A Model for Personality

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Introduction

H.J. Eysenck

This book is not an introduction to personality research, it is not a textbook, and above all it is not a model *of* personality. The title, *A Model for Personality,* was chosen on purpose to indicate that we are here concerned with a discussion of how models in this field ought to be constructed, what their functions were, and whether such models or paradigms could with advantage be produced at this stage of development. One particular aspect of personality, *extraversion–introversion* (E), has been chosen to exemplify the desiderata which emerge from such a discussion. It is not suggested that personality and E are synonymous – merely that this particular dimension is perhaps better known than any other, has had more experimental work done on it than any other and has acquired a better theoretical substructure, and more links with genetics and physiology, than any other. Hence it seems most likely to serve as an example of how a satisfactory model of personality might ultimately be constructed, i.e. by analogy with E. Other dimensions of personality, such as *neuroticism–stability* or *psychoticism–superego functioning,* are mentioned in the discussion, but only when they overlap or interrelate with E.

The book uses E as an example to illustrate the way in which a model of personality can be constructed, but it is in no way a summary of all that is known about E. Instead of reviewing the huge experimental literature which has grown up around this concept, a task which is becoming every day more and more impossible, we have concentrated on a few key issues, and given a thorough discussion of those important areas on which E has impinged – psychophysiology, conditioning, memory and learning, social behaviour and the like. These areas are important for an empirical definition of E, for an understanding of its construct validity and for a causal analysis of its theoretical underpinnings. Of particular importance in this connection is the chapter on heredity, which links psychological variables with underlying physiological and anatomical ones.

The book is informed by an underlying belief that personality is a fundamental concept in psychology, and that no experimental or applied psychology can flourish which does not incorporate concepts related to personality, such as traits, aptitudes, attitudes, etc. Psychology always deals with *people*, and people are above all else *individuals*, i.e. they behave differently in identical situations. Hence all laws based on *regularities* of behaviour have to be modified by reference to those aspects of human nature which produce *differences*, and the development of laws governing the interactions of these individual differences with the observed generalities is a vital component of a scientific psychology. In the past, the 'experimental' psychologists have tended to go their own way, leaving the task of bringing order into the field of individual differences to psychometrists, personologists and others interested in classification, correlations and nosology. Conversely, those interested in individual differences and personality have tended to

disregard the concepts and laws of experimental psychology, and have tried to construct a science of personality in disregard of what had been accomplished in the broader area. I believe that both sides were fundamentally wrong in trying to go their own way; only by working together are they likely to build up a proper science of psychology, theoretically thriving and practically useful.

It is for this reason that I believe that the model for personality here offered is of importance for the development of psychology. It demonstrates, on the one hand, that experimental psychology cannot do without taking individual differences into account; by doing this it throws away a vital part of the total experimental variance and unduly enlarges the error variance, which in psychology is usually already much too large. And on the other hand it demonstrates that by using concepts and methods of experimental psychology, those interested in personality and individual differences can acquire important conceptual tools for constructing a theoretical system which can link the two fields, which can enable them to make testable predictions, and which alone makes possible the establishment of a proper *causal* theory of personality. These are important possibilities, and in trying to establish the fact that they are based on firm empirical evidence I venture to suggest that if what I have just said can be justified, then we are indeed here dealing with a model for personality, a model which is worthy of being extended to other areas of personality.

The contributors to this book have been selected on the basis of their *expertise* in the various fields represented, and their contribution to these fields; they have all been admonished to be critical as well as constructive in their comments and conclusions. Like all scientific theories, that linking E to cortical arousal encounters many anomalies; this book attempts to stress the existence of anomalies as much as the apparent successes of the theory. As Claude Bernard once said, *scientific concepts are not right or wrong; they are useful or useless.* If the concepts employed herein are found useful – and I believe that few readers will dispute this – then we may hope that further work will clarify the apparent anomalies and extend the theory to areas hitherto not considered in sufficient detail. Indeed, the existence of anomalies proves that a theory is in fact *scientific*: only unfalsifiable theories are without such anomalies, and unfalsifiable theories are by definition outside the scientific pale. This does not mean that we should be proud of such anomalies, and cherish them forever. We should make every effort to clarify the issues, look at different parameters, and try in every way to see if the apparent anomalies cannot be made to conform to our theories; or else we may have to modify our theories in order to incorporate the anomalies. All this is part of the problem-solving aspect of normal science, as Kuhn has emphasized; the tremendous growth of interest in this paradigm, and the equally notable increase in research effort devoted to such extension and clarification suggest that the next few years will see a considerable improvement in these aspects of the theory which are still a little hazy. Perhaps the next edition of this book will contain the answer to many puzzles which are still with us at the time of writing; until then we can only present the theory, warts and all, as it stands at the moment. Readers interested in finding promising research projects will find plenty in the following pages; the theory is by no means finished, even in its major outlines. Nothing would give me greater satisfaction than to find that the book had stimulated keen and eager young scientists to test and if possible disprove some of the hypotheses here discussed; this is the greatest success that a scientific theory can have.

Chapter 1

General Features of the Model

H.J. Eysenck

1.1 Models and Explanations

The problem dealt with in this book was raised in a classical query over 2000 years ago by Theophrastus, in his book *Characters*, written when he was 99 years old: "Why is it that while all Greece lies under the same sky and all the Greeks are educated alike, yet we all have characters differently constituted?" Individuality in human beings is so pronounced, and variability so common, that many have despaired of finding any scientific basis for constructing a model of personality; Allport (1937) has given a clear discussion of the many problems raised. The ancient Greeks suggested an answer in terms of traits and types; the theory of the four temperaments which they put forward has lasted longer than perhaps any other psychological theory, but of course it is open to many criticisms. Can modern psychology do any better?

A glance at recent textbooks does not suggest any very confident affirmative answer (e.g. Hall and Lindzey 1957; London and Exner 1978; Mahrer 1970). What we find is a long list of different theorists, putting forward entirely different views and hypotheses, using entirely different measures, and even types of measures; this is a far cry from the sort of *paradigm* that we are told characterizes science (Kuhn 1962). There is not even sufficient agreement for a revolutionary to rebel against; all there seems to be is a multitude of approaches in search of a unifying principle. The prevailing mood seems to be one of excessive eclecticism; in the words of Feyerabend (1975), "Anything goes". There is indeed an honoured and honourable meaning to the term, eclecticism; it denotes an attitude of impartiality, a refusal to become committed too readily and prematurely, a desire to examine all sides of a problem and to review all the evidence, even where it seems to go counter to one's cherished theories. But it may also mean an easy acceptance of all types of view, good, bad and indifferent; a refusal to exert one's critical faculties or to pass judgments on the adequacy or otherwise of theories and experiments apparently supporting or disproving these theories; a lazy recognition that there is some good in all theories and a fatalistic acceptance of the rules of the caucus race – all have won, and all must have prizes! It is the latter type of eclecticism which is so prevalent in this field, and it is an attitude that is fatal to a proper scientific study of any topic.

The resulting mood of disenchantment seems all-pervasive. If we cannot judge objectively between rival theories, then clearly we shall never achieve the status of a science; if we refuse to elaborate criteria for accepting or rejecting theories, then the achievement of a *paradigm* in Kuhn's sense becomes impossible. I shall attempt, in this first chapter, to suggest criteria which may be useful in judging scientific models of personality; the rest of the book will be devoted to a review of the literature concerning a particular model, with special reference to the manner in which the model stands up to scrutiny, using these criteria. I believe that it is possible to decide between competing alternations along the usual lines of scientific investigation, and that a paradigm does exist even in this complex and difficult field – imperfect and incomplete, but viable and promising. It is hoped that the contents of the following chapters will enable readers to judge the correctness or otherwise of this statement for themselves.

What is that we seek when we study *personality*? What do we mean we seek for an explana-

tion (Craik 1952) of observed individual differences or try to construct a model (Hesse 1966) to help us visualize the complex of explanations contained in a theory? Essentially, we search for concepts (Jammer 1954, 1957) which will help us reduce the infinitude of observed events to a small number of variables tied together by rules or laws; ideally, these concepts and laws should enable us to predict future events and to understand (postdict) past events. Explanations must not be too broad; it is not enough to say that a person's conduct is the product of his heredity and his past experiences. We must clearly specify much more closely the laws of heredity in question (is there dominance, assortive mating, epistasis; how much of the phenotypic variation is explained by additive genetic variance; etc.), and we must state precisely and unambiguously just what environmental events produce just what types of behaviour. But above all we need intervening variables, concepts of the nature of traits, types, abilities, attitudes and what not else that is measurable and can be used to characterize a given person.

Such an approach is often called nomothetic and contrasted with a different, idiographic approach. The latter is literary rather than scientific, intra-individual rather than inter-individual, *geisteswissenschaftlich* rather than *naturwissenschaftlich*; typical of this approach are existentialist schools, phenomenologists and many types of dynamic psychology. It is not our purpose here to discuss or criticize the idiographic approach or to point out the non-scientific nature of such theories as the Freudian or Jungian; this task has been undertaken by others (Popper 1959; Lakatos and Musgrave 1970; Suppe 1974) more versed in the philosophy of science; Eysenck and Wilson (1973) may also be consulted. What we have attempted in this book is the construction of a scientific model of personality, using the term 'scientific' in its classical sense. Admittedly the 'demarcation' dispute, i.e. the question of how precisely to separate science from non-science, is by no means settled, but there is much agreement on many essential elements (Suppe 1974; Lakatos 1976), even though some maverick philosophers, such as Feyerabend (1975), still believe the whole endeavour to be misplaced.

There are three main contestants in the ring; Baconian inductionism, the verification principle of the Vienna school and the falsification principle of Popper. It has always seemed to me that all three principles have a positive contribution to make and that they are complementary rather than exclusive. In particular, it seems to me that we must see these principles as relevant at different stages in the development of a scientific theory. Figure 1.1 illustrates this view. At an early stage of development, we are reduced to fact-collecting on the basis of vague hunches, serendipitous discoveries of unforeseen regularities and inductive generalizations. When sufficient data have been collected along these lines, we are in the position of being able to put forward hypotheses of relatively small compass, and now the emphasis shifts to verification; unless we can verify these hypotheses, at least within the confines of certain parameter values, it is unlikely that they will be pursued further or interest other scientists. Given that this stage is successfully passed, we enter the realm of theory-making proper, and now falsification becomes the most important aspect of our experimental work. When a given theory is firmly established, it becomes a scientific law, and now the paradigm has become settled; only a revolution, sparked off by the accumulation of anomalous findings, and the emergence of an alternative theory, will dethrone such a theory. Thus what constitutes a scientific approach will depend on the degree of development of a particular field; too rigorous a demand at too early a stage may well prevent the proper development of a discipline from ever taking place, just as too lenient a requirement at a later stage of development will prevent the discipline from growing up and assuming its rightful place.

I would suggest that the theory here developed is entering into the third stage, i.e. theory-making proper; this is the stage where the notion of the paradigm becomes appropriate. Such a view may be criticized as unduly optimistic; readers will have an opportunity to study the evidence for themselves and thus answer the question by the use of their own criteria. I would merely venture to suggest that the answer has important implications for the future devel-

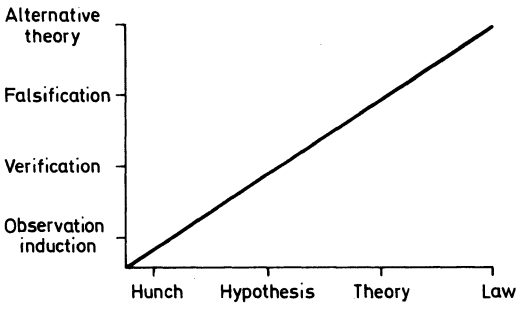

Fig. 1.1. Demarcation theories of science: A unified point of view

opment of research in the field of personality. It has become almost a joke to see the proliferation of concepts and tests in the personality field; Buros (1970) reviews hundreds if not thousands in his compendium, and of the making of further tests there is no end. Unless we are to end up in mindless eclecticism we must at least attempt to reach some sort of consensus on criteria and, if possible, on concepts; without such agreement the psychology of personality will never reach the status of a science.

The development of the model here advocated, in its historical context, is briefly discussed in the next section; as already mentioned, it embodies the Greek concepts of traits and types, and as these concepts have themselves recently been criticized as inappropriate (Mischel 1968, 1977), it may be useful here to discuss briefly the major characteristics of these concepts and the main answers to the criticisms made of them. A more detailed review of the topic has appeared elsewhere (Eysenck and Eysenck 1980). The essence of the criticism was put forward most forcefully many years ago by Thorndike (1903), when he stated that "there are no broad, general traits of personality, no general and consistent forms of conduct which, if they existed, would make for consistency of behaviour and stability of personality, but only independent and specific stimulus-response bonds or habits." In opposition, it is maintained that the following eight statements of the state-trait position are supported by so much evidence that they can be regarded as definitely established:

1) Individuals differ with respect to their location on important semi-permanent personality dispositions, known as 'traits'.
2) Personality traits can be identified by means of correlational (factor analytical) studies.
3) Personality traits are importantly determined by hereditary factors.
4) Personality traits are measurable by means of questionnaire data.
5) The interactive influence of traits and situations produces transient internal conditions, known as 'states'.
6) Personality states are measurable by means of questionnaire data.
7) Traits and states are intervening variables or mediating variables that are useful in explaining individual differences in behaviour to the extent that they are incorporated into an appropriate theoretical framework.
8) The relationship between traits or states and behaviour is typically indirect, being affected or 'moderated' by the interactions that exist among traits, states and other salient factors.

The essential point here is that the concept of 'trait' demands consistency of conduct, whereas Thorndike's and Mischel's view seems to be that such consistency is largely missing. There was much discussion of this point in the '30s and '40s, and a large literature grew up in this connection, starting with the important work of Hartshorne and May (1928, 1929) and Hartshorne and Shuttleworth (1930). All this work has been extensively reviewed by Eysenck (1970a), and the conclusion was that consistency rather than inconsistency was the order of the day – even in those studies which at first sight seemed to come to opposite conclusion, like the Hartshorne, May and Shuttleworth experiments. Mischel (1969) clearly disagrees, when he writes:

I am more and more convinced, however, hopefully by data as well as on theoretical grounds, that the observed inconsitency so regularly found in studies of noncognitive personality dimensions often reflects the state of nature and not merely the noise of measurement.

The basis for this assertion was the partial review of the relevant literature by Mischel (1968), from which he concluded that measures of consistency in personality rarely produced correlations as high as 0.30.

There are many criticisms of this conclusion (Eysenck and Eysenck 1980); here we shall only mention briefly some of the main ones.

(1) Limited and unreliable data-sampling vitiates many studies. Epstein (1977) showed that consistency of moods was only 0.20 when computed on a data-base of 2 days, but rose to 0.88 when a longer data-base was used. (2) Limited and unreliable tests. The Hartshorne, May and Shuttleworth studies found correlations of 0.2 or thereabouts between individual objective classroom tests; these rose to respectable heights when combined into batteries of 9 individual tests, giving predictive accuracies of 0.5 to 0.6 against teachers' ratings of honesty. (3) Poor and unreliable criteria. Predictions are limited in accuracy by the reliability of the criterion; criteria, even in cognitive fields, are often grossly unreliable (Hartog and Rhodes 1936), and even more so in the personality field. All these factors produce 'noise', and measurements must be corrected for attenuation before conclusions can be drawn. (4) If personality variables account for only 9% of the variance, situations only account for 10% (Sarason et al. 1975). If one adopts very stringent criteria for the minimal percentage of the variance that a factor must account for in order to warrant further consideration, then there is danger that researchers will discover that no factors at all are sufficiently important to consider! (5) 'Moderator' variables (Wallach 1962) are important in personality research, but Mischel disregards them as reducing the generality of the trait approach; yet if the situation is complex enough to demand moderator variables to account for this complexity, we cannot avoid using them. (6) Mischel averages consistency coefficients over good and bad, successful and unsuccessful studies; this is meaningless. If one good, carefully researched study based on reasonable theoretical predictions gives a consistency of 0.8, this is not negated because another scruffy, poorly designed study based on no theoretical foundations gives a consistency of 0.0! (7) Consistency may involve more than one factor; thus antisocial behaviour correlates positively with psychoticism, neuroticism and extraversion in children (Eysenck 1977a). Each factor by itself only contributes a relatively small amount of variance, but in sum they make prediction possible with a much greater degree of accuracy. (8) Interaction between personality and situation accounts for a respectable additional amount of variance; such interaction is largely discounted by Mischel, but must realistically be taken into account (Magnusson and Endler 1977). Interaction typically accounts for another 20% of some of the variance; to make this possible, personality must be a consistent variable (Bowers 1973; Sarason et al. 1975).

These and other criticisms of Mischel's position, given in more detail elsewhere (Eysenck 1970a; Eysenck and Eysenck 1980), reduce the force of its impact. He is right, of course, in applying his critical mind to the customary type of personality research, in which some multiphasic questionnaire, often chosen more or less at random (MMPI, CPI, 16 PF), is applied to a population which is also given some other test, or which is rated or measured on some behavioural or experimental variable, without any theoretical expectation of what might be found. Of the resulting 20 correlations, 1 is almost certain to be 'significant' by chance, and with luck some of the observed correlations may be reasonably high. However, these multiphasic scales usually only measure a much smaller number of underlying variables; thus Nichols and Schnell (1963) have shown that the CPI measures essentially only two variables (neuroticism and extraversion, to use our terms), and Reynolds and Nichols (1977) have shown that these two variables carry the full burden of prediction, with the specific variance of the original scales contributing nothing. This sort of situation again makes Mischel's type of averaging meaningless; if the CPI predicts

some type of behaviour, then the resulting coefficient of correlation would be divided by the total number of scales used, and the outcome would be very different depending on whether we chose the number of CPI scales as the denominator, or the two scales which carry the whole burden of predictive validity.

The position here taken would seem to derive some support from the consensus of opinion in Magnusson and Endler's (1977) book, but it may be necessary to look in detail at the alternative view to Mischel's which they offer in their advocacy of 'interactional psychology' They argue that

> Within traditional personality psychology, it is possible to distinguish among three conceptions of molar individual behavior: the *trait model*, the *psychodynamic model*, and the *situationism model*. The trait model and the psychodynamic model... have in common their stress on *person factors* as the main determinants of behavior.... Situationism, in contrast, examines the *environment* to find the important factors that determine the behaviors of individuals. Research within this model has aimed at finding general laws for behavioral reactions as functions of the kind and intensity of external stimulation.

Magnusson and Endler go on to state that on the basis of some empirical research of their own

> A fourth model, called an *interactionist model*, has been formulated. A basic element in this model is the focus on the ongoing multidirectional interaction between an individual and his or her envoronment, especially the situations in which behavior occurs. Persons and situations are regarded as indispensably linked to one another during the process of interaction. Neither the person factors nor the situation factors per se determine behavior in isolation; it is determined by inseparable person by situation interactions.

This view has an immediate intuitive appeal, but the alternatives are unreal and incorrectly perceived and presented.

'Person theories' have never been suggested as being independent of situations; the very names of the traits often researched (suggestibility, sociability, impulsiveness, conditionability, vigilance) explicitly contain mention of the situations suitable for evoking and measuring these hypothetical traits. A vigilance situation could never be used to measure conditioning, suggestibility or sociability; similarly a typical conditioning situation could never be used to measure suggestibility or sociability. Thus 'person theories' automatically include an interactionist-type implication of suitable situations for measurement and evocation in the statement of the traits involved. Similarly it would be to misrepresent Mischel to state that he completely rejects individual differences in his 'situationist theory'; he simply regards variance due to individual differences as unlikely to equal importance of situational variables. In a very real sense both Mischel and 'person theorists' are interactionists; they can be differentiated by their position along a continuum at the extremes of which lie the positions of Thorndike (who really seems to have held the views attributed by Magnusson and Endler to 'situationists'), and those of some hypothetical but non-existent 'person theorists' who completely disregarded the importance of situations. Such a position would be meaningless and self-contradictory, and in reality modern 'person theorists' and 'situation theorists' are much closer together somewhere in the middle of the hypothetical continuum, differing in a quantitative, but not a qualitative, manner. Thus Magnusson and Endler are not advocating a third possibility, but are simply recognizing the same inevitable conjunction of person and situation as producing behaviour which person theorists and situation theorists also recognize. The quarrel, if there is one, is about the general size of the contribution by person and situation factors, and this question is by its nature unanswerable, depending on the precise details of the experiment, and varying in dependence on the extreme nature of the situation, the variance in personality type, the measures used, the value of the theories tested and many other factors. The whole debate is largely semantic; it would never occur to physicists to ask whether predictions about the behaviour of elements or alloys depended more on their atomic constitution or the external influences (changes in temperature, pressure, electrolysis, etc.) to which they were exposed. They would rightly regard the whole question as meaningless; clearly some form of interactionism is implied in the very definition of physical change, as it is in relation to human behaviour.

Ultimately, of course, the argument as it applies to the concepts used in this book must be settled by reference to the experiments employing these concepts; the most important studies have been reprinted in book form elsewhere (Eysenck 1970b, 1976a). These books cover a much wider field than that covered in this volume; the literature has grown so enormously in recent years that no complete coverage would be possible, and consequently we have concentrated on a few fields where sufficient material was available to make reasoned judgment possible. Consistency should be looked at not only in the psychometric sense adopted by Mischel; much more important in our view is the consistency between theoretical prediction and experimental verification. It is with this latter type of consistency that we shall be dealing in the main; the psychometric interpretation will be dealt with only in passing.

What is the nature of the theory here discussed? We shall only state very briefly the outlines and main tenets of the theory, partly because it has been stated at much greater length elsewhere (Eysenck 1967; Eysenck and Eysenck 1969, 1976), and partly because relevant parts will be restated by individual authors in this book at the appropriate place. Essentially the theory asserts that human personality can be described in terms of traits, such as sociability, impulsiveness, activity, worrysomeness, carefreeness, etc. which are intercorrelated and form higher-order 'superfactors'. The three main superfactors have been variously named by different investigators, but will here be called E (extraversion–introversion), N (neuroticism–stability) and P (psychoticism–superego). These factors, or others remarkably similar to them, have been found over and over again in many different studies (Royce 1973), and may even be isolated in animals (Chamove et al. 1972; Broadhurst 1975). These factors have a strong genetic basis (Eysenck 1976b), and psychophysiological theories have been elaborated, linking E with the reticulo-cortical arousal loop, N with the limbic system (visceral brain) and P with the androgen hormone system (Eysenck 1967; Eysenck and Eysenck 1976). These factors make possible predictions in the experimental, social, educational, psychiatric, criminological and other fields (Eysenck 1976a; Eysenck and Wilson 1978; Eysenck and Rachman 1965; Eysenck 1977b). There are specific hypotheses, such as that the reticulo-cortical arousal loop is not independent of the limbic system, so that any activation of the latter will be shown up in the heightened arousal consequent in the former, whereas there is no reciprocal relation of this kind (Eysenck 1967). Pharmacological agents affect behaviour in predictable ways, depending on the personality type involved (Broadhurst 1978), and so does brain damage (Eysenck 1967). The range of possible predictions is therefore very wide, and verification or disproof of the theory can be looked for in many different directions. This book will be concerned largely with a review of the strictly experimental literature in psychophysiology, conditioning, learning and memory, brain damage and drug effects, but one chapter has been devoted to the broader social field.

1.2 The Development of a Paradigm

A paradigm may be expected to have a developmental history; even though it may have arisen in one man's brain, nevertheless it must have had precursors, and after its birth must go through many stages before being widely accepted. So with the model here discussed. I believe that an adequate understanding of the problems in this field, and the attempted solutions as well, is impossible without some knowledge of the history and development of the psychological theories of personality. There are roughly speaking 12 periods of development, each associated with an outstanding personality whose work marked a definite advance; it may be useful if these 12 advances are defined in some detail. It is often said that psychology has a long past, but a short history; this is equally true of the study of personality. The moment when intuitive understanding, philosophical speculation and clinical intuition, which constituted the past, gave way to experimental study, psychological theory and psychometric

analysis can be defined more easily here then in most other areas of psychology; the turning-point is associated with the extremely original and fundamental work of a man whose very name is probably unknown to most psychologists, even those who are actively working in the field of personality study. This man was the Dutch philosopher and psychologist G. Heymans (1857–1930),.who published his views and results in book form in 1929 but who had written his fundamental papers (with E. Wiersma and H. Brugmans, Heymans and Wiersma 1906–1909) some 20 years earlier. We shall see in a minute just why it is he, rather than others who are more often named and who are better known to English-speaking psychologists, who may be said to mark the transition point from unscientific past to scientific history.

(1) The story begins – if the human search for an understanding of personality, individual differences, temperamental pecularities and other deviations from the strictly average sort of behaviour can in any real sense be said to have a 'beginning' – with Galen, a Greek physician who lived in the second century A.D. and who is widely credited with the enunciation of the doctrine of the four temperaments. The notions of the melancholic, the choleric, the sanguine and the phlegmatic, shorn of the associated theory of the 'humours' which were believed to cause their striking differences, have passed into every-day language, and the man in the street still uses these phrases in characterizing certain 'types' of behaviour. As we shall see, the theory of extraversion–introversion is intimately connected with this ancient theory, ridiculous only to those who do not realize that it embodies a large slice of excellent clinical observation, without which it would never have been accepted or have lasted longer than any other psychological theory. This is not the place to go into the vexed question of Galen's originality in this respect, or to discuss possible prior claims of Hippocrates and others; I am not sufficiently expert to discuss these questions, and for the purposes of this book they are not of too great importance. The reader interested in the early development of these theories may with confidence turn to A.A. Roback's *Psychology of Character* (1927).

(2) The second chapter of our story opens with the contribution made by the great German philosopher and scientist Immanuel Kant in his book on *Anthropologie*, which was a kind of text-book of psychology and in which he brought the doctrine of the four temperaments up-to-date, popularized it and made it acceptable to philosophers, physicians, theologians and other learned men concerned with human personality. Eysenck and Eysenck (1969) have translated his descriptions of the traits characterizing the four temperaments and have drawn attention to the close relationship between these descriptions and the results of modern factor-analytic work in this field; they also point out that the main difference between his views and more modern ones lies in his categorical conception of 'types' as being unchangeable and pure. A person belongs to one of these four groups; he cannot change his position, and there are no intermediate degrees. This notion of 'types' has been rightly criticized by modern American writers, but unfortunately they ascribe such views to more recent writers than Kant – writers who in fact do not hold them, like Jung and Kretschmer.

(3) Modern typology parts company with Kant in this respect, and the person who took this important step of translating categorical types into continuous dimensions, and who thus marks our third epoch, was none other than W. Wundt (1874). His contribution has been discussed elsewhere (Eysenck 1964), and therefore little need be said here other than that he pointed out that cholerics and sanguinics both shared the characteristic of being *changeable*, while phlegmatics and melancholics were *unchangeable*; substitute 'extravert' and 'introvert' for changeable and unchangeable, and our modern theory (in its descriptive aspects) is born. Add that he considered a second dimension (emotionality – nowadays often labelled neuroticism or instability) to be formed by the two emotional temperaments, i.e. the choleric and the melancholic, as opposed to the other two, which were considered by him unemotional, and you have a two-dimensional description, continuously variable, of personality, very much as it is given by recent writings of Cattell, Guilford or the present writer (see Fig. 1.2).

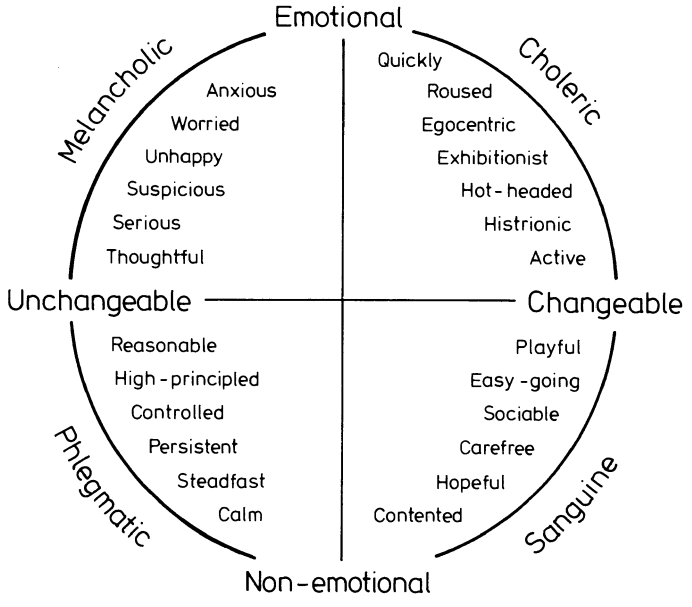

Fig. 1.2. Traits traditionally associated with the 'four temperaments' of Galen, arranged along two major dimensions of personality. (According to Wundt 1874)

Wundt, like Heymans, is seldom if ever mentioned by English-speaking writers on personality, in spite of his very important contribution. (Ebbinghaus, too, wrote along rather similar lines.)

(4) The fourth great contribution comes from O. Gross (1902, 1909), a Viennese physician, who attempted to give a physiological basis to the personality dimension of extraversion–introversion (not then so called, of course; Gross wrote at the beginning of the present century). He conceptualized mental and emotional processes in terms of a primary function, subserving sensation and perception, and a secondary function, which subserved the perserveration of primary processes; individuals differed according to the length of the secondary process – introverts had a long, extraverts a short, secondary process. He showed in fascinating detail how this conception (which tied up with the newly proclaimed theories of perseveration of the memory trace, by Müller and Pilzecker) could be used to account for the personality traits of the two types posited. His physiology is of course entirely speculative, as he himself recognized; it is fascinating to see how he (and later on MacDougall 1929) tried to invent something akin to the Ascending Reticular Activating System, and how both succeeded in describing (by deduction from behaviour) something which at that time was far beyond the ken of physiologists and neurologists. Truly, if the reticular formation had not been discovered, it had certainly been invented by psychologists anticipating later developments.

(5) We come now to the fifth epoch, and to G. Heymans and his colleagues. His contribution is threefold, and in each of his innovations he anticipated a large and important area of research (Heymans 1908; Heymans and Wiersma 1906, 1907, 1908, 1909):

(a) *Psychometric.* Heymans was the first person to realize the importance of quantifying the implicit relationships between traits which had served earlier writers; where they simply observed and noted subjectively 'what goes with what' (in Spearman's phrase), he suggested the use of correlational methods, and worked out a very crude and elementary, but nevertheless useful, coefficient of association. He also worked out methods of grouping such correlations, thus in essence anticipating factor analysis. He was not a gifted mathematician, and curiously enough rejected product-moment correlation coefficients for quite the wrong reasons, but he did have an intuitive understanding of

the logical requirements of mathematical analysis, and pioneered what are now widely used methods.

(b) *Experimental.* He was perhaps the first person to realize that observation of every-day behaviour is not sufficient to build a science of personality on, and he carried out experimental studies to measure individual differences in behaviour; these are perhaps the first truly to deserve the name of 'experiments in personality' – Galton's studies, to take but one example that seems to disprove this generalization, were not experimental in the laboratory sense.

(c) *Hypothetico-deductive method.* He realized that science is intimately tied to the use of the hypothetico-deductive method, except perhaps in its first, tentative steps, and he linked the theories of Gross with his psychometric work and his experiments into a nomological network, to use a term which would have been new to him, but the implications of which were apparent in his work. These three major contributions entitle him to be called the father of experimental personality research; unfortunately, his writings are widely dispersed and do not lend themselves to detailed exposition, but a description, with quotations, of his work has been given in *The Structure of Human Personality* (Eysenck 1970a).

(6) The next claimant for a place in our company of immortals is C.G. Jung (1933), whose contribution to personality study is often misinterpreted. C. Spearman, in his classic *Abilities of Man* (1927), sums up the work of Heymans, Wiersma and Brugmans by saying: "So far as scientific status is concerned, this Dutch work stands upon a very high plane. In it mere causal observations – shown over and over again to be grossly misleading – are replaced by most careful and systematic investigations." He goes on to characterize Jung with equal insight: "Ideas substantially the same as those mentioned above re-appeared not long afterwards in the work of Jung. But the arduous scientific research of his predecessors ... now gives way to attractive literary embellishment." Jung is often credited with giving a long list of other writers who preceded him in delineating his types of extraversion and introversion; it is interesting that although these types are so very similar to Heymans' carefully researched strong and weak secondary function types, yet Heymans is never mentioned – in spite of the fact that much of his work was published in German and must have been familiar to Jung. If Jung's descriptions are not original, neither is his use of the terms extraversion and introversion; these had been used in European writings for several hundred years before him. His main claim to originality must be his suggestion that extraversion was linked with the hysterical group of neurotic disorders, introversion with the psychasthenic group (dysthymia – anxiety, reactive depression, phobias, obsessive-compulsive disorders). There appears to be some truth in this observation, and while neurotic typology must be credited to Janet rather than to Jung, nevertheless the identification with normal personality types is important in the historical development of the concept (Eysenck 1947).

(7) Related to Jung in that his main concern was with the abnormal counterparts of normal personality types was E. Kretschmer (1948), but his main contribution did not lie in his identification of extraversion ('cyclothymia') with manic-depressive insanity and introversion ('schizothymia') with schizophrenia (Jung too had thought of schizophrenia as being linked with introversion). The evidence does not suggest that schizophrenia does in fact have such a link; such a generalization would not now be acceptable – although it must of course be realized that the term 'schizophrenia' means many things to many people, and that its use in modern Anglo-American psychiatry may not be identical with its use in German-speaking circles 50 years ago. However that may be, Kretschmer's continuing fame rests on his insistence on the importance of constitutional factors, and on his insight into the relationship between leptomorphic bodybuild and introversion. (See also his American disciple, Sheldon 1940, 1942). While again the evidence regarding bodybuild and insanity, on which he insisted so strongly, is at best inconclusive, there seems to be no doubt that in the normal field at least a relationship of the kind postulated by him exists – although much weaker than he (and Sheldon, who took up his system with minor modifications) believed. Correlations of 0.4 or

thereabouts are the most that can be expected when the elementary errors in conducting such experiments which disfigure his and even more Sheldon's work are rectified (Eysenck 1970a). (Both Kretschmer and Sheldon contaminated their judgement of temperament and of diagnosis by having knowledge of the bodybuild of the subjects in their studies; this contamination produced unacceptably high correlations often exceeding the reliabilities of the ratings involved! Furthermore, Kretschmer took little trouble to partial of the effects of age; later work has shown this to be essential.) But constitutional factors are important, as we shall see, and Kretschmer was the first to insist on their importance.

(8) The pace now quickens, and our epochs begin to overlap. After Jung, the next great writer to be noted is perhaps C. Spearman, the founder of the London School – a 'school to end schools', as he once put it, in an attempt to crystallize his belief that the method of factor analysis, which he introduced into psychology, was capable of substituting objective, quantitative fact for subjective, intuitive belief. Through his students (Webb, Garnett and Oates) and his collaborators and successors (notably Burt, Stephenson and Cattell) he exerted a profound influence, and while history remembers him more for his work in intelligence measurement, we must note here that he was the first to demonstrate the existence of the two factors, strictly defined and measured, of emotionality–neuroticism ('w' in his terminology) and of extraversion–introversion ('c' in his terminology). He also tried to elaborate experimental tests of perseveration, with which to measure these personality traits; these were unsuccessful, possibly because he and his students were thinking in terms of psychometric group tests, not in terms of experimental laboratory examinations, given to one person at a time. Whatever the defects of his work, viewed from the vantage point of hindsight, his contribution, substantive and methodological, was crucial in transplanting the Dutch work into English soil (Eysenck 1970a).

(9) The contribution of our ninth great figure, J.P. Guilford, can best be understood in terms of the problem which he set out to solve. Briefly, the situation may be summarized by saying that the success of the Woodworth Neuroticism Questionnaire, and the appearance of the English translation of Jung's book, inspired many psychologists in the U.S.A. to produce questionnaires of neuroticism and introversion respectively. The largely subjective method used for picking out items and combining them in an essentially arbitrary fashion guaranteed that these 'measuring instruments' measured nothing in particular, and when it was found that neuroticism inventories intercorrelated only about 0.3 with each other, while neuroticism and introversion inventories showed correlations of equal size, it was concluded that this whole approach had been a failure (Vernon 1938). The bitter taste of this failure survived for a long time, without the realization that it was not due to any faults in theoretical conceptualization or in the principle of questionnaire construction, but rather to inadequacies in the make-up of these particular questionnaires. It is easy to see this now, but at the time many psychologists vowed never again to use personality inventories and never again to think in terms of introversion–extraversion; in many cases this vow survived the Second World War and is only slowly losing its compulsive force. Guilford's great contribution was the realization that the intercorrelations between inventory items, and the factor analysis of these intercorrelations, constitute indispensable steps in the isolation of stable personality factors and the construction of suitable questionnaires.

Guilford also contributed experimental studies which at the time were outstanding examples of the laboratory approach to personality study. If the findings were largely negative, this was perhaps inevitable at the particular stage of development reached at that time by both personality theory and experimental psychology (Guilford et al. 1976).

(10) Our tenth author is the Russian writer B.M. Teplov, who has taken up Pavlovian teaching with respect to the 'strong' and 'weak' nervous system, and has built upon this an impressive series of experimental studies of individual differences, ably recounted in English by J. Gray (1964). It has always seemed to me that Teplov's 'weak nervous system' is analogous to the introverted type, his 'strong nervous

system' to the extraverted type; hence his inclusion in this list. A lengthy discussion of the points of similarity and difference between the two typologies is included in this book, so no more needs to be said on this point. It may be worth while, however, to point out the novelty and interest of many of the techniques pioneered by the Russian workers. The stereotyped choice by Western psychologists of such obviously poor measuring instruments as the Rorschach or the M.M.P.I. when called upon to investigate personality traits is put to shame by the inventive genius of the Moscow group. Perhaps only Cattell escapes this censure on our side of the fence, because he, too, has attempted (with considerable success) to break out of the bear-hug of tradition. The Russian work, too, has its weaknesses, of course, and these may loom larger to psychometric readers than its strengths; but Teplov's successors are taking great strides to eliminate these weaknesses and the immediate future may benefit greatly from cross-fertilization.

(11) We are now nearing the present day, and the work of our next exponent is still very much in progress. (Guilford, too, is of course still active at the time of writing, but his interest has shifted to the study of cognitive dimensions and originality.) R.B. Cattell has transferred the traditions of the London School to American soil, and has combined exceptional mastery of statistical techniques of multiple factor analysis with large-scale empirical studies employing ratings and self-ratings, and objective, experimental and physiological measurements of the most varied groups. This work goes well beyond the confines of our interests here, but it should be noted that in all his groups the two factors (usually extracted as higher-order factors derived from the intercorrelations between oblique primary factors) of extraversion–introversion and neuroticism (called 'anxiety' by him) emerge more clearly and strongly than any others (Cattell and Kline 1977). Since Cattell is undoubtedly the foremost living exponent of the factor-analytic approach, constant verification of the fundamental descriptive hypothesis on which much of the material in this book is based is most valuable and welcome, and the large area of factual agreement between him, Guilford, and the present writer on this point has been documented in great detail elsewhere (Eysenck and Eysenck 1969).

(12) Last, least, and only after much hesitation the writer would place his own contribution. In essence, what he has tried to do has been a continuation of the threefold approach of the Dutch school, as adapted by Spearman and turned by him into a characteristic of the London school. Our psychometric work has been summarized extensively, with much new material, in *The Description and Measurement of Personality* (Eysenck and Eysenck 1969). Our experimental work has been similarly summarized in *The Dynamics of Anxiety and Hysteria* (Eysenck 1957) and later papers and writings. Our hypothetico-deductive approach can best be studied in *The Biological Basis of Personality* (Eysenck 1967), in which an attempt is made to deduce extravert–introvert differences in behaviour, both social and in the laboratory, in terms of differences in cortical arousal, mediated by the reticular formation. The success of these efforts is still too doubtful, and the work itself too recent, to comment on it in any detail; the reader will be able to judge for himself after perusal of the material in this volume.

Careful reading of documents straddling 2000 years of historical development have given rise to some general impressions which may be useful to newcomers to this field. In the first place, there is a strong feeling of historical continuity. Galen's and Kant's observations do not strike the modern observer as ridiculous and outmoded; our own work may be more extensive, better controlled and statistically more defensible, but it is recognizably a development of ideas mooted all these centuries ago. Gross's and Heymans' speculations about physiological mechanism have little factual substratum, but they are not out of line with what we now know about the structure of the cortico-reticular arousal loop and its functioning. Spearman's and Guilford's early factorial studies are now very out-dated, but modern methods, aided by computers, do not give results essentially different from theirs. In fact, what we recognize throughout this historical development is the usual scientific progress, slow, step by step, brick by brick, until finally we arrive, almost by stealth,

at a splendid, well-built usable structure. So many hands have made their contribution that it becomes difficult to say: *he* built it. All those who contributed have built it, although some have made a bigger contribution than others.

In the second place, there is a feeling that for a long time contributions were made by single people, or at best small groups; others were slow to take up the contributions made. Heymans' work has been followed up in Holland, and later in South Africa, but is hardly known elsewhere; even the recent work of Cattell is carried out mostly by his students and fellow-workers, not by the general body of research students. This position is slowly changing; gradually a more general approach is being elaborated in which theories are being tested in different laboratories all over the world.

A third point which may be important is that personality study is beginning to cease to be the prerogative of a small set of psychologists who happen to be interested in individual differences, while the great body of experimental and theoretical psychologists goes its own way, profoundly unmoved by whatever may be going on in this small corner. If extraverts and introverts differ in their habitual arousal level, as well as in their sensory thresholds, orienting reactions, adaptation rates, E.E.G., E.M.G., and G.S.R. response patterns, rates of conditioning, perceptual after-effects, and a thousand and one psychological and physiological measures, then it ceases to be practical for the experimentalist to proclaim his disinterest in 'personality' and relegate individual differences to the error term in his analysis of variance; interaction terms, embodying personality in the form of extravert–introvert differences, become extremely important and should be extracted from any well-planned study, even when personality differences are not the main point of interest. I have discussed this point at some length elsewhere (Eysenck 1969) and will not insist on it here at any greater length. It is my impression that the lesson is gradually being learned, and that more and more hardbitten experimentalists are taking individual differences into account.

It is unfortunate in this connection that the terms extraversion and introversion are in many people's minds linked so closely with the putative father of this personality typology, C.G. Jung. From the point of view of scientific study, his contribution has been largely a negative one; by allowing his mystical notions to overshadow the empirical, observational data he has done his best to remove the concept of personality type from the realm of scientific discourse. His extremely complex system, involving four 'functions' arranged in contrasting pairs, all of which can be extraverted or introverted, and which compensate each other in a complex manner in which conscious extraversion may be linked with unconscious introversion, has not found much favour with even his more devoted followers; as he once pointed out when questioned on whether a given person was extraverted or introverted: 'In the last analysis I decide who is an extravert and who is an introvert!' This splendid assertion of faith mirrors Goering's famous statement when someone pointed out that his personal favourite, *Luftwaffe* General Milch was in fact Jewish: 'I decide who is a Jew!', but it proves somewhat less attractive to scientists who are attempting to construct a universal, objective science of personality structure and measurement. Psychologists will have to learn the plain historical fact that the personality types of extraversion and introversion owe very little to Jung, and the sooner this message reaches psychological textbooks the better.

It is interesting that the first appearance of the term 'extraversion' in an English dictionary, appropriately enough, is in Dr. Johnson's *Dictionary of the English Language*, which appeared in 1755; it does not tell us very much, however, as he defines it as 'the act of throwing out: the state of being thrown out.' J.A.H. Murray, in the *Oxford Dictionary* of 1897, quotes G. Coles (1692–1732) as having used the term in a rather more modern sense – 'a turning of one's thoughts upon outward objects'. M.E. Lazarus, in his book *Love versus Marriage*, which was published in 1852 in New York, speaks of 'introversion, the turning inward of the being to act against himself.... The habit of introverted thoughts has very morbid tendencies and incapacitates us from appreciating the real values and beauties that surround us.' And in 1899, W.D. Whitney in his *Century Dictio-*

nary defined introversion as 'the act of introverting, or the state of being introverted; turning or directing inward, physical or mental'. Thus the terms themselves were current long before Jung's book appeared, and they were used with a meaning not too dissimilar to that which they have now assumed – which is very different from that they assume in Jung's psychology!

This is inevitably a very brief and summary sketch, but it will give the reader a feeling for the historical roots of our present-day concepts in this field, as well as illustrating the manifold influences which have been brought to bear on it. The development of the concept of extraversion has the aspects of a typical paradigm, growing in extent, in rigour, in sophistication, in acceptability, but without losing its original meaning and identity. Galen and Kant would have recognized our present-day concept as having grown out of their own observations and theorizing; so would Wundt and Gross, Heymans and McDougall. This link with history is important in a science which thrives on fads which are here today and gone tomorrow; not thus are paradigms created!

