can account for the differential behavior of Hullian and Tolmanian strains.

Fundamentally, the reason the use of personality factors in experimental designs is vital lies simply in the difference between experiments in physics and experiments in psychology. The physicist can subdivide his or her subject matter indefinitely (or almost so, going right down to quarks) and can control the experimental situation completely. The psychologist can control the situation to some extent but has had to use indestructable individuals. His or her material is always diverse, and that diversity can, and usually does, interact with the avowed aim of the experiment. Bright and dull, extraverted and introverted, neurotic and stable—the personality of the participants will (nearly) always interfere with the actions of the independent variables. This makes experimentation much more complex and difficult, requiring experimentalists to acquaint themselves with the theories and findings of personality researchers. But there is no alternative. Scientists cannot disregard factors that can be demonstrated to affect their experiments. Conversely, the inclusion of such factors will inevitably throw new light on the personality theories in question. And what is more, researchers shall achieve the aim of having a unified psychology and shall possess a truly scientific paradigm.

Personality Paradigm and Normal Science

According to Kuhn (1974), science for the most part consists of periods of normal science, in which deductions from paradigms are tested and minor improvements are made in the formulation of the paradigms in question. Often, anomalies are discovered and attempts are made to accommodate these within the paradigm; indeed, the study of such anomalies often leads to new discoveries and a strengthening of the paradigm. Thus, the discovery that Uranus was not behaving in accordance with Newton's theory led Le Verrier and J. Adams to postulate the existence of a new planet that might produce these irregularities and to try to predict where such a planet might be found. This in turn led to the discovery of Neptune. Thus, an anomaly that might have disproved Newton's theory led to one of the most spectacular supports instead. Occasionally, these periods of normal science are disrupted by the revolutionary destruction of a paradigm (ether, phlogiston) and its replacement by another. These revolutions create the greatest excitement, but the silent work of normal science is far more characteristic of what scientists do and is quite essential to the natural growth of science.

The absence of paradigms in much of psychology, particularly social psychology, has led to the virtual absence of normal science in this field. Instead of building on past theories and past findings, there is a strong tendency to throw out previous theories and experiments when anomalies arise and start on something new, rather than try to accommodate the anomalies. Much important work was done by Hull and Spence in the prewar years, but impatience led to the wholesale rejection of the whole model when anomalies accumulated. In the personality sphere, where there should have been a gradual growth of an acceptable paradigm, there have been instead constant new starts, and new claims, that gain short-term acceptance, but only to give way to newer claims when the excitement has died down. This is no way to make personality study into a science.

An example will illustrate what I mean when I say that anomalies and failures to replicate, when properly analyzed, may actually strengthen a theory (Eysenck, 1981a). I had postulated, and found, that introverts form conditioned eye-blink responses more quickly than do extraverts. This prediction was based on the theory that introverts show higher arousal than extraverts (Eysenck, 1973), and experimental studies amply supported the theory (e.g., Eysenck & Levey, 1972; Jones, Eysenck, Martin, & Levey, 1981; Martin & Levey, 1968). They also supported the hypothesis that neuroticism would not be related to conditioning. Spence & Spence (1966), on the other hand, argued that high anxiety–neuroticism, acting as a drive, would predict better conditioning, but that introversion would not. These predictions were strongly supported in several studies (e.g., Spence & Beecroft, 1954; Spence & Parker, 1954). Clearly, such contradictory findings would count as an anomaly and serve to discredit both theories.

The facts actually support that both findings support both theories (Eysenck, 1991). In talking about "failure to replicate," it is assumed that a replication contains all the essential features of the original (i.e., those features essential to test the theory in question). But when different theories are being tested, different features may appear essential. Spence and Spence (1966) were testing a theory of anxiety–neuroticism and hence tried to induce state anxiety in his subjects by making conditions of testing as threatening as possible, thus making possible correlations between trait anxiety and conditioning. Arousal produced by the strong anxiety that was induced would drown out any less-strong differences in arousal between introverts and extraverts. Eysenck (1973), on the other hand, tried to eliminate irrelevant emotional components by making the testing as anxiety-free as possible, thus enabling the postulated differences in arousal due to extraversion to make an appearance. Thus, both the Spence and the Eysenck theories are supported when the situation is taken into account; it is widely accepted that Situation × Personality interaction is crucial to any understanding of the dynamics of individual differences (Magnusson & Endler,

1977). It is interesting that Hull (1951) recognized individual differences as important parts of his general theory. His postulate XVIII is in fact a postulate of individual differences and states that the "constant" numerical values appearing in equations representing primary motor behavioral laws vary from species to species and from individual to individual. Spence seems to have been his only follower to take this statement seriously and to have applied it in a series of experimental studies (Eysenck, 1973). Hull's law of individual differences expresses precisely what I am aiming at in this article.

This is an example to indicate the proper progress of scientific investigation. In the absence of a proper paradigm, psychologists often interpret failure to replicate, or generally negative findings, as disproof of a given hypothesis and cease to be interested in it. The proper attitude would be to take such anomalies as problems that require a solution that, if possible, would use elements of the original hypothesis to define the proper situational variables that might account for the apparent anomaly.

As an example, consider a study by Brocke, Tasche, and Beauchard (1997), which tested the prediction that introverts would show larger P300 amplitudes on the EEG in vigilance and oddball tasks. Several experimenters have found the predicted outcome, but others have not. Conditions making for exception to the rule involve (a) brief duration of oddball or vigilance tasks, (b) complex stimuli or more cognitively demanding tasks, and (c) stimuli with an emotional component. All of these situational conditions are predictable on the basis of the general arousal theory. Brocke et al. added a fourth, predicting on the basis of deduction from the general theory that introverts would show greater amplitude of the P300 at low levels of stimulation and that extraverts would show it at high levels. Using three levels of stimulation, they verified their prediction. Theories that predict the situational conditions under which a given effect becomes apparent are clearly superior to theories that do not, and experiments should always use several different conditions to compare effects, using a theoretical rationale. As another example, consider Gale's (1983) analysis of 33 studies investigating Eysenck's (1990) hypothesis that extraverts would show less arousal than introverts on the EEG. For the 38 experimental comparisons in question, extraverts were less aroused than introverts in 22 comparisons and more aroused in 5, with the remaining studies giving nonsignificant results. Gale suggested that the effects of extraversion on the EEG were influenced by the level of arousal induced by the experimental conditions. Moderately arousing conditions were the most suitable, with differences either disappearing or being reversed with conditions producing either low or high levels of arousal. Classification of the studies as high, medium, or low in arousal bore out his suggestion, with all of the studies using medium-arousing conditions supporting the theory. There has been some debate concerning Gale's analysis; as the studies were not designed to test his hypothesis, different interpretations are of course possible. I have cited it to illustrate the importance of considering situational variables in testing the theory.

There are many examples of the importance of this situation–personality interaction in testing theories such as the extraversion–arousal one (Eysenck, 1990). Thus, the theory predicts that introverts would show a stronger orienting response and lower habituation. The majority of studies support the hypothesis, particularly when stimulation is moderately arousing; less intense stimulation does not always give such support. Again, in auditory tests low frequency stimulation appears to be more effective in differentiating extraverts and introverts. Quite generally, regression effects on stimulus conditions are often curvilinear; very strong and very weak stimulation appear less successful in differentiating introverts than medium-strong stimulation. Individual experiments not taking such situational effects into account often suggest failure for a theory when in fact the results fall into line when the curvilinearity of the regression is taken into account (Stelmack, 1981). The curvilinearity of regression is itself predicted by the theory (Eysenck, 1990).

It is sometimes suggested that laws such as Pavlov's law of transmarginal inhibition and the Yerkes–Dodson (1908) law are not precise enough to indicate where in a range of intensities the turning point should appear. This indefiniteness, it is suggested, makes predictions from personality theory difficult to assess. It is true that the failure of experimental psychologists to study these important and omnipresent laws in detail is depressing; hardly a dozen studies have been explicitly devoted to their classification, whereas literally thousands of studies have been done on such fripperies as the Roschach ink blots. Yet this does not touch on the predictions made from personality that predict that the turning point would occur at a lower level of intensity for introverts than for extraverts. This prediction can be tested regardless of psychologists' knowledge of the mean level at which the turning point is located. Indeed, the existence of individual differences means that there is no identifiable point of inversion, but a *range* determined by differences in extraversion–introversion.

If one accepts the theory of extraversion–introversion as paradigmatic, and there is far more experimental evidence relating to this theory than to any other, then it would seem desirable that normal science should take over the detailed study of deductions from that theory and put it on a firmer foundation. Isolated studies in many different fields have been done, with considerable success, but not much has been done to clarify the influence of situational variables, an influence often clearly spelled out by the theory itself (Eysenck & Eysenck, 1985). The fact that extraversion and neuroticism in one form or another have been recognized as major dimensions of personality since Greek times and form part of nearly all personality questionnaires (including the Big Five) in use now suggests that they have pretty well achieved paradigmatic status and deserve the usual follow-up by normal science.