1.3 Theory Making: Correlational and Experimental Psychology

The general theory dealt with in this book has two separate but intertwined strands, the first descriptive, and based on factor analytic arguments and demonstrations, the second causal, and based on experimental tests of deductions from the theory. Both aspects have generated large numbers of empirical studies; Buros' *Eighth Mental Measurements Yearbook* lists over 700 studies using just one of the Eysenck questionnaires, and on the Department files there are some 5000 reprints relevant to the theory. However, there are many questions attaching to the attempts made to support or disprove aspects of the theory, and it seemed important to discuss some of these questions in this section. We shall deal with both descriptive and causal theories, but before going into details it may be useful to discuss briefly the position taken here concerning what Cronbach (1957) has called the two disciplines of scientific psychology. The term refers to correlational studies, concerned with individual differences, and experimental studies, concerned with general laws. Cronbach makes the point that these two disciplines have in the past had little to do with each other, but that without active cooperation between them no scientific psychology is possible; the two disciplines complement each other and each needs the support of the other. This view has been strongly endorsed by the present author (Eysenck 1967), who makes two major points.

The first point is that correlational psychology cannot in the nature of things come up with objective, universally agreed dimensions or categories; there are innumerable, mathematically equivalent ways of rotating factors, for instance, and no statistical magic key (not even simple structure) can close the door on alternative solutions. Psychological theories generate correlational studies (and sometimes the other way round); these studies can refine and partly disprove the original theories, leading to further and better correlational studies; the process is infinite, but it also hides an infinite regress. Alternative solutions and rotations are in principle, and usually in practice, not only possible but also appeal to different people. Factors shade into each other, as do concepts; there are no clear-cut dividing lines. Even the distinction between first-order, second-order and higher-order factors is relative to the selection of original items or tests; there is no fundamental hierarchy determining the level of different concepts once and for all. The failure of factor analysts to come up with an agreed solution to the problem of what and how many primary factors is a case in point; if a method is as objective as adherents often claim, then in the 50 years since its inception one would have expected a more apparent consensus! The final factors never completely escape the shadow of the initial selection of items or tests, or the selection of methods of extraction and rotation (Eysenck and Eysenck 1969). Even in the much better researched field of intelligence, there are still controversies about the need for a general factor (g), with some psychologists,

like Guilford for instance, preferring a solution solely in terms of a large number of primary factors. (Guilford's solution is in my view not reconcilable with the data he himself has provided, but the general preference of many American psychologists for some solution eliminating *g* cannot be so easily dismessed – Eysenck 1979a).

The crucial failure in all this work is of course the lack of any causal hypotheses; purely correlational studies are inevitably circular, and only the incorporation of measures external to the circle, and linked with theories concerning the causation of the factors hypothesized, can take us outside the *circulus vitiosus*. It is for this reason that I have laid such emphasis on theories linking arousal, as a reticular formation – neocortex loop, with extraversion–introversion; if it is feasible to deduce all the observed phenomena from such an hypothesis, and predict others, not yet observed, then clearly we have left behind the difficulties associated with a purely correlational approach. The contribution here of experimental psychology is vital in that it provides us with the concepts in terms of which to phrase such causal theories; the concept of arousal derives from laboratory studies of experimental psychologists and physiologists, although early theoreticians like Gross were already searching for some such concept as implied by their observational data. It is of course not impossible that the crucial concept identifying the causal element might be generated independently of experimental psychology, but the probability of this happening is not perhaps very high.

It should perhaps be said at this point that in asserting that correlational psychology has need of concepts and methods of experimental psychology, there is no attempt to downgrade the importance and value of correlational studies and multivariate analyses (Cattell 1966). Experimentalists in the narrow sense often look down with scorn on the users of correlational methods, considering this in some sense as an inferior method to that of functional analysis; this is a curious misconception. Even in physics it is becoming realized that statistical concepts and methods are fundamental for any but the grossest action sequences under investigation, sequences embodying millions and trillions of separate entities, and even there fundamental discoveries owe their existence to the simple observation of correlations. The discovery of Hubble's Law for instance, which is absolutely fundamental to modern cosmology, is based on the observation of a correlation observed by him between the distance and velocity of recession from the earth of different galaxies. In spite of some still persisting doubts as to whether the Doppler effect which is used in this correlation is of truly cosmological significance, no physicist would doubt the importance of Hubble's work, in spite of its reliance on the despised correlation coefficient.

However, it would be wrong to think of the relation between correlational and experimental psychology as consisting entirely in contributions made by the latter to the former; as remarked before, the relation is one of complementariness, and our second point therefore is that in most cases experimental psychology cannot function properly without reliance on the results of correlational psychology. The reason is simply that psychology studies the behaviour of organisms, and these organisms react differentially to identical stimuli. The differences observed may be merely quantitative, e.g. that some of Pavlov's dogs condition very quickly and strongly, others very slowly and weakly; this by itself would be of the utmost importance to any theory of conditioning, or to its application to social functioning. Often, however, the differences are qualitative, i.e. some people (e.g. introverts) react in ways that are the exact opposite to that in which other people (e.g. extraverts) react. When this happens, disregard of such personality differences by the experimental psychologist will lead to the variance due to them being accumulated in the error term, which is thus typically swollen out of all recognition, until it swamps (as it too frequently does) all the main effects variance.

One example must suffice to illustrate this effect; others will be found throughout the rest of the book. Since Urbantschitsch's (1883) original work on intersensory facilitation, there has been a considerable body of experimentation indicating that the stimulation of one sensory receptor can facilitate the perception of stimuli

in some other sensory area, presumably through an increase in the arousal level. However, many of the results reported have been quite divergent and contradictory, and no generally tenable conclusions have emerged. Recent work has been concerned with the parameters involved in this effect, such as the intensity of the heteromodal stimulus, but there has been an almost complete lack of concern with personality differences, and most writers have concluded that the effect can be either facilitatory or inhibitory (or presumably non-existent), a conclusion reminiscent of much of what passes as experimental psychology, and completely unenlightening. Shigehisa and Symons (1973a, b; Shigehisa et al. 1973) used the personality dimension of extraversion–introversion to reconcile apparent contradictions.

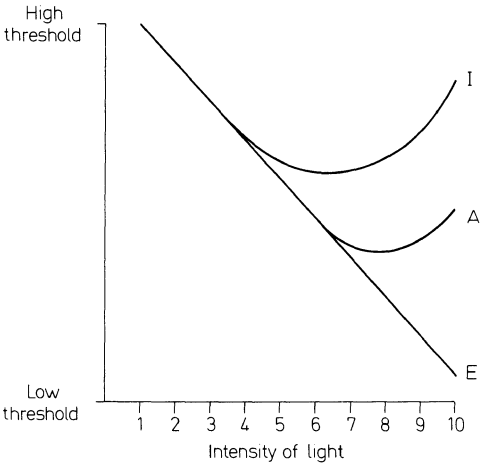

Fig. 1.3. Predicted change in auditory thresholds of introverts, ambiverts and extraverts with change in the intensity of ambient illumination

Their argument proceeds on the basis of two assumptions. The first makes use of the well-known inverted U relation between drive (arousal) and performance; this is similar to Pavlov's two laws of *strength* (an increase in stimulation will produce an increase in response) and *transmarginal inhibition* (beyond a certain point, further increases in stimulation will produce increasing inhibition to protect the neurons against possible damage). Thus the first stage postulates that as heterosensory stimulation increases from a very low level, there will at first be facilitation of the perception of stimuli in the other modality, but that after an optimal point is reached, there will be inhibition. There is much evidence in the literature for some such generalization, but we now come to the crucial second stage of the argument, which states that introverts, having higher levels of arousal to begin with, will reach this optimal stage sooner than ambiverts, and these will reach it earlier than extraverts, who start out with a particularly low level of arousal. This is a perfectly clear prediction, which was tested by submitting ten extraverts, ten ambiverts and ten introverts to an experiment in which auditory thresholds were measured under conditions in which illumination was varied in intensity in ten stages. The predicted effect is shown in Fig. 1.3, and perusal of the original paper will show that the results were precisely as anticipated. The phenomenon was capable of replication, and the procedure could be reversed, i.e. varying auditory stimulation while measuring visual thresholds, with equal results. The study shows clearly that only by designing the experiment with both experimental parameters (intensity of stimuli) and personality parameters (extraversion–introversion) in mind, as well as a usable theory of the regression of the phenomenon on arousal can we hope to achieve reproducible, meaningful results. As I have tried to argue elsewhere, and have illustrated with many examples, this is the usual kind of result when experimentalists take seriously their duty to look at the nature of the organism studied, as well as the parameters of the experimental situation (Eysenck 1976a). Arousal is such an important variable in most psychological experiments that it may be said almost axiomatically that there can be few studies in experimental psychology which would not benefit by having the extraversion–introversion dimension controlled and used as either a main effect or more frequently as responsible for an interaction effect (Eysenck 1967).

In using personality variables in this interactive and integrative fashion, however, there are many problems and difficulties which it may be useful to mention and briefly discuss at this point. The first has already been mentioned, namely the prevalence of curvilinear regressions as far as arousal (and hence extraversion–intro-

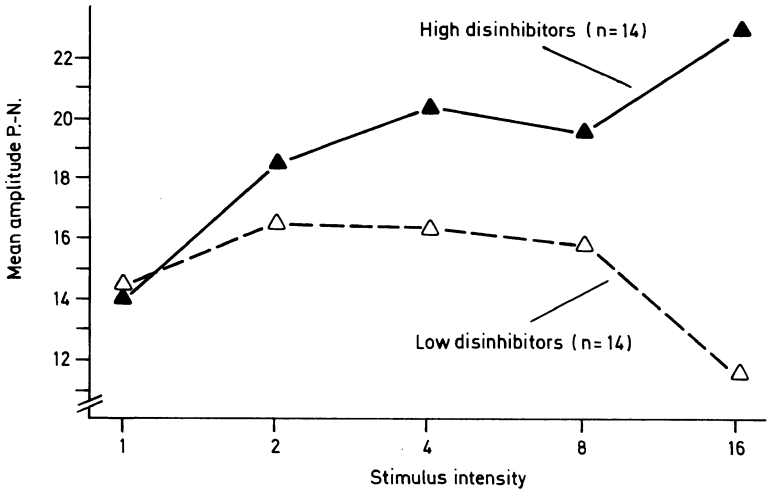

Fig. 1.4. Mean AEP (averaged evoked potential) amplitude for low and high disinhibition scorers (introverts and extraverts) at different levels of stimulus intensity. (After Zuckerman et al. 1974)

version) is concerned. The large literature on the Yerkes–Dodson law, the Pavlovian laws of strength and transmarginal inhibiton, and the inverted-U function shows the great and almost universal tendency for increases in responses as a function of increases in stimulation to be self-limiting. Figure 1.4 shows the results of an experiment using the AEP (averaged evoked potential) as a function of intensity of visual stimulation; there are five levels of stimulus intensity, and the personality variable measured was 'disinhibition', which is correlated with extraversion. It will be seen that as in the Shigeshisa studies, there is a linear regression in the high disinhibition (extraverted) group, and a curvilinear regression in the low disinhibition (introverted) group; quite probably the linear regression would turn down too if the intensity were increased by another factor of 2.

Corcoran (1965) has put forward several arguments to indicate the usefulness of the curvilinear relationship and its correlation with extraversion–introversion for experimental studies. He states, quite rightly, that the inverted-U relationship has been used rather loosely, in that some (e.g. Hebb 1955; Malmo 1959; Duffy 1949) have used the relation as an aid to theory, while others (e.g. Freeman 1940; Courts 1942; Schlosberg 1954; Stennett 1957) have found that the relation fits their data. As he points out, "the assumption is, however, loose and ill-defined since with a U-function direct prediction of the value on one axis from knowledge of the other is not always possible." This difficulty arises from the fact that on such a curve, for any given value of performance except the optimal there will be two possible values of arousal, so that although level of performance is predictable given level of arousal, level of arousal cannot be ascertained merely from knowledge of performance. As Fig. 1.5 shows, this ambiguity can be resolved, however, by experimentally increasing or decreasing arousal level; the direction of change of performance will then indicate the location of the subject

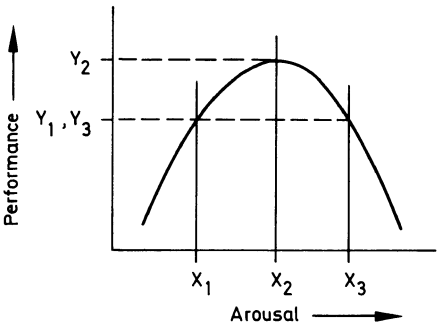

Fig. 1.5. Performance level Y_1, Y_3 would result from arousal levels X_1 or X_3. Given values Y_1, Y_3 it is possible to determine whether arousal level is at X_1 or X_3 by manipulating level of arousal and noting directional change in performance. (After Corcoran 1965)

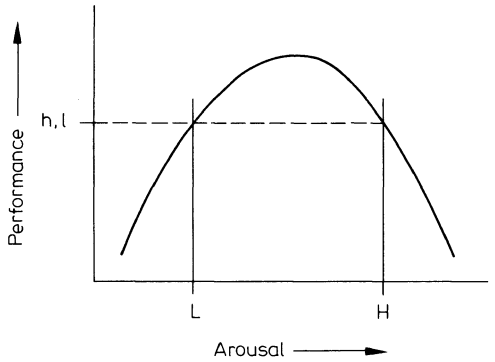

Fig. 1.6. Groups L and H both perform at h, l. By manipulating level of arousal it is possible to determine which group is at H and which at L. (After Corcoran 1965)

on the abscissa. In a similar fashion, the position of two groups differing in personality (e.g. extraverts and introverts) on a performance variable can be identical, but their shift in performance when arousal level is increased or decreased will provide predictable shifts in performance. Figure 1.6 illustrates this contingency, and Corcoran provides experimental evidence to illustrate the use of personality variables in this manner. Other illustrations, using drugs, will be given in later chapters.

A similar argument is put forward by Frith (1967), who tested critical flicker fusion performance under conditions of quiet (low arousal) and noise (high arousal), predicting improvement under the noisy conditions for extraverts, and no change or decrement for introverts. Figure 1.7 illustrates his prediction, and the experimental results were in line with expectation. The percentage of correct responses from quiet to noisy conditions rose from 52.64 to 58.56 for the extraverts, but remained almost identical for the introverts (53.12 as opposed to 53.16).

So far we have dealt with a curvilinear relation between drive or arousal, on the one hand, and performance on the other. There can be little doubt about the reality of such a curvilinear relation, but its causes are still unclear. There are numerous theories, ranging from Pavlov's notion of protective inhibition, i.e. inhibition (presumably on the receptor side) which protects the cortical neurons from overstimulation, to Easterbrooks' notion of concentration of attention, accompanied by decrement in noncentral perception and attention. Some of these theories will be referred to in later chapters, but it may be suggested that no firm theory has arisen to account for all instances of curvilinear regression, and that probably no single theory exists in this field; different responses may be subject to different laws in this respect, requiring a separate theory for each class of responses. It is unfortunate that so little seems to have been done to clarify this issue; the problem has been with us for long enough!

To complicate the picture even further, we have two additional but different types of curvilinear regression, implicating extraversion–introversion through the mediating variable,

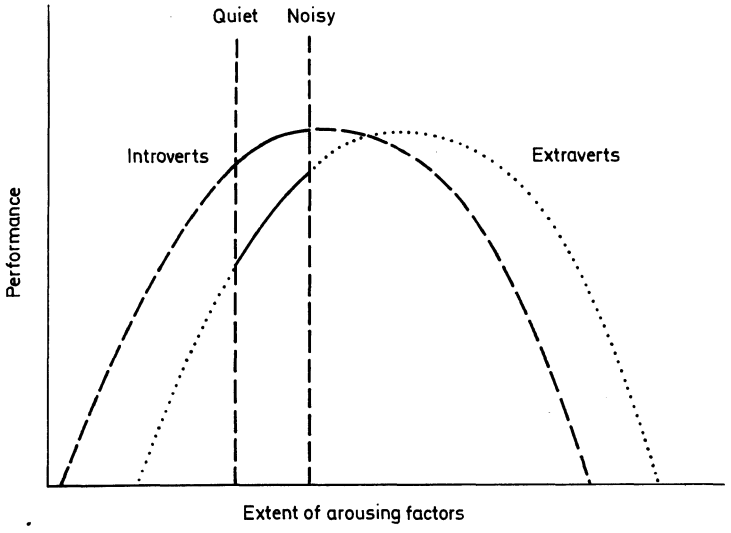

Fig. 1.7. Hypothetical relationship between the extent of arousing factors, personality and performance. (After Frith 1967)

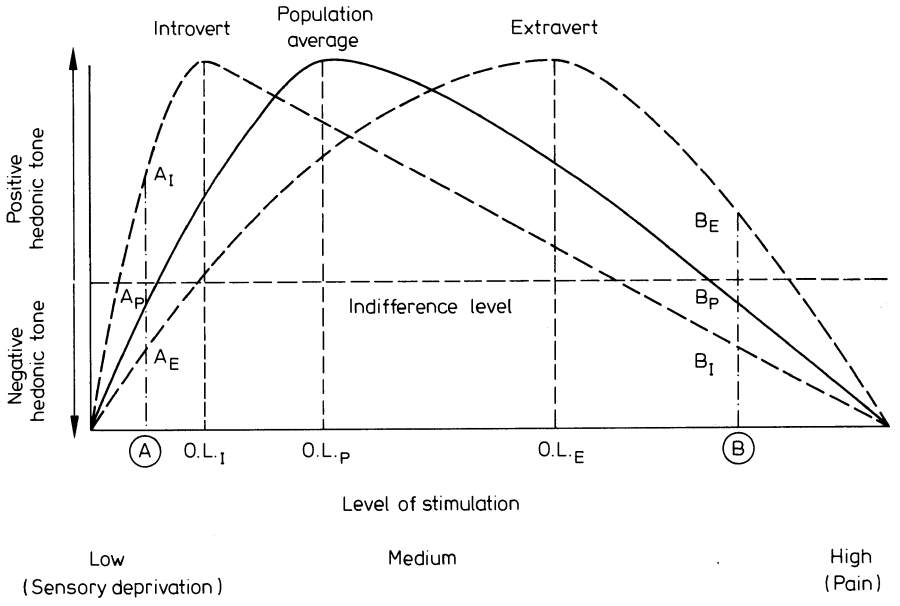

Fig. 1.8. Relation between level of sensory input and hedonic tone as a function of personality. (After Eysenck 1963)

arousal. The first of these regressions involves hedonic tone, rather than performance; Fig. 1.8 illustrates the hypothesis, first stated by Eysenck (1963). The hypothesis states, in the first place, that just as there is an optimal level of arousal for *performance,* so there is an optimal level of arousal for subjective feelings of contentment, happiness or generally preferred *hedonic tone.* This level is intermediate between low arousal, such as might accompany sensory deprivation, and too high arousal, such as might accompany painful sensory stimulation. This hypothesis, originally introduced in a slightly different form by Wundt (1874) is illustrated in Fig. 1.8 by the solid curved line; it is modified by the additional hypothesis that extraverts and introverts will show an optimum level of hedonic tone at points O.L.$_I$ and O.L.$_E$, displaced from the general average optimum level O.L.$_P$ of the population towards the lower and higher stimulation-arousal ends of the abscissa respectively. It follows from this hypothesis that at points A and B, where the hedonic tone is indifferent for the average (ambivert) person, introverts and extraverts will have respectively negative and positive hedonic feelings. It also follows that extraverts will have higher pain thresholds (e.g. Shiomi 1978), while introverts will find sensory deprivation easier to bear (Eysenck 1967). Both these predictions have been shown to be experimentally verifiable (Eysenck 1976a).

This hypothesis is clearly different from that linking arousal and performance, but it should be noted that it is quite possible that hedonic tone itself will influence performance, in the sense that performance itself, by changing a person's arousal level, will become a positive or a negative reinforcement, and hence produce motivation either to continue or discontinue the performance of the act in question. Conversely, performance, by affecting the arousal level, may lead to actions which may change the arousal level; thus Ashton et al. (1972) have shown that under boring work conditions (under-arousal) people will smoke more than under moderately arousing conditions; similarly, they will smoke under difficult work conditions (over-arousal) more than under moderately arousing conditions. (Smoking has arousing or tranquilizing effects, depending on dosage – Eysenck 1979b.)

These interactions should always be borne in mind when planning experiments in this field, or analysing experimental results. Thus Tranel (1961) studied 20 introverts and 20 extraverts

under conditions of perceptual isolation and found that "as a group extraverts tolerated the isolation conditions significantly better than introverts in terms of time spent in the room." This result is completely contrary to what was expected. However, Tranel also discovered the reasons for this apparent failure. Subjects had been instructed to lie quietly on their couch, to estimate the time every half hour and not to go to sleep. "In general, the extraverts reacted by ignoring the instructions ... while the introverts reacted by adhering rigidly to instructions." As Tranel reports, "extraverts largely ignored the instructions to lie quietly. They moved about quite freely and this movement was part of their coping behavior. In other words, extraverts resorted to a form of self-stimulation in the form of tapping, moving, or exploration of the surroundings. They seemed to be much more concerned with devising ways to endure the situation than with following the instructions." This may be interpreted as negative hedonic tone, much stronger in extraverts than introverts in this situation, motivating extraverts to increase arousal level by going counter to the instructions. The strong motivational properties of hedonic tone must therefore always be borne in mind in experiments involving arousal.

An alternative hypothesis to the one illustrated in Fig. 1.8 is of course that extraverts and introverts differ with respect to the optimal level of arousal, with extraverts preferring a higher level than introverts. The available evidence, though not sufficient to establish the original hypothesis as definitively superior, does not suggest that this alternative hypothesis is preferable. An experiment to test these two hypotheses against each other would be necessary before coming to any final decision, and no such experiment has thus far been reported. It is of course not impossible that both hypotheses may be true and complementary, rather than antagonistic; extraverts may prefer a higher level of arousal, as well as being chronically low on arousal, as compared with introverts. However, while there is much evidence, as we shall see, for the latter statement, there is none for the former, and provisionally, therefore, we shall retain the original hypothesis.

A final possibility which must be considered is related to the concept of 'arousal potential' introduced by Berlyne (1974). This may be said to denote what he calls 'collative' properties of stimuli, but has probably a much wider application to all types of stimuli which produce arousal or disarousal; such stimuli may be called *external* sources of arousal potential. *Internal* sources of arousal potential are such personality factors as may be related to arousal, i.e. primarily extraversion–introversion. The question which now arises is whether there is a linear relation between arousal potential and arousal, accompanied by a curvilinear relation between arousal and performance, or whether there is already a curvilinear relation between arousal potential and arousal, which might, in Pavlov's phrase, protect the organism against too high an arousal. If this were true, then the observed inverse-U relation between arousal and performance might in part be an artefact due to lower arousal being produced by higher arousal potential conditions in the descending limb of the U-shaped relation between arousal potential and arousal.

This rather confusing hypothesis is illustrated in Fig. 1.9 (Eysenck and O'Connor 1979), which related arousal potential to a direct measure of arousal, namely the CNV (contingent negative variation on the EEG). The arousal potential in this case was constituted by internal

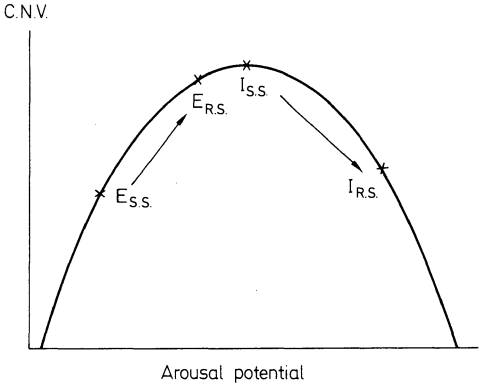

Fig. 1.9. Contingent negative variation (a measure of cortical arousal) as a function of arousal potential. Internal sources of arousal potential – extraversion and introversion. External sources of arousal potential – RS (real smoking) as opposed to SS (sham smoking)

factors (extraversion vs introversion), and an external factor (sham smoking vs real smoking, with the latter constituting the arousal-increasing factor). The figure shows the theoretical prediction; compared with the sham smoking condition, which shows extraverts having lower arousal than introverts, real smoking produces a shift upwards for the extraverts, but a shift downwards for the introverts. The outcome of the experiment, which will be described in more detail in a later chapter, was in line with prediction; thus there seems to be some truth in the hypothesis linking arousal potential to actual arousal in a curvilinear fashion.

The notion of these several and distinct curvilinear relations between arousal, arousal potential, performance and hedonic tone is confusing, but if reality is complex it serves no useful purpose to pretend otherwise, and the evidence for all the relationships observed is quite strong. Further research into all of them is obviously required, as is further work on the relation obtaining between these curvilinear relations and personality. However, there is one supremely important consequence that follows from this discussion, and it is that *theories linking personality with performance must always specify the parameters under which the relationship is to be tested.* Clearly, if curvilinear relationships are the rule rather than the exception, then correlations between a given personality trait (e.g. extraversion) and a given type of performance may be positive or negative, depending on the part of the U-shaped curve linking the two. There is so much evidence concerning this point (e.g. Eysenck 1967, 1976a), which will be raised again and again in the course of this book, that experimentalists can have no excuse for disregarding it in designing their experiments or interpreting their findings. The Shigehisa et al. experiments already described are a clear example; for low intensities of visual stimulation the relation between introversion and auditory threshold is the opposite to that which can be observed at higher intensities. Another example is the work of Eysenck and Levey (1972) on eye-blink conditioning with strong and weak UCS; the correlation with introversion is positive for weak stimuli, negative for strong stimuli, as predicted. *A personality theory cannot be regarded as acceptable unless it predicts the parameter values under which certain relations are to be found.* Such predictions may not always be quantitative prior to investigations of the precise point at which the optimal levels on a given curvilinear relationship are to be found, but at least some relative predictions should always be possible, phrased perhaps in terms of more or less, stronger or weaker. Many alleged failures of the theory discussed in this book have been produced by the experimenter's failures to take seriously the requirements of the theory to specify parameter values; in the absence of such statements the results of experiments are not interpretable.

The outcome of this section is thus that theory making in the personality field cannot proceed without taking into account the concepts of experimental psychology (such as arousal) and the findings of experimental psychology (such as the inverted-U relation between arousal/drive and performance). Conversely, the findings of correlational psychology in the personality field are directly relevant to the testing of theories in the experimental field, and the inclusion of such directly relevant personality variables (e.g. extraversion–introversion) is not just permissible, but mandatory. Theories should combine personality and experimental variables in making predictions, and should pay particular attention to the specification of the parameter values expected to produce a given type of interaction; without such specification, theories cannot properly be said to be supported or disproved. There are many other difficulties and problems in making proper predictions and testing personality theories; some of these will be discussed in the next section. But these, although important, are less fundamental than the problem posed by the specification of parameter values, particularly the intensity and duration of stimuli.

1.4.1 Theory Testing: Constraints and Complications

Given that our theory has been stated in a testable form, we are still faced with a number of problems, some of which are part of the

philosophy of science and are common to all models, others of which are more specific to the particular model under consideration. We shall deal briefly with the more general problems, then in greater detail with the more specific ones.

Both verificationist and falsificationist theories in the scientific 'demarcation' dispute already alluded to in the first section have given rise to difficulties. We can verify a particular deduction from a particular theory, but this is clearly not enough to verify the theory as such; many other deductions could have been made, and some of these might have disproved the theory. A theory, accordingly, can never be proved to be right, and as Popper has pointed out, all theories in fact are likely to be proved wrong in the end. Falsification of a theory is also difficult, however, because a theory is always only one of two or more premises in a logical argument; if the deduction is falsified, it could always be because the other premise(s) involved are wrong, rather than the theory in question. Both these possibilities must be borne in mind when considering the evidence for and against a given theory.

As an example, consider Eysenck's theory relating extraversion to pursuit-rotor reminiscence (Eysenck and Frith 1977). Accepting the Hullian notion of dissipation of inhibition as an explanation of the phenomenon, it was argued that extraverts, generating more inhibition, should show greater reminiscence. During a prolonged effort to verify this deduction and quantify the theory, in the course of which over 50 separate experiments were carried out and reported, there was overwhelming support for the superiority of extraverts in pursuit rotor reminiscence; yet the theory was in fact false, as could be shown quite clearly when another deduction was tested, namely that differences in reminiscence between extraverts and introverts should be apparent pre-rest, with rest restoring equality of performance. What was found was the opposite: differences appeared post-rest, with equal performance pre-rest! Apparently the Hullian formulation was in error, and an alternative theory, relying on consolidation of the memory trace and differential strategies was adopted and found more satisfactory.

We thus have a syllogism in which the Hullian theory of inhibition is the major premise, the Eysenck theory of extravert–introvert differences in arousal-inhibition the minor one; the deduction depends on both being true. In fact, the major premise is almost certainly not true (at least in connection with the phenomena here considered), and accordingly one would have expected the deduction to be falsified – which might have led to the erroneous conclusion that it was the minor premise which was in fact incorrect! By sheer accident it proved possible to substitute a quite different theory in lieu of the major premise, which, in combination with the minor premise, generated an identical prediction. But by the same token, we may also be in error in accepting this verification as proof of the correctness of the major premise, the minor premise or both; another, as yet unconsidered, theory might explain the observed facts equally well or better. Thus neither verification nor falsification of single deductions should be taken too seriously, particularly when there is little detailed knowledge about such vexed questions as suitable parameter values. We expect anomalies in all scientific theories, even the most useful and widely accepted; Newtonian gravitation theory was never free of embarrassing anomalies of this kind (Suppe 1974) Psychologists are perhaps too ready to discard theories which show such anomalies, and to expect perfection where more realistic physicists and chemists would prefer having a reasonable theory containing inconsistencies and anomalies to having no theory at all. When a theory is successful in 'explaining' and predicting a wide range of phenomena, it is usually considered better practice to look for errors in subsidiary theories providing parts of the scaffolding of additional premises needed for prediction, rather than to abandon the theory altogether.

Even so, testing a scientific theory can proceed at many different levels, and it is important to realize the kind of support given to a theory when deductions at these different levels are tested and verified. Deductions can be looked at as lying on a continuum ranging from direct and close to indirect and remote. The hypothesis that extraverts would have more eye contact with an interviewer than introverts is an obvi-

ous, common sense deduction from the major premise that extraverts are more sociable (which is perhaps more a definition than a premise), and the additional one that sociable people would have more eye contact than unsociable people – a minor premise which could of course be wrong. If the prediction turns out to be wrong, we would almost certainly reject the minor premise; consequently from the point of view of testing the major premise, failure is not crucial and success not very informative.

This prediction would seem to test nothing more than the descriptive aspect of the personality theory, although Zajonc (1965) has linked social behaviour with arousal, a connection which makes it possible to deduce the positive search for social contact of the extravert from his low arousal. Clearly much more direct deductions from the causal theory can be made by testing the arousal level of extraverts and introverts in carefully designed situations, using EEG, CNV or other similar measures widely accepted as indexing different states of arousal. Such tests of the theory are described in detail in another chapter, and they constitute perhaps the clearest and most direct tests of the causal hypothesis of extraversion–introversion. Failure on one of these tests might be argued away as being due to the erroneous inclusion of this test among the measures of arousal, or the faulty selection of too stimulating or too little stimulating testing conditions; more general failure of the deductions would effectively put the theory out of court. This type of test is therefore of particular importance in assessing the success or failure of a theory.

The prediction that introverts should form conditioned responses better and more quickly than extraverts (under conditions of low intensity UCS) is of interest, because it is known (or strongly suspected) that conditioning is facilitated by high arousal; if introverts show high arousal (which is an important part of the theory) then the conclusion should follow. Failure could be blamed on the hypothesis linking arousal with conditioning, but this hypothesis is pretty firmly established, and the failure of the test to confirm the original theory would redound more to the discredit of the personality postulate than to that of the conditioning-arousal theory. Vigilance (Mackie 1977) is probably as closely linked with arousal as is conditioning, and the relation between introversion and vigilance is firmly established (Eysenck 1967).

Predictions in the field of memory are more indirect than those relating to conditioning, in part because of the much more complex nature of human memory, and as a partial consequence of this, because of the intrusion of additional hypotheses whose status is not very clear. Thus Howarth and Eysenck (1968) based their prediction of the differential recall of extraverts and introverts, with the former showing forgetting over time, and the latter showing reminiscence, on Walker's theorem, according to which consolidation of the memory trace, while it is going on, inhibits recall. The prediction was successful, thus apparently validating both the personality theory and Walker's theorem, but one would clearly feel more sure of the affirmative nature of the outcome for personality theory if Walker's theorem had been more firmly established in advance of the experiment. A later chapter discusses these points more thoroughly.

Least certain, and most doubtful, are of course predictions in the social field generally, e.g. the prediction that antisocial behaviour would be correlated with extraversion, psychoticism and neuroticism (Eysenck 1977a). Other predictions in this field concern neurotic behaviour (Eysenck and Rachman 1965), sexual behaviour (Eysenck 1976c), and social attitudes and ideologies (Eysenck and Wilson 1978). In all these fields the logical chain from premises to conclusions is so long and complex that errors are perhaps inevitable and failures of prediction not unexpected; it is surprising that so many of these predictions have in actual fact stood up to empirical testing. Brody (1972), after stating that he believes that a "fully adequate scientific theory of personality which we do not as yet have will be similar in many respects to Eysenck's theory", continues with some pertinent criticisms, particularly related to predictions in the social field. "First, and perhaps most critically, the theory is not invariably in accord with empirical findings. Second, deductions from the theory often involve *ad hoc* assumptions and do not invariably rigorously follow from the central assumptions of

the theory. Finally, the theory appears to deal most precisely and extensively with biologically relevant behaviours. However, there is insufficient attention to the social aspects of behavior and there is a failure to deal in an equally thorough and sophisticated way with the socialization process." Our last chapter reviews the predictions of the theory in the social field and some of the results of testing these predictions; Brody's criticisms will probably be seen as well taken.

There are of course good reasons why developments have been along these lines, rather than others. Particularly important among the reasons for concentrating on biological factors, to the partial disregard of social ones, has been the general *Zeitgeist* which showed an opposite trend; it seemed important to emphasize biological factors, in the secure knowledge that most other research workers in the field would concentrate their efforts on social factors. Man is of course a bio-social organism, and both social and biological factors must be taken into account in any rational model of man; nevertheless, biological factors are so fundamental and were so disregarded, that concentration on them seemed not unreasonable. In any case, the conditioning model constructed to account for much of criminal and neurotic conduct does contain cognitive elements; as Pavlov always insisted, the second signalling system is a vital part of his theoretical construct. As he pointed out, "owing to the entire preceding life of the human adult a word is connected with all the external and internal stimuli coming to the cerebral hemispheres, signals all of them, replaces all of them and can, therefore, evoke all the actions and reactions of the organism which these stimuli produce." This view was strongly endorsed by Platonov (1959), whose original and important researches into the second signalling system have been rather disregarded in the Western world. Also, recent work by Martin and Levey (1978) on evaluative conditioning suggests that the principles of conditioning can with advantage be extended to social fields where previously purely cognitive theories predominated.

Again, the development of cognitive and other theories on the social side has been disappointing, to say the least; they are not in a proper state to be incorporated within a model which claims to have some degree of scientific status. Allport (1975) has recently given a succinct, critical but not unjust review of cognitive theories; this is his conclusion: The field, he says, is characterized by "an uncritical, or selective, or frankly cavalier attitude to experimental data; a pervasive atmosphere of special pleading; a curious parochialism in acknowledging even the existence of other workers, and other approaches, to the phenomena under discussion; interpretation of data relying on multiple, arbitrary choice-points; and underlying all else the near vacuum of theoretical structure within which to interrelate different sets of experimental results, or to direct the search for significant new phenomena." These are strong words, but they do not seem unjust when viewed against the background of the cognitive theories produced in such profusion.

When all is said and done, however, it must remain obvious that the application of the model of personality here developed to social activities in the larger sense is hazardous because of the length of the chain of reasoning involved, and because of the multiplicity of influences which determine social action. The application of the model to antisocial conduct (Eysenck 1977a), for instance, makes predictions which are verified best with children and adolescents; it does work also with incarcerated adults, but the reasons for incarceration are many, and so are the effects of incarceration; thus we have a much more complicated situation in which personality factors making for antisocial behaviour only form a part of the total background. Many prisoners are inadequate, rather than antisocial; they are less likely to fall into the same personality pattern as other offenders whose crimes testify to their antisocial nature. Prison may make inmates more introverted in their behaviour; thus the very act of incarceration may reflect back on the personality factors involved. In some cases offenders were under the impression that their answers to the personality questionnaire might be instrumental in procuring earlier parole, or other advantages; high lie scores and low neuroticism scores testify to this, but of course under these circumstances

the results become meaningless. When all these difficulties are borne in mind, and only the better studies taken into account, the agreement between model and fact is not too poor. Personality is always only one of many influences which determine complex social phenomena, and hence correlations are unlikely to be very high; consistency of direction is all that may reasonably be expected.

So far we have dealt with problems in the verification or falsification of predictions made from the model which are real, in the sense that the questions raised are meanigful and have an answer, even though that answer may be difficult to produce. There are also questions and problems which are unreal, although this has not prevented many psychologists from raising them or from criticizing the model in terms of the putative answers given to these questions. Two questions in particular have been raised many times and discussed as if they had a meaningful answer. The two questions most frequently asked regarding extraversion, both of which lack any scientific meaning, are these: "Is extraversion–introversion a unitary dimension?", and "Is extraversion–introversion independent of adjustment?" (Carrigan 1960.) Consider what is implied in asking these questions. Philosophers make a clear distinction between concepts and things; questions about existence and relationships can be meaningfully asked of things, but not of concepts. We can ask: "Is this a table or a pig?" and expect to receive a meaningful answer. But concepts are entirely different; they form part of a theoretical network which defines their coordinates and renders them useful or useless, in varying degrees. To ask whether extraversion–introversion is independent of adjustment is to ask whether extraversion–introversion correlates with neuroticism, in terms of our theory. But such a question presupposes that there is in nature, somewhere, a thing called extraversion–introversion, and another thing, called neuroticism, and that we are able to look upon these God-given things and confidently make assertions about the existence or otherwise of certain observable relations between them. But this is surely quite untrue of psychological (or physical or chemical) concepts; these do not have an existence independent of the theory of which they form a part, and hence this type of question cannot be asked about them.

Consider Fig. 1.10, which shows the relative position of six extraversion and six neuroticism questions in a two-factor framework (Eysenck 1970a). Clearly these two dimensions are independent; does this mean that we can answer

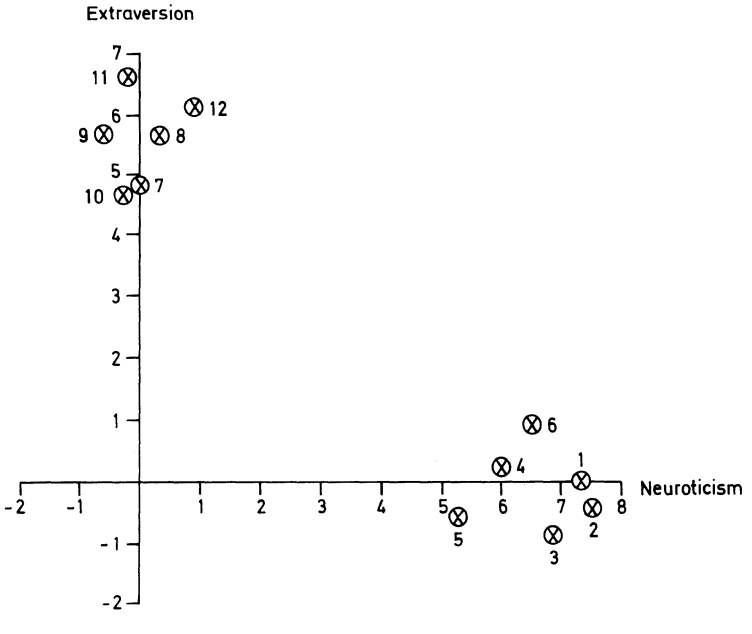

Fig. 1.10. Relative position in two-dimensional space of six neuroticism and six extraversion questionnaire items

Carrigan's question about independence in the affimative? Surely not, unless we accept that the dimensions in question are completely and solely defined in terms of these two sets of six questions! Suppose we were to add two questions concerning impulsiveness and risk-taking, both of which would have loadings on both E and N, administer the new questionnaire to a new group of subjects and reanalyse the new set of data. We would now have the choice of retaining simple structure, and obtaining oblique axes, thus giving us correlated E and N factors; or of retaining orthogonal axes, and allow the two new questions to lie in the first quandrant, separate from both our factors, and having loadings on both. By a suitable choice of additional questions we could make the correlation between E and N assume almost any value between $+0.2$ and -0.2; clearly there is a good deal of subjectivity involved in the whole process, and a question about the *real* state of affairs is meaningless. We can construct scales of E and N which possess the desired quality of independence, or we can construct scales of E and N which are positively or negatively correlated; thus the question we are dealing with really involves properties which we desire our scale to have, rather than properties which some concept existing independently of human thoughts, desires, and aims may have – some immaterial *Ding an sich*, which in the nature of things we could never comprehend.

The same is true of the question concerning unitariness. We can make up inventories approaching unitariness (i.e. giving rise to matrices of rank 1, or apporaching this rank), or we can make up inventories which have a much higher rank; this is a subjective decision. Clearly, if we conceive of E and N as super-factors, emerging from the intercorrelations between primary factors, then our inventory will only give rise to matrices of rank 1 if we are careful to select for our inventory only a single question from each of the many primaries that generate E or N; even Spearman was quite clear on this point (which arises with equal clarity in relation to the concept of intelligence – Eysenck 1979a).

It may be answered that by allowing so much room for subjectivity, we are abjuring science, and that a scientific approach must be entirely objective. This is a major misconception, unfourtunately widespread among social scientists who have little acquaintance with the procedures used by physicists in constructing measuring instruments or defining concepts. I have elsewhere given an extended example from the field of temperature measurement to illustrate this point (Eysenck 1979a). Consider two measuring devices, a resistance thermometer and an ordinary mercury-in-glass thermometer. Even in the classical ice-point to boiling-water point range these do not give identical readings, and as every text-book of physics makes clear, the choice between the different readings is arbitrary. Outside this narrow range differences between different methods of measurement become much more pronounced, yet the choice still remains subjective. Should we be expected to be more objective in this respect than are the physicists?

Even within the simple field of liquid-in-glass measurement of temperature, differences in results arise. To take but one example, if we compared water thermometers with mercury thermometers, we would find that in rising from freezing point to 4° C, mercury would expand, water contract! In actual fact, the liquids most widely chosen (Mercury and alcohol) were selected in part because they fit in best with the kinetic theory of heat, which predicts that the final temperature reading of a fluid obtained by mixing similar fluids of masses m_1 and m_2 at the initial temperatures t_1 and t_2 should be:

$$t_f = \frac{m_1 t_1 + m_2 t_2}{m_1 + m_2}$$

The linseed oil thermometer was discarded because measurements made with this instrument did not tally with the predictions made by the kinetic theory; mercury and alcohol thermometers do tally. Thus the choice of a measuring instrument is in part based on its agreement with theory; the same is true of psychological measurement. If the theory says that extraversion and neuroticism are independent, then a measuring instrument will be constructed and chosen which will give independent readings for the two concepts (if possible); if extraversion–introversion is conceived as a super-factor made

up of a set of traits or primaries, then the inventory resulting will either be multidimensional (superfactor) or unindimensional (containing only one item from each primary), depending on the choice of the experimenter. To ask whether the concepts are *really* independent or unidimensional is scientifically as meaningless as to ask whether the temperature indicated by the mercury or the resistance thermometer is the *real* temperature; the use of the term *real* in this connection implies an independent existence which can be asserted of things (although even there philosophers would enter many caveats), but not of concepts. For these reasons we have not discussed the large literature which has accumulated around these questions; the facts there presented answer questions about the relations between measuring instruments constructed by different psychologist, and administered to different populations, they are not relevant to any theoretical problem that can be answered.

An illustration of the hollowness of terms like 'unitary' in this connection is furnished by Eysenck's (1956) analysis of Guilford's allegedly unitary scale for the measurement of social shyness (S). The hypothesis tested was that there were essentially two sorts of social shyness, one connected with introversion (I don't care much to be with other people), and the other connected with neuroticism (I am afraid of other people). Factorial analysis showed clearly that the allegedly unitary scale broke up into two orthogonal parts, which correlated quite highly with E and N respectively, and contained items whose content indicated conformity with the hypothesis. This study indicates that the questions raised by Carrigan can be phrased in a meaningful manner by suggesting alternative hypotheses concerning a particular inventory or concept. It indicates that while there is an element of subjectivity involved in the construction of a quetionnaire or other measuring instrument, there are also objective constraints which are on the whole much more powerful. It would be impossible to construct an inventory of E and N which bore any relation to the concepts of the model and which produced very high positive or negative correlations between the two scores.

Similarly, it would be very difficult to construct an inventory of this kind which failed to produce the two factors of E and N, relatively independent of each other; in fact, even when there is no intention of doing so, the simple inclusion of large numbers of personality questions pretty well ensures the emergence of these factors (Eysenck and Eysenck 1969; see particularly the reanalysis by Eysenck 1978b, of a study by Browne and Howarth 1977). This fact also speaks powerfully against the attempts of some psychologists to restructure the personality field around axes rotated through 45°, such as suggested by Gray and Claridge; these attempts will be discussed in some detail in the final chapter. What is clear is that in all the numerous analyses conducted during the past 50 years, many of them not at all concerned with E and N as major personality factors at their inception have nevertheless ended up with factors identical in nature to these, although sometimes differently named (Royce 1973). None has ended up with the 45° rotated factors, suggesting that if we start with a random sample of items, or with a quota sample of the total universe (like Cattell), then their intercorrelations will define areas of clustering corresponding to E and N. This is an empirical finding, just as is the finding of a 'positive manifold' among the correlations between cognitive test (Eysenck 1979a).

Having said all this, it remains to be stressed that there are meaningful questions that can be asked about E, N and the contributory primary factors. It is meaningful to ask questions about the factorial complexity of sets of items included in popular or technical definitions of traits like 'impulsiveness' or 'sociability', or 'sensation-seeking', or their position in the factorial space defined by the superfactors (e.g. Zuckerman et al. 1978; Eysenck and Eysenck to be published). It is equally meaningful to enquire to what extent the observed correlations between impulsiveness and sociability, which are basic to the concept of 'extraversion', are due to genetic or environmental factors (Eaves and Eysenck 1975); this and similar problems are dealt with in some detail in a later chapter. It is also meaningful and indeed important to ask to what extent the major superfactors or

dimensions of personality found in Western countries are replicable in studies carried out in countries behind the Iron Curtain, or in non-Western industrialized countries like Japan, or in third-world countries like India and Nigeria. All these cultures have been investigated, with very positive results (e.g. Lojk et al., in press); identical factors emerge in all these widely different cultures. It is with soluble problems of this kind that scientists should be concerned (Medawar 1968), rather than with attempts to answer questions that are outside the realm of science altogether (Eysenck 1977b).

1.4.2 Theory Testing: Some Sources of Error

One major source of error in testing the theory of extraversion (or indeed any theory of personality) is to treat the concept in question in isolation and to disregard the fact that it is likely to interact with other personality concepts and, indeed, with ability concepts as well. Not all extraverts are likely to behave in an identical fashion, regardless of whether they are bright or dull, neurotic or stable; the interaction with these other dimensions of personality may be crucial in mediating a successful prediction. Consider a very simple example. In my original formulation of the personality theory of antisocial activity, I suggested that fundamental to this type of conduct would be a lack of positive conditioning mediating socialization processes, and that these would be produced more readily in the introvert than the extravert, because of the greater arousal in the former, producing stronger and quicker conditioning (Eysenck 1977a and earlier references given there). I also suggested that neuroticism would act as a powerful drive, multiplying the established habits of socialized or anitsocial conduct in the manner suggested by Hull. The prediction was therefore made that it would be the high E–high N subjects who would be predisposed to antisocial conduct, as compared particularly with the low E–low N subjects, but also partly with subjects in the other two quadrants.

In spite of this explicit prediction, most investigators who tested this prediction simply compared extraverts with introverts, leaving out of account completely the N dimension. Results were mixed when this was done, but certainly the outcome cannot be considered very positive as far as the prediction is concerned. Burgess (1972) has recently reanalysed some of the published data, using N as a moderator variable as demanded by the original theory, and has shown that when this is done all the data involved give positive results. It seems clear that reliance on only one single dimension when theory predicts interaction between several dimensions may generate a false impression of failure.

Consider another example, taken from the field of expressive movements (Wallach and Gahm 1960; Wallach and Thomas 1963). They subdivided their population in terms of neuroticism (anxiety) and introversion–extraversion, and measured an area filled with doodling as a measure of graphic constriction and expansiveness. Eysenck (1967) has reviewed evidence to suggest that the high N group would be in a state of higher stress than the low N group, leading to extraverts high on N having constrictive scores, extraverts low on N having expansive scores, while introverts would reverse this relation. The actual figures bear out this prediction, as shown in Fig. 1.11. Clearly no prediction of any meaningfulness is possible on the basis of E alone; both E and N come into the prediction, and both have to be measured and used to produce a meaningful result.

A third, rather more complex illustration comes from work on eyeblink conditioning; a

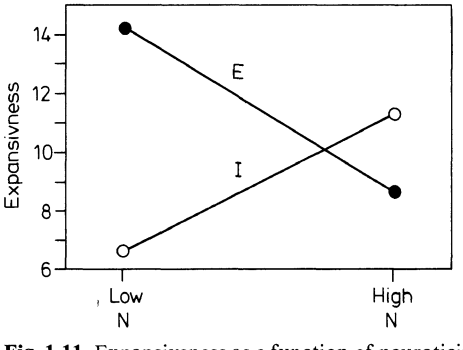

Fig. 1.11. Expansiveness as a function of neuroticism and extraversion–introversion. (After Wallach and Gahm 1960)

more detailed treatment of the problems raised will be given in a later chapter. Spence originated work on the relationship between personality and eyeblink conditioning, suggesting that anxiety (as measured by the Taylor MAS) should correlate positively with speed of conditioning, in view of Hull's additive law of drive; several studies from his laboratory verified this prediction. Kimble and others, however, working at Duke and elsewhere, failed to replicate these studies. Eysenck predicted that introversion, rather than neuroticism, would be found to correlate with speed of conditioning, in view of the higher arousal level of introverts; he also provided a number of studies verifying his hypothesis, and demonstrating an absence of correlation of conditioning with N. Spence published another study, again finding positive correlations between conditioning and anxiety, but none with introversion. Thus we would appear to have here a complete set of contradictory results, impossible to disentangle. Yet attention to the interaction of N and E would seem to make the apparent disagreement perfectly lawful and predictable.

Kimble noted that the Spence studies were done under very anxiety-provoking conditions, thus producing varying degrees of emotional responses in persons differing in N; his own work, and that of Eysenck, provided conditions lacking in these anxiety-provoking aspects. As a consequence we may be able to explain the apparent failure of Kimble and Eysenck to replicate Spence's results in terms of experimental conditions; where conditions are not anxiety-provoking, differences in N will not be able to affect performance. Spence's failure to discover correlations between conditioning and introversion follow from Eysenck's postulate that high degrees of anxiety or emotional response inevitably affect cortical arousal level, both directly and indirectly (i.e. through the reticular formation); thus the anxiety produced by Spence's experimental set-up would wash out individual differences in cortical arousal and eliminate correlations with introversion (Eysenck 1967). When we note further that the MAS is a dual measure incorporating both N and introversion (in varying degrees; correlations are higher with neuroticism than with introversion), we have a reasonable explanation of all the observed phenomena. The predicted correlation between introversion and conditioning only holds when (a) the UCS is reasonably low in intensity, and (b) conditions are such as to evoke only small degrees of anxiety. These conditions of testing can be derived from the general theory, and are not ad hoc; failure to adhere to them will inevitably reduce or eliminate the predicted correlation, and may in fact turn them in the opposite direction, i.e. when Pavlov's law of transmarginal inhibition comes into play. Individual differences in N cannot be left out of account when making predictions in relation to E, unless conditions of testing are such as to assure that emotional reactions to the total situation are minimal.

It is unfortunate that conditions of testing have attracted so little attention among personality theorists and research workers; it may be said that such lack of attention could easily be fatal to any hopes of producing replicable results in this field. Some of these conditions are fairly obvious, such as the anxiety-provoking nature of the situation into which subjects are placed (although until Kimble pointed out this feature in Spence's laboratory it had not been remarked upon by any of the psychologists who took part in the debate concerning the failure to replicate his results); others are much more obscure, and seldom attended to. One such condition, here chosen as an example, is time of day when testing is carried out; the relevance of circadian rhythms to test performance, and their interaction with personality, has been demonstrated with particular clarity by the Cambridge group (Colquhoun 1971).

Figure 1.12 shows the body temperature rhythms of introverts and extraverts respectively (Blake 1967), and Fig. 1.13 shows a similar diagram giving alertness ratings of introverts and extraverts. It will be seen that for both measures (which might be regarded as indirect indices of cortical arousal) introverts have higher scores in the morning, extraverts in the evening; in other words, introverts are larks, extraverts owls, to use the terms frequently employed in the vernacular. (Similar differences are found in diurnal variations in pain – Folkard et al. 1976.) Many studies have shown performance

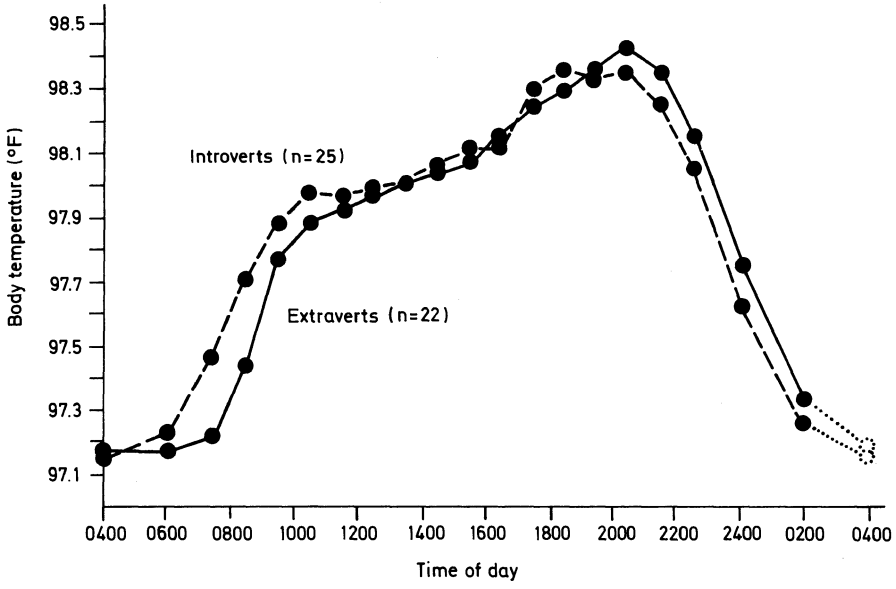

Fig. 1.12. Body temperature rhythms of introverts and extraverts. (After Blake 1967)

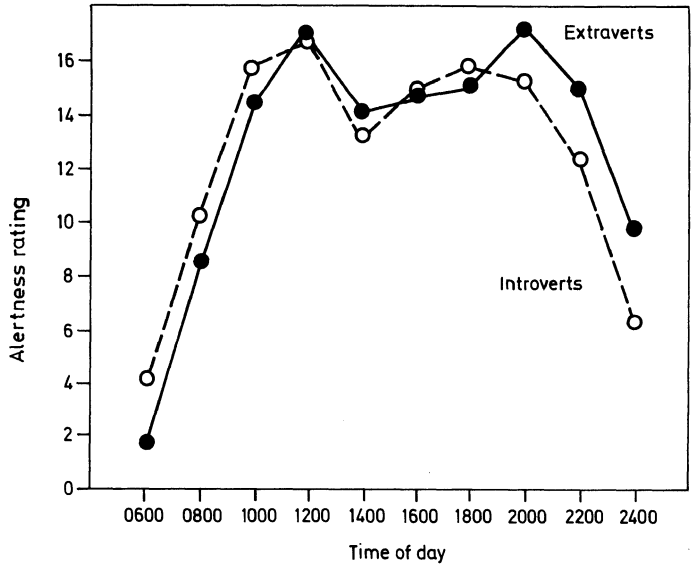

Fig. 1.13. The diurnal variation of subjective alertness for introverts and extraverts. (After Folkard 1975)

differences corresponding to these changes in body temperature and alertness; typical are two studies by Colquhoun (1960) and Colquhoun and Corcoran (1964). In the former, using a vigilance task, he found introverts to score better than extraverts in the morning, worse in the afternoon. In the latter, using a task requiring the cancelling of letters in a piece of English prose, he obtained similar results (cf. Fig. 1.14).

Blake (1971), reporting a number of additional experiments, summarized his results as follows: "Taken altogether, the results described in this chapter therefore favour the view (1) that introverts have higher arousal levels than extraverts in the morning, (2) that there is a general increase in level of arousal in both "types" throughout the day, (3) that the level of arousal increases at a greater rate in extra-

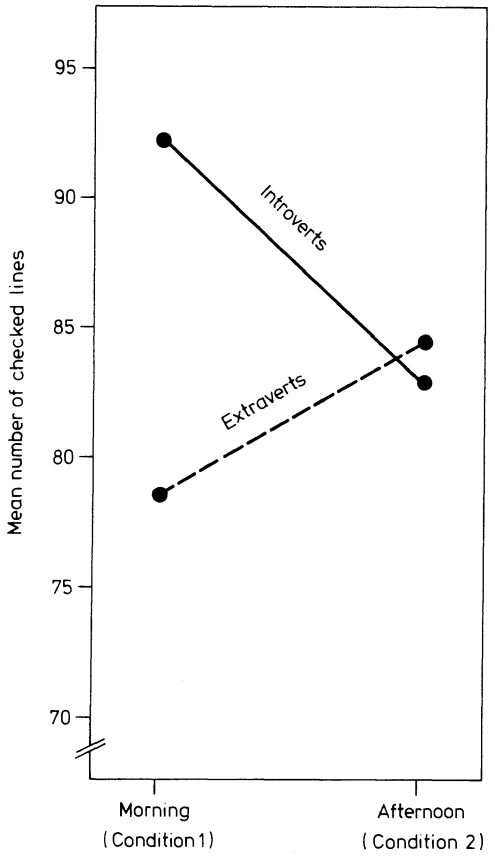

Fig. 1.14. Mean output of extraverted and introverted subjects at different times of day. (After Colquohoun and Corcoran 1964)

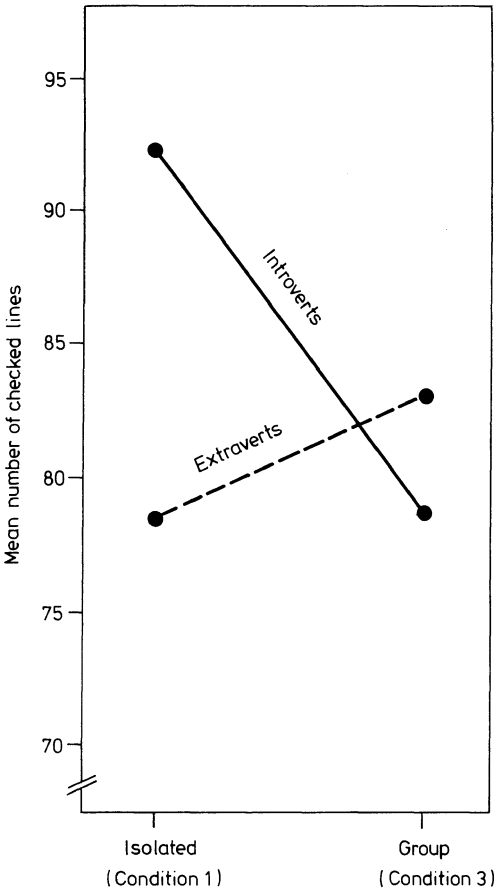

Fig. 1.15. Mean output scores of extraverted and introverted subjects in different social situations. (After Colquhoun and Corcoran 1964)

verts than in introverts, with the result that (4) when an additional arousing factor is added to the task situation the level of arousal in extraverts may be post-optimal for performance later in the day. Changes in temperature during the day mirror the performances of introverts and extraverts quite neatly under "normal" conditions, and could therefore ... be considered a reasonably valid indicant of variations in arousal, and in resultant levels of efficiency, in both types. However, because of the "inverted-U" relationship between performance and arousal it can now be seen that the use of temperature as a simple predictor of actual behaviour will, in certain circumstances, be misleading ... The value of temperature as a direct index of performance efficiency is limited to conditions in which arousal does not exceed the optimal level for the particular task under investigation."

Among the conditions studied by Colquhoun and Corcoran (1964) which produced increases in arousal differentially for extraverts and introverts was testing in isolation or in groups; Fig. 1.15 shows results of testing in the morning under these two types of condition. The interpretation favoured by Colquhoun would be that under isolated conditions introverts are near optimum arousal, extraverts well below that level; under group testing conditions introverts would be pushed beyond this optimum on to the descending arm of the inverted-U relationship, while extraverts would be pushed up the ascending arm. The work of Zajonc (1965) lends support to the hypothesis that group testing (or

indeed the presence of other human beings at all) produces increases in arousal.

In addition to isolation vs group testing, other manipulations of the environment that have been used include white noise and drugs. As an example of the latter type of modification, consider the work of Revelle et al. (1980) on the use of caffeine in relation to performance on a verbal achievement task. (The data were clearest in relation to the impulsiveness component of extraversion, and hence the discussion is restricted to this aspect of their work.) The major finding of the study was that low impulsives were hindered and high impulsives were helped by caffeine in the morning of the first day of testing, but that this pattern was reversed in the evening. Revelle et al. (1980) argued that this result could be accounted for by means of the following assumptions: (a) caffeine increases arousal level; (b) there is a time of day effect (or circadian rhythm) in arousal; (c) there is a phase difference of several hours between introverts and extraverts in this circadian rhythm, with introverts reaching their peak arousal level before extraverts; (d) there is a curvilinear inverted-U relationship between the level of arousal and performance. This interpretation, while it agrees with some of the literature mentioned above, suffers from various serious faults which have been pointed out in detail by M.W. Eysenck and Folkard (1980); we shall not here repeat their criticisms, but simply mention the fact that there are still many problems in the interpretation of circadian rhythm data in so far as they relate to performance data. This does not of course invalidate the major point made here, namely that attention to time-of-day effects in designing and interpreting psychological experiments involving personality is mandatory; the facts regarding the existence of interrelations between personality, performance and time of day are not in dispute. What is arguable is the applicability of some very simple hypothesis to explain all of these data.

Circadian rhythm effects have obvious links with imposed changes on these rhythms, such as occur in night-shift work, spring and autumn daylight time changes and jet lag. Such changes should be easier to impose on extraverts than on introverts, either using Wundt's notion of extraverts being more 'changeable', or else the simple view that extraverts, being less easily conditioned, should extinguish the conditioning experiences which linked their circadian rhythms to specific internal experiences of time change more easily. The evidence certainly favours such a view; Colquhoun and Folkard (1978), Folkard et al. (1979), Monk and Aplin (1980) and Monk and Folkard (1976) have found consistent evidence for the hypothesis that extraverts more easily adjust to these types of changes. Some of the correlations reported are quite impressive, and clearly the effect is of considerable importance.

The two sources of error in testing predictions from the arousal theory of personality already mentioned (treating E in isolation from other major dimensions of personality, and disregarding conditions of testing, such as circadian rhythm effects) are fairly general and might apply to other dimensions of personality and the testing of theories regarding them also; the same is true of the third source of error which we propose to discuss in relation to a particular hypothesis involving figural after-effects. The error consists in applying inappropriate statistical tests (in this case test-retest reliability measures) to the data obtained and drawing conclusions from these data which are unjustified. It is unusual for psychologists to think very much about the conditions which must be observed if such tests are to be applicable to a given body of data, and hence errors may often be made which are not obvious to experimenter or reader. I have chosen for discussion a particular example, largely because the evidence regarding it is extensive, and also because the amount of research originally rejected because of such errors of interpretation was very large; other examples could have been given.

The story begins with Eysenck's suggestion that individual differences in kinaesthetic after-effects could be used as an index of personality, particularly extraversion (Eysenck 1955); experimental studies were reported showing reliable differences in such after-effects between introverted and extraverted neurotics. Broadbent (1961) and others replicated the study, with varying amounts of success, but it was not until

the large-scale research efforts of Petrie (1967) that what became known as stimulus intensity modulation attracted much attention. As can be deduced from our Fig. 1.8, introverts would augment the impact of external stimulation, extraverts would reduce it; Petrie used the KAE (Kinaesthetic After-Effect) test, in a modified form, to measure these properties of stimulus intensity modulation, and succeeded in producing a large amount of evidence, relating to many areas such as pain perception, criminality and smoking behaviour. In all this work highly significant correlations were found in the direction predicted from extraversion–introversion (arousal) theory, although as we shall note presently some negative findings were also reported (Barnes 1976). Zuckerman et al. (1974) extended the theory to psychophysiological measures, i.e. the average evoked potential.

Gradually the favourable climate of opinion changed with the discovery that repeat reliability of the KAE test was poor (Morgan and Hilgard 1972), and more recent writers have tended to dismiss all this work as practically worthless, on the basis that totally unreliable tests cannot produce valid information (e.g. Weintraub et al. 1973). As Baker et al. (1976) note, "recently, the literature of KAE has taken a sharp turn. The current consensus is that (a) KAE lacks test-retest reliability..., (b) KAE shows only marginal, if any, validity, and (c) use of these KAE scores as an index of stable individual differences should be discontinued." (p. 2.) Baker et al. take up this critical view of the KAE and attempt to demonstrate that it is completely mistaken. "In challenging this current consensus, we argue that (a) first-session KAE is valid; (b) poor retest reliability simply reflects later-session bias; (c) hence, multisession studies should not be used to assess validity without taking this bias into account. Those recent studies which failed to support KAE validity were each multisession in design. If our bias contention is correct, these studies should be ignored, and the claim of intermittent validity is thus rebutted." (p. 1.)

Barker et al. have taken this criticism beyond a merely verbal and conceptual argument; they have demonstrated that lack of reliability is indeed due to persisting effects on later sessions of earlier experiences, invalidating multisession scores, and they have reanalysed published and apparently unsuccessful replication studies. They summarize their results of these reanalyses as follows: "Reanalysis of the most recent multisession, nonsupportive validity study indicates (a) Session 1 validity, (b) later-session bias, and (c) later-session validity when multisession scores are combined to avoid bias. Thus, KAE validly measures personality." (p. 1.) As they point out, "the major import of this study is not in the particular findings of validity and bias but rather in terms of its implications for the area of KAE individual differences research; it destroys the logical and empirical basis for the current critical consensus which has consigned the KAE measure to oblivion as a measure of individual differences.... In conclusion, the original excitement and interest which many psychologists showed in the KAE task and the stimulus intensity modulation hypothesis after the early supportive studies seems fully justified."

We would go even further than this and argue that what has happened here is not unusual in personality research, namely the rejection of a perfectly good theory, leading to interesting and fruitful results, on the basis of negative findings based on methodology and analysis which do not take into account the actual theory under examination and hence lead to apparently negative findings. These findings are then used to discredit the theory for which they are in fact irrelevant. Taken together with the frequent lack of attention to parameter value requirements of a given theory, so that parameters are chosen for the testing of a theory which are not in accord with the predictions of that theory, this leads to failure to replicate, a failure which is not due to faults in the theory under investigation, but rather to lack of attention on the part of the investigator to the dictates of the theory in question. Psychology is not in possession of so many good theories that it can afford to reject such theories on the basis of work which pays scant attention to the actual requirements of such theories.

It might be said in extenuation that those who doubted the validity of the KAE as a measure of extraversion did so on the excellent

grounds that a test having low reliability cannot have high validity. This is only true under quite circumscribed conditions, and clearly these conditions did not apply (and were never shown to apply) to the KAE test. But even more compelling is another argument, which should have acted as a warning to critics – if a test is valid, it cannot be unreliable! The strong support which Petrie's early studies, as well as those of others, gave to the theory suggests very strongly that the measures used (i.e. first-session KAE scores) cannot have been unreliable; had they been so unreliable, positive results could not have been obtained. Thus it may be suggested that critics inverted the proper argument, leaving out of account the demonstrated success of the method as used by Petrie and arguing from the failure of the changed method used by them. Quite generally, failure to replicate (usually with many changes in experimental procedure, population studied, methods of analysis, etc.) is taken as a sign that the original research was faulty in some respect and does not deserve further attention. It might equally well be taken to mean that the failure is due to certain aspects of the replication study and that it is incumbent on the experimenter to explore the differences between the two studies in order to clarify the situation. Failure to replicate may often mean just that – a failure on the part of the experimenter to properly replicate the major features of the original study and thus an implied responsibility to account for this failure. Too frequently do psychologists in particular neglect this responsibility and assume that by failing to replicate somebody else's work they have thrown doubt on that work, when in reality they may only have thrown doubt on their own competence. A scientist replicating another scientist's work has an explicit and implicit responsibility to pay attention to all aspects of the older work and of the theory informing it; changing parameters at will, and disregarding important aspects of design and analysis, are only likely to mislead and make it impossible to evaluate the true contribution of the original or the replication study.

In looking at the possible sources of error in testing theories in the personality field, it may be useful to consider the alternatives in terms of Fisher's notion of Type 1 and Type 2 errors. It is possible to accept theories too readily on the basis of apparently favourable results which only test few and possibly atypical deductions from the theory, or which are not in fact rigorously deducible from the theory at all. It is equally possible to reject theories on the basis of apparently unfavourable results where the failure of the experiments in question to give positive results is due, not to defects of the theory, but to errors in the minor premises of the syllogism used to make the prediction, to assumptions about the tests used which are in fact erroneous, or to failure to take into account the whole of the theory to be tested, including the deductions about parameter values contained in it. It is the point of this section to point out that errors of this second type are more frequent in modern psychology than errors of the first type; there are many ways in which predictions can be falsified without involving the failure of the theory in question. Research workers should always be on the lookout for this type of error which, in the early stages of theory development, is the more serious. It is when a theory has been extensively tested that scientists become more aware of the finer details to be taken into account, and it is then that errors of the first type become more unacceptable.

I would not like what I have said to be misinterpreted as advocating that we should relax our standards of rigour; quite on the contrary. What I am suggesting is that these rigorous standards should be applied not only to experiments favourable to theories and hypotheses under investigation, but also to the critiscism made of them theoretically and the invalidation based on experimental studies and 'failures to replicate'. We should always ask whether such invalidation really follows from strict adherence to the dictates of the theory, or whether perhaps the alleged disproof rests on more shaky foundations. It may be noted that Newton's *Principia Mathematica* was widely criticized and rejected by the French physicists for a long time because of lack of rigour in the mathematics (a lack of rigour which was not abolished until 150 years later, by Cauchy in his *Cours d'Analyse*) and because of the numerous anomalies

which appeared on the observational side. All scientific theories are beset by anomalies (Popper 1959); it is only in psychology that the existence of such anomalies is regarded as justification for abandoning an otherwise respectable theory. This is carrying rigour to extremes and must ultimately be destructive; no paradigm can be built up under such a regime. Scientific theories should be abandoned only when a better theory is available, not when anomalies arise. Scientific rigour is obviously necessary if we are to avoid errors of the first kind, but it must be kept within limits by fear of making errors of the second kind. There is a fine balance between extremes which is often difficult to maintain, but vital for success in building up a science of psychology.

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Chapter 2

The Psychophysiology of Extraversion and Neuroticism

Robert M. Stelmack

2.1 Introduction

The personality dimensions of extraversion and neuroticism, which are defined at the descriptive level of Eysenck's system, have been linked to constructs at an explanatory level which are common to hypothetical deductive models in perception, learning, motivation, memory and emotion. As a comprehensive personality system, it is commendable for the attempt to relate personability dimensions to the biological foundations of individual differences in those fundamental psychological processes. The excitation-inhibition hypothesis which was proposed by Eysenck in 1957 referred in particular to the hypothetical constructs of excitation and inhibition which were drawn from those concepts as they were employed by Pavlov (1927) and Hull (1943). Excitation and inhibition were conceived as hypothetical neural processes upon which the acquisition and extinction of behaviour depended. If introverts were characterized by higher levels of cortical excitability and lower levels of cortical inhibition than extraverts, they would be expected to display enhanced sensitivity and efficiency in the processing of sensory stimulation and in conditioning. It was also proposed that such constitutional dispositions may account for individual differences in the social and psychiatric behaviour of introverts and extraverts. The excitation-inhibition hypothesis provoked a good deal of controversy, much of it still unresolved, but the proposal served an important discipline-bridging function and provided a useful and necessary framework for the exploration of the foundations of individual differences in extraversion and neuroticism.

In the 1967 publication of the *Biological Basis of Personality*, Eysenck outlined more specific neural terms for the excitation-inhibition hypothesis by drawing on developments in physiological psychology Gellhorn and Loofbourrow 1963; MacLean 1958, 1960; Samuels 1959; Routtenberg 1966), which led to the proposal of a two level arousal system to account for individual differences in extraversion and neuroticism. In this proposal, the extraversion dimension was identified with differences in levels of activity primarily in the corticoreticular loop, which modulates cortical excitation and inhibition, with introverts characterized by increased levels of activity. Neuroticism, defined as an emotional stability-instability dimension, was linked with differences in level of activity primarily in the limbic system. This proposal, which is an extension of the excitation-inhibition hypothesis rather than a revision, has carried earlier contradictions and controversy with it and in addition has provoked some controversy on its own terms (cf. Koriat et al. 1973; O'Gorman 1974). This chapter begins with a brief outline of the position presented in the *Biological Basis of Personality* on which the hypothesis linking individual differences in extraversion to differences in level of corticoreticular activity was founded. The issues and evidence offered since that time by psychophysiological research addressing that hypothesis are considered in an attempt to clarify the basis of disparate findings and to develop promising research directions. The problematic issue of the physiological basis for the interaction of E and N is considered on similar grounds. Since these questions have been explored primarily with electrocortical and electrodermal techniques, the review emphasizes those procedures; pupillometric measures are briefly mentioned.

2.2 The Physiological Basis of Extraversion

Moruzzi and Magoun (1949) provided the first evidence which identified behavioural arousal with a distinct neural structure when they demonstrated that electrical stimulation of parts of the brain stem reticular formation elicited a general activation pattern in the cortical EEG. When individuals who are resting in a relaxed, quiet state are asked to pay attention to an event or are alerted by a novel change in the environment, the pattern of their EEG activity changes from a slow, synchronized pattern to a fast, desynchronized pattern. Since the time of that discovery, 30 years ago, activity in the ascending reticular activating system (ARAS) has been considered to play an important role in wakefulness, alertness, vigilance and in the regulation of sensory input.

The ARAS is located in the brain stem reticular formation, the central gray core of the brain stem, and is composed of a lattice-work of short nerve cells that are encompassed by the classic sensory pathways, the specific thalamic projection system. Collaterals from the ascending sensory pathways excite cells of the ARAS, which then relay the excitation to widely dispersed sites in the cerebral cortex. This cortical arousal is reflected in the EEG desynchronization. The ARAS also innervates the diffuse thalamic projection system, synchronizing excitation between the thalamus and cortex. It is thought that this reticulo-thalamo-cortical activity constitutes a state of enhanced sensitivity and attention to subsequent excitation from direct sensory pathways and other cortical sources (Lindsley 1970). Eysenck (1967) has suggested that individual differences in this corticoreticular activity may favour the enhanced perceptual sensitivity and vigilance of introverts and facilitate their conditioning. (See Fig. 2.1).

The descending branch of the reticular formation, the reticulospinal tract, also has important motor functions. By exercising excitatory and

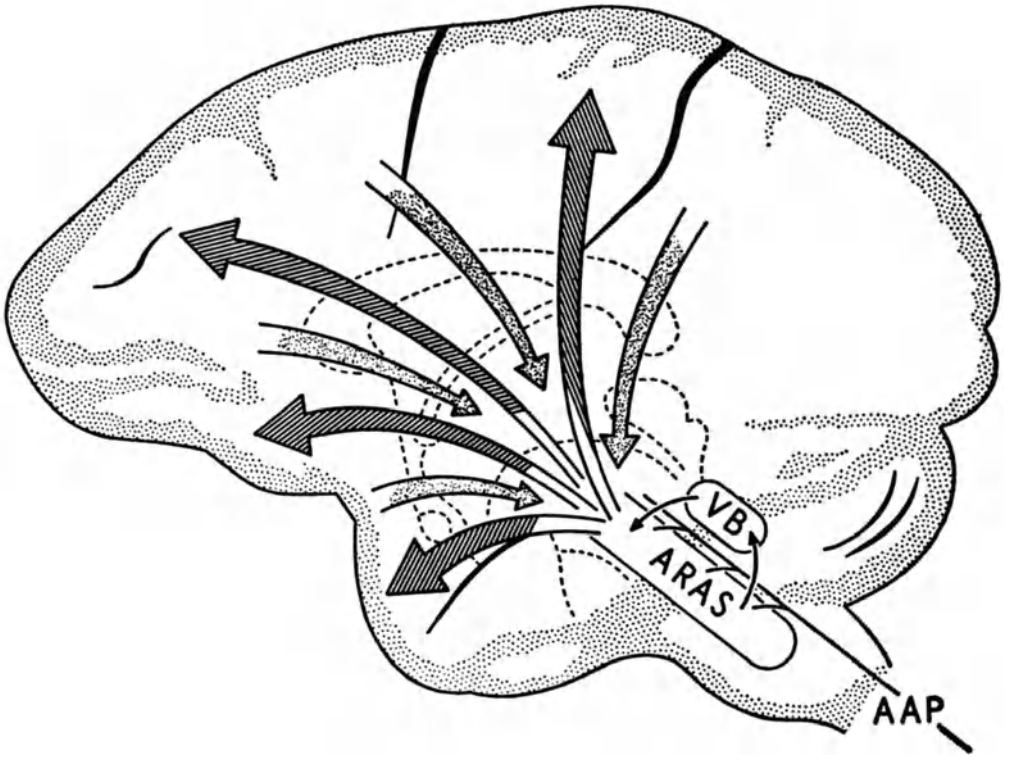

Fig. 2.1. Interaction of reticular formation, visceral brain and cortex. (After Eysenck 1967)

inhibitory control over muscular stretch reflexes, the brain stem reticular formation regulates muscle tone and postural adjustments that are also implicated in states of attention, arousal and emotion. In this regard, it should be noted that the hypothalamus and other limbic structures do not innervate the spinal cord directly. Rather, input from the limbic system is projected to the midbrain reticular formation where interactions involving input from the ascending sensory pathways and other brain centres take place. The corticoreticular system is not an isolated system: it overlaps with systems serving autonomic functions, including the limbic system. In general, it seems clear that the reticular formation is implicated in the initiation and maintenance of motivation, emotion and conditioning by way of excitatory and inhibitory control of autonomic and postural adjustments and by way of cortical coordination of activity serving attention, arousal and orienting behaviour.

The physiology of the reticular formation outlined here admittedly understates the complexity of that system and does not consider the uncertainties and debates attending the exploration of the neurophysiological substrates of attention and arousal. This sketch, however, may adequately convey the context within which the hypothesis that introverts are characterized by greater levels of cortical arousal than extraverts has been considered in the psychophysiological research reviewed here. It is also appropriate to note at this point that there is a rather large speculative leap required to take one from the neurophysiology of the reticular system to the human behaviours which the reticular system is thought to serve. There have been a number of demonstrations with animal preparations which have helped to bridge this gap by showing the direct influence of reticular activation on performance measures. With monkeys, for example, moderate levels of electrical stimulation of the midbrain reticular formation have been shown to facilitate optimal performance in reaction time tasks (Fuster and Uyeda 1962). Stimulation above the optimal level and stimulation at other sites did not show the facilitative effects. Recordings from electrodes implanted in the midbrain reticular formation have also indicated that optimal performance in reaction time tasks are preceded by moderate levels of reticular activity (Goodman 1968). The psychophysiological research reported here, which provides indirect evidence of differences in levels of corticoreticular activity, can also contribute to the neurophysiology of behaviour in general and the biological basis of individual differences in particular.

2.3 Extraversion and Electrocortical Activity

For the most part, the rationale for the research cited in this section was based on the hypothesis that introverts are characterized by higher levels of cortical arousal than extraverts (Eysenck 1967) and from attempts to replicate the report by Savage (1964), where this hypothesis was confirmed with EEG measures. Subsequently, there have been numerous attempts to test the hypothesis of increased levels of cortical activity for introverts by employing EEG indices of cortical arousal, with high levels of arousal in the waking state described by low amplitude, high frequency activity in the alpha frequency range of 8–13 Hz. The outcome of reports published since 1967 presents a collage of inconsistencies and contradictions. Reports identifying increased levels of cortical arousal with introversion have been presented by Gale et al. (1969), Marton (1972), Morris and Gale (1974) and Frigon (1976). Failures to support the hypothesis have been cited by Fenton and Scotton (1967), Gale et al. (1971), Winter et al. (1972) and Becker-Carus (1971). The contrary hypothesis indicating high levels of arousal for extraverts has been reported by Broadhurst and Glass (1969) and Gale et al. (1972). A good deal of the inconsistency in these findings appears to stem from differences in recording and scoring methods and in the subject selection and preparation procedures. Many of these studies have been reviewed by Gale (1973); to some extent, his review spares one the necessity of detailing the differences which mark the studies cited.

The difficulties in comparing and evaluating the outcomes of the studies reviewed may be underscored by pointing out that these studies are virtually idiosyncratic in their electrode placements and in the methods of reducing the EEG data and defining indices of alpha activity. Similarly, the task demands on the subject vary from reclining in a semi-somnolent state with eyes closed to procedures where the subject sits upright and performs difficult arithmetic problems. Strictly speaking, any one of these differences preempts the possibility of replicating results. Sex differences within the subject classifications were not controlled in a number of the studies reported. Broadhurst and Glass (1969) and Winter et al. (1972) comment on the possible role that sex differences contribute to their results, but the extent to which these sex differences confound EEG effects due to extraversion was not certain. Sex differences are not regarded in the mixed samples employed by Gale et al. (1969), Marton (1972), Becker-Carus (1971) and Morris and Gale (1974).

Several of these studies have other limitations which diminish the strength of their findings. Hand scoring techniques, which are vulnerable to experimenter error, have been employed by Becker-Carus (1971), Fenton and Scotton (1967) and in treating part of the data in the work by Broadhurst and Glass (1969). Only Fenton and Scotton report reliability data for their measures, but as Gale (1973) has pointed out, their presentation of the stimuli was contingent on presence of alpha activity during the experiment. As a result, individual differences may be confounded by differences in the prominence of alpha activity to visual inspection, differences in rate of presentation of stimuli and differences in the duration of the experiment. Frigon (1976) does not report filter frequency ranges nor his scoring criteria for distinguishing alpha activity. His analysis of the EEG appears to depend solely on visual inspection.

The study by Becker-Carus (1971) appears to have been particularly beset with difficulties, as almost 50% of his 36 subjects were not included in the statistical analysis of the data for technical reasons. In addition, Becker-Carus employed a German translation of the MPI (Brengelmann and Brengelmann 1960) in the classification of subjects. While there may be some reservations regarding the construct validity of this instrument, it is the omission of an adequate description of score distributions which limits the assessment of the relationship between personality variables and EEG activity. These same complaints apply to Marton (1972), who also used the Brengelmann and Brengelmann translation.

Both of the studies which report higher levels of cortical arousal in extraverts reveal some problems in the classification of subjects. Broadhurst and Glass (1969) noted that the classification of extraversion and neuroticism into high and low groups resulted in four groups of unequal size, but the number of subjects assigned to each group was not indicated and the degree of precision in evaluating each treatment effectively is uncertain, especially with the small samples involved. Gale et al. (1972) indicated that the distribution of extraversion scores in a relatively small sample of 20 subjects tended to favour scores in the extraverted range. It should also be noted that the principal aim of this study was to consider the effects of time of day on EEG arousal indices and that the increased arousal was observed only during an early morning session. From their discussion of results it is clear that the authors did not place much confidence in their findings.

The study conducted by Gale et al. (1971) is exemplary for the way in which subjects were selected and classified, for the double blind procedures adopted for recording and scoring their data and for the meticulous techniques applied in reducing and analysing their data. High and low extreme scorers on the neuroticism dimension were classified according to high, middle and low extraversion scores to form six groups of ten subjects each. No differences between introverts and extraverts were observed, a result which signalled a failure to replicate their earlier report (Gale et al. 1969). Apart from technical improvements, the principle distinction between the two studies is in the degree of attention or arousal induced by the instructions to the subjects. In the former study, subjects were instructed to either open or close their eyes ten times in alternate 2-min intervals; in the latter study, subjects were instructed to close their

eyes and relax while 65-dB tones of 5-s duration were presented every 2 min. Subjects were reclining in both experiments. Their data indicated that conditions in the first experiment elicited higher arousal levels overall. The authors also suggested that higher levels of arousal in introverts are observed under conditions which induce moderate levels of arousal (Gale 1973).

The study by Winter et al. (1972) reports a failure to replicate their earlier findings of higher levels of arousal for extraverts. The experiment involved eyes open and eyes shut (low arousal) and solving arithmetic problems with eyes open and eyes shut (high arousal). The temporal effects of these conditions were not considered. The authors indicate that under the eyes open, mental arithmetic condition, "the neurotic extravert showed higher cortical arousal than the neurotic introvert, while the stable introvert showed higher cortical arousal than the stable extravert." (p. 47). The significance of this effect was not indicated by post-hoc statistical analysis, however, and the graphic presentation of this interaction is not entirely convincing.

The relationship of imagery and extraversion to EEG alpha abundance was investigated in the study by Morris and Gale (1974). Subjects viewed a series of words presented on slides (5-s duration each) and then were instructed to shut their eyes and to allow images associated with the words to form. The statistical analyses were based on the mean alpha abundance for each subject averaged across the different conditions defined in the procedure and are thus confounded in the analysis. The significant positive correlation between alpha abundance and extraversion, indicating high levels of cortical arousal in introverts, endorses the previous findings by Gale et al. (1969).

The study by Frigon (1976) was a complicated project investigating the congruence of the extraversion dimension with the dimension of strength of the nervous system from the Russian typology of the nervous system (cf. Gray 1964). His procedures described conditions designed to determine habituation, conditioning, differential conditioning, the establishment of a conditioned response baseline and extinction with reinforcement. Differences between introverts and extraverts were obtained only in the latter condition. This paradigm consisted of 48 CS-UCS pairings (70-dB tone with pictorial slides) with interstimulus intervals of 4–5 s. Analysis of CR magnitude, i.e. the mean duration of alpha blocking during the presentation of the CS alone on four blocks of three successive test trials, indicated greater cortical arousal for introverts. The principle drawback of this project pertains to the uncertainty of the recording and scoring procedures mentioned previously.

Perhaps the most appropriate conclusion which can be drawn from this survey is that the demonstration of higher levels of cortical activity for introverts using EEG measures of arousal remains equivocal. The most adequate study (Coles et al. 1971) did not confirm the hypothesis. The conditions under which their recordings were taken, however, were designed to induce low levels of arousal. Studies which have obtained higher levels of arousal in introverts could be described as having conditions which induced moderate levels of arousal as Gale (1973) has suggested. This observation, however, should be viewed as a suggestion to guide future research rather than a generalization. Even within those studies supporting the hypothesis, conditions cannot be described as 'moderate' with confidence. Marton (1972), for example, employed 'weak' low frequency tones in an habituation paradigm. Although intensity level was not reported, one may be inclined to interpret the 'weak signals' as a condition which would induce low levels of arousal. Similarly, the study by Gale et al. (1969) had the subject reclining while opening and closing his eyes. One is hard pressed to describe this condition as inducing moderate levels of arousal. On the other hand, the stable introverts who showed higher levels of cortical arousal than stable extraverts in the study by Winter et al. (1972) were engaged in solving mental arithmetic problems with their eyes open, a condition which can be described as inducing higher levels of arousal. It is only with a great deal of optimism, then, that one could state that introverts show higher levels of EEG activity under conditions of moderate arousal. Nevertheless, one must concede that the general direction of the results of these inquiries is towards higher levels

of cortical activity for introverts under conditions intermediate between semi-somnolence and stressful.

2.4 Extraversion and Cortical Evoked Potentials

The technique of averaging cortical potentials which are evoked by specific stimulation permits the investigation of stimulus related cortical activity that is difficult to distinguish in the ongoing EEG activity. In general, increases in intensity of stimulation are reflected in increased evoked potential amplitude. Indeed, evoked potential amplitude and intensity have been found to be related by power functions similar to those obtained by psychophysical measures for visual, auditory and somatic sensory modalities (Regan 1972). While the congruence of the relation between intensity and evoked potential amplitude and the relation between intensity and sensory magnitude has been shown, the degree of correspondence between evoked potential measurement and estimates of sensory magnitude is still rather unclear. It is clear, however, that averaged evoked potentials provide a useful technique for investigating sensory information processing. Enhanced evoked potential amplitudes are also thought to reflect increased levels of attention (cf. Näätänen 1975). If evoked potential measures can serve as reliable indices of sensory sensitivity and attention, they would seem to be appropriate techniques for exploring the psychophysiological basis of differences in sensory sensitivity (Smith 1968; Stelmack and Campbell 1974; Siddle et al. 1969) and vigilance (Krupski et al. 1971; Harkins and Geen 1975) between introverts and extraverts.

Increased levels of cortical activity for introverts inferred from the somatosensory evoked response have been reported by Shagass and Schwartz (1965), but attempts to replicate this finding have been unsuccessful (Häseth et al. 1969; Burgess 1973). In these cases, correcting for individual differences in sensitivity by applying different levels of stimulus intensity according to the subject's absolute threshold to somatic stimulation may have precluded the observation of individual differences in evoked response. Although a direct relationship between somatosensory evoked response and subjective sensitivity has not been confirmed, it is apparent that correcting individual differences in perceptual sensitivity by applying different stimulus intensities to different individuals would confound the effects of personality differences on the evoked potential amplitude.