Making Factor Analytic Analysis Hypothetico-Deductive

We may now return to Goldberg's (1993) problem, which he declared for all practical purposes insoluble and which in a nutshell represents the fundamental weakness of all correlational psychology. Are A (agreeableness) and C (conscientiousness) subfactors of P (psychoticism), or is P an artificial shotgun marriage of A and C? The claim made here is simply that an answer to such a problem can only be found by joining the causal, experimental approach to the correlational one; this does not guarantee a correct answer, but it does provide a testable one. Consider the following arguments.

Let us begin with the question of nomenclature. Naming fac-

tors has always been extremely subjective, and it relies essentially on interpreting rather arbitrarily the meaning underlying high-loading items (Ravensdorf, 1978). In a recent study by Dreger, Lichtenstein, and Cattell (1995), the authors tried to discover by factor analysis the major personality variables in preschool children and to relate these to the Big Five. They did find five factors, and they asked four independent judges to name them in line with the items having high loadings. Factor A was named "reserved versus outgoing" by Judge 1, "responsive to aversive events" by Judge 2, and "as making no sense psychologically" by the other two judges. Factor D was named "mature versus immature" by Judge 1, "making no sense" by Judge 2, "self-monitoring" by Judge 3, and "acquiescent" by Judge 4 (Dreger et al., 1995, p. 66). The other factors were judged equally diversely. What is the true psychological content of each factor? Clearly, factor analysis is not sufficient to bring this out, and the same criticism applies to the Big Five generally: What is the true nature of the factor variously named "openness," "culture," or "intellect"?

The experimental approach suggests that we should start with a testable theory underlying the assumed factor. Underlying the theory of psychoticism is the hypothesis of a quantitative predisposition underlying functional psychotic disorders (Eysenck, 1952; Eysenck & Eysenck, 1976; Eysenck, Granger, & Brengelmann, 1957). This construct has many similarities to the later notion of schizotaxia or schizotypal taxon developed by Meehl (1962). The hypothesis is directly testable, using the P scale, developed by factor analysis, as an (imperfect) measure of the hypothetical dimension. If the theory is correct, it would follow (inter alia) that experimental variables differentiating between schizophrenics (representing functional psychotics) and normals should also discriminate between high-P-scoring and low-P-scoring normals, and between high-P-scoring and low-P-scoring schizophrenics. I have called this the *proportionality criterion,* and numerous studies have shown that it makes correct predictions (Eysenck, 1992a). In the list of variables used to illustrate the proportionality criterion, I have on purpose included several different types of measures. One class deals with biological variables (HLA B27, MAO, dopamine) of different kinds. A second deals with laboratory behaviors (eye tracking, dichotic shadowing, sensitivity levels). A third is concerned with learning–conditioning variables (latent inhibition, negative priming). Yet a fourth is concerned with psychological variables (creativity, hallucinatory activity, word association). Physiological variables (EMG, autonomic–perceptual inversion) constitute yet a fifth set of variables. "It is the variety of variables which makes the results impressive, together with the theoretical congruence; to obtain successful results over such a wide array of variables suggests that the underlying hypothesis may be along the right lines" (Eysenck, 1992a, p. 777).

Related to this attempt of proof is the requirement that the theory of schizophrenia should also fit P, so that deductions from a theory of schizophrenia that had experimental support could be tested using normal subjects differing in P. Taking the theory advanced by Gray et al. (1991), Gray, Pickering, and Gray (1994) tested the deduction that P should be related to dopaminergic mechanisms. Investigating dopamine D2 binding in the basal ganglia using single photon emission tomography, they found, as predicted, a significant correlation between psychoticism and dopamine D2 binding in the left-side basal ganglia. A similar prediction regarding latent inhibition, also deriving from a theory of schizophrenia, has also been verified several times, as well as predictions on negative priming (Eysenck, 1992a). These are examples of predictions that follow directly from the identification of the factor P with the theoretical concept of psychoticism; they could not have been made from any combination of (negative) agreeableness and (negative) conscientiousness, although quite probably A and C will be found to correlate negatively with the variables mentioned in this paragraph.

It is not suggested that all of the studies mentioned, and many more not mentioned, definitively prove the correctness of the theory underlying P, or that negative results may not appear in the future. I am suggesting that the methodology adopted clearly enables researchers to test the underlying theory and thus constitutes an advance on the purely descriptive nature of the usual factor analytic process. The combination of the correlational and experimental methods are the necessary and sufficient conditions for the creation of a paradigm in the personality field.

It is possible to take further the predictive aspects of the model. I have elsewhere tried to trace the natural history of creativity (Eysenck, 1995b), itself usually concerned only with correlational studies (Glover, Ronning, & Reynolds, 1989). Starting with the well-established link between genius and psychopathology, I suggested that P would be a good measure of this link, a hypothesis for which a large amount of evidence has been found. A causal explanation of this link was sought in the dopamine–latent inhibition connection with P, which would account for the overinclusiveness apparent in schizophrenics and the shallow associative gradients apparent in creative people. It has proved possible to construct a complete theory of creativity, included in which is an account of individual differences in creativity and intuition.

Consider now the advantages of the theory. The dopamine–latent inhibition theory predicts and causally accounts for the observed high correlations between P and creativity. No doubt similar correlations may in the future be found between A (negative) and C (negative) and creativity, but these would follow from the known negative correlations between A and C, on the one hand, with P, on the other. There is no theoretical lead from Big Five theory to creativity. There is in fact no theory of A and C from which deductions could be made; the only predictions possible would be identifiable as behaviors essentially synonymous with A and C. But such are not predictions that follow from a nomological network; in purely correlational studies there is no theory and no nomological network. (It might be said that the postulation of the lexical hypothesis amounts to a theory, but that is not so. Adherents of the Big Five treat the lexical hypothesis not as a theory to be tested but as an axiom; one looks in vain for any attempts to assess the true value of this axiom along experimental lines.) If one assesses the paradigmatic value of a given factor on the basis of its theoretical basis and its predictive powers, clearly P has considerable advantages over A and C.

Creativity is not the only concept linked with P through a proper nomological network. Another example is addiction. There is ample evidence that high dopamine levels in the nucleus accumbens (NAc) are found in certain types of addiction (Jo-

seph, Young, & Gray, in press). As already explained, dopamine production is linked with P, and this would suggest that addicts of many kinds should have high P scores. There is a large amount of evidence to support this hypothesis, and psychological and behavioral links between dopamine levels in the NAc and addiction strengthen the hypothesis still further (Eysenck, 1997). Again, no predictions follow from the postulation of factor A and C.

In thus attempting to answer Goldberg's (1993) query, I am not suggesting that psychoticism, extraversion, and neuroticism will always remain the only major dimensions of personality. It may even be possible that in due course A and C will develop the necessary theoretical basis to produce testable predictions independent of their correlation with P. What I am suggesting is that those who claim such elevated status for A and C are issuing promissory notes drawn on a nonexisting bank account. Claims for paradigmatic status should be made after, and not before, the necessary theory construction and experimental testing have been completed.

My own system is of course not the only one to have developed a nomological network making testable predictions of the kind here considered; we also have, for instance, the theories developed by Gray (1991), Cloninger (1986), Zuckerman (1991), and Zuckerman et al. (1993). These are essentially three-dimensional and resemble mine in many ways that have made differential testing difficult. But essentially these systems claim paradigmatic status, transcend the purely descriptive level of factor analysis, and are eminently testable in principle, however difficult it may be to find decisive experiments.

A Paradigm for Intelligence

Intelligence has suffered the same fate as the noncognitive aspects of personality, in that nearly all research has dealt with the taxonomic aspects of the concept, using factor analytic methods (Carroll, 1993). The hierarchical model with some two dozen primary factors at the bottom and *g* at the top is certainly favored by most of the evidence (Eysenck, 1992b; Gustafsson, 1984; Snow, Kyllonen, & Marstalek, 1984), but it is certainly not accepted universally (Horn, 1985). In recent years, a beginning has been made of completing the intervening stages of a more complete model of intelligence ranging from DNA through proximal antecedents (biological intelligence) through measures of *g* to proximal consequences (elementary cognition tasks, such as reaction time and inspection time) to distal consequences (such as scholastic achievement, occupational selection, and vocational guidance). Figure 7 shows the sequence envisaged.