Significant negative correlations between extraversion and amplitude of the evoked response to 1000-Hz, 60-dB tones have been observed by Hendrickson (1973). Negative findings, however, were reported by Rust (1975) in an elaborate but unsuccessful attempt to replicate the previous results. In his first study, the auditory evoked response (AER) to 1000-Hz tones at 95 dB were recorded from 84 subjects. In the second case, the AER to 95, 75 and 55-dB tones were recorded from 212 subjects. Of the more than 100 correlations noted, positive correlations between extraversion and the latency of the negative peak at 100 ms and the peak to trough amplitude from that same point during the 75-dB condition were the only significant results observed and these effects were in the opposite direction to those reported by Hendrickson (1973).

Stelmack et al. (1977) have reported that introverts obtained greater amplitude of the AER than extraverts with low frequency stimulation (500 Hz) at 55 dB for one group of 30 subjects and 80 dB for another group of 30 subjects (see Fig. 2.2). No differences between groups were observed with high frequency (8 kHz) stimulation. The level of attention required for the subject was enhanced with instructions to count the series of alternating high and low frequency tones. This requirement may be contrasted with conditions in the study by Rust (1975), where subjects received a train of stimuli at a single frequency every 33 s. Stelmack et al. (1977) argue that the determination of differences between introverts and extraverts may have been facilitated by employing low frequency auditory stimulation. It has been demonstrated that inter-individual variability of the AER is greater at low frequencies than at higher frequencies

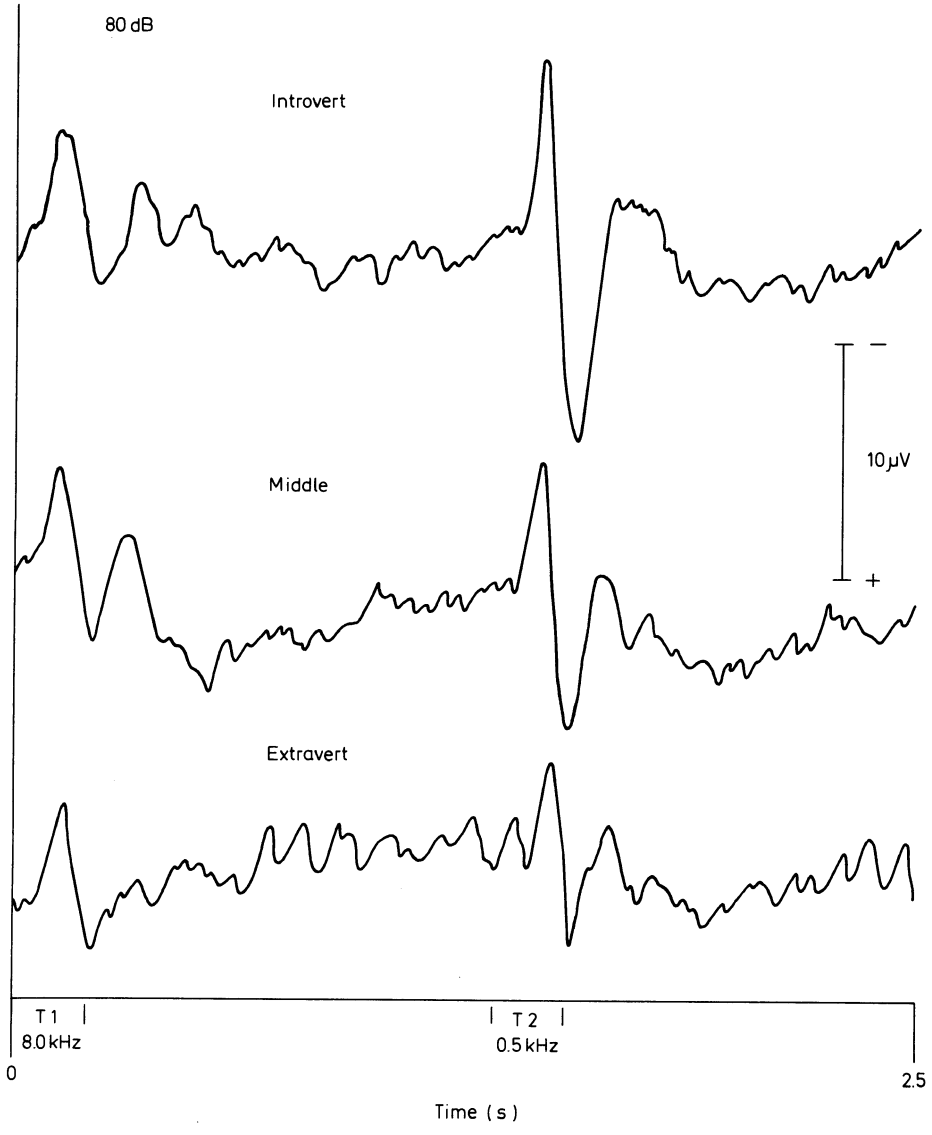

Fig. 2.2. Typical auditory evoked response for introvert, middle and extravert subjects for high (8.0 kHz) and low (0.5 kHz) frequency tones at 80-dB intensity. (After Stelmack et al. 1977)

(Davis and Zerlin 1966; Rothman 1970). In any case, only further research can determine the efficacy of the conditions under which these positive results have been observed.

An intriguing result bearing on extraversion and individual differences in the level of central excitatory and inhibitory activity was observed during an investigation of the effects of stimulant and depressant drugs on magnitude of contingent negative variation (CNV) (Ashton et al. 1974). The CNV is a negative baseline shift in the EEG which develops prior to an expected stimulus (Walter et al. 1964); this event related cortical activity is also considered to be a useful indicator of attention and arousal functions (Tecce 1972). Ashton et al. reported that extraversion and the rate of nicotine intake was correlated with percentage change in CNV magnitude, a result which indicated that the rate of nicotine intake in extraverted smokers was slower and perhaps associated with a stimulant effect, while in introverted smokers the rate

was faster and associated with a depressant effect. It was also speculated that "introverts and extraverts appear to select doses of nicotine when smoking in order to obtain respectively the depressant and stimulant effects needed to offset their innate cortical disposition." (p. 68).

A somewhat similar effect was reported by Janssen et al. (1978), with introverts showing a significantly lower mean CNV amplitude during exposure to white noise than without white noise. Both of the CNV studies cited here were primarily concerned with the effects of depressant and stimulant drugs on the CNV; the differential effect of stimulant and depressant drugs on groups differing on degree of extraversion was ancillary to their principal purpose. It is clear, however, that the CNV may provide a useful tool for exploring individual differences in extraversion, perhaps within the context of the orienting reaction (Weerts and Lang 1973; Loveless and Sanford 1974) or the distraction-arousal hypothesis (Tecce 1974) since both of these paradigms bear directly on attention and vigilance.

2.5 Extraversion and the Orienting Reaction

Pavlov (1927) observed that behavioural arousal accompanied the first few presentations of the conditioned stimulus, behaviour which suggested that the organism was orienting to change in the environment; it was also indicated that this orienting reaction (OR) may be an important precursor of the conditioning process. Research on this mechanism, particularly in the West, was virtually neglected until the publication in English of Sokolov's *Perception and the Conditioned Reflex* in 1963. This work, in which Sokolov proposed his model of the orienting reaction, has had a considerable impact on Western psychophysiology, an influence which extends to the psychophysiology of individual differences in extraversion and neuroticism (O'Gorman 1977).

In Sokolov's model, characteristics of stimulus input such as intensity, frequency and duration are said to be stored in a neuronal model. An OR would be elicited when the neuronal model of current and previous stimulation do not match; habituation is said to occur on subsequent presentation of the stimulus when a match is made. The components of the OR system are somatic, (including movements of the body, head and eyes towards the stimulus) autonomic (including increases in skin conductance, cephalic vasodilation, digital vasoconstriction, heart rate deceleration, momentary cessation of respiration and pupillary dilation) and central (including EEG desynchronization). It is generally agreed that in the basic chain of events leading to an OR (cf. Lynn 1966) cortical excitatory impulses contingent on changes in sensory stimulation and impulses via collaterals from the ascending sensory tracts impinge on the reticular formation, which in turn activates the hypothalamic sites that initiate the autonomic components of the OR. With repetitious sensory stimulation, cortical inhibitory impulses impinge on the collaterals that transmit impulses from the ascending sensory tracts to the reticular formation, whereby the autonomic response diminishes and habituation occurs.

As the title of Sokolov's work declares, the functional significance of the OR is to facilitate the selection and analysis of stimulation. Although there has been considerable work devoted to defining the stimulus parameters which maintain the elicitation and habituation of the OR (cf. Graham 1973), the facilitative effects of the OR on sensory processing remain to be clarified and the role of the OR in conditioning constitutes an issue which remains to be decided (cf. Stern and Walrath 1977). It is in this uncertain context that those who would exploit the OR paradigm to explicate the psychophysiology of extraversion boldly tread.

Sokolov's model provided a conceptual framework and delineated a number of psychophysiological indices for investigating fundamental psychological processes such as sensitivity, attention and conditioning, making this model a particularly attractive one for exploring individual differences in extraversion. Because of the central role which cortical exitatory and inhibitory activity plays in the elicitation and

habituation of the OR, inferences regarding levels of corticoreticular activity can be made by observing the level of autonomic activity in response to repetitious sensory stimulation, with higher levels of activity evident in greater response amplitude and less habituation. Such considerations have made the manipulation of the orienting reaction, in particular habituation of the electrodermal OR, a popular strategy to test the validity of the hypothesis linking introversion with increased levels of corticoreticular activity.

There is a good deal of consistency in the electrodermal OR studies which consider individual differences in extraversion, although even within those studies which endorse Eysenck's hypothesis the consensus is complicated. Evidence which is consistent with the hypothesis of greater cortical arousal for introverts has been noted by Mangan and O'Gorman (1969), Crider and Lunn (1971), Wigglesworth and Smith (1976), Smith and Wigglesworth (1978), Stelmack et al. (1979), Gange et al. (1979), Nielsen and Petersen (1976), Fowles et al. (1977) and Desjardin (1976). Negative findings have been reported by Coles et al. (1971), Siddle (1971), Sadler et al. (1971), Krupski et al. (1971), Bohlin (1972), Koriat et al. (1973), Feij and Orlebeke (1974), Bartol and Martin (1974) and Mangan (1974).

This count, however, does not stand alone; the credibility of the effect rests on the adequacy of the experiments and in replications. In addition, the failures to support the hypothesis must be accounted for as well as the successes. Such an analysis is facilitated by two extensive reviews which have been recently published. In an analysis of OR habituation and the autonomic nervous system, Graham (1973) has reviewed the effect of stimulus parameters on the habituation of autonomic response measures, and O'Gorman (1977) has reviewed the role of individual differences in habituation.

The strongest case among those studies reporting significant positive results would seem to rest with several authors who have reported a series of investigative studies exploring the conditions under which significant differences between introverts and extraverts may reliably emerge. It may be appropriate to begin with studies by Mangan and O'Gorman (1969) and Mangan (1974), who were among the first to investigate directly the apparent similarities between the extraversion dimension and the strength-sensitivity dimension in the neo-Pavlovian typology of nervous system properties (cf. Gray 1964; Eysenck 1967; Nebylitsyn and Gray 1972).

Eysenck (1967) drew attention to similarities between introverts and the weak nervous system type on the basis of such characteristics as relatively low sensory threshold, low thresholds of arousal, low thresholds of transmarginal inhibition (response decrement at high intensity) and persistent orienting reflexes. Mangan and O'Gorman note, however, that habituation of the OR is regarded as an index of the property, dynamism of inhibition, which is considered to be orthogonal to the strength-sensitivity dimension (Nebylitsyn 1972). In their first study, they argued that weak nervous system subjects, who seem to be similar to introverts (Eysenck 1967), should have greater initial OR magnitude than strong nervous system subjects. They also explored the possibility that rate of OR extinction (dynamism of inhibition) was linked with extraversion. Their results show that extraverts displayed larger initial amplitude electrodermal responses to moderate intensity tones. This finding is contrary to the expectations of Eysenck's hypothesis. A second result reported, however, notes that introverts habituated less than the extraverts when subjects had moderate neuroticism scores and low frequency stimulation was employed. While this study is methodologically sound in most respects, the description of the electrodermal response measures in arbitrary units rather than absolute values limits the analysis and makes comparisons with other reports difficult. The second study (Mangan 1974) was an ambitious attempt to explore personality, cognitive and psychophysiological parameters of classical appetitive GSR conditioning. This study did not replicate any of the findings of their previous work but the initial amplitude to tactual stimulation was positively correlated with extraversion. Some uncertainty in the analysis of this report is introduced by the omission of statistical summaries of the electrodermal response and extraversion score distributions.

Since repeated testing of the same subjects can also introduce a source of error in electrodermal responding (cf. Bishop and Kimmel 1973), some question remains concerning the temporal effects of the ten sessions in which all 21 subjects participated. The larger initial amplitude responses for extraverts observed in these reports is unique among the electrodermal studies reviewed here.

In the first of two studies, Wigglesworth and Smith (1976) selected 90 subjects to form a matrix of high, middle and low extraversion and neuroticism groups with five male and five female subjects in each group. At the end of 30 tone presentations in the habituation series, introverts showed larger response amplitudes than extraverts to the stimulus tone following the interposition of a novel 500-Hz tone (see Fig. 2.3). This dishabituation effect (cf. O'Gorman 1974) was evident for introverts but not for extraverts. No significant differences in trials to criterion habituation rate were observed, but the introvert group showed significantly larger initial response amplitudes than extraverts at the 80-dB intensity level, while extraverts showed significantly larger initial response amplitudes than introverts at the 100-dB intensity level. This reversal was interpreted by identifying the extraversion dimension with the Russian dimension of strength of the nervous

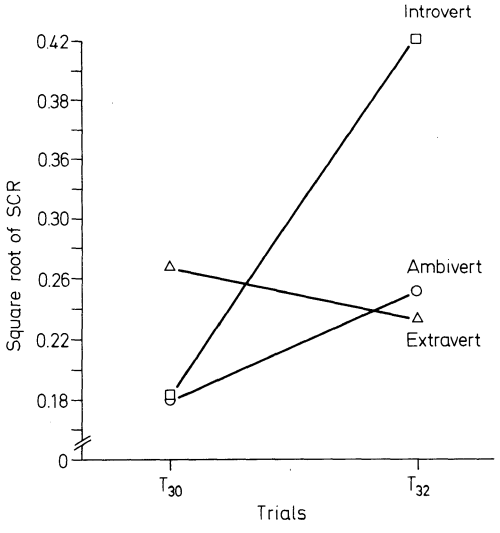

Fig. 2.3. Dishabituation response amplitude as a function of extraversion. (After Wigglesworth and Smith 1976)

system (Gray 1964, Eysenck 1967) with introverts, the weak nervous system type, reaching the level of transmarginal inhibition (response decrement) at lower levels of stimulus intensity than extraverts. Their second study was essentially similar to the first, with an additional 60-dB condition introduced. The dishabituation effect was replicated. In this case, however, no significant differences between introverts and extraverts were observed with the initial amplitude measures. It was found, however, that extraverts habituated more rapidly than introverts at the 100-dB intensity level. While greater electrodermal activity of introverts is the most consistent result reported in both experiments, the effect was observed with different response measures and under different conditions. This disparity, which is not an atypical finding, suggests that the relationship between extraversion and electrodermal response is complex and may emerge only under specific conditions.

Fowles et al. (1977) conducted a series of four experiments which departed from the OR paradigm employed in the majority of studies cited here by reporting only skin conductance level rather than skin conductance responses. While the experimental manipulations are compatible with OR studies, the OR, which is indexed by an increase in conductance from the onset of stimulation, is not considered, In all four of the studies reported by Fowles et al. (1977), between groups designs were employed involving two levels of extraversion, auditory stimulus intensity and task difficulty as the independent variables. Subjects were classified with the ego control scale (EC-5M and F) and ego resiliency scale (ER-S) which are the first two orthogonal factors of the MMPI and which have been identified as independent extraversion and neuroticism dimensions that are significantly correlated with the EPI scales (Wakefield et al. 1974). In the first experiment, 80 male and female subjects were categorized into extraversion and introversion groups on the basis of extreme scores on the extraversion scale. Following a difficult paired associate learning task, extraverts were observed to have a higher skin conductance level than introverts during 20 presentations of 1000-Hz, 103-dB tones. This result was replicated in a second study employing 40 subjects.

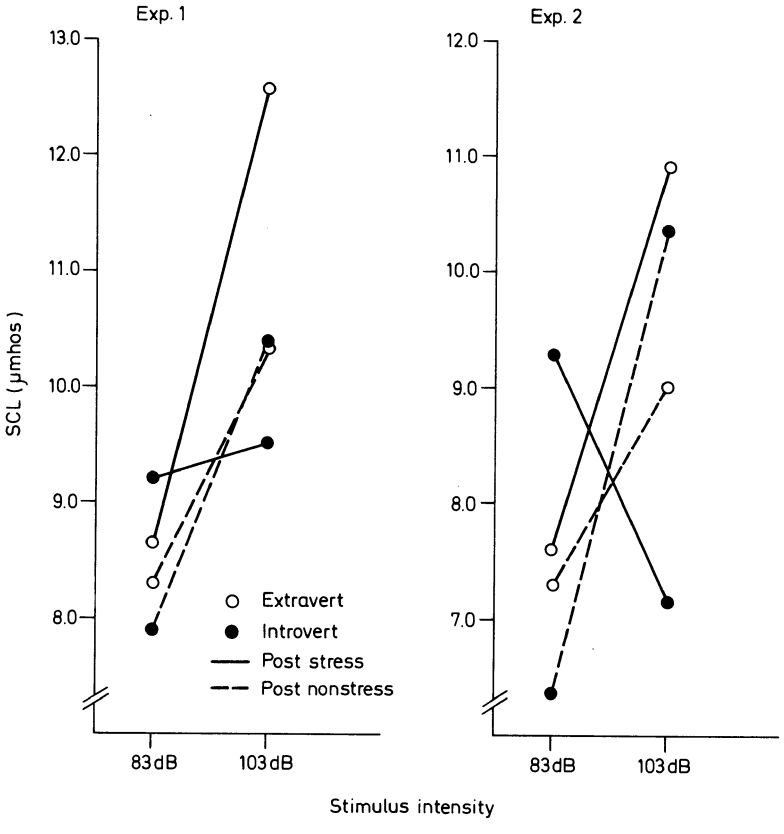

Fig. 2.4. Pre-stimulus skin conductance level for introverts and extraverts during 83-dB and 103-dB tone presentations following easy (post non-stress) and difficult (post-stress) tasks. (After Fowles et al. 1977)

No differences were evident between extraversion groups with tones at 103-dB intensity following an easy paired associate task, nor with tones at 83-dB intensity following either the difficult or easy task. In effect, the greater responsiveness of extraverts was evident only during the high intensity condition following a difficult task (see Fig. 3.4).

In a third similar experiment, 120 female subjects, classified into high, middle and low extraversion with neuroticism scores restricted to the middle range, were treated as before, but no learning task was given prior to the presentation of the tones. The higher skin conductance level of the introvert group was apparent during the presentation of tones at 83-dB intensity but no differences were evident at 103-dB intensity, i.e. the greater responsiveness of introverts emerged in a non-stress, moderate intensity condition. In a fourth experiment, female subjects were classified on the basis of extreme scores on both the extraversion and neuroticism dimensions. Tone presentations were made at 75-dB and 100-dB intensity levels and the experiment was repeated with the same subjects. In both sessions, introverts revealed higher skin conductance levels during initial tone presentations at 75-dB intensity, replicating the effects observed in their third experiment. These results were seen to be consistent with the view that the extraversion dimension and the dimension of strength of the nervous system were congruent (Gray 1974; Eysenck 1967), with the introverts showing "greater responsiveness at low stimulus intensities and the decline in responsiveness at high stimulus intensities as a result of transmarginal inhibition (p. 142)." These conclusions are also in agreement with the first study of Wigglesworth and Smith (1976). More importantly, this series of experiments would seem to constitute a convincing demonstration of higher skin conductance levels for introverts at moderate intensity levels (75 and 83 dB). One reservation which ought to be considered, perhaps, is the use of the MMPI in the classification of subjects. While there would seem to

be good arguments favouring the identity of the scales employed with the EPI dimensions, their application to Eysenck's hypothesis is nevertheless indirect.

In a series of experiments which attempted to consider the rapprochement between Eysenck's system and the Russian typology of the nervous system, Stelmack et al. (1979) reported that the introverts showed less habituation than extraverts to chromatic stimuli as evidenced by cardiac, skin resistance response and vasomotor indices of the OR. Introverts displayed more electrodermal and vasomotor ORs than extraverts to red, blue, and grey stimuli and more cardiac ORs to the blue stimulus. Extraverts displayed more cardiac ORs to red than introverts. In an independent experiment employing the same 30 female student nurses as subjects, introverts again displayed more ORs than extraverts to visually presented neutral and affective words with the electrodermal and vasomotor indices of the OR. The psychophysiological measures taken in these studies were rather coarsely analyzed with only trials to criterion habituation reported for the three measures. Tonic levels were not indicated, and the extent to which responses are independent of initial levels is not evident. The distribution of the electrodermal responses were also badly skewed. The confidence in this experiment rests mainly on the observation of more persistent electrodermal and vasomotor ORs for the same group of introverts on two different occasions. In a third experiment using an independent sample of 60 female subjects, the greater number of electrodermal ORs to chromatic stimuli for introverts was replicated. Introverts also obtained greater initial amplitude of the skin conductance response. This experiment would appear to be adequate in most respects and would support the hypothesis of higher levels of cortical arousal for introverts.

In an unpublished inquiry into the effect of meaning on habituation of the OR conducted in the Ottawa laboratory by Desjardins (1976), extraversion was significantly and inversely related to two separate estimates of spontaneous skin conductance and to skin conductance level. The stimulation procedure involved visual presentation of words differing in concreteness–abstractness and associative–connotative linguistic meaning to a sample of 96 women. Correlations based on the average electrodermal response across the four word conditions indicated a significant negative relationship between extraversion and skin conductance level. A significant negative correlation between extraversion and the number of spontaneous SCRs during the resting period prior to habituation trials was also observed. Similarly, a negative correlation between extraversion and the number of SCRs during the 30 interstimulus intervals was noted. The study would appear to be technically correct and would endorse Eysenck's hypothesis.

Of the remaining studies which report significant correlations between extraversion and electrodermal activity, the report by Crider and Lunn (1971) is commendable. They reported significant negative correlations between the extraversion factor of the MMPI and the habituation rate and number of spontaneous fluctuations of the skin potential response to 1300-Hz, 90-dB tones. Nielsen and Petersen (1976), recording the skin resistance response to auditory stimulation, reported significant negative correlations between extraversion and number of spontaneous fluctuations as well as the number, amplitude and recovery of the orienting reaction. Their habituation series was atypical in that it comprised a random series of 60-dB white noise stimuli and 70-dB, 200-Hz pure tones. There are also questions which may be raised in light of the omission of minor details in their report. For example, it is not clear whether their OR amplitude measure, defined as a decrease in resistance from stimulus onset divided by skin resistance level, refers to the amplitude of the initial response or to the average of the habituations series. Overall, however, the study appears to have been carefully executed.

Krupski et al. (1971) examined electrodermal and personality correlates of auditory vigilance performance. Higher levels of cortical arousal have been hypothesized as determinants of the introverts' superior performance in vigilance tasks, perceptual sensitivity and conditioning, but there have been few studies which have tested the hypothesis directly by recording psychophysiological measures and performance mea-

sures concurrently. The approach taken by Krupski et al. (1971) is notable. Vigilance performance, quantified as the number of reports of signals where signals did not occur (false alarms) was positively related to extraversion and negatively related to the electrodermal response amplitude of the first stimulus presented and to the average amplitude for all points where a signal appeared and was detected. No significant correlation between extraversion and electrodermal response was reported. The omission of a summary or statistical statement of the electrodermal data and the distribution of extraversion scores limits the analysis of the adequacy of this experiment. In a more recent report employing the same strategy (Gange et al. 1979), introverts were observed to display more electrodermal responses to signal stimuli, and to trial markers in a visual vigilance task. Introverts also displayed more spontaneous responses both during the vigilance task and during an observation condition in which they were asked to pay attention to the visual display but not to detect signals. No difference in nonspecific responses were observed in a condition where subjects were asked simply to sit in the room with no visual display presented. Heart rate levels during all three conditions were also higher for introverts than extraverts. The experiment demonstrates the concomittance of arousal indices and superior vigilance performance of introverts.

An analysis of the reports reviewed offer a number of possible conditions which may influence the differentiation of introverts and extraverts with electrodermal measures. Differences in stimulus parameters and modalities, subject selection procedures and choice of response measurement seem to merit consideration.

2.5.1 Stimulus Characteristics

The measurement of the electrodermal OR to auditory stimulation has been the method of choice for the majority of studies investigating the relationship between extraversion and the OR. The most consistent observation in these studies is that the application of 1000-Hz, 60–75-dB tones typically fail to differentiate introverts and extraverts (Mangan and O'Gorman 1969; Coles et al. 1971; Siddle 1971; Sadler et al. 1971; Koriat et al. 1973; Feij and Orlebecke 1974; Mangan 1974). In the second study by Mangan and O'Gorman (1969) and in the report by Nielsen and Petersen (1976) where significant effects were reported, the stimulus intensity was in the 60–70-dB range but low frequency tones were employed (380 Hz and 200 Hz respectively). As noted in the previous section, there is evidence that inter-individual variability of the auditory evoked response is greater under low frequency conditions. Similar effects have not been demonstrated with electrodermal activity, however, and indeed there seems to have been no recent systematic investigation of the effects of frequency on OR elicitation or habituation. (Also note that differences in absolute sensitivity have been observed only under low frequency conditions. Smith 1968; Stelmack and Campbell 1974). The Nielsen and Petersen habituation training trials also contained an additional level of complexity in randomizing the presentation of two sounds, a manipulation which could conceivably raise the level of interest or arousal value of the condition.

Greater electrodermal responsiveness for introverts has been observed in studies employing auditory stimulation in the 75–90-dB range (Crider and Lunn 1971; Wigglesworth and Smith 1976; Fowles et al. 1977). Bohlin (1972) also employed 1000-Hz tones in this intensity range (80 dB) but failed to observe differences between introverts and extraverts. The experimental procedure was designed to induce sleep during a habituation procedure. The monotonous stimulation procedures described clearly differ in arousal potential from those studies employing similar levels of intensity.

There is a lack of consistency among those studies which have employed tones at the 100-dB intensity level. Fowles et al. (1977) observed greater skin conductance levels for extraverts when the habituation series was preceded by a stressful arithmetic task but not under nonstressful conditions. Wigglesworth and Smith (1976) found that extraverts presented larger initial response amplitudes than introverts at this high intensity level and under minimally

stressful conditions. This effect did not replicate in a subsequent study (Smith and Wigglesworth 1978), where in fact extraverts were again observed to habituate more rapidly than introverts and to give smaller responses to a novel test stimulus following habituation trials and the subsequent standard stimulus.

Several of the studies employing visual stimulation have observed significant effects. Verbal stimuli were employed by Stelmack et al. (1979) and Desjardins (1976). Differences between introverts and extraverts were also observed by Gange et al. (to be published) during a visual vigilance task. Stelmack et al. (1979) and Bartol and Martin (1974) employed chromatic stimuli. In the latter case, initial amplitude differences failed to reach acceptable confidence levels (p 0.10) but the direction of differences was consistent with the greater electrodermal responsiveness of introverts observed in the other reports employing visual stimulation.

It appears that conditions which favour differentiating between extraversion groups with the electrodermal measures of the OR can be described as moderately arousing, a consideration which may serve as a rough guide in the selection of stimulus conditions. From among the stimulus conditions in the studies reviewed here, low frequency tones in the 75–90-dB intensity range and visual stimuli provide the base from which such an inquiry would commence. A precise definition of what constitutes a moderately arousing stimulus has not been stated. One criterion for a precise definition of a moderate stimulus condition may refer to that point on a stimulus dimension which maximizes individual differences in autonomic response. This case is analogous to the situation in the construction of achievement tests, where it is items (rather than stimuli) of moderate difficulty level (rather than arousal level) which increases discriminative power.

2.5.2 Subject Selection

Extraneous subject characteristics are also thought to contribute to inconsistencies of results observed in psychophysiological research generally and the psychophysiology of extraversion particularly (Averill 1974; O'Gorman 1974). It would appear, however, that subject factors such as age (Surwillo 1965, 1969; Surwillo and Quilter 1965) and sex (Kimmel and Kimmel 1965; Purohit 1966), which are known to contribute to the between subject variability of electrodermal measures, can be ruled out as factors which confound differences between introverts and extraverts. Of the four studies which employed mixed samples, Bartol and Martin (1974), Wigglesworth and Smith (1976), Fowles et al. (1977) and Smith and Wigglesworth (1978) obtained positive effects, while only Bohlin (1972) did not. Virtually all of the studies reviewed drew their subjects from undergraduate programmes and the ages of subjects ranged from 19 to 35 years of age. The inadequate description of score distributions along the extraversion dimension was a characteristic of several reports which failed to support Eysenck's hypothesis. (Siddle 1971; Koriat et al. 1973; Feij and Orlebecke 1974; Mangan 1974). Since the magnitude and the extent of variation of extraversion scores is omitted, one cannot determine whether a full range of scores along the extraversion dimension was represented or whether the sample was skewed or restricted to the middle range of scores.

The notion of autonomic response specificity advanced by a number of authors (Malmo and Shagass 1949; Lacey and Lacey 1958; Engel 1972; Sersen et al. 1978) is another subject characteristic which contributes to between subject variability and may be particularly relevant to studies relating extraversion differences to differences in autonomic response. Despite the evident tendency of individuals to demonstrate a preferred or 'stereotyped' autonomic response even to moderate stressors, featuring a component measure of autonomic activity rather than a holistic response of this system, there were few studies designed to accomodate that principle. Some insight into the effects of this individual response specificity, wherein some individuals are disposed to blush in a moderately arousing situation, while others perspire or palpitate, is gained in the study of Stelmack et al. (1979) in the habituation to neutral and affective words where multiple autonomic mea-

sures were recorded. As indicated by a standard multiple regression analysis, the conjoint influence or cardiac, electrodermal and vasomotor OR components accounted for 54% of the variation in extraversion (multiple r – 0.73) while no single component accounted for more than 24 per cent. A mechanism of the kind implied by the notion of autonomic response sterotype can account for such an increase in prediction and suggests that consideration of the individual autonomic response preference of subjects merits deliberate attention and that the application of multiple autonomic measurements may be worthwhile. The question which must be considered is whether the electrodermal response, by itself, is an adequate index of level of activity in the corticoreticular system, particularly as it applies to individual differences in extraversion, given the prospect that the between subject variability of specific autonomic systems may be idiosyncratic to some extent. Other studies recording from more than one autonomic system concurrently (Koriat et al. 1973; Feij and Orlebeke 1974) did not assess the conjoint effect of electrodermal and cardiac measures in the prediction of individual differences in extraversion, and the question remains quite speculative.

2.5.3 Measures of Electrodermal Recording

The several measures which are derived from electrodermal recording can be generally described in terms of tonic levels and phasic responses or changes from tonic levels. The principal determinant of tonic levels of electrodermal activity is thought to be the number of active sweat glands (Montagu and Coles 1966; Edelberg 1972), and this activity is considered to be a good index of general arousal level (Malmo 1959; Raskin 1973). Differences related to fast and slow recovery rate of phasic responses suggest a complexity of physiological determinants that involve at least two different mechanisms. This complexity extends to the psychological significance of phasic responses as well. Responses elicited by specific stimulation have been variously employed as indices of anxiety, arousal, attention and orienting; responses elicited spontaneously in the absence of specific stimulation have been frequently interpreted as an index of altertness (Raskin 1973). Further differentiation among the measures depends on the recording technique or circuitry employed and the type of transformations applied to these measures (Prokasy and Raskin 1972, 1973). The significance of these measures and the appropriateness of the transformations have given rise to the lively debates that have marked the progress of electrodermal measurement. Although these issues are implicated in the results reported here, for the most part, their consideration is beyond the scope of this chapter.

Significant differences between introverts and extraverts have been observed with both tonic and phasic measures, with differences in phasic response measures more frequently noted. With regard to response measurement, there is no apparent basis on which studies which endorse Eysenck's hypothesis can be differentiated from those which do not. It may be interesting to note, however, that the significant differences between introverts observed by Smith and Wigglesworth (1978) failed to emerge when a range correction transformation was applied to skin conductance response scores. With the range correction procedure, each score is expressed as a ratio of the maximum response of which the subject is capable. It has been argued that this transformation, which attenuates the dependence of phasic responses on tonic levels, reduces residual error variance and sharpens the contrast between treatment conditions (Lykken 1972). As Montagu and Coles (1966) have stated, "if an experiment has been designed to compare the responsiveness to a standard stimulus of two groups differing in arousal level, the use of a unit of measurement that has been selected because it is independent of background level may defeat the object of the investigation (p. 264)." The appropriateness of range correction procedures for studies of individual differences in psychophysiological response remains an issue which has not been satisfactorily resolved and consequently the range correction procedure should be applied warily rather than ritually.

Among the studies which differentiate between introverts and extraverts with electrodermal measures, Fowles et al. (1977) have demonstrated high skin conductance levels for introverts in a series of four independent experiments. Desjardin (1976) also reports higher skin conductance levels for introverts, while Nielsen and Petersen (1976), Stelmack et al. (1979) and Gange et al. (1979) report no differences. Mangan and O'Gorman (1969), Crider and Lunn (1971), Smith and Wigglesworth (1978), Stelmack et al. (1979) and Gange et al. (1979) state that introverts showed greater numbers of phasic responses to repetitive stimulation. Since each of these authors employed recording techniques or transformations which were different in some way from those employed by the others, the strength of concurrence for the effect is somewhat constrained. Crider and Lunn (1971), Nielsen and Petersen (1976), Desjardins (1976), and Gange et al. (1979) observed that introverts displayed greater numbers of spontaneous electromal fluctuations than extraverts. In this case, Crider and Lunn recorded skin potentials, Desjardin employed a constant voltage circuit, Nielsen and Petersen (1976) and Gange et al. (1979) employed constant current circuits and transformed their scores to conductance units; because of these differences the consensus is once again constrained. Introverts have also obtained greater OR amplitudes than extraverts. For Wigglesworth and Smith (1976) the amplitude differences were observed to the initial stimulus and to the standard training stimulus which followed a novel stimulus at the end of a training series (1976; 1978); Stelmack et al. (1979) also observed greater amplitudes to the initial stimulus for introverts, while Nielsen and Petersen indicated greater average amplitude across the series of training stimuli. Mangan and O'Gorman (1969) observed greater initial amplitude for the extraverts.

While the robustness of electrodermal measurement can be invoked to account for the emergence of the effect despite idiosyncracies and minor disparities in the recording techniques, the inconsistency between different authors, and in some cases with the same authors, using the same techniques and transformations is nevertheless perplexing. Recent attempts to standardize electrodermal measurement (Lykken and Venables 1971), the widespread endorsement of constant voltage circuitry and the expression of response measures in conductance units should ultimately help to resolve the discrepancies. Some convergence of these results, however, may be seen in the light of the frequent finding that the number of spontaneous responses is related to trials to criterion habituation rate and sometimes to higher tonic levels (cf. Bull and Gale 1973; Siddle and Heron 1976; Smith and Wigglesworth 1978).

2.5.4 Conclusions

Differences in electrodermal activity between introverts and extraverts have been demonstrated with both simple auditory stimuli of moderate intensity and visual stimulation and usually under non-stress conditions where more than passive participation is required. Electrodermal activity is typically greater for introverts than extraverts. Differences in phasic response, in particular, with introverts showing more persistent electrodermal responses to repetitive stimulation, has been the effect most frequently observed and concurs with O'Gorman's (1977) conclusion that extraversion is related to electrodermal habituation. The cautious optimism which O'Gorman (1977) expressed regarding the relation of extraversion and electrodermal habituation is supported by the additional work which has appeared since the publication of his review. There is also some evidence that introverts demonstrate higher skin conductance levels and greater frequency of nonspecific responses than extraverts. These observations imply differences in basic arousal processes and suggest that the effect is not exclusively stimulus bound.

2.6 Extraversion and Pupillary Response

Pupillometrics is a relatively recent development in psychophysiological measurement that promises to be an effective technique for study-

ing psychosensory functions (Hess 1972). The iris muscle that circumscribes the pupillary aperture is reciprocally innervated by the autonomic nervous system, and the effects of this system are particularly dominant in the pupillary light reflex. With the onset of a light stimulus, activity of primarily parasympathetic origin can be inferred from an initial rapid constriction phase that is then moderated by increasing sympathetic opposition, while the rapid redilation at the offset of the stimulus signals parasympathetic relaxation followed by slower redilation that is due to peripheral sympathetic activity (Lowenstein and Loewenfeld 1969). Under conditions that do not elicit the pupillary light reflex, such as tonic pupil size prior to stimulation, inferences of general level of autonomic arousal can be made; and changes in other sensory modalities, such as auditory stimulation, yield sympathetic effects (pupillary dilation) which can also be understood in terms of an OR model. The studies which follow have approached the question of individual differences in psychophysiological response between extraversion groups using the pupillometric methods described above.

Holmes (1967) employed a white adapting light to achieve maximum constriction and then measured the extent of dilation from photographs taken at intervals of 5, 10 and 15 s after the offset of the light. Subjects were then classified into extreme groups of eight fast and eight slow dilators (one SD from the mean) on the basis of their mean proportion of dilation observed at those intervals relative to maximum dilation. It was then noted that the fast dilators had significantly higher extraversion scores on the *Maudsley Personality Inventory*. By a similar procedure, subjects were classified into extreme groups of eight fast and eight slow constrictors from photographs taken at 1-, 2- and 3-sec intervals following the onset of a light stimulus. The fast constrictors were noted to have lower extraversion scores than the slow constrictors, results which were interpreted to be indicative of relatively greater amounts of acetylcholine at cholinergic synapses for introverts and which were linked to the introverts' greater awareness of their environment and more rapid conditioning.

In a recent paper, Frith (1977) investigated the effects of auditory stimulation on pupil size and the pupillary light reflex for 33 male subjects who were administered by *Eysenck Personality Questionnaire* (Eysenck and Eysenck 1975). The pupil was photographed on 16 mm film at three frames per second during two sequences of conditions consisting of no stimulation, a green light flash, a 100-ms, 95-dB tone, a 6-s, 95-dB tone and a 6-s, dB tone with the light flash. High scores on the impulsivity scale (a subfactor of the extraversion dimension) were significantly correlated with smaller pupil size during the no-stimulation condition and with less extensive constriction during the two conditions which elicited the pupillary light reflex, suggesting that the more impulsive subjects were less aroused and less reactive than less impulsive subjects.

Effects which were independent of the pupillary light reflex and which perhaps were more sensitive to cognitive influences have been reported by Stelmack and Mandelzys (1975). The pupillary response to auditorily presented neutral, affective and taboo words was recorded to 33 male subjects classified with the EPI into three groups of high, middle and low extraversion and equivalent in moderate neuroticism. A television monitoring system was employed that provided a continuous graphic record of pupillary activity. Introverts showed a significantly greater increase in pupil size (dilation) from pre-stimulus levels than the extravert and middle groups, particularly in response to the taboo words. This effect can be described as an OR. Pre-stimulus pupil size was also greater for the introvert group, indicating that they maintained a higher level of arousal throughout the entire experiment (see Fig. 2.5). The design of the experiment did not permit one to determine whether the differences in pre-stimulus levels between groups were due to differences in initial level of arousal or to stressful effects generalized throughout the experiment. It is clear, however, that the introverts were more reactive.

Owing to the marked differences in recording, scoring and stimulus conditions, coupled with the absence of replications, the few inferences which can be drawn from these pupillometric

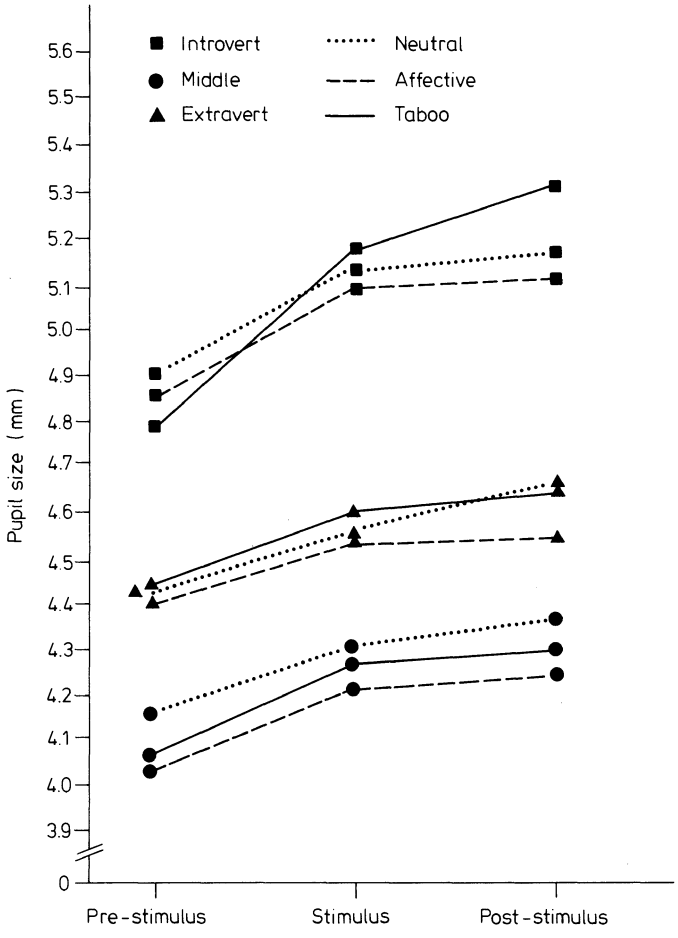

Fig. 2.5. Mean pupil size of introvert, middle and extravert subjects to neutral, affective and taboo words (After Stelmack and Mandelzys 1975)

studies must be entertained with some caution. Nevertheless, one's attention may be directed to some consistency in the observations which are reported. The introvert's larger tonic pupil size prior to stimulation (Frith 1977; Stelmack and Mandelzys 1975) would support the association of introversion with higher levels of cortical arousal. The less intensive pupillary constriction during the pupillary light reflex for extraverts (Holmes 1967; Frith 1977) cannot be interpreted unequivocally. Although introverts can be seen as more reactive, the pupillary light reflex can be inhibited under stress or higher arousal levels (Plouffe and Stelmack (1979), since the dependence of the pupillary light reflex on pre-stimulus levels was not assessed, the ambiguity cannot be resolved.

2.7 The Interaction of Extraversion and Neuroticism

An important issue relevant to the ontogenesis of neuroticism that has been considered in several of the reports reviewed concerns the question whether the psychometric independence of extraversion and neuroticism which has been established is paralled by similarly distinct physiological processes, as Eysenck (1967) proposed. This issue, which is attended by a good deal of ambiguity and confusion, emerges from the finding that some psychophysiological measures correlate with both neuroticism and extraversion. Claridge and his associates (Claridge et al. 1963), after considering individual differences

in sedation threshold, spiral after-effects and blood pressure, were among the first to question this notion of independent neurological dimensions. For EEG indices of cortical arousal, neuroticism has been related to both higher levels of arousal (Winter et al. 1972) and lower levels of arousal (Coles et al. 1971). With those exceptions, neuroticism has not been related to EEG indices of cortical arousal in cases where the psychometric independence of extraversion and neuroticism has been established. A number of authors, however, have reported significant, and usually positive, relationships between neuroticism and electrodermal activity (Mangan and O'Gorman 1969; Siddle 1971; Coles et al. 1971; Fried et al. 1967; Sadler et al. 1971; Nielsen and Petersen 1976; Mangan 1974). On the other hand, no differences in electrodermal activity between groups differing in neuroticism were reported by Kelly and Martin (1969), Koriat et al. (1973), Wigglesworth and Smith (1976), Desjardins (1976), Fowles et al. (1977), Stelmack et al. (1979) and Smith and Wigglesworth (1978).

The psychometric independence of extraversion and neuroticism in studies reporting a significant relationship between neuroticism and electrodermal response has been less certain than in the case with studies reporting a significant relationship between introversion and electrodermal response. Sadler et al. (1971) classified their 40 subjects into four groups "by splitting the distributions of extraversion and neuroticism scores at the means of this sample (p. 34)", after the psychophysiological responses had been recorded. It would be very fortuitous if that method of classification resulted in four groups of equal number without some misclassification. With Fried et al. (1967), extraversion was not considered in the classification of their subjects, and the extent of the interaction of the two dimensions cannot be identified. While Coles et al. (1971) report a negative correlation of -0.09 between extraversion and neuroticism for their population of 131 subjects to whom the test was administered, the extent of correlation between extraversion and neuroticism for the 60 subjects from whom the electrodermal responses were recorded is not – an omission determined by the exclusion of subjects scoring in the middle range on the neuroticism dimension (and which also inflates the difference). For Siddle (1971), extraversion and neuroticism are negligibly correlated, but score distributions are not described. With Mangan (1974), extraversion and neuroticism are correlated ($r-0.26$) and score distributions are not described.