The causal model in question has been outlined several times (Bates & Eysenck, 1993a; Deary & Caryl, 1992; Eysenck, 1986, 1987; Eysenck & Barrett, 1985). Essentially, intelligence is viewed in terms of cerebral efficiency, measured in terms of error-free transmission of information across the cerebral cortex, leading to fast responding (Eysenck, 1987). This model has used experimental tests involving averaged evoked potentials, with complexity of the waveform indicating relatively error-free transmission (Barrett & Eysenck, 1992), reduced uptake of glucose in high-IQ subjects (Haier, 1993), neural adaptability (habituation) to repeated stimuli (Schafer, 1985), and others.

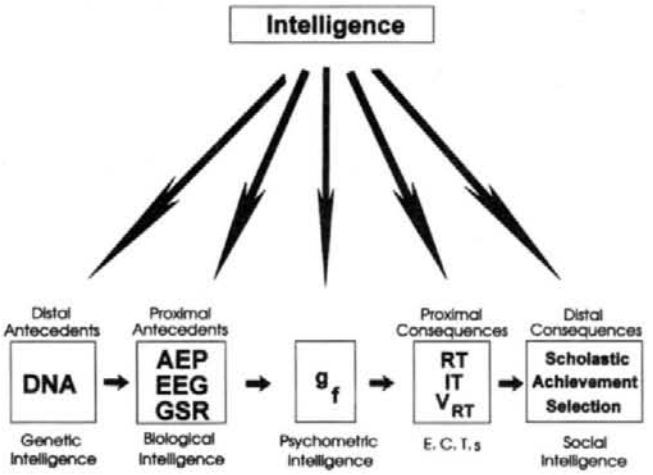

Figure 7. Diagrammatical representation of five steps in any causal theory of intelligence. AEP = average evoked potential; EEG = electroencephalograph; GSR = galvanic skin response; g = general intelligence; f = fluid intelligence; RT = reaction time; IT = inspection time; V_{RT} = verbal reaction time; E.C.T.s = elementary cognitive tasks.

All these methods can be interpreted in terms of differences in the speed of transmission of information across the cortex, caused by differential individual proneness to errors in transmission (Eysenck, 1987). Thus, this is a two-stage theory, with errors in transmission, possibly occurring at the synapse (Hendrickson & Hendrickson, 1982) or due to faults in the brain myelination (Miller, 1994; there are many possibilities, not mutually exclusive). There is a considerable amount of evidence for this theory, embracing psychophysiological studies of proximal antecedents, and studies using elementary cognitive tasks, such as reaction time or inspection time as proximal consequences. Speed of transmission can be gauged by measuring latencies of evoked potentials. Errors can be measured by degree of complexity of the evoked potential waveform.

Again as in the field of personality, I would suggest that this theory may serve as a paradigm, requiring a large amount of normal science to solve the mystery of the many anomalies that have already accumulated. Looking at the average evoked potential (AEP) studies, for instance, one can see that Barrett and Eysenck (1994) failed to find the positive correlation between IQ and trace complexity that appeared in the earlier studies, and Barrett and Eysenck (1992) and Bates and Eysenck (1993b) reported experiments in which the direction of the correlation was actually reversed. These reversals have led many critics to decry the whole approach on the basis of "failure to replicate," but that is not a useful reaction. It seems more reasonable to try to disentangle the reasons for such seemingly opposite outcomes of seemingly similar experiments. In other words, it seems appropriate to use the methods of normal science to look at the anomalous results of experiments and try to disentangle them.

In a large-scale study, Barrett and Eysenck (1994) showed that the complexity–IQ correlation could be found on one group of participants, whose AEP P180 component amplitude was greater than some specified target value, but not in another

group, whose AEP P180 was smaller than the specified target value. This was not a finding capitalizing on chance errors, but was repeatable for different samples. The authors argued that attention is the moderator of the AEP–IQ relationship. They concluded that "conventional averaging of evoked potentials is no longer sufficient for future work in this area. Evidence from our data indicates individual difference measurement information being lost as well as distortion created in the AEP waveforms envelope" (Barrett & Eysenck, 1994, p. 28). The same AEP P180 difference was found to determine degrees of correlation between inspection time and psychometric intelligence.