Notwithstanding the reservations regarding the psychometric independence of extraversion and neuroticism, analysis of the data of studies reporting significant relationships between neuroticism and electrodermal activity reveal a good deal of confusion. Initial OR amplitude was negatively related to neuroticism for Mangan and O'Gorman (1969) and Mangan (1974) and positively related to neuroticism for Siddle (1971). Longer habituation rates were obtained by high neuroticism subjects in the study by Coles et al. (1971) and Nielsen and Petersen (1971); on the other hand, longer habituation rates were obtained by low neuroticism subjects in the study by Fried et al. (1967). Sadler et al. (1971) report only a lower number of responses for high neuroticism subjects, while on the contrary, Nielsen and Petersen report a greater number of spontaneous responses for high neuroticism subjects. On the strength of technical merit, the least equivocal effect would seem to be the less habituation of high neuroticism scorers noted by Coles et al. (1971) and Nielsen and Petersen (1976).

It should also be considered that in several ways the studies cited here are tangential to Eysenck's proposal relating neuroticism and differences in autonomic activation. Eysenck (1967) takes the view that neuroticism is characterized by individual differences in emotional responsiveness, excitability and agitation. The autonomic activation concomitant with the emotional expressions of fear, anger and distress, which characterize neurotic states, contrasts with the relatively low levels of autonomic activity which may be implicated in differences in sensitivity, attention and specific cases of conditioning between introverts and extraverts. It can be argued that it is only under stressful conditions, where strong emotions are elicited or high levels of arousal are induced, that dominant differences in autonomic activation would

be implicated. With the exception of Kelly and Martin (1969) and a specific condition with Nielsen and Petersen (1976), the studies cited were conducted under conditions which could be described as inducing low or moderate levels of arousal and consequently do not provide a direct test of the hypothesis.

2.7.1 Neuroticism and Stress

Psychophysiological research bearing on the hypothesized differences in cortical arousal between extraverts and introverts has been facilitated by the research interest in the OR and habituation where psychophysiological techniques have been employed in the attempt to elaborate basic perceptual and learning processes. The demonstration of differences in autonomic activation along the neuroticism dimension has proven to be more difficult and, in fact, few attempts have been made. Because of the significance of establishing the determinants of introverted (phobias, anxiety reaction) and extraverted (psychopathy, hysteria) neurosis, it is worthwile to consider research issues on this problem which can contribute to the psychophysiology of neuroticism. Specifically, attention will be drawn to the distinction of the neuroticism trait as it is manifested in normal and patient populations and to the consideration of neuroticism in the context of emotional response patterning.

2.7.2 Normal and Patient Populations

Eysenck (1967) has suggested that behavioural differences between high and low neuroticism subjects may be interpreted 'in terms of differential thresholds for hypothalamic activity (p. 237)' and in particular to differences in responsivity of the sympathetic nervous system 'with high neuroticism scores associated with greater responsivity'. This suggestion can be considered by exposing subjects from normal populations to stressful stimuli or stressful conditions of varying intensity. A second suggestion with different implications is derived from the discussion of differences between corticoreticular arousal and automonic activation (Eysenck 1967, p 235), where it is stated that for individuals who have frequently experienced strong emotions for long periods of time, the distinction between activation and corticoreticular arousal may not apply; for these individuals "quite mild stimuli are emotionally activating (p 233)". This suggestion can be considered by comparing normal control subjects with subjects from patient populations under moderate or low levels of stimulation.

This distinction may be considered with the study of Kelly and Martin (1969), who reported significant differences between patient and control groups differing in degree of neuroticism for tonic levels of heart rate, blood pressure and blood flow during a non-stressful control period, a result consistent with expectations of high sympathetic activity for high neuroticism subjects who have experienced chronic or reactive anxiety states. No differences in these measures were evident during a stressful mental arithmetic task, a result interpreted as a failure to support the hypothesis that neurotic patients demonstrate over-reactivity of the autonomic nervous system in response to stressful stimuli. The failure to differentiate between groups during the stressful task would seem to be ceiling effect. A review of similar studies employing anxiety-neurotic patients (Lader 1969) also suggested that patient groups are generally autonomically less reactive than controls and draws attention to the possible limiting of responsiveness due to initially raised pre-stimulus levels in patient groups. On balance, however, the attempts to differentiate anxiety patients from control populations with electrodermal measures has not been impressive (Stern and Janes 1973).

Among the studies employing stressful conditions with non-psychiatric subjects, Nielsen and Petersen (1976) observed significant positive correlations between neuroticism and habituation to a 105-dB unconditioned stimulus in a classical conditioning paradigm and between neuroticism and the number of spontaneous fluctuations throughout a series of manipulations which included a 105-dB habituation series. In a recent study, Plouffe and Stelmack (1979) recorded the pupillary light reflex for low, moderate and high neuroticism subjects under control,

stress (induced by the cold pressor test) and fatigue conditions. The only effect observed was a negative correlation between neuroticism and extent of redilation to the offset of a light stimulus during the post-stress condition, an effect with reflected greater sympathetic fatigue of the pupillary light reflex. Katkin (1975), in reviewing his own work on individual differences in electrodermal response and manifest anxiety in normal subjects, has observed that under high stress conditions (threat of shock) no differences between groups emerge, but that under moderate levels of stress (mild ego involving threat) subjects with higher trait anxiety showed greater increase in number of electrodermal responses than subjects with lower trait anxiety scores. These reviews suggest that successful differentiation between high and low neuroticism subjects may not only depend on selecting stressors which achieve maximum between subject variability of response without ceiling effects but also distinguishing the neuroticism trait as it is manifested in normal or patient populations.

2.7.3 Neuroticism and Emotional Response Patterning

The emotional stability–instability described by the neuroticism dimension has been linked solely to autonomic activity. The perplexing complexity of emotional behaviour has been oversimplified, and the considerable difficulties involved in identifying fundamental patterns of emotional expression and their psychophysiological concomitants have been avoided. The behaviours encompassed within the extraversion and neuroticism classification, such as dysthymia and psychopathy, may be explicated by consideration of specific emotional response patterns or systems rather than emotional behaviour in general (cf. Izard 1972).

This direction has been advanced by Gray (1973), who has proposed a modification of Eysenck's (1967) position. The strength of Gray's proposal rests in the development of a model of emotions which was derived from an analysis of learning theory and physiological psychology. An attempt was then made to explain differences in extraversion and neuroticism from the model. Gray identifies introversion with behavioural inhibition or fear in response to signals of either punishment or frustrative non-reward which are mediated by a system linking the orbital frontal cortex, the hippocampus, the medial septal area and the ascending reticular activating system. Extraversion is identified with approach behaviour in response to signals of reward which are mediated by a system linking the septal area, medial forebrain bundle and medial hypothalamus. Neuroticism is depicted as a dimension of increasing sensitivity to both reward and punishment. In this view, introversion and extraversion are served by functionally distinct emotion systems, while neuroticism is determined by both (see Fig. 2.6). Gray's modification addresses two fundamental features of Eysenck's (1967) proposal. Eysenck's hypothesis that introverts are more easily conditioned than extraverts is replaced by the hypothesis that introverts are more susceptible than extraverts to threats of punishment and signals of frustrative non-reward. Secondly, the hypothesis that individual differences in neuroticism are related to individual differences in the limbic system is articulated by specifying two functionally distinct limbic subsystems on which differences in degree of neuroticism may depend.

Gray's proposal outlines a cogent neurological substrate for emotional behaviour that provides a useful context for exploring individual differences in extraversion and neuroticism. By extension from animal experiments surveyed in the development of his model of emotional behaviour, learning paradigms are specified that can be appropriate for testing the hypotheses outlined. There are additional implications for the psychophysiology of extraversion and neuroticism which can be considered. The salient stimuli for differentiating introverts and extraverts are those which have aversive characteristics. In the studies cited in this review, Stelmack et al. (1979) observed that introverts showed less electrodermal and vasomotor habituation than extraverts to taboo words. In a more convincing demonstration, Stelmack and Mandelzys (1975) reported that introverts gave greater pupillary dilation to auditory taboo words than extraverts. Both these studies support Gray's hypothesis of greater susceptibility

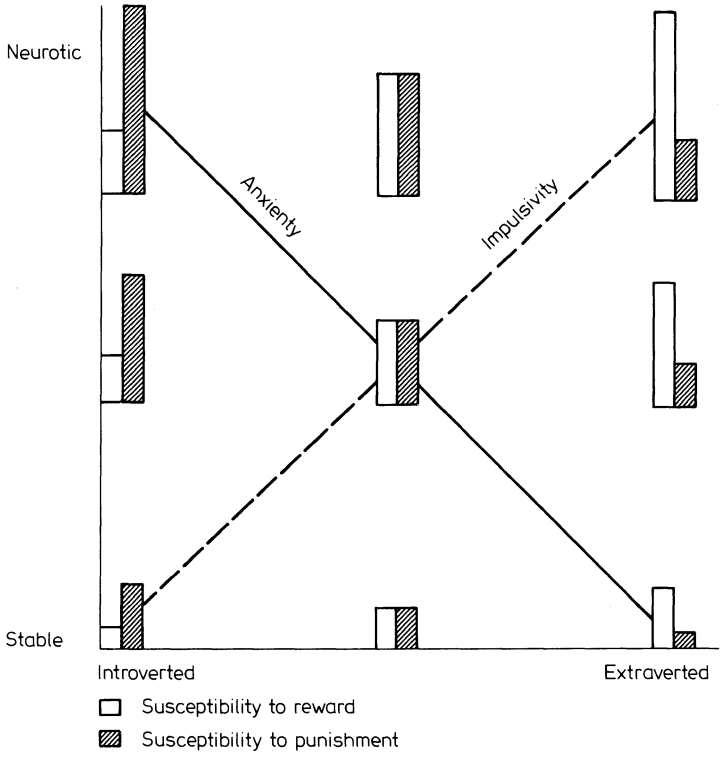

Fig. 2.6. Proposed relationships of (a) susceptibility to signal of reward and susceptibility to signal of punishment to (b) the dimensions of introversion–extraversion and neuroticism. The dimensions of anxiety and impulsivity (diagonals) represent the steepest rate of increase in susceptibility to signals of punishment and reward respectively. (After Gray 1973)

to aversive stimulation for introverts. Neuroticism was not implicated, because the scores were restricted to the middle range. Mangan (1974) noted a significant negative correlation between neuroticism and the electrodermal amplitude to erotic nude pictures, which were employed as the unconditioned stimulus in a classical appetitive conditioning paradigm. Correlations with extraversion were positive but did not reach the 5% confidence level. The results are consistent with expectations from Gray's model in as much as subjects low on neuroticism and high on extraversion showed greater responsivity to the appetitive stimulus. In a novel preparation, Mangan's (1974) data indicated that low neuroticism and high extraversion scores were also associated with greater initial electrodermal amplitude to a tactile stimulus, a puff of warm air, applied near the subject's navel. If, as it may be presumed, this delightful procedure is considered an appetitive stimulus, the effect is consistent with Gray's hypothesis.

The shift from Eysenck's (1967) emphasis on autonomic nervous system activity and hypothalamic regulation to the limbic structures, which Gray (1973) proposes as the biological basis of extraversion and neuroticism, places a different perspective on the autonomic response measures that have been employed to test Eysenck's hypothesis relating neuroticism and emotional activation. Basically, the physiological sites that effectively control autonomic response measures are less immediately relevant to the physiological basis of extraversion and neuroticism. This view, coupled with the innocuous levels of stimulation typically employed, may account to some extent for the relatively small amount of variation in extraversion accounted for by autonomic response measures.

The demonstrations of greater responsiveness for introverts with electrodermal and electrocortical measures are not easily reconciled with Gray's hypothesis relating introversion and susceptibility to punishment. With few exceptions, explicit aversive stimulus values are not apparent in those cases, but it can be argued that the conditions of 'moderate intensity' under which differences between extraverts and intro-

verts emerge is so vaguely stated that an aversive component may be encompassed. One would be hard pressed to describe as aversive the stimulation employed in demonstrations of differences in absolute sensitivity (Smith 1968; Siddle et al. 1969; Stelmack and Campbell 1974) and vigilance (Harkins and Geen 1975; Gange et al. 1979). It may be implied from Gray's view, however, that introverts are more sensitive to the implicit 'signals of punishment' which may be imposed with the task demands and social context of the experiment. The issue is whether a hypothesis of sensitivity to signals of punishment can account for the range of behaviours on which introverts and extraverts differ or whether a more general hypothesis of sensitivity (excitation) must be maintained. That this issue is more apparent than real is suggested by the fact that differences in ARAS activity which presumably account for the introverts enhanced psychophysiological response to moderate levels of stimulation is accommodated in Gray's proposed physiological determinants of extraversion.

There is some merit in applying the distinctions in emotional response pattern which Gray proposes to the elaboration of the physiological basis of neuroticism within the extraversion and neuroticism framework at the present time. While fear and anxiety can be seen to predominate in the emotional response repertoire of introverts high in neuroticism, anger can be conceived as predominant in the emotional response repertoire of extraverts high in neuroticism. The identification of introverted neurotics with the traditional psychiatric classification of anxiety, phobias and obsession-compulsion has been considered (Eysenck 1967), and such psychophysiological data as is available (Gray 1972; Mathews 1971) and as cited in this review does not contradict the identification of fear as a predominant emotion of introverted neurotics, though the notion has yet to be put to an adequate test. In this respect, fear may be distinguished with psychophysiological techniques by increased blood flow to the striate muscles, as Kelly and Martin (1969) observed with the forearm blood flow of high anxiety patients, and by cephalic vasoconstriction (Hare 1973) and cardiac acceleration (Klorman et al. 1977) to phobic stimuli. The application of such psychophysiological procedures may facilitate the resolution of this question.

The identification of extraverted neurotics with psychopathy has been controversial (cf. Passingham 1972). The distinction between primary and secondary psychopathy (cf. Hare 1970) can be explored by linking the former to the psychoticism dimension of the *Eysenck Personality Questionnaire* (Eysenck and Eysenck 1975) and by linking secondary psychopathy to high extraversion and high neuroticism scores. Since these scales have only recently been made available, there has been little opportunity to employ them in psychophysiological research applications. Although the psychophysiology of psychopaths in prison populations has often reflected the confusing difficulties in adequately classifying subjects, psychopaths have displayed lower amplitude electrodermal activity than controls under rather severe conditions, such as anticipating painful stimulation (Hare 1973). Under similar conditions, psychopaths have shown greater heart rate acceleration to a CS and greater deceleration immediately prior to stimulation (Hare and Craigen 1974) – a pattern which may reflect physiological concomitants of fear. The development of such speculations which consider neuroticism in terms of differences in emotional response patterning merit consideration for the advancement of the psychophysiology of neuroticism.

2.8 Conclusions

The enhanced responsiveness to stimulation of introverted subjects has been demonstrated with a wide range of electrophysiological techniques, with the most consistent effects observed with electrodermal measures. The effects are typically observed under conditions which can be described as moderately arousing and are consistent with effects that can be ascribed to individual differences in level of corticoreticular activity. On the strength of these observations, proposals relating extraversion to differences in

corticoreticular activity cannot easily be dismissed.

Under the conditions employed in the studies reviewed, correlations between neuroticism and psychophysiological responsiveness have not been reported with sufficient consistency to permit inferences of the physiological determinants. In the designs of those experiments, the psychometric independence of extraversion and neuroticism is often uncertain, obscuring the assessment of the interaction with psychophysiological measures. It is doubtful that the low or moderate stressors applied in most of these studies are sufficiently stressful to provide an adequate test of Eysenck's hypothesis linking neuroticism with emotional activation.

Analysis of the available evidence, as well as work with patient groups, illustrates the difficulty in defining stimulus conditions that are sufficiently stressful to elicit individual differences in activation without obtaining ceiling effects. A clear distinction between the neuroticism trait as it is manifested in normal or patient populations would seem to be in order, since the effects of the neuroticism trait expressed in patient groups may be confounded by the enduring effects of stressful life experiences. Explication of the psychophysiology of neuroticism may be advanced by considering neuroticism in terms of differences in emotional response patterning, with a predisposition of fear and anxiety predominant in the emotional response repertoire of introverted neurotics and a predisposition to anger predominant in the emotional response repertoire of extraverted neurotics.

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Chapter 3

A Survey of the Effects of Brain Lesions upon Personality

Graham E. Powell

3.1 Introduction

There are two reasons, above and beyond the purely academic ones, why clinical neuropsychologists should concern themselves with the relationship between brain and personality. The first is that up to 80% of adults who suffer brain injury are likely to suffer personality deterioration or some psychiatric disability (see Lishman 1968). Therefore personality disorders should be thought of as just as important (defined in terms of frequency) as the more commonly assessed and studied disabilities relating to language, memory, motor, visual and intellectual functions. The second reason why the analysis of brain and personality relationships is important is that whereas deliberate lesions are virtually never made to alter, say, language or memory functions, deliberate lesions in the form of "psycho"-surgery *are* made to alter personality. It is somewhat paradoxical that such intentional lesions are made according to a brain-personality model that is far weaker and less developed than the equivalent models for language and memory and so on. The onus is therefore upon the clinical neuropsychologist, who should be the member of the surgical team best equipped to conceptualize and measure personality, to provide the data on brain-personality models and to elaborate the model in such a way as to encourage more fruitful research.

It can be seen that both of these reasons are practical – they have the well-being of the patient at heart. In the first place the aim is to assess some of the subtle personality changes that accrue from brain assault, so that rehabilitation can be better planned and the patient and his family helped in coping with any problems the personality change may engender. The second implied clinical aim, in the field of psycho-surgery, is to better understand the overall effects of specific operations, so as to improve the pre-operative evaluation of gains and costs and to assist in the refinement and modification of surgical procedures.

In this chapter, some of the evidence relating personality to the brain will be reviewed and it is hoped that certain key questions will be framed that will indicate profitable directions in which brain-personality research might proceed.

3.2 The Brain-Damaged Personality

There is no such entity as 'the' brain-damaged personality, in the same way as there is no such thing as the epileptic personality (Rutter 1977) or the aphasic personality (Lezak 1976).

In reality, very few studies have looked at personality changes attendant upon brain injury, and fewer still have done so with any degree of refinement. The lack of proper measures of personality and reliance upon clinical observation and 'typical' case reports has tended to perpetuate the stereotype of the brain-damaged individual and to obscure nearly all of the details of what must be a very complex relationship between brain and personality. As an example, Roberts (1976) studied the sequelae of closed head injuries in 359 cases who had been amnesic or unconscious for at least a week. Personality was apparently assessed on a single, simple scale but no actual figures are given. Yet Roberts comes up from somewhere with the orthodox stereotyped view that the pattern is one of 'frontal euphoria, disinhibition or

anergia, which was usually associated with intense irritiability [*sic*].'

However, such studies in spite of all their inadequacies manage to make one of the most basic points absolutely clear: brain damage does have an effect upon personality and personal functioning. Russell and Smith (1961), for example, show that in 661 cases of closed head injury 56% suffered from anxiety and depression; Logue et al. (1968) in 79 cases of cerebral aneurysm found that 42% underwent a personality change as observed by their relatives; and Storey (1967) showed that in 261 cases of subarachnoid haemorrhage 55% suffered a psychiatric effect.

It can be argued that personality change is not a direct consequence of the physical injury, that there is no 'hard' link between a particular piece of neural tissue and personality – the alternative view being that the personality change is a psychological reaction to the physical or neurological impairment following injury. There is much to said for this view, for it is obviously reasonable to suggest that physical impairment can have such effects. A nice example is to be found in the work of Pratt and McKenzie (1958). They cite 12 cases in which a disturbance of balance due to vestibular disorders (vertigo) accounted for the presenting symptom of anxiety. As the disorder was treated, so anxiety abated. There are several possible explanations of this. The most appealing psychologically is that we are used to associating feelings of dizziness, unsteadiness and faintness with extreme anxiety, so that when these internal states are experienced they are interpreted as anxiety regardless of the true cause, which in these cases was unknown to them.

The handicap-mediation hypothesis is therefore accepted as one aspect of brain-personality linkage. But it is not the whole story, for it will be shown as we go along that individuals with very similar physical deficits can suffer different personality changes dependent upon the exact site of lesioning – we can control for handicap and still get effects upon personality, and, of course, the situation arises many times where lesions with no apparent physical consequence may have a profound impact upon the individual's personality processes.

To return to concrete findings, there are a few very general trends that relate non-specific damage (i.e. damage considered irrespective of its localization) to personality change. One can formulate a simple mass-action principle to describe these trends, which has been dubbed the damagedness hypothesis (Powell 1979). It is that the more damage there is to the brain, the more damage there is to personality. The direction of change in personality is not specified, for it seems that in whatever direction personality does move (i.e. up *or* down on any particular scale) the change is always likely to be detrimental to the functioning of the individual.

Evidence for a crude mass-action effect comes from various sources, the first being those studies that examine the link between the extent of brain injury and psychiatric breakdown. Here, we have to assume some kind of relationship between personality breakdown and psychiatric breakdown, but this seems a reasonable assumption given that a good personality both protects the individual from psychiatric illness and promotes rapid and permanent recovery from the illness (Mayer-Gross et al. 1969). A fine example of such studies comes from Lishman (1968), who followed up 670 cases of penetration head wounds and categorized each as exhibiting either (a) no psychiatric disability ($n=93$), or (b) mild psychiatric disability ($n=433$) or (c) severe psychiatric disability ($n=144$). Psychiatric disability was defined quite widely by Lishman to include almost any psychological change detrimental to the social and personal functioning of the individual.

The relationship between disability and degree of damage is given in Table 3.1. It can be seen that the more damage there is, the greater the disability. It is a relationship that stands independent of pure intellectual loss which can be seen by the effects of partialling out intellectual loss (which Lishman also assessed). For example, the correlation between depth of penetration and disability is 0.26 ($p<0.01$), which remains high and significant even with intellectual loss statistically partialled out, i.e. $r_{1.2,3}=0.20$, $p<0.01$.

A second source of evidence for the damagedness or mass-action hypothesis comes from research into the epilepsies, where it seems that

Table 3.1. Relation between measures of damage and psychiatric disability (Lishman 1968)

	n	Psychiatric disability		
		Nil	Mild	Severe
Depth of penetration				
< 3 cm	219	25%	65%	10%
> 3 cm	373	9%	66%	25%
To ventricles	78	6%	59%	35%
Total brain tissue destroyed				
Grade 1	52	54%	33%	13%
Grade 2	224	26%	35%	39%
Grade 3	66	10%	17%	73%
Grade 4	3	–	33%	67%
Post-traumatic amnesia				
< 1 h	329	19%	69%	12%
< 7 days	131	13%	63%	24%
> 7 days	210	7%	59%	34%

bilateral lesions have a more disruptive effect on personality than do unilateral lesions. For example, Meier and French (1965) gave the MMPI to 53 psychomotor epileptics to find that the bilateral subgroup had distinctly elevated scores in comparison with the unilateral subgroup. As a second example, Flor-Henry (1969 a, b, 1973) shows that epileptics with a bilateral focus are three times more likely to become psychotic than are epileptics with a unilateral focus. As a last brief example, Rutter (1977; Rutter et al. 1970) shows that neurotic and antisocial disturbances in brain-lesioned children are significantly greater with bilateral than unilateral lesions.

The analysis of cognitive changes in tumor cases provides the third source of evidence for the mass-action idea. Hécaen (1964) in a study of 439 tumour cases, shows that the presence of raised intracranial pressure (which indicates more widespread cortical dysfunction) is a significant predictor of confusional and deteriorated states and, to a somewhat lesser extent, of disturbances to mood and character. For example, if papilloedema is taken as the index of cranial hypertension, then 38.4% of mesodiencephalic tumour cases *with* papilloedema have character disorders, whereas only 8.5% of mesodiencephalic cases *without* papilloedema have similar mood and character disturbances. This trend is also found in subtentorial tumour cases and in cortical tumour cases (excluding temporal and frontal cortex cases).

At this point we might consider whether brain damage ever *improves* the patient's personality. In fact, Logue et al. (1968) found 11 cases of a series of 79 aneurysm patients whose relatives saw an improvement in personality, and Storey (1967) described 13 'improved' personality cases (as rated by spouse) of 261 haemorrhage patients. The type of improvement seems to be the same in both instances: less tense, anxious and irritable, less fussy and overmeticulous. These data seem to run contrary to the damagedness hypothesis, but really they do not apply to a discussion of *generalized* brain damage, as there was a distinct pattern to the site of lesioning in these cases. With Storey et al., the improved cases all had an anterior aneurysm, and Storey's cases mainly had haemorrhaging of the anterior communicating artery. In other words, these patients had undergone an involuntary frontal leucotomy which has quite specific effects concerning the reduction of negative feelings, as described above and as will be discussed in full later.

Overall, we can conclude that the presence of brain damage per se may not indicate a particular type of personality change, but does predict a non-specific breakdown of personality liable to be detrimental to the personal and social functioning of the patient. Having made this very general statement, we can now begin to refine the picture by moving from the topic of general damagedness to a consideration of site of lesion.

3.3 Laterality of Lesion and Personality

There is some suggestion in the literature that left lesions have a more severe impact upon the personal functioning of the individual than do right lesions. This is particularly evident in some of the data presented by Flor-Henry (1969a, 1973). He looked at the side of the lesion in 50 psychotic epileptics to find that of

the 28 who had unilateral lesions, 19 were left cases and only 9 were right cases. This ratio was compared with that obtained from 38 matched control (i.e. non-psychotic) epileptics in which the left:right ratio was 13:25. The resultant χ^2 is significant at beyond the 0.01 level.

A trend in the same direction has also been found by Lishman (1968) in his study of penetration wounds. In Table 3.2 it can be seen that left cases seem to be over-represented in the severely psychiatrically disabled category. However, the result falls short of statistical significant ($\chi^2 = 1.82$).

These hints of more severe disruption of personal functioning after left lesions – which might be accounted for by disturbances to the regulatory functions of language – may apply only to adults rather than children, in whom hemispheric specialization is less clear cut. Shaffer et al. (1975) examined 98 children who had received depressed compound fractures and who had suffered visible damage to the cortex. The children were all rated on a scale completed by teachers which describes neurotic, antisocial and age-inappropriate behaviours. They generally scored in an elevated manner on this scale when compared to a normal sample (e.g. Rutter et al. 1970), which is in line with the previous discussion on the generally detrimental effects of brain damage. But when results were analysed according to laterality of lesion (Table 3.3) there were no significant findings.

Turning from the severity of left vs. right sequelae, it may be further noted that left and right lesions can give rise to differing *types* of personality dysfunction. For example, Louks et al. (1976) gave the MMPI to 15 left and 15 right matched cases. They ascertained for each subject the neurotic index (hysteria plus psychasthenia) and the psychotic index (lie scale plus paranoia plus schizophrenia) to yield an overall index of P–N. This index was higher in the left group than the right group (61.5 vs. 38.1, $t = 2.17$, $p < 0.05$), even though the groups did not differ in their intellectual, language, memory or motor problems as measured by the Reitan-Halstead impairment rating.

This indication that left lesions give rise to impairment in thinking or logical processing whereas right lesions influence affective processes finds support elsewhere. Lishman (1968) describes how after left penetration wounds changes are primarily intellectual (intellectual, dysphasic and memory changes), but after right lesions more personality-related problems accrue – such as depression, irritability, facile behaviour, apathy or 'frontal lobe syndrome' effects. Similarly, Flor-Henry (1969a) finds that confusional and schizophrenic states are more common after left lesions whereas manic-depressive disorders are more common after right lesions (see Table 3.4).

These findings cause trouble for those brain-related theories of personality that do not differentiate the brain into two distinct hemispheres. For example, Eysenck's (1957, 1967) theory

Table 3.2. Laterality of lesion and psychiatric disability (Lishman 1968; 1977)

	Psychiatric disability		
	Nil	Mild	Severe
No. with unilateral lesions	71	92	108
% having left lesion	51%	56%	62%
% having right lesion	49%	44%	38%

Table 3.3. Mean scores on teacher's questionnaire[a]

Lobe	Laterality	
	Left	Right
Frontal	8.23	8.22
Temporal	6.37	6.57
Parieto-occipital	10.00	7.50

[a] Laterality, lobe and laterality X lobe effects are NS on two-way analysis of variance.

Table 3.4. Laterality and type of psychosis (Flor-Henry 1969)

	Manic-depressive	Mixed	Confusional	Schizophrenic
Right	44%	18%	11%	9.5%
Left	22%	36%	44.5%	43%
Bilateral	33%	46%	44.5%	47.5%

$\chi^2 = 4.4$; $p < 0.05$

which concerns a loop between the cortex and the ascending reticular arousal system (ARAS) does not distinguish between left and right cortex. Nor does a related theory of Gray (1970, 1972), and nor do the Russian school (e.g. Teplov 1964) who consider general properties of the cortex, such as Strength and Mobility, without considering, say, left–right differences in strength and mobility. This lack of consideration of such a basic property of the brain (i.e. left vs. right) means that all three models mentioned fall short of being true neuropsychological models of personality. These models will be returned to as more evidence is presented.

Having discussed generalized damage and the issue of lateralization, the effects of lesions to more explicitly defined sites can now be reviewed.

3.4 Frontal Lesions and Personality

Several authors have stated fairly persuasively that frontal lesions precipitate an increase in extraversion (Willett 1960; Blakemore 1967; Eysenck 1967; Gray 1970). An excellent review of the evidence as gained from the effects of frontal lobotomies and leucotomies has been presented by Passingham (1970), who casts doubts upon this proposition, as does a further review by Powell (1979). Evidence for and against the frontal–extraversion hypothesis comes from various sources: research with the MMPI, observations on changes in sexuality, studies using an extraversion questionnaire scale and research using standardized performance tests.

Investigations using the MMPI consistently show that frontal lesioning leads to a decrease in the experiencing of negative mood states (i.e. depression, fear and anxiety) as assessed particularly by the depression scale (D) and the psychasthenia scale (Pt). Significant findings in this direction come from Anderson and Harvik (1950), Vidor (1951), and Walsh (1977), all of whom found a reduction in D and Pt in frontals. Clearly, this looks much more like a change in emotionality (and perhaps neuroticism) than in extraversion.

As for the sexuality research, rumour has it that patients become sexually disinhibited after frontal lesions, which is potentially related to the fact that extraverts have more frequent and varied sexual experiences. This supposition has not been confirmed in studies by Post et al. (1968), Miller (1954) or Pippard (1955). The Kinsey sexual interview has even been given to patients before and after frontal topectomy, by Freeman (1973). Of the 64 patients only 10 took more interest in sex after the operation, and precisely 10 took less interest.

Research with the extraversion questionnaire scale is slightly more positive, given some interesting data supplied in a brief report by Smith et al. (1977). Here, 31 cases were assessed prior to the pre-frontal leucotomy and 30 months afterwards. There was markedly significant change in the target variables, for anxiety and depression as measured by the Hamilton and Beck scales reduced, as did the patients' neuroticism scores (at the 0.001 level). As for extraversion, this increased from a mean of 6.5 to 10.3 ($p < 0.005$).

It has been argued elsewhere (Powell 1979) that much of this increase in E scores could be due to the statistical regression to the mean effect, since Smith et al.'s subject formed an extremely introverted group (i.e. mean E of 6.5). But analysis of data kindly provided by S.B.G. Eysenck runs contrary to this explanation. The test-retest data on 260 subjects (213 male) were examined and the information extracted for all subjects who had an initial E score of nine or less. The test and retest means for these subjects ($n = 20$) are given in Table 3.5. It is evident that although there is a clear-cut regression to the

Table 3.5. Test and retest means on extraversion for subjects ($n = 20$) initially scoring 9 or less

	Mean	SD
Test	7.0	2.28
Retest	9.1	4.76
Difference Scores	+2.1	4.36
No. of *Ss* showing increase in E		14
No. of *Ss* showing no change in E		2
No. of *Ss* showing decrease in E		4

mean of 2.1 points, this is only half of the change found in Smith's study. This suggests, then, that there is a 'real' rise in extraversion after frontal leucotomy that needs explanation.

However, two other studies do not find such support for the frontal–extraversion hypothesis. First, Levinson and Meyer (1965) administered the Maudseley Personality Inventory to 179 frontal cases to find a mean E score of 24.34, which is not above the published norm of 24.91. Of these cases, 29 had both pre- and post-tests, and the slight trend for a rise in E (16.3 to 18.2) was not statistically significant. Second, Kelly et al. (1966) gave the MPI to 40 modified leucotomy cases pre- and post-operatively, to find once again that a slight trend towards a rise in E of 15.4 to 17.2 was not significant.

It has been suggested by Eysenck (1979, personal communication) that these last two trends in the direction of the extraversion–frontal hypothesis should be looked upon favourably in spite of their lack of significance, because of the nature of the items on the extraversion scale. The argument is that extravert items concern long-term habits, so that responses to these questions are based upon ingrained self-perceptions not easily shifted by any changes that might have occurred over the previous few months. This would be in contrast, say, to items on the neuroticism scale that often concern mood states – and here we are used to moods going up and down and going through good periods and bad periods. There are two problems with this explanation of the small changes in extraversion found by the Levinson and the Kelly studies. First, the explanation must predict that test-retest reliability on the neuroticism scale is lower than on the extraversion scale. There is no evidence for this, since in the manual to the Eysenck Personality Questionnaire (Eysenck HJ, Eysenck SBG 1975) the one month test-retest reliability for E is 0.89 and for N is 0.86. This difference of 0.03, with a sample size of 257, does not approach significance. Second, Smith et al. (1977) *did* obtain a significant change in extraversion, so why should not the Levinson and Meyer studies?

Passingham (1970) discusses these and other studies in greater detail and reaches the conclusion that frontal operation evidence does not provide support the extraversion hypothesis.

The final area relevant to this hypothesis concerns two studies that look at changes in cognitive performance after frontal operations. Tow (1955) and Petrie (1952) both claim that post-operative changes are in the direction of more extraverted performance. The implication is that the change has been mediated by a change in extraversion, but a more parsimonious explanation is that there has been a simple performance decrement due to loss of cortical processing facilities since the changes are all in a deterioration direction – there are reductions in full scale IQ, verbal IQ, vocabulary, matrices, mazes, word fluency, word similarity, object sorting, persistence, accuracy on letter cancellation and on tracing. There seems no reason to invoke the concept of extraversion, which there might have been if the patient's performance could have been shown to *improve* under those conditions that favour the extravert rather than the introvert.

The aspect of personality that does seem to change after frontal operations and lesions is not, then, extraversion, but emotionality, as expressed by anxiety, fearfulness, depression and neuroticism. This is also the case after accidental frontal lesions (see Logue et al. 1968). More precisely, these terms reflect negative mood states, since research into any changes in the experiencing of positive mood states such as contentment, happiness, love and so on has not been forthcoming. This is a serious obstacle to our full understanding of the nature of the change in the pattern of emotional responding.

At a more theoretical level these data furnish virtually no support for Eysenck's model of the neural basis of extraversion – in which a frontal lesion should disrupt the cortical-ARAS loop whose activity level determines the level of extraversion. Nor for Gray's model – in which the frontal region is the highest level-controlling factor in an ARAS-medial septal area-hippocampal loop whose activity once again determines extraversion (although there is good support for Gray's ideas regarding this negative feedback loop's relation to *neuroticism* (see Gray 1970).

3.5 Cingulate Gyrus Lesions

The structural aim of the cingulectomy or cingulotomy operation is to sever the fibres linking the frontal lobes to the limbic system and to disrupt Papez's circuit, (see Fig. 3.1) in which the cingulate gyrus is taken to be the projection area for emotions, in the same way that the occipital lobes are the projection area for the visual modality (Papez 1937). The therapeutic aim is, as with frontal operations, to reduce the experience of negative emotions – fear, anxiety, guilt, depression and pain.

Results presented by Mitchell-Heggs et al. (1976), and shown in Table 3.6, are typical of the area. They studied 66 cases (23 men and 43 women, mean age of 38 years) mainly of obsessional neurosis (n=27), chronic anxiety (15), depression (9) and schizophrenia (7). A stereotaxic freezing technique was used, with bilateral lesions to the cingulate gyrus and the lower medial quadrant (lesions were also occasionally made to the genu of the corpus callosum).

A satisfactory improvement rate of between 70% and 90% was claimed for the operation, quite remarkable given that the patients had been chronically ill for an average of 11 years. It can be seen from the Table that this improvement rate is part of an overall pattern of positive change occurring mainly within 6 weeks of the operation and apparently non-specific with re-

Table 3.6. Mean psychometric values before and 6 weeks and 16 months after limbic leucotomy (n=57) (Mitchell-Heggs et al. 1976)

	Pre	6 wks	16 months		p Pre vs 16 mths
MPI					
Neuroticism	33.8	24.2	23.8		0.001
Extraversion	14.4	16.7	16.7		NS
Depression					
Beck	25.5	17.2	15.3		0.001
Hamilton	22.6	10.6	12.8		0.001
Anxiety					
Taylor	32.2	24.2	22.3		0.001
Hamilton	24.0	12.6	14.4		0.001
Middlesex Hosp. Questionnaire					
Anxiety	11.6	9.1	8.3		0.001
Phobic	7.5	6.0	5.8		0.001
Obsessional	11.3	9.1	8.6		0.001
Somatic	7.8	5.2	4.5		0.001
Depressive	9.9	7.5	6.9		0.001
Hysteric	5.5	5.1	4.3		0.001
Leyton Obsessional Inventory					
Symptoms	26.7	16.9	15.7	mixed	0.001
Traits	12.1	9.6	9.0	diag-	0.01
Resistance	39.8	21.6	17.1	nosis	0.001
Interference	41.6	21.6	16.0	n=25	0.001
Symptoms	32.6	19.0	18.3	ob-,	0.001
Traits	12.5	9.6	9.0	sessio-	0.05
Resistance	56.0	24.2	17.2	nals	0.001
Interference	57.7	20.9	14.1	n=12	0.001

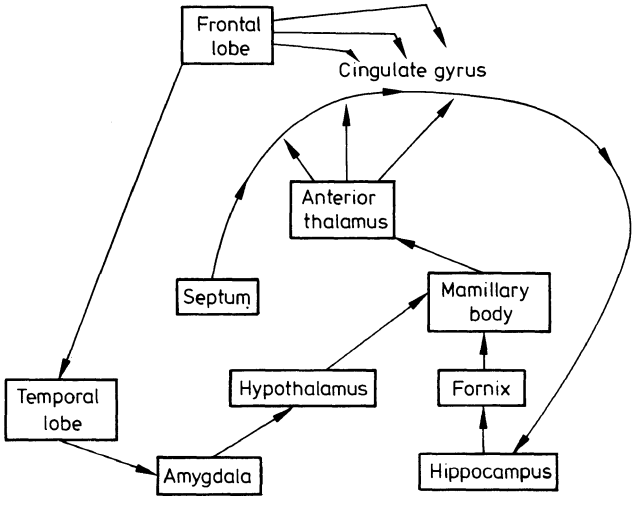

Fig. 3.1. Diagrammatic representation of the Papez's circuit

gard to precise symptomatology (since, for example, non-obsessionals improved just as much on the obsessional symptom questionnaire as did actual obsessionals).

The personality findings of decreased N and no change to E are a replication of an earlier study by Laitinen and Vilkki (1973). Here 18 patients underwent bilateral stereotaxic lesioning to the most rostral part of the cingulum, rather below and in front of the knee of the corpus collosum. This operation, known as an anterior mesoloviotomy, caused a decrease in neuroticism ($p<0.05$), with no significant change in extraversion. This fall in neuroticism is paralleled by changes in certain physiological indices of anxiety, as shown by Kelly et al. (1973), examining 40 cases of cingulectomy. Forearm blood pressure, heart rate and systolic blood pressure all changed significantly for the better, whilst N scores fell from 30.8 to 23.1 ($p<0.001$).

The clinical success rate of the operation has also been found elsewhere. For example, Ballantine et al. (1967, 1972) claimed a satisfactory response rate of 79% in 66 cases of cingulectomy for 'mood disturbance' e.g. depressive, obsessive and anxiety states. Broager and Olesen (1972) find with 63 cases of intractable pain, depression and neuroses a significant improvement rate of 66%. Finally, Mingrino and Schergna (1972) find an immediate (but not sustained) improvement in nine of ten cases of violent, aggressive behaviour.

It might further be mentioned that these beneficial changes apparently accrue without any attendant loss of intellectual functioning. Studies by Laitinen and Vilkki (1972) and Meyer et al. (1973) do not show any changes in IQ as measured by the WAIS.

To return to central theoretical issues, the finding that the cingulectomy does not influence extraversion is something of a blow to Gray's theory (1970, 1972), which is presented diagrammatically in Fig. 3.2. It can be seen that the frontal lobe governs the limbic circuit which controls sensitivity to punishment, this sensitivity being, in Gray's terms, the key component to extraversion. Put simply, cingulate lesions should sever connections between the frontal and limbic circuits (this being the basic rationale for the psycho-surgery operation) and therefore change extraversion. Since cingulate lesions do not have this effect, Gray must rework his model as far as the role of the orbital frontal cortex in extraversion is concerned.

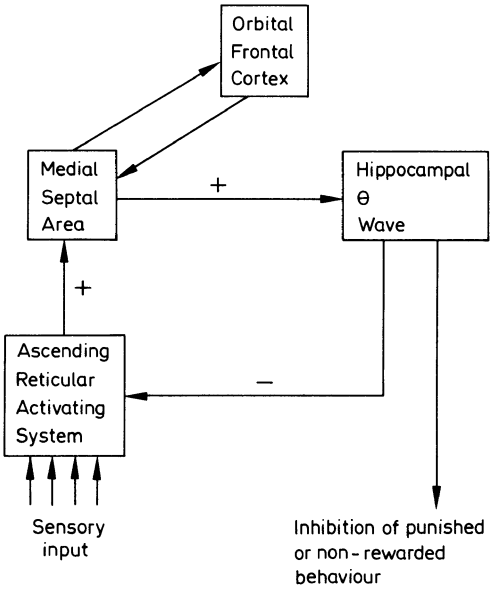

Fig. 3.2. The physiological basis of extraversion: Gray's theory (Gray 1972)

3.6 Amygdala Lesions and Violence

Looking at Fig. 3.1 it is apparent that a further way of disrupting Papez's emotional circuit is to destroy the amygdala. This will go some way towards disconnecting the temporal cortex from the limbic system. It will also tend to disconnect the frontal region from the limbic system, given the major baso-lateral fibres extending from the frontal to the temporal lobe.

Narabayashi et al. (1963; Narabayashi and Shima 1973) noted that after temporal lobectomy operations for epilepsy, there are often highly beneficial changes in temper tantrums and aggressive episodes, especially if the deep structures such as the amygdala were excised.

Table 3.7. The clinical effectiveness of stereotaxic amygdalotomy for violent and aggressive behaviour

Author	n	Clinical response					
		F Dead	E No change	D Transient change	C Slight change: less excited	B Marked change: easier to control	A Excellent: greatly improved, no violent outbursts
Narabayashi et al. (1973)	60	–	2		7	22	29
Vaernet & Madsen (1980)	12	–	1	–	–		11
Mark et al. (1972)	10	–	1	–	–	–	9[a]
Balasubramaniam et al. (1972)	128	9	5	15	45	45	9
Totals	210	9	9	67		125	
Percentages		4.3%	4.3%	31.9%		59.5%	

[a] Immediate response data. There was some relapse over 1 year follow-up.

They therefore undertook a series of operations specifically aimed at this psychological effect – their goal was to 'improve the emotional state of the patient with behaviour disorders' by the stereotaxic bilateral lesioning of the amygdala. The clinical response found in this study and others is given in Table 3.7.

The interpretation of these results is problematic, since outcome is measured on such a simple scale with absolutely no proper psychometric assessment. For instance, different facets of aggression have not been delineated – verbal aggression, physical assaults, self-directed hostility and so on. In particular, tough-mindedness or aggressiveness as a personality trait has not been distinguished from specific, situationally dependent incidents. The question of emotionality changes other than aggressiveness also lies open, since there were no multiple base line measures of anxiety, fear, depression, contentment, happiness and so forth. Added to this, many of the operations were on subnormals, so we may be dealing with a population that already had abnormal or lesioned brains.

Therefore, although there is some evidence that the operation has a placating effect upon behaviour, the precise nature of the psychological change is totally obscured by the crude measurement technique.

3.7 Hypothalamic Lesions, Aggression and Sex

The hypothalamus has also served as a target for stereotaxic psycho-surgery, since in the posteromedial region there is an 'ergotropic triangle' which, when stimulated, causes tachycardia, rising blood pressure and pupillary dilation. Sano et al. (1972) reason that since these effects are noted during aggressive episodes, the destruction of the area could have beneficial effects. Undertaking the hypothalamotomy on 66 cases exhibiting violent and aggressive episodes, they report a 'good' or 'excellent' outcome in all but two patients. Once again, because of poor assessment measures (i.e. a single three-point scale) these results are virtually uninterpretable.

More recently, the hypothalamus has received surgical attention because of its links with sexual performance – since in animals excision of the ventromedial nuclei dampens hypersexuality. Roeder et al. (1972) therefore report the hypothalamotomy as a treatment for sexual perversions. Of the ten patients who volunteered for the operation (four homosexuals, one exhibitionist and five pedophiles) six were claimed to have a complete cure, one a fair

response and three a poor response. There was no psychometric evaluation.

The mechanism of change (if we assume there is something more to the results than a placebo effect) seems to be one of simple, non-specific suppression of overall sex drive – i.e. the action of the operation is not solely upon the deviant tendencies. Dieckman and Hassler (1977) also found a dramatic reduction in sex drive after hypothalamotomy in the treatment of three rapists and one sexually motivated arsonist.

Clearly, because of the poor data collection, we have no real idea of general personality changes that might attend hypothalamotomy.

3.8 Thalamic Lesions

There is little to say here other than that thalamotomies have been performed to reduce hyperkinesia, aggression and abnormal affect. A moderate success rate has been reported and discussed by Andy and Jurko (1972a and b) who get good results in only 6 out of 30 cases.

More important than these clinical findings, though, is a report by Choppy et al. (1973), showing how the collection of even a little proper psychometric data can be informative. Choppy compared left thalamotomy cases ($n=18$) with right thalamotomies ($n=11$). Interestingly they found that the left thalamus is more linked to mood states than is the right one (in contrast to the general finding, discussed earlier, that right hemisphere lesions lead to more affective type changes). After left thalamotomy, depression and introversion scales derived from the MMPI reduced ($p=<0.02$ and <0.05 respectively) and self-criticism, which is linked to neuroticism, also fell ($p<0.01$). Intelligence also reduced after left ($p<0.02$) but not right lesions.

If all other practitioners of stereotaxic surgery were as diligent in data collection as Choppy et al. and the Kelly group, then we would be in a far better position to understand, for example, the structural basis of emotional behaviour.

3.9 Temporal Lobe Lesions and Personality

There are several straightforward reasons why temporal lobe lesions should have a particular link with personality. First, the lobe surrounds the amygdala and other structures that form a limbic circuit to do with aggressive and emotional states – so that temporal cortex can be viewed as the cortical controlling element of these feelings and behaviours. Second, the temporal lobes contain important language and memory mechanisms that must assist in the organisation and regulation of behaviours.

Livingston and Escobar (1972) phrase it thus: "while the deep lying amygdala has received major emphasis as the dominant component of the basolateral limbic circuit in terms of aggressive behaviour, it seems likely that the anterior temporal cortex and its connections play a more sophisticated role – that of modulating emotional and behavioural activity related to sensory perception and its associated ideation and feeling. It seems reasonable to suggest that the anterior temporal cortex and its basolateral limbic connections may be involved in clinical disorders that can be visualised as malfunctions of perceptual and interpretive mechanisms."

The special link with personality dysfunction is reflected in Lishman's (1968) findings. In 63 cases of uni- or bilateral temporal damage due to penetration injury, only 10% had no psychiatric disability, whilst 61% were severely disabled – a degree of association higher than for frontal, parietal or occipital lesions. Also, in Hécaen's (1964) tumour series, 42.4% of temporal cases had confusional or deterioration disorders and 22.2% suffered mood and character changes. Similarly, Falconer (1973) found that in his sample of 100 cases of temporal lobe epilepsy only 13 were psychiatrically normal. In particular, 47 were diagnosed as psychopaths and 27 showed extreme violence and aggressiveness. Finally, it will also be remembered that Flor-Henry (1969) showed the association between temporal lesions and psychosis.

Unfortunately, description of the 'temporal lobe personality' has been largely non-psychometric, relying on case reports and general ob-

servation. Description of the aggression association, and the reduction in aggression after temporal lobectomies which remove the amygdala and hippocampus, can be found in Falconer (1955, 1973) and Hill et al. (1957).

As for a more generalized assessment of the emotional disorders that can follow temporal lesions, an interesting paper by McIntyre et al. (1976) is relevant. They measure impulsivity with the Matching Familiar Figures Test (a trait with features in common with extraversion), and the ability to detect and label common affective states using the Davitz-Mattis Metaphor Test (in which the subject has to label each of 55 verbal descriptions with either Anger, Anxiety, Joy, Love or Sadness). The subjects formed three matched groups of left temporals, right temporals and controls (n: 11, 11 and 12 respectively). It was found that left temporals were more reflective than normals, i.e. took longer to match the familiar figures without making any more errors, whilst right temporals were more impulsive (one-way analysis of variance $F=7.2$, $p<0.01$).

The slowing of left temporal performance could be a reflection of a subtle memory disorder linked to a reduction in the feeling of 'familiarity' – a theory of amnesia gaining in popularity and experimental support (Baddeley 1975). The loss of a feeling of familiarity might be seen as bordering on a change in emotional or intuitive responding. The right temporal change can be seen as an enhancement of this feeling or a tendency to react without thought to an 'emotional' cue. This impulsivity could be one aspect of the affective disorders shown by Lishman's (1968) right temporals, i.e. over-emotionality or over-reactiveness as indicated by irritability.

As well as a deficit on the Familiar Figures Test, the emotional labelling test showed another deficit in the left temporal group – since they made significantly more errors than the other two groups. Further research will show whether this emotional perception deficit is merely a verbal labelling problem associated with some subtle undetected language impairment – or whether it is a 'real' perceptual problem that will cause the patient to misinterpret and react inappropriately to other people's emotions. The finding of a high proportion of psychopaths amongst Falconer's temporal epileptic sample (especially left temporals) tends to support the latter view of something more profound than a linguistic response handicap.

As a final speculation it might be that only left (rather than right) temporal lobectomics will improve aggressiveness by increasing reflectiveness. Indeed, right lobectomies might enhance aggressiveness by increasing impuslivity.

This hypothesis can be tested by re-analysing data presented by Hill et al. (1957), who describe personality changes in 27 temporal lobectomy cases (19 dominant and 8 non-dominant). As part of the assessment procedure, all patients were rated pre- and post-operatively on a three-point scale of aggressiveness: 0 = notoriously frequent outbursts, 1 = occasional tractable outbursts, 2 = no outbursts. Of the 27 cases, 24 exhibited aggression pre-operatively. Changes in this aggressiveness are given in Table 3.8 separately for dominant and non-dominant cases.

Despite the small numbers involved, the results reach significance at the 0.05 level. The dominant operation has more beneficial effect upon aggressiveness than the non-dominant operation. Since this hypothesis was not in the mind of the original authors, it cannot be put down to an experimenter effect or other bias effect. The suggestion can be made, therefore, that dominant lobectomies change aggressiveness by decreasing impulsivity, i.e. aggressive thoughts and ideas are not immediately acted upon. In contrast, non-dominant operations, which can lead to an *increase* in impulsive responding, do not have such a beneficial effect on aggressiveness, although aggression does not actually get worse.

Table 3.8. Changes in aggressiveness after temporal lobectomy

	Non-dominant operation	Dominant operation
No. exhibiting less aggression	3	12
Same aggression	3	3
More aggression	1	2

$\chi^2 = 6.22$, $p<0.05$

Overall, we see reiterated the point that a few appropriate tests have led to the construction of testable hypotheses and have 'filled in' the picture of change. However, we are still a long way from describing the full relationship between the temporal lobes and personality, although we might say first that the left and right lobes potentially have different roles and second the temporal cortex and the deeper amydaloid structures also have different roles, with the cortex regulating emotional integration and responding, whilst the amygdala helps generate the emotional feeling or impetus itself. Hence we must expect left and right, and cortical and subcortical lesions to have differing effects upon emotionality.

3.10 Brain-Stem Arousal Systems and Personality

One of the most fundamental states of the brain is its position on a continuum of readiness to operate, or activation. In an activated state, the organism is alert and neural transmission is facilitated by a lowering of the neurons' threshold for firing. In a non-activated state the individual is drowsy, there is a condition of generalised neural inhibition in which neurons are more difficult to fire.

The brain's position on this continuum is determined by mechanisms within the brain stem that have generally become known as the Ascending Reticular Activating System (ARAS), and by the descending, controlling action of the cortex. Hence an ARAS-cortex loop is formed. According to one brain-based model of personality, "this loop then is concerned with information processing, with cortical arousal and inhibition, and in its application to personality differences with introversion and extraversion" (Eysenck 1967). Eysenck's theory, which is well-known, is that activity in this loop varies from person to person, with introverts being generally more activated than extraverts.

Lesion work in this area is rare, but it can be noted that frontal lesions particularly influence activity in the cortical-reticular loop. Extreme under-arousal following frontal lobectomy is often observed clinically as severe apathy and inertia (Post et al. 1968; Miller 1954; Pippard 1955; Freeman 1973; Tan et al. 1971).

Alternatively, or additionally, lowered cortical arousal can appear as behavioural disinhibition and be observed as over-reactiveness, irritability or outspokenness. Often the two sets of behaviour are seen in the same population, as in the data of Logue et al. (1968) presented in Table 3.9. All cases (n=79) were of anterior cerebral aneurysm. It can be seen that more than half the cases are rated by relatives as evidencing loss of energy, and about a third show behavioural disinhibition. (The tendency for frontal cases to worry less is also replicated.)

Table 3.9. Changes in personality features after anterior cerebral aneurysm (Logue et al. 1968)

	Worry	Irritability	Outspokenness	Physical energy
Increased	9	21	26	4
No change	45	50	49	31
Decreased	25	8	4	44

Certain physiological measures reveal this under arousal. For example, Homskaya (1973) and Miller (1954), as well as others, have observed significantly increased alpha-wave activity in the EEG after frontal lesioning.

However, the fact that frontal lesions reduce arousal but (as discussed previously) do not cause a particularly strong increase in extraverted patterns of behaviour reveals that the concepts of arousal and extraversion are not entirely synonymous. Many factors other than corticoreticular activity add together to produce the typical picture of the extravert as defined, say, by the items he or she endorses on an extraversion questionnaire scale. In other words, extraversion can be defined on several different levels that are only loosely coupled or related.

3.11. Individual Differences in Response to Cortical Stimulants and Depressants

Although lesion work in this area is Spartan, the effects of drugs that change activity in the corticoreticular loop have been fairly extensively studied. The basic argument is that a given dose of a given drug will have differing effects upon the individual, dependent upon his initial position regarding corticoreticular activation. If introverts and extraverts generally respond in some consistently different way, then this can be taken as evidence of an underlying difference in brain function.

A good example of the way a drug can differentially influence the performance of various personality groups is given by Gupta and Kaur (1978), who studied the effects of a stimulant (dextroamphetamine) upon kinaesthetic figural after-effect (KFAE). They found that the drug, which increases activity in the corticoreticular loop, improved the performance of extraverts, since the cortex becomes more efficient at the judgement task when it is slightly more aroused. But the drug adversely influenced introverts' performance as Eysenck predicts – because the introvert is already quite aroused, so that the stimulant pushes the arousal level of the cortex past the point of maximal efficiency as described by the Yerkes-Dodson principle.

The precise dose-response surface is depicted in Fig. 3.3. The three groups of *S*s were E^+ (14+), E (10 to 12) and E^- (8 and less), all matched for neuroticism, and the drug doses were 7.5, 10.0 and 12.5 mg. In the figure the height of the response-surface indicates magnitude of KFAE. In brief, the E^+ group have higher KFAEs than E^- subjects under placebo conditions, indicating greater initial activity in the corticoreticular loop of introverts. At each drug level the KFAEs of E^+ subjects falls ($p < 0.01$ for each of the three doses), indicating improved perceptual judgement, whereas the KFAEs of E^- subjects increases (at the 0.05 level for the 7.5 mg dose and at the 0.01 level for the two larger doses).

This finding that individuals with low corticoreticular activity can have performance improved with moderate doses of stimulants has been found elsewhere. For example, hyperkinetic children are cortically under-aroused (Satterfield 1973, 1978), and so stimulants (e.g. methylphenidate, dextroamphetamine, deanol and caffeine) can be shown to improve their performance on many cognitive tasks involving vigilance (Yepes et al. 1977; Werry and Aman 1975), reaction time (Reichard and Elder 1977), impulsivity (Campbell et al. 1971) and sensory thresholds (McManis et al. 1978). It can be seen that these cognitive changes are all in the introverted direction (i.e. introverts show better vigi-

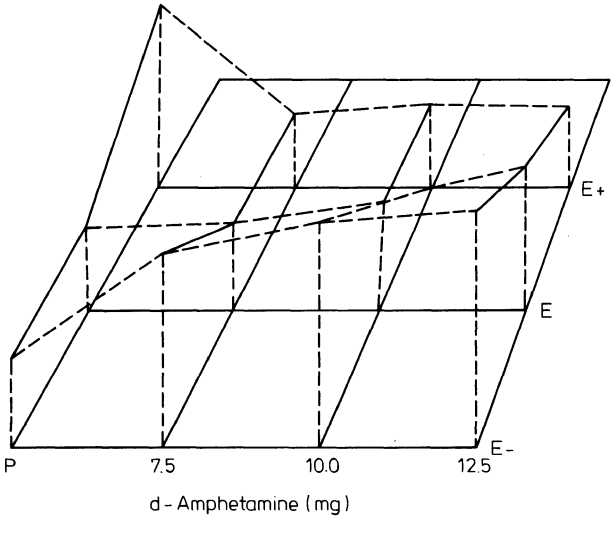

Fig. 3.3. Response surface: personality and drug treatments on kinaesthetic figural after-effects (Gupta and Kaur 1978)

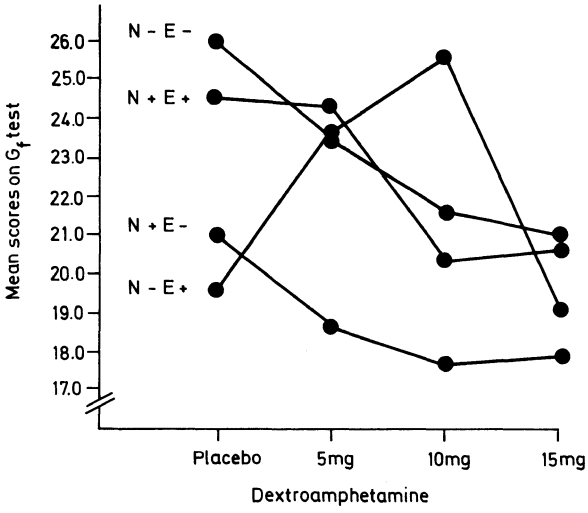

Fig. 3.4. Personality, dextroamphetamine and fluid intelligence (Gf)

lance, less impulsivity, quicker reaction times and more sensitive thresholds), thus lending support to Eysenck's notion that introverts generally exhibit more activation in the cortico-reticular loop.

A third example of the effects of dextroamphetamine comes from Gupta (1977), who studied its effects upon measures of fluid and crystallized intelligence (Gf and Gc). Fluid intelligence concerns the immediate ability to analyse and process information and may be affected by temporary changes in state as produced by drugs. Crystallized intelligence refers to well-learned material and knowledge and thus should be less easily affected by drugs. Gupta predicted on the basis of Eysenck's work that dextroamphetamine would improve the performance of extraverts on Gf by increasing arousal to an optimum level, but would cause a performance decrement in introverts whose arousal level would be taken past the optimum. Gupta used the Culture Fair Intelligence Test to assess Gf and the Group Test of Mental Ability to measure Gc (performance on which should be the same for introverts and extraverts regardless of drug dose).

As for the experiment itself, 320 subjects were divided into four groups: N+ E+, N− E+, N+ E−, and N− E−. Each group was subdivided into a further four groups according to the dosage of drug they were to receive: placebo, 5 mg, 10 mg and 15 mg dextroamphetamine. Hence there was a total of 16 groups, each with 20 subjects. All subjects took the drug and were tested for Gf and Gc 1 h after its administration. The results for the crucial variable, Gf, are given in Fig. 3.4.

Gf was affected by drugs ($p < 0.001$), by personality ($p < 0.001$) and by a personality X drug interaction ($p < 0.001$). It is this significant interaction term which shows that subjects must have started off the experiment in differing states of arousal.

The figure looks somewhat complicated at first glance, but if we consider just the 5-mg condition (in which there is no danger that the arousal level of extraverts as well as introverts is pushed too high) then the picture clears. We can compare the N− E− group with the N− E+ group – i.e. the comparison is between introverts and extraverts with neuroticism held constantly low. It can be seen that the stable introverts suffer a performance decrement with 5 mg dextroamphetamine ($p < 0.01$), but that stable extraverts improve ($p < 0.01$), which is exactly in line with prediction. Similarly we can compare N+ E+ with N+ E− to find that once again the introverts deteriorate ($p < 0.02$), although this time there is no significant change in the extraverts' performance.

Gupta further showed that although Gc was generally adversely influenced by dextroamphet-

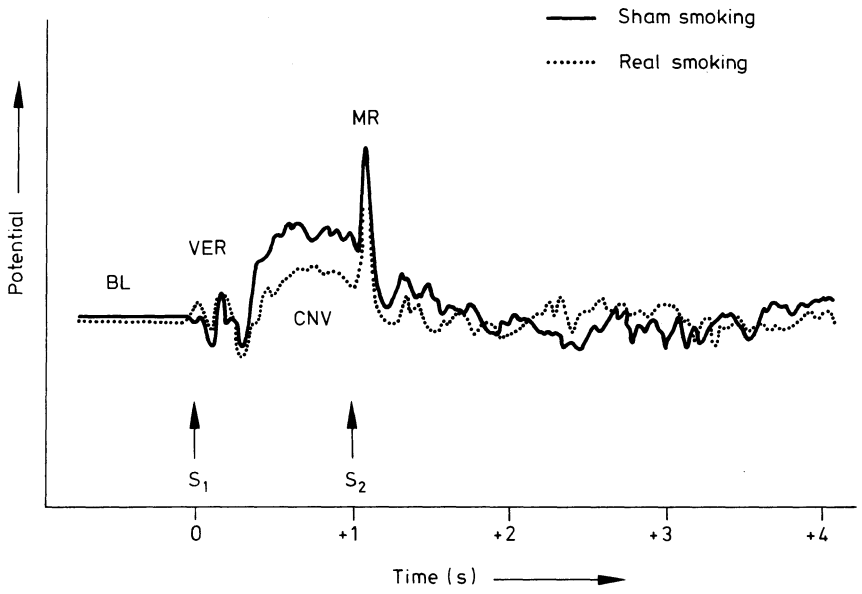

Fig. 3.5. Average CNV for four introverted subjects under the 1-s condition during sham and real smoking. *Abbreviations:* BL, baseline; VER, visual evoked response to warning light; S_1, warning light; CNV, expectancy wave; MR, motor response; S_2, response tone

amine ($p < 0.02$), there was no personality or personality X drug effect, which was concordant with his predictions.

There is reasonable support, then, in Gupta's (1977) study and in Gupta and Kaur's (1978) work for the proposition that introverts and extraverts will react differentially to small doses of dextroamphetamine because of their initially differing levels of cortical excitation.

It is important at this stage to consider whether the results obtained might be specific to dextroamphetamine rather than stimulants in general, although this seems unlikely. There are three recent studies that use stimulants other than dextroamphetamine.

In the first study, O'Conner and Eysenck (1979) look at the effects of smoking upon the Contingent Negative Variation (CNV) or the 'expectancy' wave, as it is often called. The assumption here is that if an already aroused cortex is made more aroused, then it will pass into a state of inhibition, causing the CNV to be depressed in size. Hence the prediction is derived that nicotine will enhance CNVs in extraverts but depress CNVs in introverts.

O'Conner tested four extraverts ($E > 16$) and four introverts ($E < 9$) at 9 a.m. in the usual CNV paradigm, using a 1- and 4-s foreperiod reaction-time task. All subjects were tested twice, once while smoking real cigarettes and once while smoking nicotine-free cigarettes (i.e. 'sham' smoking). The results for real vs sham smoking are given for the introverts in Fig. 3.5 and for the extraverts in Fig. 3.6. It can be seen that, as predicted, introverts show a decrease in CNV amplitude and extraverts an increase.

The second experiment using nicotine is by Warburton and Wesnes (1978). They make the prediction that nicotine will improve vigilance in extraverts but cause a decrement of vigilance in introverts. All 48 subjects were required to engange in the Mackworth Clock Test (in which one is required to indicate when the clock hand stops for 0.1 s) for a continuous period of 80 min. The first 20 min were drug free, and the last 60 min were under the influence of nicotine taken in cigarettes or tablet form. The change in the subjects' performance was correlated with their extraversion scores, with a positive rho being anticipated. In fact, a small negative correlation was obtained ($r = -0.124$, not significant). However, the authors point out that there was an especially narrow range of extraversion scores, with only two being below

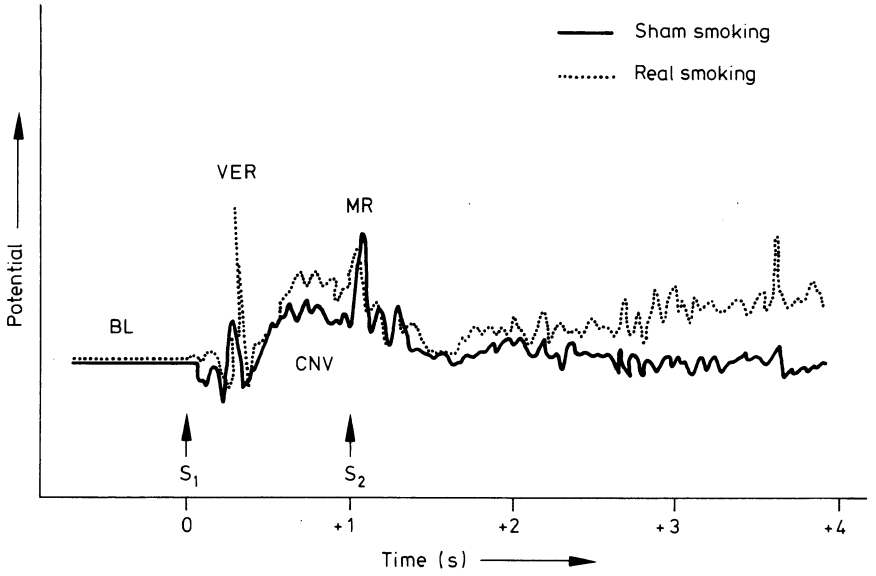

Fig. 3.6. Average CNV for four extraverted subjects under the 1-s condition during sham and real smoking. BL, baseline; VER, visual evoked response to warning light; S_1, warning light; CNV, expectancy wave; MR, motor response; S_2, response tone

12, which decreased the likelihood of obtaining any significant correlations at all. After this rather unsatisfactory study, which highlights the usefulness of the extreme group approach as used by O'Conner, comes the third study, by Janssen et al. (1978), which is rather different from all previous ones in that the stimulant condition is one of additional sensory stimulation (white noise), rather than a drug being used.

Janssen predicted, as did O'Conner and Eysenck, that the stimulant, white noise, would increase CNVs in extraverts but depress CNVs in introverts. Twenty-four subjects were randomly assigned to one of three groups: 10 and 20 mg chlordiazepoxide; 150 and 300 mg caffeine; and finally a placebo or no-drug group. The CNV paradigm was that of an auditory warning tone and a 1.5-s interstimulus interval, and all subjects were tested both with and without white noise. Analysis of variance revealed a significant noise X personality group interaction ($f=4.53$, $p<0.05$) as expected. Examination of mean CNV amplitudes during base measurement (i.e. before any drugs were taken) shows that CNV amplitude as diminished by white noise in introverts, as anticipated, but did not actually rise for extraverts. The precise means are given in Table 3.10.

As for the effects of chlordiazepoxide and caffeine, Janssen et al. unaccountable fail to consider drug effects in relation to personality. They make *general* predictions for the stimulant and depressant, collapsing the introverts and extraverts together, and therefore fail to get any significant differences between the two drugs. Obviously, if one expects caffeine to raise CNV in extraverts but to lower it in introverts, then the net effect of caffeine upon a mixed personality group is likely to be nil.

Overall, according to this brief review of the effects of stimulants, there is moderate but consistent support for Eysenck's arousal postulate.

Table 3.10. Mean CNV amplitude during base measurement with and without white noise (Janssen et al. 1978)

	Without white noise	With white noise	p diff.
Introverts (n=8)	12.05	8.50	<0.05 (t=7.07)
Extraverts (n=16)	11.32	10.78	NS

Research on the effects of depressants provides rather more positive findings, particularly from the recent work of Jones (1974; Jones et al. 1978; Jones and Vega 1972), who studies the effects of alcohol upon performance.

The simplest hypothesis to be derived from Eysenck's work is that alcohol will cause an increase in extraversion. Jones and Vega (1972) attempted to test this by measuring extraversion in 20 subjects after taking a placebo and in 20 subjects after ingesting 1.32 ml of 95% USP ethanol per kilogram body weight. In fact the mean extraversion score of the placebo group was 33.5, whilst the mean score of the alcohol group during the ascending limb of the blood alcohol curve was lower (but not significantly so) at 30.0. This insignificant find is not surprising, given that the two groups were not initially matched for extraversion, and given that the E-scale items are not of the type likely to respond in any immediate fashion to a drug effect, i.e. they concern quite long term habits, as been discussed previously.

This led Jones (1974) to study the effect of alcohol upon performance measures and not upon the E scale itself. The general prediction here is that extraverts have a lower degree of cortical arousal and therefore will get drunk more quickly. A given dose of alcohol should, then, have more effect upon extraverts' than introverts' performances. Forty male subjects were divided into those who would ingest alcohol at 1.32 ml/kg and those who would receive a placebo (n:20). The groups were further subdivided into introverts and extraverts. Prior to the administration of the drug or placebo, all subjects completed the EPI to ascertain extraversion and the Shipley scale to match groups for intelligence. After drug administration, subjects completed Ravens I on the ascending limb of the blood-alcohol curve and Ravens II on the descending limb. It is the score on the Ravens that is the dependent variable.

The results are presented, for the descending limb, in Fig. 3.7, where it can be seen that the effects of alcohol are detrimental for both groups ($p < 0.01$), but that it is worse for extraverts, as predicted ($p < 0.05$).

One problem with this study is that it does not take neuroticism scores into account. This

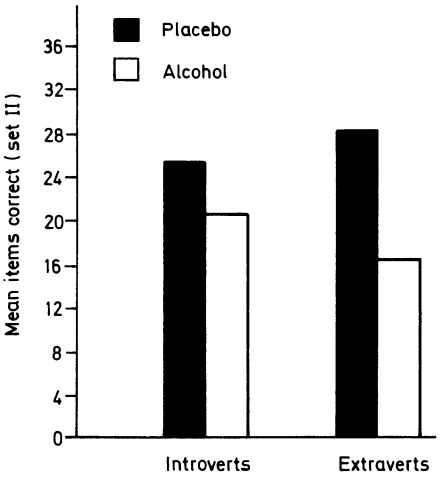

Fig. 3.7. Performance of introverts and extraverts in the alcohol and placebo groups on Set II of the Raven's Progressive Matrices

Table 3.11. Scores on the Raven's Matrices (Set II) after alcohol consumption

	n	Score	F	p diff.
Introverts	51	32.08	14.69	<0.01
Extraverts	48	27.00		
Low N	51	31.16	4.57	<0.05
High N	48	27.98		

was corrected by Jones et al. (1978). Subjects (n=99, male:36) were divided into four groups: N+ E−, N+ E+, N− E−, and N− E+, and further subdivided into males and females, yielding eight groups in all. All subjects completed the Shipley and the EPI before alcohol consumption, and the Ravens Matrices after taking 0.52 gm/kg of 95% USP ethanol. Scores on the Ravens are given in Table 3.11.

It is apparent that stable introverts are liable to perform best under alcohol and neurotic extraverts worst. Indeed, the N− E+ group has the highest mean on the Ravens (34.00), and N+ E− group has the lowest (26.04). It should be noted that the extraversion effect cannot be accounted for by suggesting that extraverts *always* score lower on Ravens whether they have alcohol or not, since Jones (1974) has previously shown that under placebo conditions extraverts actually score slightly *higher* on Ravens (see Fig. 3.7).

This section can be concluded by reiterating that the arousal level of the individual is influenced by damage to the frontal areas and to brain-stem mechanisms; by environmental conditions such as the presence of additional sensory stimulation like white noise; by the ingestion of central stimulants and depressants and by the constitution of the individual as reflected by personality measures, particularly extraversion.

Eysenck's hypothesis that extraverts are in general less cortically aroused than introverts has consistently received support and suggests, amongst other things, that the anergic effect of frontal and brain-stem lesions may be modulated in part by a pre-morbid level of extraversion – i.e. frontal cases who were introverts may be those who do not exhibit the frontal picture of inertia, apathy and verbal disinhibition. For the moment, this remains a hypothesis for further research.

3.12 Personality Processes

The discussion has conveniently led to the problem of definition. Until now a dimensional or trait approach to personality has implicitly been assumed, as described, say, by the factor analytic models of Eysenck or Cattell. Sticking too closely to this approach could prove a fatal handicap if we are to understand the neuropsychological basis of personality.

Consider research into the neuropsychology of memory. Progress here has centred upon the breaking up of the total function into more discrete processes – coding, modality specificity, time-tagging, serial and parallel processing, cue strategies, recall strategies, short and long-term storage strategies and so on. The description of each process has led to the development of a wide variety of experimental tasks and methods. Similarly, research into language dysfunction has benefitted greatly from the processes delineated by modern psycho-linguistics (Goodglass and Blumstein 1973) – distinctions between fluent and dysfluent disorders, aspects of phonemic and morphemic structure, the analysis of depth structure and the analysis of the interface with memory are examples of processes that can be utilized to better understand the structure of an aphasic dysfunction.

In contrast, the concepts of dysthymia, or extraversion, or psychasthenia, and so on, tend to come as 'lumps'.

However, recent work in the field of personality has broken away from this trait tradition and is beginning to define personality as a set of related cognitive processes linked to the perception and interpretation of the environment or of 'situations' (Mischel 1973, 1977; Hogan et al. 1977; Endler and Magnusson 1976). This newer approach is more amenable to an information-processing account of personality and is hence more likely to be mapped onto brain processing.

There are four basic processes alluded to in situational models of personality: (1) an active searching for, and filtering of, certain stimulus features or elements, (2) the evaluation of stimulus elements as they are liable to influence the person, (3) the evaluation of several alternative plans of action to cope with the needs of the situation and (4) the organisation and implementation of the selected plan for action. It can be seen that there is still considerable scope for individual consistencies (or traits) to develop – in the stimuli that are attended to, for example, or in the number of alternative plans that are considered before just one is chosen. Also, there are many ways in which persons may differ – here, Miller et al. (1960) suggest variation in the time span, detail, flexibility and speed of plan construction. To this we might add, for example, the number of stimulus elements attended to, or their type, or the kind of probabilistic logic used to predict situation or plan outcome. The list of areas for investigation seems almost endless.

As an example of how this approach yields new ideas, consider the possible neuropsychological mechanisms that may influence cue selection or filtering. The following hypotheses spring to mind (see Powell 1979): First, individuals with more weakly lateralized brains (e.g. males in general, Buffery 1976) that are arranged more suitably for parallel, spatial processing, will pay more attention to non-verbal

aspects of situations. Conversely, strongly lateralized brains will concentrate upon verbal cues in the situation.

Second, we can predict that the state of arousal in the corticoreticular loop will influence the number of situational cues that are attended to by the individual. M.W. Eysenck (1979) discusses how arousal reduces the range of cue utilization or concentrates attention, and shows how introverts (assumed to be more aroused) encode material less elaborately and encode fewer stimulus attributes. This type of work leads one to expect that the extravert's behaviour will be responsive to, or dependent upon, more aspects of the situation than in the behaviour of the introvert. The extra sensitivity of the extravert to his environment might make behaviour more situationally dependent. In contrast, the introvert, who focuses attention upon a few cues, is less likely to differentiate between situations and may appear to be behaviourally more rigid. The problem of what kind of person shows 'trait'-like consistency has been taken up in other contexts (Bem 1972; Alker 1972). In any given situation there is likely to be an optimum number of cues that are appropriate to attend to (e.g. a lecture vs a party), hence the extravert's behaviour is likely to be more 'efficient' or appropriate for some settings than others, with the reciprocal holding true for the introvert.

Third, the effects of cortical damage upon situationally dependent behaviour can be considered. The damage may reduce arousal (as previously discussed regarding frontal lesions), causing a broadening of attention and responsiveness and influencing behaviour in the direction of increased extraversion, as predicted by Eysenck's (1967) theories. If arousal level is unaffected but associative processing impaired (e.g. after temporal lesions), then the individual may compensate for this by reducing his processing load through the narrowing of attention and the analysis of fewer situational cues; i.e. a change in the introverted or reflective direction, as found by McIntyre et al. (1976) with left temporal cases.

Finally, individuals who are lesioned in areas such that the flow of information between the cortex and limbic system is disrupted (e.g. cingulectomies, amygdalectomies and orbital undercuts) will show reduced attention to affectively toned cues or elements, or will show distortions in emotional perception, and so on.

To recap: there is no one single key feature of the brain that will explain personality. The term peronality implicates many separate processes, states and mechanisms, each of which will become an object of research in its own right.

3.13 Brain and Personality: A Synopsis

The areas of brain and certain connections that have been mentioned throughout this chapter can be diagrammatically summarized as in Fig. 3.8.

Cortical lesions have a generalized detrimental effect on personality functioning (Lishman

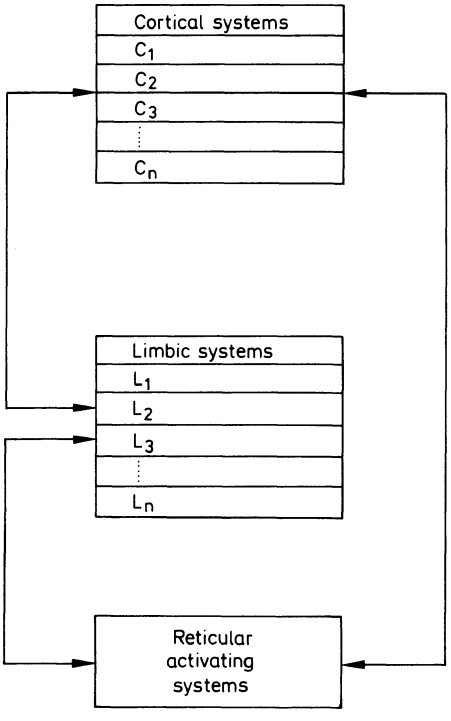

Fig. 3.8. A compilation of current neuropsychological models of personality, indicating the main places where the effects of lesions are evaluated

1968; Hécaen 1964; Rutter 1977), although there may well be hemispheric differences, with dominant lesions affecting logical, intellectual aspects of behaviour organisation but non-dominant lesions relating to affective mood (Flor-Henry 1969; Lishman 1968; Louks et al. 1976). Individual differences in the arrangement of cortical faculties and processes between the two hemispheres therefore becomes a factor relevant to individual prognosis after brain injury (Buffery 1976). Further, the separate lobes can contribute differentially to various personality processes, with, for example, the frontal lobe being involved in strategic organisation (Luria 1966; Miller et al. 1960) and the temporal lobes having a special function in regard to impulse control (McIntyre et al. 1976).

Limbic or sub-cortical lesions more involve the generation of emotional, drive, or pleasure-related states (Weil 1974). Specific circuits or 'centres' can be lesioned to change, for example, sexual drive (Roeder et al. 1972; Dieckman and Hassler 1977). One specific circuit has been linked with extraversion or sensitivity to punishment (Gray 1970), although most work centres upon the role of the limbic system for neuroticism, because of its links with emotionality.

Corticolimbic connections can be severed, as in the cingulotomy operation, to prevent emotions that might be generated in limbic circuits from receiving cortical expression (Kelly et al. 1973; Mitchell-Heggs et al. 1976). These lesions therefore create a disconnection syndrome, as described by Geschwind (1965), and cause a lowering of neuroticism by reducing the experience of negative moods.

Lesions to the *Corticoreticular loop*, as described by Eysenck (1967), change cortical tone and probably influence a wide variety of personality and other processes. Changes in extraversion form a small part of this overall change, since extraverts tend, too, to be cortically underaroused.

Lesions to the *limbic-reticular loop,* again as described by Eysenck (1967), are more difficult to define anatomically. Lesioning the ascending activating fibres from the ARAS to the limbic circuits will change activation in the limbic system (Weil 1974) and will probably be found to influence the same processes that can be altered with actual limbic lesions, but with less specificity. This is the third way, then, that neuroticism could be changed (i.e. along with limbic lesions or corticolimbic loop lesions), although a different mechanism pertains to each instance.

In conclusion, it can be stated quite safely that the problem of rehabilitation of personality after brain injury is a wide-open field. This chapter emphasises the need for a greatly improved descriptive system of personality changes, which in turn demands much greater theoretical and experimental endeavour towards defining those cognitive processes which underlie what we term 'personality' and which are affected by physical damage to the brain. Pinpointing these processes and their malfunction is the first step towards defining therapeutic goals. Without such a definition of the problem, attempts at therapeutic intervention may as well not proceed – they will be shots in the dark.

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Chapter 4

The Genetic and Environmental Architecture of Psychoticism, Extraversion and Neuroticism

D.W. Fulker

4.1 Introduction

It is the purpose of the present chapter to discuss the causes of individual differences in Eysenck's major personality dimensions, psychoticism, extraversion and neuroticism, using the concepts and analytical methods of biometrical genetics. From the point of view of the biological theory underlying these dimensions (Eysenck 1967), resolution into genetic and environmental components is of some importance. The presence of a strong genetic component, implying stable, constitutionally based individual differences combined with a relatively weak effect of the environment, particularly the social environment, would lend strong support to the theory. For this reason alone the investigation of the effects of nature and nurture on these personality dimensions is justified. However, the choice of biometrical genetics as the method of analysis, a relatively recent development in this area (Jinks and Fulker 1970), has the additional advantage of allowing a more searching investigation of these causes than alternative approaches based on the simple nature-nurture dichotomy. Using the biometrical approach, genetic and environmental effects can be further subdivided into more informative components and additional insight gained into how these components interact. As a result, biometrical analysis not only provides broad evidence concerning the validity of the underlying psychological theory but also allows us to place the theory within the wider framework of evolutionary biology by providing more detailed information concerning the genetic and environmental architecture of the traits.

Unfortunately, the biometrical approach, while being the one most suited to the present purpose, is not well understood by psychologists and is seen to present difficult conceptual and statistical problems (Buss and Plomin 1975; Mittler 1971). In this respect it is not unlike factor analysis, particularly in its more recent developments. Fortunately, however, as is the case with factor analysis, it is possible to outline basic concepts and techniques in a relatively straightforward manner if unnecessary complications are avoided. Consequently, a simple account of the biometrical approach will be included in the present chapter, using Shields' (1962) twin data on neuroticism as illustrative material before proceeding with the main review of the literature which necessarily assumes some familiarity with basic concepts and techniques. The reader familiar with this approach may therefore wish to omit the next section.

4.2 The Biometrical Approach

4.2.1 Basic Model

The statistical model underlying the biometrical approach recognizes that an individual's observed, or phenotypic, score has been influenced both by his genetic make-up and his environmental circumstances. Thus, at its simplest level, the phenotype (P) is assumed to be made up of two additive effects, a genetic effect (G) and an environmental one (E). If P is expressed as a deviation from the mean of the population under investigation we can write

$$P = G + E.$$

Even at its simplest level this formulation is frequently misunderstood. The commonest mis-

understanding probably concerns the almost grotesque level of abstraction such a formulation seems to imply. We can only observe a particular individual's phenotypic score, so in what sense can it be made up of two parts? How, for example, can a high neuroticism score of 7 points above average in the Eysenck Personality Questionnaire (EPQ), about 18 points, be made up of two parts, one due to genetic make-up, say 4 points, and the other to environmental influences, the remaining 3 points, when all we can observe is a score of 18? However, such a question demonstrates a misunderstanding not only of the biometrical model but of additive linear models in general. What the model implies is that this individual's particular genetic make-up is of the kind that generally raises neuroticism scores by 4 points under a wide variety of environmental circumstances. That is, he is genetically predisposed to be somewhat neurotic. Similarly, the model implies his environmental circumstances are of the kind that also generally increase neuroticism, but by about 3 points in this case.

The problem arises because we cannot usually determine what these average genetic and environmental effects are for any particular individual, so that the model appears to involve an unrealistic degree of abstraction. In principle, though, we can see how these effects can be observed by considering studies carried out in the animal laboratory, where the effects can be measured easily and with a high degree of accuracy. In this case we can use genetically uniform strains of rats or mice, for example, to replicate any particular genetic make-up and assess its average effect by systematically observing the phenotype under a variety of environmental conditions. Average performance then defines the effect of that particular genotype, that is the value of G.

For example, with the Maudsley strains of rats, strains that were selectively bred for high and low emotional defecation in the open field (Broadhurst 1960), we know exactly what their typical response will be under a variety of environmental conditions from a great many experimental studies, this response being of course entirely genetic in origin. The measurement of the effects of the environment presents even less difficulty and is undertaken, in effect, whenever we carry out some environmental manipulation on a representative sample of laboratory animals.

Another, more subtle, objection to the model is that genetic and environmental effects are unlikely to be strictly additive. That is, the magnitude of genetic differences between strains of laboratory animals, for example, might well be expected to vary depending on environmental circumstances. In the case of the Maudsley strains it is quite reasonable, in fact, to expect the strain difference to be greater under normal laboratory conditions than under those that favour a high degree of habituation of the response.

Up to a point this objection is quite reasonable, for such genotype-environmental interactions are quite common in the behaviour genetic literature (Fulker et al. 1972). However, they only usually occur to any degree when either genetic make-up or environmental conditions are extreme. For the vast majority of genotypes and environmental conditions that make up the normal range, interactions appear to be of negligible importance (DeFries 1979). Fortunately, should such interactions be found to be of importance, the basic additive model can be elaborated by the addition of an interaction term. The main effects, G and E, are still defined as average effects across a variety of conditions, except now there is an additional background variability due to the interaction effects. The interaction effects themselves are defined as the effects of specific combinations of genotype and environment after allowance has been made for the general effects of G and E. The model is thus formally equivalent to that underlying two-way analysis of variance (ANOVA), with interaction between the main effects.

However, while we can specify the model accurately in the laboratory, it is much more difficult to do so in human populations. In this case we are usually forced to adopt a quasi-experimental approach (Campbell and Stanley 1963) in order to operationalise the model, just as we do in many other situations in the social sciences. Thus, rather than combining variables systematically as we do in the laboratory, we are usually forced to exploit what systematic

features occur naturally in the population, taking it as we find it. In the present context we can exploit, for example, the systematic genetic difference in similarity that exists between pairs of identical or monozygotic twins (MZ) and fraternal or dizygotic pairs (DZ). Or we can make use of the systematic environmental differences between adopted individuals and those who are reared by their natural parents. Sometimes we exploit both these systematic features in the study of separated MZ twins. In all, some dozen or so such naturally occurring quasi-experimental designs are available for investigating the nature-nurture problem in this way (Fulker and Eysenck 1979).

Unlike the situation with true experimental designs, in the quasi-experimental approach we need to use additional checks to satisfy ourselves that the design is valid. In the true experimental design, validity is ensured by the elegant device of randomization (Fisher 1960). In the quasi-experimental designs of the social sciences we must rely on various internal and external checks and the cumulative effect of combining a number of independent sources of information. These principles lie at the heart of the biometrical approach to the analysis of human behaviour.

The nearest we are able to get with human subjects to the animal laboratory situation employing inbred strains is in the study of MZ twins reared apart. Ideally, from a scientific point of view, we would like the twins to be separated immediately at birth and placed quite randomly into foster homes. The two situations would then be exactly equivalent. Under these conditions, any resemblance between the pairs could be directly ascribed to their identical genetic make-up and their differences to different environmental experiences taking place during their lifetime. In practice, of course, this is not how things happen. Individuals are often separated weeks or months after birth and sometimes they are brought up in a related branch of the same family. At first sight such difficulties appear to present insuperable problems for valid inference. As we shall see, however, this need not be the case if we take the trouble to combine different sources of information and look at our data searchingly.

Table 4.1. Neuroticism (N) scores for 26 pairs of female MZ twins reared apart (Shields 1962)

Pair	Higher N	Lower N	Mean	Difference
1	5.0	4.5	4.75	0.5
2	10.0	1.0	5.50	9.0
3	8.0	4.0	6.00	4.0
4	7.0	5.0	6.00	2.0
5	8.0	7.0	7.50	1.0
6	9.5	7.0	8.25	2.5
7	10.0	8.0	9.00	2.0
8	11.0	10.0	10.50	1.0
9	14.0	7.0	10.50	7.0
10	12.5	10.5	11.50	2.0
11	14.0	9.5	11.75	4.5
12	14.5	9.0	11.75	5.5
13	12.0	12.0	12.00	0.0
14	16.0	9.5	12.75	6.5
15	14.0	12.0	13.00	2.0
16	14.5	12.0	13.25	2.5
17	13.5	13.0	13.25	0.5
18	14.0	13.0	13.50	1.0
19	19.0	8.5	13.75	10.5
20	18.5	9.5	14.00	9.0
21	16.0	14.0	15.00	2.0
22	18.0	13.5	15.75	4.5
23	17.5	14.5	16.00	3.0
24	19.0	16.0	17.50	3.0
25	18.0	17.0	17.50	1.0
26	18.0	17.0	17.50	1.0

Mean N score, 11.84

The largest study of MZ twins reared apart relevant in the present context is that of Shields (1962), in which 42 pairs of separated twins were assessed using an early form of the Maudsley Personality Inventory (MPI), an early forerunner of the EPQ. Let us assume for the moment that the ideal requirements of this kind of study are met. Then the phenotypic scores for these subjects define the Gs and Es of our simple additive model directly in the following way.