The inclusion of attention in the general theory also formed part of Bates and Eysenck's (1993b) discussion of their finding of a negative correlation between string length (a measure of trace complexity) on the AEP and IQ. They argued that experimental demands on attention decided the direction of the correlation between AEP complexity and IQ—positive when minimal demands were made, negative when high demands were being made. Bates, Stough, Mangan, and Pellett (1995) directly tested this hypothesis, with positive results. The addition of attention as a moderator variable complicated the overly simplistic picture of the original model but served to explain the apparent anomalies. Future work will demonstrate the degree to which this added hypothesis can improve prediction of experimental results and the correctness or otherwise of the interpretation of the intervening variable as "attention." For the moment, it seems likely that researchers must take into account individual differences in P180 amplitude and attention demand characteristics of the testing situation. The interaction of these two variables (personality and situation) will form a particularly interesting feature of future experimentation.

Can the experimental method help in confirming factor analysis distinctions and in deciding between different interpretations of factors isolated by correlation of methods? I have argued in favor of this possibility (Eysenck, 1995a), using the g_f-g_c theory of Cattell (1963) and the verbal–nonverbal factorial solution of the Wechsler scales as an example. The factorial solution contrasting the verbal and the nonverbal scales is well supported, but are the resulting factors properly distinguished in terms of the verbal or nonverbal nature of the tests involved? The verbal tests might be viewed as power tests, the nonverbal as speed tests, suggesting a different interpretation. Or could one view the verbal tests as examples of crystallized ability (g_c), and the nonverbal as examples of fluid ability (g_f)? Do g_c and g_f react differently to an experimental intervention designed to separate them? These were the questions stimulating the experimental studies involved.

Basing ourselves on the well-established fact that IQ can be increased in children by vitamin–mineral supplementation (Eysenck & Schoenthaler, 1997), we argued that this improvement should occur only for g_f, and not for g_c, for obvious reasons. Crystallized ability is already firmly acquired and is unlikely to be changed by supplementation, but fluid ability should be improvable. Thus, using the Wechsler Intelligence Scale for Children (WISC) as the test, both hypotheses can be tested simultaneously: (a) The verbal and nonverbal scores on the WISC measure g_c and g_f, respectively, and (b) micronutrient supplementation improves g_f but not g_c. The prediction that it would be the nonverbal IQ, but not the verbal IQ, has now been tested in 10 studies and received firm support in 8; the 2 nonsignificant studies suffered from low statistical power in the one and from too short a period of supplementation in the other (although both gave results in the expected direction). Thus, the results demonstrate that taxonomic problems in intelligence can be attacked and answered through the use of proper experimental methodologies. (Micronutrient status using blood samples supported the hypothesis that increases in IQ would occur only in participants low in such status and would improve through supplementation.)

Conclusion

I would conclude that purely taxonomic studies, inevitably correlational in kind, and using factor analytic, multidimensional scaling and similar methods of analysis, cannot achieve paradigmatic status because of the inevitable subjectivity involved in such studies. What is required is a more theoretical approach seeking causal connections and using experimental tests of deductions from the theories in question. Existing theories have already shown the possibility of this approach in the field of personality and intelligence, enabling researchers to answer questions that a purely correlational approach cannot answer.

The advantages of combining the correlational, psychophysiological, and experimental approaches do not all lie on one side. Experimental psychology has suffered greatly by neglecting the whole field of individual differences, and it can be demonstrated that taking personality variables linked theoretically with the dependent and independent variables into account can substantially increase the amount of variance accounted for. Much of this variance is currently thrown into the error term, thus seriously reducing the predictive accuracy of the theory and even leading to failure of replication. As already pointed out by Cronbach (1957), the unification of psychology through the coming together of the correlational and the experimental approaches is one of the most important tasks before psychologists today. What has been done since Cronbach's (1957) address suggests strongly the correctness of his analysis.

Ultimately, the need for such unification rests on one undoubted characteristic of psychology as a science. Psychologists have to deal with persons, not atoms. It is a person who comes into the laboratory: a person with his or her own ideas, emotions, prejudices, bits of knowledge and information; a person with a specific position on the major dimensions of personality; a person with his or her special IQ and specific abilities. All of this must interact in diverse ways with performance on most, if not all, experimental conditions; it must affect memory, learning, perception, conditioning, emotional reactions, psychophysiology—indeed, anything he or she does. The evidence for such large-scale interaction is now conclusive (Eysenck & Eysenck, 1985) and makes it imperative for the relevant personality factors to be included in any experimental design. Conversely, such inclusion, governed always by theory, will throw much needed light on the value of the theory in question and will hopefully lead to improvements in that theory, or even substitution by a better theory and the creation of a paradigm. The failure of both sides to recognize this need is the major factor for the present lack of such paradigms in the study of individual differences.

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