In Table 4.1, neuroticism scores are listed for the 26 pairs of female subjects for which full information was available. The questionnaire involved 22 items similar to those found in the neuroticism scale of the EPQ. Subjects were scored 1 for a neurotic response, otherwise 0, with a score of $^1/_2$ for a 'not sure' response.

Clearly a wide range of variation exists among the twins in the table, the scores ranging from 1 to 19, almost the complete range of

the test. However, the most striking feature of the twins' scores is their similarity. The lowest pair of twins with a mean of 4.75 differ only by half a point. The two highest pairs averaging 17.50, nearly 15 points higher, still differ by a mere 1 point. More than half the remaining pairs differ by 3 points or less. Clearly the pairs correlate to a considerable degree, suggesting strong genetic determination of the phenotypic differences among the twins.

However, the main point in listing the twins in this way is to show how their scores relate in a direct and simple manner to the basic form of the biometrical model and to illustrate some of its associated computational methods.

Recall that our individual's phenotypic score, P, is represented by the sum of a genetic and environmental effect G and E

$$P = G + E.$$

Since pairs of MZ twins have identical genetic make-up, they also have identical values of G and the pair differences in Table 4.1 can only reflect the effects of the environment. As we have seen, these are generally quite small, the majority being 3 points or less, although occasionally they are large, the largest being 10.5. In our ideal study of separated MZ twins, where separation is assumed to be at random, the variation in these differences can be taken to reflect the full range of the effects of relevant life experiences on this trait. In this population the implication would be that only 1 in 26 people, or about 4%, can expect to have experiences that will modify the effects of their basic genetic make-up by as much as 10.5 points. The majority, 17 out of 26 or about 65%, should be affected only by about 3 points or less. When we consider that these environmental effects include day-to-day variation in response to the questionnaire items, or unreliability as the psychologist would call it, so that repeat testing can easily result in differences of up to 3 points anyway, the long-term effect of the environment on individual variation is seen to be quite small for the majority of people. For the 65% of twins with differences as small as 3 points we are probably getting as close to an individual's true score as is possible within the limits of the reliability of the measuring instrument, not by measuring the individual directly but by measuring a genetically identical individual with whom he has had little or no contact during his entire lifetime! This simple table thus bears direct witness to the importance of genetic make-up for this particular measure of neuroticism.

Twin differences measure nothing but environmental effects, whether the twins are randomly separated or not. Randomization is only necessary if we want to assume that these environmental influences cover the full range. However, only if separation is at random do the pair means give a valid measure of genetic make-up. In addition, unlike the estimated environmental effects, these estimates of genetic effects are subject to a degree of inaccuracy due to the effects of the environment. We can see how this inaccuracy arises if we consider any pair in the table. Take the first pair, with a mean of 4.75, a value of G 7.08 points below average. Now if this pair were, in fact, only two of three MZ triplets, the missing triplet's score would improve our estimate of G. Taking G now as the average of all three scores, our estimate would be unlikely to remain exactly -7.08. Any deviation from this value would reflect in part the different environmental influences to which this triplet had been exposed. His phenotypic score might easily be 2, for example, giving a revised estimate of -8.01 for G. Consequently, while twin differences reflect only environmental variation, differences in pair means, while reflecting mainly genetic variation, must also reflect some environmental variation too.

Because these estimates of G are affected to some extent by E we can most accurately separate the relative effects of genotype and environment not by looking at raw score means and differences but by a comparison of their variation. The appropriate measure of this variation is the variance defined in the usual way as the mean of squared deviations from the population mean. The observed phenotypic variance for the N individuals in our table is therefore given by

$$V(P) = \frac{1}{N-1} \sum_{i=1}^{N} (P_i - \text{sample mean})^2$$

which provides an estimate of a little over 19. In our ideal separated twin study where

$$P = G + E$$

and the G and E are quite independent of each other, V(P) will also be made up of two parts, V(G) the genotypic variance, and the variance of environmental effects, V(E). Thus

$$V(P) = V(G) + V(E),$$

where V(G) is simply the average of squared Gs in the population and V(E) the average of squared Es.

It is often argued that the independence of G and E is unlikely in human populations, and for some traits this seems to be the case. For example, with cognitive traits we often find that favourable genetic make-up goes along with a favourable home environment, and vice versa (Fulker and Eysenck 1979). When this occurs, the basic model has to be elaborated by the addition of a co-variance term to take account of the lack of independence between G and E. However, with randomly separated individuals this co-variance cannot arise, even if it exists in the general population, and the separation of V(P) into its components will be valid (Jinks and Fulker 1970). As we shall see, this source of variation is almost certainly absent in the personality domain.

We can estimate the two components V(G) and V(E) from the variances of the pair means and pair differences shown in Table 4.1, where these variances are defined as follows:

$$V(\text{pair means}) = \frac{1}{n-1} \Sigma (\text{pair means} - \text{sample mean})^2$$

$$V(\text{pair differences}) = \frac{1}{n} \Sigma (\text{pair differences})^2,$$

where n is the number of pairs. The pair differences require no mean correction, since they have an expectation of zero when twins are taken in random order, rather than the higher scoring twin first, as we have done in Table 4.1. In terms of our biometrical model, these two variances have the following expectations:

$$V(\text{pair means}) = V(G) + \tfrac{1}{2}V(E)$$
$$V(\text{pair differences}) = 2\,V(E).$$

Performing these calculations on the data in Table 4.1, which the reader may care to carry out for himself, gives:

$$14.325 = V(G) + \tfrac{1}{2}V(E)$$
$$19.471 = 2\,V(E).$$

Thus V(E) is estimated as half 19.471, and we obtain our estimate of V(G) by taking half the estimated V(E) from the variance of pair means, giving estimates:

$$V(G) = 9.457$$
$$V(E) = 9.736.$$

The total phenotypic variance is the sum of these, 19.193, and the ratio of V(G) to the phenotypic variance is 0.49. This fraction, the proportion of total variation due to genetic make-up, is often referred to as the broad heritability of the trait.

Of course, what we have done in these calculations is to carry out a simple Between and Within Pair analysis of variance (ANOVA) and used the variances to estimate an intra-class correlation of 0.49 for twin pairs. In fact the ANOVA usually involves variances or mean squares calculated as half the variances of pair *sums* and pair *differences*, not of pair means and pair differences as we have done. Thus the Between Pair mean square is just 2V(Pair means) and the Within Pair mean square is $\frac{1}{2}$V (Pair differences). This ANOVA and the expectations on the biometrical model are shown in Table 4.2.

The expression for the intra-class correlation (r_i) is

$$r_i = \frac{B - W}{B + W},$$

which from the expressions given in Table 4.2 can be seen to have the expectation

$$r_i = V(G)/[V(G) + V(E)]$$
$$= V(G)/V(P)$$

and expresses the between pair component of variance, in our case V(G), as a fraction of total variation.

In the analysis of Shields' data we assumed both early and random separation, in order that V(E) reflects the full range of environmental effects. We also assumed the absence of G × E

Table 4.2. ANOVA of Shields' neuroticism data in Table 4.1

Item	df	Mean square	Expectation
Between pairs (B)	25	28.65	V(E)+2V(G)
Within pairs (W)	26	9.74	V(E)

Table 4.3. MZ-twin differences in Shields' study, divided according to age and manner of separation

	Separated before 1 year	Separated after 1 year and/or reunited
Adopted into related homes	5.5 2.0 3.0 1.0 0.5 2.5 1.0 4.5	6.5 1.0 0.5 2.0 9.0 4.5 1.0 4.0 3.0 10.5
Mean	2.50 ± 0.63	4.20 ± 1.10
Adopted into unrelated homes	2.5 1.0 2.0	7.0 0.0 2.0 9.0 2.0
Mean	1.83 ± 0.44	4.00 ± 1.70

interaction. Are these valid assumptions? The first assumption appears, on the face of it, to be false, since only 11 pairs were separated during the 1st year and remained separate into adulthood, and of these only three were adopted into unrelated families. Of the 15 separated later, or reunited at some time, only five were adopted into unrelated families. However, before dismissing the results of the study we must ask if there is any evidence that these factors are at all relevant as regards neuroticism and could have produced an inflated degree of similarity among the twins. If they are not relevant, then the twins may well have been separated in an adequately random fashion as regards environmental factors relevant to the development of the trait.

We can examine this point by dividing the twin differences in Table 4.1 according to the manner of separation as shown in Table 4.3. Clearly there is no problem so far as these two factors are concerned, since pairs are actually slightly, though non-significantly, *less* alike when either separated at a late stage or adopted into related homes. The means in Table 4.3 are in reverse order to that expected if these variables were indeed relevant in the development of the trait in question.

More light is shed on this unexpected result when we adopt the additional strategy of combining independent sources of information and look at Shields' control group of 29 pairs of unseparated female MZ twins. Compared with the separated pairs, which correlate 0.49, the unseparated pairs only correlate 0.46, actually a little less than the separated pairs. Thus, as the analysis of Table 4.3 suggests, the extent of separation is quite unimportant as a source of twin similarity, since control twins, who were not separated at all, were in no degree more alike than separated pairs. Of course, what this suggests is that environmental experiences typically shared by individuals brought up together in the same home, such factors as general parental influences, social class and geographical location are of no importance in the development of neuroticism. As we shall see later, this systematic aspect of the environment can be built into our biometrical models explicitly and investigated further using twin and other, more powerful, designs.

So far as G × E interactions are concerned, we can obtain some information from the means and differences in Table 4.1, although the kind of detailed information available from animal studies is not really obtainable with human subjects.

Each difference in the table measures an environmental effect, and these effects are conceptualized as originating from a single population of influences. If we have a G × E interaction, then the same environmental influence will have a different effect depending on the genotype of the individual experiencing it. This would be expected to result in a mixture of distributions in place of the single distribution for E we have assumed. Thus if we could examine the 26 twin differences for signs of heterogeneity we would have a good indication of G × E interaction.

Another way of looking at the same problem is in terms of differential stability. With inbred

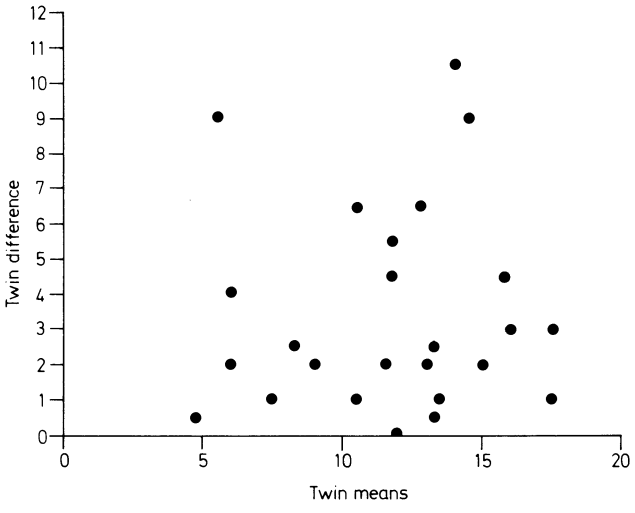

Fig. 4.1. The graph shows separated MZ twin differences in female neuroticism plotted against the pair means. The apparent absence of a systematic relationship suggests there is no genotype-environmental interaction for this trait. (Based on data taken from Shields 1962)

strains of animals, within-strain variation is a measure of the stability of the strain as regards environmental influences. If strains differ in this respect, then we have evidence of G × E interaction. In our case we can view the twin pairs as equivalent to our strains, two individuals per 'strain', and the differences as a measure of their environmental stability. Again, heterogeneity of these differences would indicate G × E. Unfortunately with such a small sample there is little chance that any of the standard tests of heterogeneity of variance will be powerful enough to detect the effect. As an alternative, however, we can look at the pattern of environmental differences in relation to the genotypic effects indicated by the pair means. A plot of these differences against the means is shown in Fig. 4.1.

To the eye at least there seems very little systematic relationship, beyond a slight possibility that the E effects get smaller at the extremes of the genotypic range. In fact we expect a small floor and ceiling effect of this kind with tests involving only 20 or so items. These effects are really just scalar, resulting from the test items following a binomial distribution rather than that of the normal curve. For this reason, in some of the more elaborate analyses we shall be considering later in the chapter this scalar effect is removed by transformation.

Should G × E interaction exist, from the way we have defined our model, its effect is generally confounded with that of the environment and will not, therefore, bias our estimate of genetic variance. For this reason, although it is of great interest to detect interactions where they exist, for the insight they provide regarding genetic architecture, it is of no great importance regarding our estimate of heritability if they are present but remain undetected.

We have looked at Shields' sample of separated female MZ twins in detail to illustrate the basic statistical model underlying the biometrical method and to explain the correlational and ANOVA computational methods used in analysis. In the next section we shall illustrate how the biometrical model can be built up using MZ and DZ twins reared together and illustrate the statistical procedures used to estimate genetic and environmental parameters.

4.2.2 Estimation of Parameters in the Model Using MZ and DZ Twins

We have seen how statistics derived from data on MZ twins reared apart have a very simple interpretation on the biometrical model and allow a complete separation of genetic and environmental factors. However, while they allow a unique separation of these two effects, they are not very informative regarding any finer subdivision. For this purpose we need to include information from additional family relation-

ships. The most common source of this information is that obtained from monozygotic and dizygotic twins reared together.

As soon as individuals are reared together in the same family we have to recognise that pair resemblance may result from shared environmental experiences as well as from any shared genetic make-up. This shared, or common, environmental effect will be denoted for the time being as CE. Since this common family environmental influence can, in large part, be equated with the effects of social environment, its presence is of particular interest to the psychologist. As well as CE there will also be specific environmental effects (SE) which are unique to the individual and typically cause members of the same family to differ from each other. These specific effects are often referred to as 'within-family environmental effects' in the biometrical literature. Thus what we formerly called E, which was defined to cover the full range of environments, is split into two parts

$$E = CE + SE$$

and

$$V(E) = V(CE) + V(SE).$$

Thus we see that, when MZ twins are reared apart, that which normally makes individuals living together similar, CE, now only makes them different and V(CE) appears in the expression for the variance of pair differences. The within-family environmental effects, SE, are what we measure when we control perfectly for genetic make-up within the family. That is, they are measured directly by the pair differences of MZ twins reared together. On the other hand, the pair means of these twins now reflects not only G, as in the case of separated MZ pairs, but CE as well. Thus, if we use half the variances of the pair differences and pair sums to calculate the between and within-pair mean squares in an ANOVA, their expectations on the biometrical model are V(SE) for the within-pair mean square and $V(SE) + 2\,V(G) + 2\,V(CE)$ for that between pairs. This latter mean square is made up of twice the between-pair component, $V(G) + V(CE)$, together with the within-pair variance, V(SE), as a kind of error component tending to obscure the G and CE effects, just as the mean square for separated MZ twins was made up of twice the between-pair component, V(G), obscured by the whole of environmental variance, V(E), or $V(SE) + V(CE)$ as we have now redefined it. Taking the previously referred to expression for the intra-class correlation for MZ twins reared together

$$r_i = \frac{2\,V(G) + 2\,V(CE)}{2\,V(SE) + 2\,V(G) + 2\,V(CE)}.$$

The twos cancel and we have

$$r_i = V(G) + V(CE)$$

where these components are now expressed as fractions of the total variation. We can now see formally in terms of the biometrical model the implications of the separated and unseparated MZ correlations being 0.46 and 0.49 respectively. Since $r_i = V(G)$ for the separated MZ pairs again expressed as a fraction, a value of -0.03 or -3% is implied for V(CE) and 0.49 or 49% for V(G). However, the presence of this negative variance component in this simple model is not permissible, the definition of V(CE) being the average of squared CE effects in the population, a quantity that cannot be negative. Since the estimate was obtained from a rather small sample and is numerically small, it is reasonable to put the value of this parameter V(CE) equal to zero and estimate V(G) from the pooled correlations as 0.47 or 47%.

In much of the behaviour genetics literature the biometrical models are developed solely in terms of intra-class correlations, the reason being that since they only reflect the between-pair components of variance and not the within-pair component as well, as do the between-pair mean squares, their expectations are much simpler to grasp intuitively. With so much of the literature developed in terms of intra-class correlations, we are forced to use this approach too if we are to reanalyse and synthesize others' findings. Since the correlations only reflect between-pair components, valuable information is lost using this approach. Both approaches, the correlational and that based on ANOVA, will be developed in this introductory section.

We have seen how the addition of MZ twins reared together allows us to explore the nature

of environmental variation in a more informative fashion. The addition of DZ twins reared together has the further advantage of allowing us to explore the genetical component, V(G), in more detail. The reason we obtain this additional information is that DZ twins are genetically related in a different manner from pairs of MZ twins, and this difference can be given a precise biometrical expectation for a number of common genetical situations.

The simplest form of genetic architecture exists when two conditions are met. The first is that all the genes influencing a trait combine in an additive fashion, that is the genes express themselves in the same way, irrespective of the rest of the genotype. The second condition is that spouses do not consider the trait in question when they select each other as mates and therefore effectively mate at random with respect to the trait.

When both these conditions are met, the genetic resemblance of DZ twins, or siblings who behave in exactly the same way genetically, will be exactly half that of MZ twins. Since MZ twins have all their genes in common, this means that the genetic component in pair resemblance for DZ twins should be exactly $\frac{1}{2}$V(G) under these assumptions. The remaining half of the genetic variation causes DZ pairs to differ and is therefore reflected in their pair differences, together with the effects of the specific environment. Thus the within-pair mean square, W, still calculated as half the variance of pair differences, will have the expectation

$$W = \tfrac{1}{2}V(G) + V(SE).$$

The expectation of the between-pair mean square, B, calculated as half the variance of pair sums, is now made up of the above source of variation, as an error component plus twice the between-pair component $\frac{1}{2}$V(G)+V(CE), giving

$$B = V(SE) + 1\tfrac{1}{2}V(G) + 2\,V(CE).$$

The intra-class correlation has the simple expectation of the between-pair component of variation

$$r_i = \tfrac{1}{2}V(G) + V(CE),$$

where these components are now expressed as a fraction of the total variation.

Shields' study contained a small sample of 16 female pairs of DZ twins with an intra-class correlation for neuroticism of 0.27. We can use this correlation to see if the simple genetical model above is appropriate. Before doing so, however, we must consider what happens if the two basic assumptions of additivity of gene action and random mating fail to hold.

The most common cause of non-random mating occurs when individuals choose partners similar to themselves, a process known as positive assortative mating. In this case, parents will now resemble each other to some extent genetically as well as phenotypically, given a genetic component in the trait. Consequently, although their children are receiving half their genes from each parent, many of these genes are effectively the same ones. The result is that their children resemble each other rather more than we would expect in the absence of assortative mating, and the genetic resemblance among sibs or DZ twins is greater than $\frac{1}{2}$V(G). Assortative mating can usually be inferred from the presence of a phenotypic correlation between spouses, and although it is known to be quite high for many physical and cognitive traits, there is not much evidence of a spouse correlation in most personality traits (Vandenberg 1972). This is a problem we will return to later in the chapter.

When the second assumption, that of genetic additivity, fails to hold, sibling genetic resemblance is decreased rather than increased as it is under assortative mating.

The commonest form of non-additivity is that of Mendelian dominance. Here the expression of each of the two allelic forms of a particular gene depends on which other one is present. When two different forms are present, the allele that expresses itself is said to be dominant and the suppressed one recessive. Many genetically determined diseases are caused by the presence of double recessive alleles, and it seems likely that alleles for low IQ are recessive in this way too (Fulker and Eysenck 1979). The reason that this form of gene action reduces the shared genetic component for siblings or DZ twins is that more extreme individuals occur within the family than would otherwise be the case, in-

creasing genetic variation within the family at the expense of variation between.

DZ twins can therefore be used to explore the genetic architecture further by seeing if the between-pair genetic component of variance is less, equal to or greater than $\frac{1}{2}$V(G). Of course, the presence of both assortative mating and dominance will tend to cancel and might mislead us into thinking we have only a simple genetical system. This appears to be the case for IQ (Fulker and Eysenck 1979), where the sibling genetic variance is $\frac{1}{2}$V(G), but independent evidence of assortative mating from a high marital correlation and of dominance from the deleterious effects of inbreeding alert us to the problem and allow us to develop an appropriate biometrical model. In the case of personality measures where we know assortative mating is slight or non-existent, we are unlikely to be faced with this particular problem.

We can now look at Shields' data on MZ and DZ twins reared together in order to explore the genetic and environmental architecture of neuroticism in more detail. The analysis will be developed both in terms of correlations and mean squares in order to illustrate both approaches.

First, the simpler approach in terms of correlations. These are shown for the three kinds of twins in Table 4.4 together with our two-parameter biometrical model written as coefficients in columns five and six, a procedure that facilitates estimation. This 3×2 matrix of coefficients is known as the model matrix, denoted A in matrix notation. In the table we have three observed correlations but only two parameters to estimate from them, V(G) and V(CE). Consequently there are a number of different possible solutions. For example, we could estimate the two parameters from the MZ data alone, obtaining

$$V(G)=0.49, \quad V(CE)=-0.03$$

Or we could use only the MZ and DZ twins reared together, obtaining V(G) as twice the difference between the two correlations and V(CE) as the difference between the MZ correlations and our estimate of V(G). This approach gives

$$V(G)=0.38$$
$$V(CE)=0.08.$$

Or yet another approach would be to use the correlations for separated MZ twins and the unseparated DZ twins to obtain

$$V(G)=0.49$$
$$V(CE)=0.02.$$

Clearly there is no obvious way to choose between these different estimates, and we need some kind of averaging procedure that takes account of all three observed correlations.

One simple procedure to use in this situation is to adopt the criterion of a least-squares fit, just as we do in regression analysis. In this case we attempt to obtain estimates of V(G) and V(CE) that give expected values of the correlations as near as possible to the observed ones, according to the least-squares criterion, that is, that the sum of the squared discrepancies is as small as possible. If we write E(r) for the expected value of the correlations, obtained on the basis of our estimates of V(G) and V(CE),

Table 4.4. Correlations for neuroticism for three kinds of twins in Shields' study, with solution of two biometrical models

Twin	Observed correlation (r)	No. of pairs	Information	Model		Expected r	
				V(G)	V(CE)	1	2
MZ together	0.46	29	47	1	1	0.47	0.48
MZ apart	0.49	26	45	1	0	0.48	0.48
DZ together	0.27	16	19	$\frac{1}{2}$	1	0.23	0.24
Parameter estimates	V(G)					0.48 ± 0.14	0.48 ± 0.10
	V(CE)					-0.01 ± 0.17	0.00
	χ^2					0.039	0.041
	df					1	2
	P					0.8	0.9

then we want estimates that minimize the quantity Q, where

$$Q = \sum_{i=1}^{3} [r_i - E(r_i)]^2.$$

We could use a numerical computer routine to find these estimates, and this approach is often used where more complex models are concerned. However, regression theory gives an explicit solution to the problem in our simple case. If we regress the observed correlations simultaneously on to the two columns of coefficients in the model matrix, A, the beta-weights we obtain are the least-squares estimates of the parameters we require.

In practice we can improve on this procedure if we also take into account the different precision with which the observed correlations are determined. For example, the correlations for MZs are not only larger than those for DZs, and therefore more accurately determined, but are also based on larger samples and for that reason can be expected to be more reliable. In our model fitting procedure, therefore, we would ideally like to give the MZ correlations more weight than the DZ. With appropriate weights, w_i, we would therefore minimize

$$Q = \sum_{i=1}^{3} w_i [r_i - E(r_i)]^2,$$

which also has an explicit solution in terms of weighted regression analysis.

An appropriate weight in this context is the square of the inverse of the standard error of the correlation, a quantity known as the 'amount of information'.

$$I = \frac{N}{(1-r^2)^2}.$$

If we use the values of I as weights, we use all the information contained in our correlations to estimate the parameters in a very efficient manner, a procedure known as Maximum Likelihood estimation. In addition, we obtain a χ^2 goodness of fit test for model, since Q is now a sum of squared residuals divided by their expected variance, and standard errors can also be obtained for the parameters. This Maximum Likelihood approach is therefore optimal in the present context. The computational procedures are illustrated below using matrix notation.

We denote the observed correlation by a vector, x, the model matrix by A, the matrix of weight a diagonal matrix I and the vector of parameter estimates by θ.

$$\text{Thus } x = \begin{bmatrix} 0.46 \\ 0.49 \\ 0.27 \end{bmatrix} \quad A = \begin{bmatrix} 1 & 1 \\ 1 & 0 \\ 0.5 & 1 \end{bmatrix}$$

$$I = \begin{bmatrix} 47 & 0 & 0 \\ 0 & 45 & 0 \\ 0 & 0 & 19 \end{bmatrix} \quad \theta = \begin{bmatrix} V(G) \\ V(CE) \end{bmatrix}$$

In this notation our expression for Q is written

$$Q = [x - E(x)]' I [x - E(x)],$$

E(x) being the vector of E(r)s.

Regression Theory shows that the solution to the following simultaneous equation, the so-called normal equations, provides our estimates of θ. The equations are

$$A' I x = A' I A \theta$$

By inverting the matrix $(A I A)$ we obtain the solution

$$\theta = (A' I A)^{-1} A I x$$

These equations are set out in full and their solution is quite simple, once the rules for multiplying and inverting matrices are understood. These rules can be obtained from any elementary text on matrix algebra.

The normal equations are

$$\begin{bmatrix} 1 & 1 & \frac{1}{2} \\ 1 & 0 & 1 \end{bmatrix} \begin{bmatrix} 47 & 0 & 0 \\ 0 & 45 & 0 \\ 0 & 0 & 19 \end{bmatrix} \begin{bmatrix} 0.46 \\ 0.49 \\ 0.27 \end{bmatrix}$$

$$= \begin{bmatrix} 1 & 1 & \frac{1}{2} \\ 1 & 0 & 1 \end{bmatrix} \begin{bmatrix} 47 & 0 & 0 \\ 0 & 45 & 0 \\ 0 & 0 & 19 \end{bmatrix} \begin{bmatrix} 1 & 1 \\ 1 & 0 \\ \frac{1}{2} & 1 \end{bmatrix} \begin{bmatrix} V(G) \\ V(CE) \end{bmatrix},$$

which reduce to

$$\begin{bmatrix} 46.24 \\ 26.75 \end{bmatrix} = \begin{bmatrix} 96.75 & 56.50 \\ 56.50 & 66.00 \end{bmatrix} \begin{bmatrix} V(G) \\ V(CE) \end{bmatrix}.$$

Thus

$$\begin{bmatrix} V(G) \\ V(CE) \end{bmatrix} = \begin{bmatrix} 96.75 & 56.50 \\ 56.50 & 66.00 \end{bmatrix}^{-1} \begin{bmatrix} 46.24 \\ 26.75 \end{bmatrix}$$

$$= \begin{bmatrix} 0.0207 & -0.0177 \\ -0.0177 & 0.030 \end{bmatrix} \begin{bmatrix} 46.24 \\ 26.75 \end{bmatrix}$$

$$= \begin{bmatrix} 0.48 \\ -0.01 \end{bmatrix}.$$

These parameter estimates lead to the expected correlations in the first column of expectations in Table 4.4 and a χ^2 goodness-of-fit test of

$$\chi^2 = (0.46-0.47)^2\ 47 + (0.49-0.48)^2 + 45 + (0.27-0.23)^2\ 19 = 0.039.$$

This χ^2 has 1 df since we have used three observations to estimate two parameters. The square roots of the diagonals of the inverted matrix above, the inverse of the information matrix, provide the standard errors of the estimates.

The conclusion is quite clear. There is a large genetic component of 0.48 or 48% of variation. There is no evidence of V(CE) and no evidence from the expectation of the DZ correlation that their genetic resemblance differs from $^1/_2$ V(G). The adequacy of this latter assumption is more obvious if we drop V(CE) from the model and fit a model involving only V(G) to the three correlations. Our normal equation is now quite simply

$$46.24 = 96.75\ V(G)$$

taken from the appropriate elements of the previous matrix equations, giving $V(G) = 0.48 \pm 0.10$ and a non-significant $\chi^2_2 = 0.404$. The expectations are shown in column 2 and hardly differ from before. The expectation of the correlation for DZ twins is 0.24, trivially different from the observed correlation of 0.27.

Using correlations to estimate the parameters in our model is equivalent to standardizing all the phenotypic variances to unity. However, it is preferable to fit our biometrical models to the mean squares of the ANOVA, a procedure which uses all the information in the data and does not assume each sample can be standardized about its own variance. This is the method advocated by the Birmingham School of Biometrical Genetics (Mather and Jinks 1971), and the procedure is illustrated in the final part of this section.

The three pairs of mean squares in the analyses of variance of Shields' data, from which the correlations were obtained, are shown in Table 4.5, together with the expectations developed earlier for mean squares. The estimation procedure is the same as that for correlations, except that the weights in the analysis are now derived from the amount of information in a mean square rather than that in a correlation. These weights are the inverse of the sampling variance of a mean square (MS)

$$I = \frac{df}{2MS^2}.$$

The weights are shown in the table. The mean squares are more informative than the correla-

Table 4.5. ANOVA approach to fitting a biometrical model to Shields' data on neuroticism

Twin type	Item	df	MS	I	Model		Expected MS
					V(G)	V(SE)	
MZ together	Between pair (B)	28	22.16	0.029	2	1	24.90
	Within pair (W)	29	8.12	0.220	0	1	9.04
MZ apart	B	25	28.65	0.015	2	1	24.90
	W	26	9.74	0.274	0	1	9.04
DZ together	B	15	22.81	0.014	1.5	1	20.94
	W	16	13.10	0.047	0.5	1	13.01

Estimates $V(G) = 7.93 \pm 2.30$ $h^2 = 0.47$ 0.14
$V(SE) = 9.04 \pm 1.39$ $\chi^2_4 = 0.80$ $p = 0.9$

tions. For example, the correlations of MZs together and apart imply a negative V(CE). The mean squares, however, suggest a somewhat more complex picture. If we compare the two 'within' mean squares we see that for MZ twins reared apart it is a little larger than for MZs together, implying at least a positive effect of V(CE) if any at all. However, the 'between' mean squares suggest the reverse picture. Here we find the separated MZ mean square is larger, not smaller, as we would expect in the presence of V(CE). However, as the third column of figures in Table 4.5 indicates, the amount of information associated with the 'within' mean squares is so much more than that associated with those between that the picture for the 'withins' is more reliable. In addition, working with mean squares we can see the effects of genetic segregation directly, the 'within' mean square for DZ twins being greater than that for MZs.

Using the mean squares to estimate the parameters in our simplest of biometrical models gives the values at the foot of the table.

$$V(G) = 7.93 \pm 2.30 \text{ and } V(CE) = 9.04 \pm 1.39$$

V(G) expressed as a proportion of total variance is 0.47 ± 14, and this heritability estimate is very similar to 0.48 ± 10 estimated from the correlational data. Again the χ^2 indicates a very good fit of the model.

Yet another approach to estimation that gives true maximum likelihood estimates of variance components, even in small samples, is that suggested by Fulker (1978), which minimizes a likelihood ratio statistic using a standard computer non-linear optimization routine. This procedure takes into account the skew of the sampling distribution of mean squares when they are based on only a few degrees of freedom. The approach, applied to the mean squares in Table 4.5, minimizes

$$F = \sum_{i=1}^{6} df_i[\log_e(EMS_i/MS_i) + (MS_i/EMS_i) - 1],$$

where the EMS_i are the six expected mean squares estimated on the basis of the model and the MS_i the six observed mean squares shown in the table. This procedure gives

$$V(G) = 8.17 \pm 2.62 \text{ and } V(SE) = 8.89 \pm 1.71,$$

again with a heritability of 0.48 ± 0.15 and a $\chi^2_4 = 0.71$ $p = 0.9$.

The consistency of the estimates, their standard errors and the χ^2 goodness-of-fit tests clearly suggest the statistical robustness of these estimation procedures, which we shall use to evaluate the empirical studies in the next section.

4.3 Empirical Studies

We have discussed Shields' study of neuroticism in some detail mainly to illustrate the biometrical approach. Much of the recent literature is inaccessible without some familiarity with the approach and its associated analytical methods. In the present section we come to the main purpose of the chapter, which is to discuss studies providing information concerning the genetic and environmental architecture of Eysenck's major personality dimensions. The discussion is in two parts. The first mainly involves studies using questionnaires other than the EPQ and related tests. Many of these studies are quite old, involve relatively few subjects and employ scales of doubtful validity. Nevertheless, they are of some interest looked at from the point of view of genetic and environmental architecture. The second part involves only studies using the EPQ and related tests. Most of these are recent, involve large numbers of subjects and employ the biometrical approach outlined in the previous section.

4.3.1 Older Studies

Shields' (1962) study of separated MZ twins was discussed in detail in order to illustrate the biometrical approach. Its substantive findings are, of course, of limited value due to the small number of twins involved, especially so far as DZ pairs are concerned. However, in spite of this limitation the questionnaire used was related to the MPI, which makes it of inter-

est in the present context, and certain findings are reasonably secure. Taking the evidence from both separated and unseparated MZ twins as a whole there seems little doubt of a substantial degree of genetic determination and little or no effect of shared family environment V(CE). We demonstrated the adequacy of this simple model for female neuroticism, which showed a quite remarkable consistency considering the small number of twins involved. Even when we include the small volume of the less satisfactory male data, the same general finding holds both for neuroticism and extraversion. The separated and unseparated MZ correlations for neuroticism are now 0.53 and 0.38 respectively, and for extraversion 0.61 and 0.42, the number of pairs being 42 and 43 in each case. Both pairs of correlations are in the reverse order of magnitude required to suggest any effect of shared family environment. Even with some degree of inadequate separation this reverse order makes the conclusion of no V(CE) inescapable. In consequence, twin resemblance can only be due to genetic make-up which must be in the region of 0.5 or 50% of total variation for both traits. When we remember that the reliability of these two scales is only about 0.7, implying an unreliability variance of 30%, then of the remaining 50% within-family environment variation, V(SE), only 20% can be due to stable and enduring features of the environment. Total stable variation is 0.7. Consequently, the heritability, corrected for unreliability, is in the region of 0.5/0.7 or 70%, implying a very high degree of genetic determination.

Jinks and Fulker (1970) subjected all Shields' data to a number of biometrical analyses, and although there were a number of statistical problems expected of such small samples, the findings were broadly those inferred above from a simple comparison of the intra-class correlations. In the case of extraversion, there was some suggestion of a simple form of genotype-environment interaction, in which extraverts showed slightly greater environmental variation among the separated MZ twins, but examination of unseparated pairs failed to confirm the finding. Since in the absence of shared environmental effects the two kinds of twins should interact in the same way, it is unlikely that any appreciable interaction is present in these data.

Only two other studies provide any direct evidence of shared family environmental effects for a trait related to neuroticism. One of these is Newman et al.'s (1937) well-known study of 19 pairs of separated MZ twins. In addition to the cognitive measures in their study, they recorded the number of neurotic symptoms present according to the Woodworth-Matthews Inventory, not only for the 19 pairs of separated MZ twins, but also for the 50 unseparated pairs and 50 pairs of unseparated DZ twins. Although this study has been rightly criticised (Eysenck 1967) for administering this test to children as young as 12 years old, the consistency of its findings with those of Shields' study makes it worth examining.

The intra-class correlations are shown in Table 4.6. As in Shields' study, the separated MZ correlation is actually larger than that for unsep-

Table 4.6. MZ and DZ correlations for the number of neurotic traits in Newman et al.'s (1937) study

Twin type	Correlation	No. of pairs	Information	Model		Expected correlations	
				V(G)	V(CE)	1	2
MZ together	0.56	50	108	1	1	0.59	0.58
MZ apart	0.58	19	43	1	0	0.54	0.58
DZ together	0.37	50	67	0.5	1	0.32	0.28

Estimates Model 1 $V(G) = 0.54 \pm 0.14$
$V(CE) = 0.05 \pm 0.13$
$\chi_1^2 = 0.33 \quad p = 0.6$

Model 2 $V(G) = 0.58 \pm 0.08$
$\chi_2^2 = 0.52 \quad p = 0.8$

arated MZ pairs, again strongly indicating the absence of an effect due to common family environment. In addition, as in Shields' study, there was no direct evidence that factors normally shared by children in the same home, in this case differences in educational opportunity and social status, had any effect on the pair differences of separated twins. These factors correlated non-significantly 0.04 and −0.08 respectively. A suggestion that differences in physical health, which would be more likely to contribute to specific environmental variation, played some part was indicated by a small negative correlation of −0.29.

The general consistency of the DZ pairs is also evident if we fit a two-parameter model (Model 1). Heritability as the standardized V(G) is 54% and V(CE) is a non-significant 0.05. The small χ^2 indicates a very good fit for the model, as does the χ^2 for the reduced model (Model 2) omitting the non-significant V(CE). The reduced model gives an expected value of 0.28 for DZ twins, which suggests that $\frac{1}{2}$V(G) slightly underestimates the degree of genetic resemblance for this group (though non-significantly) and may indicate a very slight degree of assortative mating for this measure. Although the marital correlation is unknown for this particular scale, there is some evidence of a very slight degree of assortative mating for neuroticism from a few small studies (Vandenberg 1972).

The only other study that potentially bears in a direct manner on the importance of V(CE) in personality traits related to extraversion and neuroticism is that of Cattell et al. (1955), involving the Junior Personality Quiz. While the scales in this questionnaire are of low reliability, and information on the second order factors would have been more appropriate for our present purpose, some of the scales relate to extraversion and neuroticism, and the study does include two foster groups – separated siblings and unrelated children reared together, which makes it valuable for assessing the effects of V(CE).

Four scales appear to relate to neuroticism and extraversion. These are, using Cattell's nomenclature, C, or emotional stability, and Q4, nervous tension, which relate to neuroticism; Q3, or uncontrolled vs controlled, a measure of impulsivity and A, reserved vs. outgoing, a measure of sociability, both of which relate to extraversion (Cattell and Beloff 1953).

The study includes data on 52 pairs of MZ twins reared together (MZ_T), 32 pairs of DZ twins reared together (DZ_T), 91 pairs of full sibs reared together (FS_T), 31 pairs of full sibs reared apart (FS_A) and 36 pairs of unrelated individuals reared together (U_T). The data is presented in terms of the variance of pair differences for each group, the between-pair variance being omitted, presumably because between-pair variation was unstable, a feature frequently found in small studies. However, estimates of the total phenotypic variation for 540 individuals are included in the study and provide very stable comparison groups for evaluating the within-pair variances in terms of the parameters in the biometrical model. The observed variances and the biometrical model are shown in Table 4.7.

So far as V(CE) is concerned, the critical comparisons are FS_A vs FS_T, the former variance of pair differences being inflated by V(CE) and U_T vs individual or total variance, the latter being inflated by V(CE), as can be seen from the appropriate rows of the model matrix for these four groups of subjects. At first sight the evidence is somewhat conflicting. In each case the sibling comparisons suggest some V(CE), but the comparison of unrelated individuals reared together and apart indicates a complete absence of this effect for the first three factors, with some indicated, perhaps, in the case of Q3, the measure of impulsivity. The latter comparison is, however, more reliable than the former, due to the very large sample size of the unrelated individuals. In addition, the within-pair variances of FS_A for the first three scales are actually larger than our reliably determined estimate of total population variance, clearly an impossibility on any biometrical model. This anomaly suggests that the small number of FS_A is, overall, more variable than the other groups. This suggestion might explain the rejection of this group by the authors when estimating the total variance in the population (see their Table 3).

With the likelihood of the apparent V(CE) in the FS_A being due to sampling variation,

Table 4.7. Within-pair variances for four childhood (11–15) personality factors (Cattell et al. 1955)

Group	df	Raw-score variances				Model		
		Neuroticism		Extraversion		V(SE)	V(G)	V(CE)
		C	Q_4	A	Q_3			
MZ_T	52	2.55	3.05	2.21	2.51	1	0	0
DZ_T	32	4.09	4.75	2.39	2.71	1	0.5	0
FS_T	91	3.57	4.66	2.40	3.52	1	0.5	0
FS_A	31	4.29	6.16	3.80	4.21	1	0.5	1
U_T	36	4.05	5.72	3.96	3.88	1	1	0
Individuals	539	4.00	5.55	3.31	4.83	1	1	1
Reliabilities		0.52	0.55	0.32	0.47			
Estimates with								
V(CE)	V(G)	1.27 ± 0.52	2.34 ± 0.65	1.24 ± 0.39	1.85 ± 0.42			
assumed zero	V(SE)	2.84 ± 0.44	3.29 ± 0.53	2.08 ± 0.32	2.82 ± 0.33			
Model fit	χ^2_4	1.48	1.44	2.84	4.09			
p level		0.8	0.8	0.6	0.4			
Heritability		0.31 ± 0.13	0.42 ± 0.12	0.37 ± 0.12	0.40 ± 0.09			
Heritability corrected for unreliability		0.60	0.76	1.00	0.85			

it was decided to assess its importance by fitting a two-parameter model involving only V(G) and V(SE) and judging the adequacy of the assumption of zero (V(CE) by the goodness-of-fit of the model. This model was fitted to all four scales, using the weighted Maximum Likelihood approach illustrated in the previous section, the weights being the amount of information contained in each variance (V) as $df/2V^2$. This procedure uses all the available information in an optimal fashion, giving more weight to small variances with large df.

Cattell applied an elaborate system of analysis to these same data, Multiple Abstract Variance Analysis (MAVA), which he sets out in the paper. Unfortunately, conceptual problems (Loehlin 1965) and statistical problems of estimation (Jinks and Fulker 1970) render his analysis quite unreliable.

The results of our analysis are shown at the foot of Table 4.7 and are remarkably satisfactory, given the unreliability of the measures. In all cases a model assuming no V(CE) provides a very good fit to the data, p-values ranging from 0.4 for Q3, where a small amount of non-significant V(CE) seems likely, to 0.8 for the neuroticism measures where none at all is indicated. Raw score heritabilities are all highly significant and range from 0.31 to 0.42. These figures are high if we take into account the authors' estimates of reliability given in Table 4.7 and used to calculate corrected heritabilities in the final row of the table. These range from 60% to 100%, and although with such unreliable data the precise figure is uncertain, it seems fairly certain that the proportion of heritable variation is at least as high as in Shields' study.

Concerning the assumption that the genetic variance of siblings is $\frac{1}{2}V(G)$, the χ^2s indicate that this is entirely reasonable, although comparison of observed variances with those expected, which are not included in the table for the sake of clarity but which the reader may easily calculate for himself, would indicate that within-pair genetic variance is greater than $\frac{1}{2}V(G)$ for neuroticism and less for extraversion. This implies possible non-additivity for the former and some assortative mating for the latter. Since we have no independent evidence of either of these possibilities, since the relevant groups are quite small and since our models fitted extremely well, it is probably wiser to

ignore this statistically non-significant feature of the data. The parsimonious interpretation of Cattell's study is that a simple additive genetic model, involving neither assortative mating nor non-additive gene action and with environmental effects entirely specific to the individual, is entirely adequate to explain the data. Thus all three available studies involving adoption are in perfect agreement regarding the genetic and environmental architecture they reveal.

There are numerous small studies of unseparated MZ and DZ twins, as well as a few studies of parent-offspring resemblance for various measures of neuroticism and extraversion. These have been reviewed elsewhere (Buss and Plomin 1975; Claridge et al. 1973; Eysenck 1967, 1976; Mittler 1971). In general these studies point to some and often a considerable degree of genetic determination, but they are either too small or employ unsuitable tests to permit any useful information to be extracted from them for our present purposes. The unreliability of small studies can be illustrated, for example, by that of Canter, reported by Mittler (1971). This study involved 25 pairs of MZ twins and 29 pairs of DZs. The twins were assessed on Cattell's 16 PF for the broad second-order factor of neuroticism and found to correlate 0.36 and 0.06 respectively. These correlations indicate some degree of genetic determination but a rather poor fit to any biometrical model, the DZ correlation being too low compared with that for MZs. The very same twins assessed for the same trait using the EPI now correlated 0.53 and 0.70 respectively. Now not only is no genetic influence indicated at all, but again no biometrical model can really explain the data adequately, since this time the DZ correlation is much too high. Thus the same people measured on two related tests show two quite different patterns of MZ and DZ correlations – both of which are bizarre! When the same study was augmented later by another 20 or so pairs of twins (Canter 1973), the correlations for the 16 PF factor remained the same, while those for the EPI became 0.37 and 0.23 for MZ and DZ twins respectively, again quite different from the earlier report. Clearly, studies of this kind are quite unable to explore genetic and environmental architecture effectively.

Unfortunately, large studies involving unstandardized tests prove little better. Again a study reported by Mittler (1971) but this time by Brunn, Markkanen and Partanen, involving their own test of neuroticism and a sample of 157 pairs of MZ and 189 pairs of DZ twins, proves little better. The two correlations in this study were a mere 0.28 and 0.21. These equally low correlations, hardly different from each other and implying a heritability of only 14%, appear to reflect mainly the unreliability of the test.

Large studies involving reliable tests tell another story. Loehlin and Nichols (1976) administered the California Personality Inventory (CPI) to 481 pairs of MZ and 312 pairs of DZ twins. Two broad second-order factors identified with Eysenck's neuroticism and extraversion factors were extracted from the test items. Intra-class correlations for the twins on these factors are shown in Table 4.8, together with the simplest biometrical model, that is,

Table 4.8. CPI factors of neuroticism and extraversion for MZ and DZ twins (Loehlin and Nichols 1975)

Twin	No. of pairs	Neuroticism		Extraversion		Model V(G)
		Observed r	Expected r	Observed r	Expected r	
MZ male	197	0.58	0.52	0.57	0.59	1
female	284	0.48	0.52	0.62	0.59	1
DZ male	122	0.26	0.26	0.20	0.30	0.5
female	190	0.23	0.26	0.28	0.30	0.5
Estimate V(G)			0.52 ± 0.03		0.59 ± 0.03	
χ^2_3			2.96		2.26	
p			0.3		0.5	

no V(CE) and DZ genetic resemblance assumed to be $\frac{1}{2}$V(G). This simple model provided an excellent fit to the data, as shown by the non-significant model fit χ^2s, and gave heritability estimates of between 50% and 60%. Assuming a reliability of about 0.75, we obtain corrected heritabilities of 69% for neuroticism and 79% for extraversion. The inclusion of V(CE) in the model resulted in a small non-significant negative estimate of −0.04 and −0.10 for neuroticism and extraversion respectively and non-significant χ^2s for 2 df of 2.83 and 1.23. Both these estimates are non-significant too, and result in implausible negative values. The simplest of biometrical models provides quite a realistic account of this extensive data.

Loehlin and Nichols point out that zero V(CE) appears to be appropriate for most of their personality measures and conclude that it is probably a general feature of the personality domain. However, lest it be imagined that zero V(CE) is an artefact of the twin design, the authors point to substantial V(CE) for the cognitive measures in the very same study. V(CE) is well established from many studies of IQ and other cognitive traits (Fulker and Eysenck 1979).

One small twin study of the Bernreuter measure of neuroticism (Carter 1933) is of interest, because it allows us to combine results from a family study using the same scale (Crook 1937). The latter study provides correlations for all combinations of father, mother, son and daughter which have been reduced to three groups of full sibs reared together (FS_T), parent and offspring together (PO_T) and husband-wife (PP). Reduction of the number of correlational categories was carried out to render the reported correlations, which use the same subject many times over, less dependent on each other and make them suitable for our estimation procedure. Although full independence cannot be achieved by this device the data are probably sufficiently independent for the present purpose. These correlations are shown, with approximate df, together with Carter's data in Table 4.9. Taken at face value, the data once more support the simple biometrical model, the χ^2 goodness-of-fit test indicating a very good fit.

Table 4.9. Familial correlations from two studies: Carter's (1933) twin study and Crook's (1937) family study for the Bernreuter measure of neuroticism

Type	Observed correlations	No. of pairs	Model V(G)	Expected correlations
MZ_T	0.63	55	1	0.62
DZ_T same sex	0.32	44	0.5	0.31
DZ_T opposite sex	0.18	34	0.5	0.31
FS_T	0.25	101	0.5	0.31
PO_T	0.31	134	0.5	0.31
PP	0.07	79	0.0	0.00

Estimate V(G) = 0.62 ± 0.07
Model fit χ^2_5 = 1.43
p = 0.9

Turning to psychoticism, there is little real evidence available from studies other than those using the EPQ, since the scale is a relatively new one (Eysenck and Eysenck 1976). However, some supportive evidence from studies using the Minnesota Multiphasic Personality Inventory (MMPI), which attempts to measure psychotic tendencies, and from studies of schizophrenia is available.

So far as the MMPI literature is concerned, it does little more than demonstrate the presence of a genetic effect. McClearn and DeFries (1973) combine data on 120 pairs of MZ and 132 pairs of DZ twins from three separate studies (Gottesman 1963, 1965; Resnikoff and Honeyman 1967), and the intra-class correlations they obtain for the ten main subscales are given in Table 4.10.

While the MMPI is not related in any obvious way to the EPQ, psychoticism scales 4, 5 and 6 probably reflect something of the same trait. These have average correlations of 0.40 and 0.20 for MZ and DZ twins respectively; equal, in fact, to the average of all ten scales. Combining correlations in this way implies a surprisingly good fit for the simple biometrical model where we predict, in the absence of V(CE) and $\frac{1}{2}$V(G) for DZ twins, that the MZ correlation should equal twice that for DZs, which is precisely the case. It is not worth calculating any statistics, therefore, since the χ^2 goodness-of-fit test would be zero, with a probability of 1.

Table 4.10. MZ and DZ correlations for ten subscales of the MMPI. Combined data from three studies (McClearn and DeFries 1973)[a]

Item	MZ correlations (n = 120 pairs)	DZ correlations (n = 132 pairs)
Social introversion	0.45	0.12
Depression	0.44	0.14
Psychasthenia	0.41	0.11
Psychopathic deviate	0.48	0.27
Schizophrenia	0.44	0.24
Paranoia	0.27	0.08
Hysteria	0.37	0.23
Hypochondriasis	0.41	0.28
Hypomania	0.32	0.18
Masculinity-Femininity	0.41	0.35
Mean correlations, 1–10	0.40	0.20
Mean, 4, 5 and 6	0.40	0.20

Estimate of V(G) $= 0.40 \pm 0.13$
V(CE) $= 0.00$
Scale 5 V(G) $= 0.45 \pm 0.06$
with V(CE) fixed $= 0$

[a] In both cases the fit for both mean correlations is perfect, the expected correlations equalling the observed ones exactly.

The schizophrenia scale, taken alone, suggests a very similar picture, with V(CE) appearing to be of negligible importance, a finding consistent with much of the adoption literature on schizophrenia.

The best known of the adoption studies and the one with the most clear-cut result is that of Heston (1966), in which 47 children born to schizophrenic mothers were removed at birth. These children were reared in institutions, by adoptive parents or by the paternal side of their families. In no case was there any subsequent contact between the child and its natural mother. The control subjects comprised 50 children closely matched for experience following separation from their mothers. When examined during their mid-30s, five of the children from schizophrenic mothers had become schizophrenic themselves. Not one of the control group had developed the disorder.

The incidence of schizophrenia in the experimental group, 11%, is very similar, if not a little higher than the incidence among unseparated children of affected mothers, clearly suggesting little or no effect of home environment, at least so far as the influence of mothers is concerned. Since the population incidence of schizophrenia is around 1%, a degree of genetic transmission is clearly suggested. More recent adoption studies, which have been reviewed by DeFries and Plomin (1978), indicate much the same picture.

The literature demonstrating a degree of genetic determination for schizophrenia is vast. In fact, schizophrenia is probably the most extensively investigated human characteristic in the whole of genetics, intelligence coming a close second. Slater and Cowie (1971) provide a good summary of this research, and Gottesman and Shields (1976) review more recent adoption, twin and family studies.

In outline, the case for a genetic component rests on the fact that the more closely someone is related to a schizophrenic individual, the higher his own risk of becoming schizophrenic. These risks are very roughly of the order of 30–50% for MZ twins and 5–15% for DZ twins, siblings, parents and children, all of whom are genetically related to the same degree. The risk is even lower for more distantly related relatives. For the population as a whole the risk is only 1%. In the absence of a family environmental influence, demonstrated by the adoption studies, genetic transmission is clearly indicated.

From the point of view of genetic architecture these concordance rates are difficult to interpret, although polygenic control, that is, control by many genes, is indicated, since concordance rates are much higher in families where the index case is more severely affected, these being the families with more deleterious genes than those involving milder cases.

If the disorder is polygenic we can carry out a biometrical analysis by first converting the concordance rates to correlations, these correlations reflecting familial resemblance in liability to the disorder. The statistical details are quite complex (Reich et al. 1972), but a simple account is given in Fulker (1979). The method may be understood, in principle, if we consider an analogy with stature. Suppose we wanted to estimate the sibling correlation for stature. We would measure each individual's height and

calculate the intra-class correlations for sibs in the usual manner. Now suppose we only know whether individuals are tall or short according to an arbitrary criterion of, say, 6 feet. If we knew the frequency of all three kinds of sibs, tall-tall, tall-short and short-short, we could use the tetrachoric correlation (McNemar 1969) to estimate from these proportions what the correlation would have been had we used the conventional approach. In fact, if we only knew the first two categories and the proportion of tall people in the general population we could still estimate the correlation for stature. This is the situation in the typical family study of schizophrenia, tall corresponding to schizophrenic and concordance corresponding to the number of tall individuals with tall brothers. What we are assuming in this approach is that there is a continuous distribution of liability to schizophrenia in the general population, but only if an individual has sufficient liability will he manifest the disorder and be diagnosed schizophrenic. Gottesman and Shields (1967) used this approach in order to estimate the heritability of liability to schizophrenia, and Fulker (1973) used it to fit a biometrical model to data on unseparated MZ and DZ twins and separated parents and offspring. The raw data on which the analysis was based is shown in Table 4.11.

An attempt was made to include only studies using a fairly uniform diagnosis and involving reasonably adequate sampling. Concordance rates within the three categories were shown to be homogeneous and combined. The corresponding tetrachoric correlations were computed and are given at the foot of the table. These correlations were then subjected to biometrical analysis using the now familiar simple model, the results being shown in Table 4.12. Again this simple model was entirely adequate and heritability was high, being in the region of 70%.

We have discussed studies of the inheritance of traits related to Eysenck's personality dimensions from a biometrical point of view, restricting the discussion to studies that provided either direct evidence concerning family environment or were sufficiently reliable to provide a reasonable test of the biometrical model. The overwhelming finding was a complete absence of evidence for family environmental effects and good support for the simplest of additive genetical systems, with heritability being, in general, high.

This genetic and environmental architecture can provide us with some insight into the evolutionary and social significance of these traits.

Table 4.11. Concordance rates for recent schizophrenia studies

Source	Concordance		
	MZ_T	DZ_T	PO_A
Gottesman and Shields (1966)	10/24	3/33	–
Harvald and Hauge (1965)	2/7	3/59	–
Tienari (1963)[a]	1/16	–	–
Kringlen (1966)	17/55	14/178	
Heston (1966)	–	–	5/47
Rosenthal et al. (1968)	–	–	3/54
Heterogeneity χ^2	3.64 (ns)	0.57 (ns)	0.77 (ns)
df	3	2	1
% Concordance	29.41	7.41	7.92
Tetrachoric correlation based on p=1.14%	0.76 ± 0.04	0.35 ± 0.01	0.37 ± 0.02

[a] Given in Slater and Cowie (1971). p denotes population incidence (Fulker 1973)

Table 4.12. Simple biometrical model of liability to schizophrenia

Type of family	Correlation	Model (V(G)
MZ_T	0.76	1.0
DZ_T	0.35	0.5
PO_A	0.37	0.5

Estimates $V(G) = 0.73 \pm 0.06$

$\chi^2_2 = 1.64 \quad p = 0.4$

Traits for which high expression has been vital for survival, and have therefore been under strong natural selection for extreme expression during the evolutionary history of the species, are usually associated with gene action of a strongly non-additive kind. This gene action involves either directional dominance or some other form of interaction favouring high expression of the trait. A good behavioural example of this kind of trait is IQ, which almost certainly contributed to man's evolution from the apes, and, together with other measures of cognitive ability, shows considerable directional dominance toward high expression (Fulker and Eysenck 1979). Traits for which an intermediate optimum exists will have been subject to natural selection against extremes, a form of selection known as stabilizing selection. These traits are characterized by mainly additive gene action, this kind of genetical control producing more optimal intermediate individuals in the population than produced by directional dominance. The arguments relating types of gene action to biological fitness are given by Mather (1953).

Our finding of an additive genetic system thus argues for an intermediate optimum for the major dimensions of personality (or for the maladaptive nature of extremes), a picture that seems intuitively reasonable.

The finding of no common family environmental influence is of importance, since it helps focus on appropriate theories of personality development. However, it probably also indicates that an intermediate optimum is recognized as socially desirable as well as being biologically so, since it demonstrates that parents do not attempt to encourage any particular personality type. This finding is again in contrast to that for cognitive traits, where family environmental effects are quite strong.

The absence of evidence for strong assortative mating, also in contrast to the picture for cognitive traits, suggests men and women may choose a mate to complement their own personality rather than choose someone like themselves. This process would also lead to an increase of frequency of intermediate optimal individuals in the population. These matters are discussed in more detail in Fulker (1979).

4.3.2 Studies Involving the EPQ and Similar Questionnaires

Apart from a few small studies involving earlier versions of the EPQ, the most informative studies of Eysenck's major personality dimensions have been carried out in a series of recent studies by Eaves, Eysenck and collaborators, using the extensive Institute of Psychiatry Twin Register.

These studies have all used the analytical methods and notation of the Birmingham School of Biometrical Genetics (Mather and Jinks 1971). The methods have been described and demonstrated in this chapter. The notation used was an obvious one in terms of V(G), V(CE) and V(SE). This notation was adopted for the sake of clarity and consistency with other elementary accounts of the approach (Fulker and Eysenck 1979). However, the notation adopted by the Birmingham school is slightly different, although the two are completely equivalent. Since their notation is used in all the published accounts of the studies to be discussed in the following section, it is necessary to introduce it at this stage. The two notations are set out alongside each other in Table 4.13. The environmental components of variation, E_1 and E_2 are equivalent to V(SE) and V(CE) respectively. V(G) is equivalent to $\frac{1}{2}D_R$. The reason for calling additive variation $\frac{1}{2}D_R$ is as follows: D refers to the variance of additive *differences* between homozygous forms in a two-allele system. The subscript R indicates that mating is at random. The coefficient, $\frac{1}{2}$, arises because only half the maximum possible variation from homozygous combinations can arise in a random mating population.

In terms of a two-allele system *A*, *a*, there are two homozygotes, AA and aa. Half the dif-

Table 4.13. Equivalent biometrical notations

	Present notation	Birmingham notation
Additive genetic variance	V(G)	$\frac{1}{2}D_R$
Common family environment	V(CE)	E_2
Specific or within-family environment	V(SE)	E_1

ference between these is defined as the additive effect d_a. By simple algebra it can be shown that the variance of these effects in a random mating population, in which the two alleles are equally frequent, is just $\frac{1}{2}d^2$. The half arises because half the individuals will be heterozygous, *Aa*, and have no additive effect. If several loci operate, the additive variance will be $\frac{1}{2}\Sigma d^2$, summing all the individual genetic effects. We denote $\frac{1}{2}\Sigma d^2$ by $\frac{1}{2}D_R$. Random mating and the random nature of the genetical system during the reproductive process allow us to add these separate d^2 effects, since the random processes guarantee independence.

When we do not have equal gene frequencies and the frequency of *A* is *u* and the frequency of *a* is *v*, the additive genetic variance, $\frac{1}{2}D_R$, becomes $2\Sigma uvd^2$, which reduces to $\frac{1}{2}\Sigma d^2$ when $u=v=\frac{1}{2}$. The presence of dominance further complicates the expression somewhat, since it now includes dominance deviations, denoted by h to refer to the effect of a *heterozygote*. Now the expression of $\frac{1}{2}D_R$ becomes $2\Sigma uv\{d+(v-u)h\}^2$, which again reduces to $\frac{1}{2}\Sigma d^2$ when $u=v$. In the presence of dominance the total genetic variance is increased by $\frac{1}{4}H_R$ becoming

$$\tfrac{1}{2}D_R+\tfrac{1}{4}H_R$$

where $\frac{1}{4}H_R=4\Sigma u^2v^2h^2$, an expression reflecting only dominance variation. The effect of assortative mating is to further augment the genetic variance by a quantity $\frac{1}{2}D_RA/(1\text{-}A)$, where A is the genetic correlation of additive effects in the spouses, brought about by the assortative mating.

With fairly straightforward genetic designs all these sources of variation can be separated in human populations (Jinks and Fulker 1970). However, for our present purpose, although we will be referring to dominance and assortative mating, it will generally be sufficient to remember the simple conversions in Table 4.13, where, in the absence of dominance and assortative mating, $\frac{1}{2}D_R$ is equivalent to V(G) and is understood to be $2\Sigma uvd^2$ in terms of gene action.

Details of the tests used in the recent studies will be found elsewhere in this volume. Briefly, the earlier studies used the PEN, a questionnaire including a new scale, psychoticism, as well as the extraversion and neuroticism scales taken largely from the earlier MPI. Development of the PEN resulted in the 80-item Personality Inventory, the PI. This inventory was used in a number of genetical studies. Further development led to the most recent measuring instrument, the Eysenck Personality Questionnaire, or EPQ. The EPQ has been used in the most sophisticated studies in the present series.

The subjects of these studies are the twins on the Volunteer Twin Register originally set up at the Institute of Psychiatry in 1969. The register now includes about 1000 pairs of adult twins aged 18 to 84 years, and a smaller sample of about 200 juvenile pairs under 16 years. In addition to the twins, the register now includes spouses, parents, grandparents, uncles, nieces, etc., as well as a few hundred adopted individuals. These more extensive groups have yet to be fully exploited in the published research.

Zygosity determination of the twins is by means of two questions regarding physical similarity and the extent to which people mistook one for the other during childhood. These questions are those that Cederlöf et al. (1961) found resulted in 97% accuracy when classification was confirmed by blood grouping. A small blood-group study using 178 pairs of MZ and DZ twins, drawn from the present register, found that the questionnaire method of diagnosis resulted in only 3.9% mis-classification (Kasriel and Eaves 1976).

The first major study employing the twin register was published in 1975 (Eaves and Eysenck 1975) and was concerned with extraversion measured by means of the 80-item Personality Inventory. The paper presents an exhaustive analysis of two subscales of extraversion, sociability and impulsiveness, using the biometrical approach. It included a detailed examination of underlying assumptions and set a new standard for the analysis of twin data. In all 837 twin pairs, both male and female MZ and DZ pairs, as well as opposite-sex DZ pairs, were involved. These five twin groups were examined for homogeneity of mean scores before analysis and for homogeneity of variance as part of the subsequent genetical analysis. It was concluded that the groups were homogeneous and the

Table 4.14. Biometrical analysis of extraversion (Eaves and Eysenck 1975)

Twin type	Item	df	MS	Model	
				D_R	E_1
MZ_f	B	330	4.22	1	1
	W	331	1.53	0	1
MZ_m	B	119	3.48	1	1
	W	120	1.44	0	1
DZ_f	B	197	2.88	0.75	1
	W	198	2.26	0.25	1
DZ_m	B	58	3.63	0.75	1
	W	59	1.82	0.25	1
DZ_{os}	B	128	2.96	0.75	1
	W	129	2.47	0.25	1

Parameter estimates $\frac{1}{2}D_R = 1.12 \pm 0.23$
$E_1 = 1.53 \pm 0.09$
Model fit $\chi^2_8 = 7.05$
$p = 0.5$
Narrow heritability h^2_N defined as
$\frac{1}{2}D_R/(\frac{1}{2}D_R + E_1) = 0.42 \pm 0.09$
Corrected for unreliability $= 0.57$

twins could be considered representative of a single population.

Subscale variances were standardized to unit variance, using the average within-group standard deviation as an appropriate base. The mean squares, between (B) and within (W), were calculated for each of the five groups, using the total extraversion score, defined as the sum of the scores on the two subscales and shown in Table 4.14. Sex of subjects is indicated by subscripts *m* for male, *f* for female and *os* for opposite sex.

The two parameter in our simple biometrical model, additive genetic variance, $\frac{1}{2}D_R$, and within-family environmental variance, E_1, were estimated from the mean squares. The result is clear cut. The non-significant χ^2 demonstrates the adequacy of the simple additive genetic model and the assumption that common family environment, E_2, plays no part in determining the trait. The heritability corrected for unreliability is 57%, close to the values we have been finding in previous studies.

The study went on to look at the genetic architecture of extraversion in more detail by analysing individual differences in the trait profiles of the two subscales, sociability and impulsiveness. Statistically this was achieved by subjecting subscale-difference scores to the same form of analysis as the total scores just described. These difference scores reflect individual differences in the balance of sociability and impulsiveness that go to make up a total score and formally refer to subject × subscale interaction within the framework of ANOVA. Twin resemblance among these difference scores indicates similarity in the balance of sociability and impulsiveness in their extraversion scores, and the genetical component in this similarity indicates the extent to which this balance is under genetic control. Once again, the simple biometrical model was adequate ($\chi^2_8 = 8.76$ $p = 0.35$), and the variance estimates were 0.43 ± 0.05 for $\frac{1}{2}D_R$ and 0.84 ± 0.05 for E_1, giving a heritability of 0.34 ± 0.04.

The authors go on to discuss the relationship between sociability and impulsiveness from the point of view of the genetic and environmental components in the phenotypic correlation between the two subscales. In order to do this, the ten mean squares in Table 4.14 were replaced by ten 2×2 mean cross-product matrices representing the between and within variances and covariances for the two measures. Formally the computation of these cross-product matrices follows that of analysis of covariance (ANCOVA), and the simple biometrical model is extended to include genetic and environmental components of variance and covariance. A more extensive example of this form of analysis will be described later.

The analysis resulted in unreliability-corrected genetic and environmental correlations of 0.42 and 0.66 respectively. These correlations suggest that the unitary nature of extraversion, so far as these two subscales are concerned, owes more to environmental influences than to genetical ones. This finding is in agreement with an earlier multivariate analysis of PEN items (Eaves 1973), employing a discriminant function approach (Bock and Vandenberg 1968).

The first analysis of the EPQ scales was published shortly after the above study and involved the psychoticism scale (Eaves and Eysenck 1977). This scale, which relates to irrational fears, lack of sensitivity and disregard

for social conventions, not surprisingly displays a very skewed distribution of responses, the majority of people producing a low rate of endorsement of the items. Thus, although the full range of the scale is 22 items, mean male and female responses are only around 3 and 2 respectively. In addition, the unreliability variation varies a great deal along the scale, being much higher for high P subjects. Thus one form of environmental variation, unreliability variation, a component of E_1, increases with genetic predisposition; one of the simple forms of genotype-environment interaction we discussed in the introductory section.

In order to remove this scalar G × E interaction, the authors used a simple square root transformation of the proportion of items endorsed. As a result, the transformed scores no longer displayed heterogeneity, rendering them suitable for biometrical analysis. While transformation of raw test scores may appear at first sight somewhat undesirable, provided the transformation facilitates the detection of additive relationships in other studies (a sensible requirement of a useful psychometric scale), the transformation is preferable to the raw score. Provided the convention is adhered to generally in other studies, there need be no confusion.

The psychoticism data and its analysis is given in Table 4.15 for the simple biometrical model. Once again a satisfactory fit is obtained with no suggestion of E_2 variation.

With the square root transformation used in this study an estimate of expected error variation may be calculated. Correction of the heritability estimate for this source of variation produced the high value of 81%.

In the same paper the authors illustrate a competitive model in which the behaviour of one sib is allowed to affect the other's, either directly or indirectly through the unequal allocation of limited resources within the family. The data are derived from male psychoticism scores obtained from an earlier study using the PEN, the effect being absent in females. This kind of model is suggested by inflated total variation for DZ pairs, and a similar model was suggested by Jinks and Fulker (1970) for extraversion in Shields' data. Unfortunately, unequal variances can also arise from poor sampling

Table 4.15. Biometrical analysis of psychoticism (Eaves and Eysenck 1977)

Twin type	Item	df	MS	Model	
				D_R	E_1
MZ_f	B	240	0.0339	1	1
	W	241	0.0141	0	1
MZ_m	B	78	0.0513	1	1
	W	79	0.0138	0	1
DZ_f	B	132	0.0423	0.75	1
	W	133	0.0212	0.25	1
DZ_m	B	50	0.0307	0.75	1
	W	51	0.0201	0.25	1
DZ_{os} [a]	B	72	0.0362	0.75	1
	W	72	0.0218	0.25	1

[a] W for this group is corrected for the mean sex difference

Estimates $\frac{1}{2}D_R = 0.0136 \pm 0.0016$
$E_1 = 0.0142 \pm 0.0012$
Theoretical unreliability variance $= 0.0110$
$\chi^2_8 = 9.81$
$p = 0.3$
$h^2_N = 0.46 \pm 0.06$
h^2_N corrected for unreliability $= 0.81$

and the authors concluded that we should regard such evidence as merely suggestive unless the finding is confirmed by other studies.

Both adult and juvenile EPQ measures of neuroticism and extraversion have been analysed by Eaves (1978). The paper discusses a variety of technical problems associated with biometrical analysis and explores a number of different genetic and environmental models. Raw scores in this study were subjected to an angular transformation in order to correct the attenuation of variation at either end of the scale. This device retains the rank order of subjects and also their relative spacing for the most part in all but the extremes of the scale. The resulting mean squares of the ANOVA, multiplied through by Eaves by 1,000, for ease of presentation are given in Table 4.16. Scores were also age-corrected by ANCOVA, hence the loss of an extra degree of freedom for the between-pair mean squares. Again the fit of our simple model was extremely good for both adult and juvenile data on both measures, yielding

Table 4.16. A biometrical analysis of adult and juvenile extraversion (Ex) and neuroticism (N) (Eaves 1978)

Type	Item	Adult			Juvenile			Model	
		df	MS		df	MS			
			Ex	N		Ex	N	D_R	E_1
MZ_f	B	231	109	105	52	59	92	1	1
	W	233	40	43	54	13	37	0	1
MZ_m	B	68	143	129	63	53	93	1	1
	W	70	32	43	65	14	37	0	1
DZ_f	B	123	108	78	41	41	58	0.75	1
	W	125	75	68	43	28	23	0.25	1
DZ_m	B	45	79	71	42	34	63	0.75	1
	W	47	54	62	44	34	63	0.25	1
DZ_{os}	B	66	75	87	80	36	96	0.75	1
	W	67	53	62	81	30	53	0.25	1
Male individuals	V[a]	–			117	43	66	0.5	1
Female individuals	V[a]	–			102	42	60	0.5	1
Estimates	$\frac{1}{2}D_R$		39 ± 4	31 ± 4		21 ± 2	28 ± 5		
	E_1		40 ± 3	44 ± 3		16 ± 2	35 ± 4		
χ^2			10.96	5.25		10.09	12.63		
df			8	8		10	10		
p			0.2	0.7		0.4	0.3		
h_N^2			0.49 ± 0.05	0.41 ± 0.05		0.57 ± 0.05	0.44 ± 0.08		
h_N^2 corrected for unreliability (reliability 0.75)			65%	55%		76%	59%		

[a] phenotypic variance of singletons

fairly high heritabilities in the expected range, especially when corrected for unreliability.

Eaves also considers a number of alternative models that might be fitted to the twin data, models involving E_2, dominance variation and competition effects between siblings. Although the twin design is clearly inadequate to permit a rigorous comparison of these various models, it is clear that in nearly all situations the simple model is the more plausible as well as being, in all cases, the more parsimonious. So far as the twin data are concerned, there seems little doubt that our simple model is entirely reasonable.

However, with twins we are looking at individuals matched for age as well as sex (apart from opposite DZ pairs) and at only two among the many kinds of genetic relationship that can arise in human populations. It is therefore reasonable to ask how stable are the effects of genotype and environment over the wide age range typical of these studies, across sex and across different generations. Eaves attempts to look at a number of these problems concerned with the fine grain of the genetic and environmental architecture.

His most ambitious analysis is an attempt to fit models to neuroticism and extraversion data for subjects covering a wide age range and involving many different genetic relationships. The analysis involves in all the 13 different kinds of relationship to be found in the augmented Twin Register. Some two and a half thousand individuals are included in the study,

distributed among the various groups as shown in Table 4.17.

The method of analysis used was an adaptation of the pedigree approach of Lange et al. (1976), which allows maximum likelihood estimates of parameters of the biometrical model to be obtained, taking into account the high degree of overlap among the pairings bound to occur given the variety of relationships shown in Table 4.17. The method is quite complex and involves time-consuming computer optimization, but in its essentials it is quite straightforward. Consider the following simple example, where we have only two kinds of pedigree: a number of cases, say 40, of two parents and their two children, who are themselves sibs, and 20 cases, say, of two sibs reared with an adopted sibling. For each of these two kinds of pedigree we can generate respectively a 4×4 and 3×3 variance-covariance matrix by calculating all possible pairs of mean cross-products among the observations. On our biometrical model we expect these variance-covariance matrices to reflect the following combinations of parameters in the simple biometrical model.

For the 4×4 matrix we expect:

	Parent 1	Parent 2	Sib 1	Sib 2
Parent 1	$\frac{1}{2}D_R + E_1$	0.0	$\frac{1}{4}D_R$	$\frac{1}{4}D_R$
Parent 2		$\frac{1}{2}D_R + E_1$	$\frac{1}{4}D_R$	$\frac{1}{4}D_R$
Sib 1			$\frac{1}{2}D_R + E_1$	$\frac{1}{4}D_R$
Sib 2				$\frac{1}{2}D_R + E_1$

This covariance matrix is symmetrical, the lower part being the same as the upper.

For the pedigree of Size 3 we have an expected covariance matrix of the following kind:

	Sib 1	Sib 2	Foster sib
Sib 1	$\frac{1}{2}D_R + E_1$	$\frac{1}{4}D_R$	0.0
Sib 2		$\frac{1}{2}D_R + E_1$	0.0
Foster sib			$\frac{1}{2}D_R + E_1$

Thus each diagonal term simply reflects the total phenotypic variance $\frac{1}{2}D_R + E_1$ and the off-diagonals the resemblance, expressed as a covariance, expected on our simple model.

Table 4.17. Correlational pairings used in pedigree analysis of extraversion and neuroticism by Eaves (1978)

Relationship	No. of pairs
Spouse	153
Parent	545
Grandparent	57
Uncle (aunt)	314
Great-uncle (aunt)	13
Sibling	418
DZ twin	229
MZ twin	314
First cousin	113
First cousin once removed	32
Foster parent	230
Foster child – natural child	36
Foster child – foster child	22
Total number of individuals	2469
Total number of fostered individuals	340

In this simple case of two non-overlapping pedigrees we could solve for the parameters in the model using the method of maximum likelihood by minimizing the matrix expressions:

$$\chi^2 = \sum_{i=1}^{2} df_i \{\log_e |EC_i| - \log_e |C_i| + \\ + \text{trace } C_i EC_i^{-1} - p_i\},$$

where the two EC_i are the 4×4 and 3×3 expected matrices shown above, with their true numerical values inserted and the two C_i the observed covariance matrices for 39 and 29 df, obtained from the data of the two kinds of pedigrees. The vertical bars stand for the determinants of these matrices and p_i is the size of the matrices, 4 and 3 respectively. The minimization is readily carried out by standard computer routines which automatically produce the most likely parameter estimates, given the two observed C_i. This approach can be used with non-overlapping, balanced pedigrees like those we have described above (Eaves et al. 1978), as will be shown shortly. In addition, the χ^2 expression may also be used in multivariate genetic analysis, which will also be discussed later.

However, the calculation can also be carried out as follows. Suppose we list all 60 individual pedigrees as 40 sets of four scores (x_1, x_2, x_3, x_4) and 20 sets (x_1, x_3, x_3), which we denote

as 60 sets of x_i in matrix notation. Now associated with each pedigree are 60 expected covariance matrices EC_i. In our example, the first 40 are all the same 4×4 matrices and the second 20 the same 3×3 matrices shown above. Now, assuming a normal distribution of scores with overall mean μ, the likelihood function we wish to minimize can be built up one pedigree at a time as:

$$Q = \sum_{i=1}^{60} 1_i = \sum_{i=1}^{60}$$

$$\tfrac{1}{2}\log_e |EC_i| + \tfrac{1}{2}\log_e (x_i - \mu)' EC_i^{-1} (x_i - \mu).$$

Whereas with balanced discrete pedigrees we can use the more compact method previously outlined, this method allows any kind of irregular pedigree to be accommodated, simply by adjusting the EC_i to an appropriate size and form in each case. This is the method devised by Lange et al. (1976), which Eaves used to fit models to the several hundred pedigrees in Table 4.17.

The analysis is one of the most comprehensive ever attempted for a behavioural phenotype and gave valuable new information concerning the gene action of neuroticism and extraversion. A variety of complex models were fitted for both traits. In both cases there was absolutely no indication of any shared family environment, E_2, and for extraversion numerous more complex models failed to give any improvement in fit over the basic random-mating, additive genetic model. Heritability was, however, slightly lower, being 35% compared with around 50% for the twin analyses. Possible age differences between the related individuals used in this analysis, but absent among twins, was responsible for this lower heritability. If genetic expression changes with age, a form of genotype environmental interaction, then genetic resemblance will decline as age differences between relatives increase. Eaves fitted a model of this kind to the extraversion data, allowing for the absolute difference between relatives to increase with the gap in age, and found a small but non-significant improvement in fit, concluding little G × age interaction was present for this trait.

For neuroticism the picture was rather different. Although no E_2 was indicated in any of the models, some form of non-additivity was indicated, and from the goodness-of-fit of the various models it was difficult to decide if this was due to dominance or genotype × age interaction. However, the following twin studies concerned with the consistency of genetic expression at different ages appear to suggest that enough genotype × age interaction might exist to account for the non-additivity found with this trait.

Evidence of a simple form of age × genotype interaction was found for neuroticism by Eaves and Eysenck (1976b), who correlated age with pair scores and pair differences for 402 pairs of MZ twins and 212 DZ pairs. They found that pair means went down for both MZ and DZ twins, reflecting the general tendency for N scores to decline with age. Correlations were -0.25 and -0.19 respectively, both significant at the 1% level and indicating that age results in at most -0.25^2, or 6%, of common variance for twins. This small amount of variation, which would emerge as E_2 in our analyses if ignored, justifies linear age correction of data before analysis. Using a simple test of G × E interaction suggested by Jinks and Fulker (1970), they correlated pair scores and pair differences to see if high N subjects were more or less subject to the effects of the environment. This correlation was an insignificant 0.04 for the MZ twins, indicating a complete absence of this form of linear interaction. The interesting correlations in the present context, however, were those of pair differences with age, which were -0.02 and 0.19 for MZ and DZ twins respectively. Thus while E_1 effects measured by MZ differences remained constant with age, the combined effects of segregating genes and E_1, measured by DZ differences, did show a statistically significant increase. The simplest explanation of this finding is that additional genes controlling neuroticism operate at later periods during an individual's life, a form of G × age interaction. Since we have only detected the linear form of this interaction it is reasonable to assume that other more complex forms of G × age interaction, such as the pedigree analysis would be expected to detect, might exist too. Only a true longitudinal study could establish such effects unambiguously.

Although there are no relevant longitudinal studies, Eaves and Eysenck (1976) were able to look at the stability of neuroticism over a 2-year period by comparing responses to 12 items drawn from the EPQ and an earlier Personality Inventory, both given to the same 441 pairs of twins. Although there was evidence of significant subjects × occasions interaction, indicating genuine changes in neuroticism over the 2-year period, there was no real evidence that twins resembled each other in respect of these changes. This finding, which rules out either a genetic or common environmental explanation of the changes, leaves only specific environmental effect, E_1, to explain them. During this short period of 2 years, then, there was no evidence of the G × age interaction found in the previous studies involving much longer periods of time.

Another approach to the problem of the change in genetical control over time is described by Eaves (1978). Based on the work of Young (1977), it involves analysing data on both adult and juvenile twins as well as data on the parents of the juvenile pairs. The approach combines the parent-offspring relationship, which involves both adult and juvenile EPQ scores, with the two kinds of twin relationship, adult and juvenile, involving only separate forms of the test. Consequently, it is able to examine the stability of the genotypic component in adult and juvenile scores.

This important study is reported in full by Young et al. (1979). The simplest model adopted in this study defines components of additive genetic and E_1 variation for both adult and juvenile EPQ scores separately. These are denoted $\frac{1}{2}D_{RA}$, E_{1_A} and $\frac{1}{2}D_{RJ}$, E_{1_J} for adults and juveniles respectively. In addition, a single genetical covariance is defined, $\frac{1}{2}D_{RAJ}$, to represent the additive genetic effects responsible for parent-offspring resemblance. This parameter reflects the stability of the additive genetic variation across generations, or the extent that $\frac{1}{2}D_R$ changes with age. A comparison of this parameter with its juvenile and adult counterparts, in the form of a correlation coefficient

$$r_G = \tfrac{1}{2}DR_{AJ}\,(D_{RA}\,\tfrac{1}{2}D_{RJ})^{\frac{1}{2}},$$

gives an index of the stability of the additive genetic component across generations.

The analytical approach used was that of discrete balanced pedigrees previously described using three kinds of variance-covariance matrices based on the data for the three different groups in the study. There were five 4 × 4 matrices involving a father, mother and two children, the children being the five kinds of twins (male, female, MZ pairs and male, female and male-female DZ pairs). In addition, there were five 2 × 2 matrices calculated from the pairs of adult twins. Finally, there were two 3 × 3 matrices calculated from mother, father and child data in male and female singleton families.

The specification of the model can be illustrated with reference to one of these matrices, the 4 × 4 matrix for DZ twins given below:

	Mother	Father	DZ twin 1	DZ twin 2
Mother	$\frac{1}{2}D_{RA} + E_{1_A}$	0.0	$\frac{1}{4}D_{RAJ}$	$\frac{1}{4}D_{RAJ}$
Father		$\frac{1}{2}D_{RA} + E_{1_A}$	$\frac{1}{4}D_{RAJ}$	$\frac{1}{4}D_{RAJ}$
Twin 1			$\frac{1}{2}D_{RJ} + E_{1_J}$	$\frac{1}{4}D_{RJ}$
Twin 2				$\frac{1}{2}D_{RJ} + E_{1_J}$

Thus the appropriate adult and juvenile total variances appear in the diagonal, the four parent-child covariances, $\frac{1}{4}D_{RAJ}$, representing the inter-generational cause of resemblance, form four of the off-diagonal elements and $\frac{1}{4}D_{RAJ}$ the juvenile genetic variance in the remaining element, representing the causes of juvenile DZ twin resemblance. Assortative mating is assumed absent, hence the zero mother-father covariance. The other variance-covariance matrices are made up analogously. For MZ twins the covariance between pairs changes from $\frac{1}{4}D_{RJ}$ to $\frac{1}{2}D_{RJ}$, the other elements remaining the same. For singleton families only the first three rows and columns need be employed.

The results of fitting this simple model using the log-likelihood matrix equation given previously is shown in Table 4.18. Taken at face value the three analyses appear to provide a good fit for this simple additive model, particularly in the case of extraversion and psycho-

Table 4.18. Solution of simple biometrical cross-generational model for EPQ scales (Young et al. 1979)

Parameter	Extraversion	Neuroticism	Psychoticism
E_{1_A}	0.035 ± 0.003	0.038 ± 0.003	0.24 ± 0.02
$\frac{1}{2}D_{RA}$	0.037 ± 0.003	0.027 ± 0.003	0.23 ± 0.02
E_{1_J}	0.017 ± 0.002	0.036 ± 0.004	0.25 ± 0.03
$\frac{1}{2}D_{RJ}$	0.021 ± 0.003	0.028 ± 0.005	0.18 ± 0.03
$\frac{1}{2}D_{RAJ}$	0.012 ± 0.003	0.023 ± 0.004	0.07 ± 0.03
χ^2_{72}	73.80	87.20	70.11
p	0.4	0.1	0.5
r_G	0.44	0.84	0.32

ticism. The parameters of main interest, r_G, representing the stability of genetic variation across generations, indicates a very high stability for neuroticism and implies a remarkably stable constitutional basis for this trait. The authors attempted to improve the fit for this trait by introducing additional parameters but found no real evidence of an improvement on the simple additive model with no E_2. An earlier analysis by Young (1977) on a smaller sample found r_G to be 0.67, somewhat less than that found in the present study, but which agrees within expected sampling variation. The extent this correlation deviates from unity indicates the extent of age × genotype interaction and suggests a figure consistent with the non-additivity found by Eaves in his extensive pedigree analysis of neuroticism.

The picture for extraversion shows slightly less stability of the genotypic component with age but still sufficient to regard the two forms of the EPQ to be measuring similar traits at the genotypic level. The instability of the genetic component combined with the absence of G × age interaction in the pedigree analysis suggests some form of unsystematic interaction in which genetic resemblance decreases with age for some individuals but increases for others. This form of interaction would tend to inflate adult E_1, for which there is some evidence in Table 4.18, and may indicate the importance of the interaction between unique life experiences and genetic make-up for extraversion.

The model for psychoticism appears to fit well but indicates a low genotypic stability over time. In the absence of other information this could indicate considerable G × age interaction or merely reflect a low communality between adult and juvenile forms of the test. Being measured by a relatively new scale, this trait is inevitably less firmly based than either of the other two EPQ traits, extraversion and neuroticism.

In spite of the good fit, however, the authors chose to reject the simple model in view of the finding of a significant marital correlation of 0.18 ± 0.04. They went on to fit other more complex models, allowing for certain kinds of assortative mating effects, with a significant improvement in fit. However, they do not feel able to offer more than a tentative conclusion regarding which model is correct, in view of the limitation of the data and the design. One possibility, perhaps, is that the spouse correlation, which, although statistically significant, is nonetheless quite small, merely reflects the effects of one spouse on the other and does not influence the composition of the gene-pool. In this case the simple model of gene action indicated in the initial analysis would still be appropriate.

So far we have considered the genetic and environmental architecture of psychoticism, neuroticism and extraversion in univariate terms, that is, we have considered each trait separately. The finding has been one of striking consistency. No E_2, just E_1, and nothing but additive gene action, perhaps showing some interaction over time, but always highly heritable after correction for unreliability, a picture suggesting a strong constitutional basis for these fundamental personality traits.

The absence of E_2 rules out a host of social influences as likely determinants of individual differences in these traits and leaves only E_1 or environmental influences specific to the individual to require investigation. However, the complete absence of E_2 combined with a substantial genetic constitutional component raises the interesting possibility that much of E_1, after correction for unreliability, might also have a constitutional basis too, a possibility explored in the next section.

At first sight the suggestion may appear illogical, for while genetic factors must clearly originate at a constitutional level, environmental influences necessarily arise ultimately externally

to the organism. However, the effect of the environment may still be to modify the individual's constitution, especially if acting at an early developmental period, perhaps even before birth. Accidental factors affecting neural and hormonal balance during pregnancy, for example, would most likely appear in our model as E_1 effects, rather than in those associated with E_2. The effects of illness may similarly result in constitutional differences and be reflected in E_1 variation.

Of course, simple twin studies and the like cannot directly identify specific environmental causes of variation but merely indicate their presence. However, recent developments in multivariate genetic analysis (Martin and Eaves 1977; Fulker 1978) indicate how some relevant information may be gathered concerning the nature of E_1 through the comparison of genetic and environmental covariance structures.

Multivariate genetic analysis involves extending family studies to include a number of measures simultaneously. Thus in the twin study, for example, in place of between and within mean squares we generate between and within-pair mean cross-product matrices. The computational procedures are the same as those involved in ANCOVA and were briefly touched upon earlier in this section. Subsequently, in place of the univariate genetic and environmental models, multivariate components of genetic and environmental covariation are generated and fitted to the observed mean cross-product matrices.

The approach can be illustrated using part of Eaves et al.'s (1977) data on impulsiveness. This study involved four scales measuring different aspects of the impulsiveness component in extraversion (Eysenck and Eysenck 1977). The components are called impulsiveness in the narrow sense (IMPIN), risk taking (RISK), non-planning (NONP) and liveliness (LIVE). In all, 588 pairs of MZ and DZ twins from the Institute of Psychiatry Twin Register were used, but the mean cross-product matrices and the simple biometrical model for MZ twins on the first two scales only are shown in Table 4.19 in order to illustrate the approach.

In this simple example there are three environmental components, two variances E_{111} and E_{122}, which correspond to our univariate E_1 components for the two measures and one com-

Table 4.19. Illustration of a multivariate biometrical model for two variables in the case of MZ twins only

	Between-pair, cross-product matrix		df	Model	
	IMPN(1)	RISK(2)	231		
1	0.12	0.05		$E_{I_{11}}+D_{R_{11}}$	$E_{I_{12}}+D_{R_{12}}$
2		0.12			$E_{I_{22}}+D_{R_{22}}$
	Within-pair, cross-product matrix				
	1	2	233		
1	0.05	0.02		$E_{I_{11}}$	$E_{I_{12}}$
2		0.06			$E_{I_{22}}$
	Differences between the two cross-product matrices			Estimates of D_{Rij}	
	1	2			
1	0.07	0.03		$D_{R_{11}}$	$D_{R_{12}}$
2		0.06			$D_{R_{22}}$

E_I correlation $=0.2/(0.05\times0.06)^{\frac{1}{2}}=0.37$
D_R correlation $=0.03/(0.07\times0.06)^{\frac{1}{2}}=0.46$

ponent of covariation E_{112}. These components are estimated directly by the within-pair mean cross-product matrix. The between-pair matrix is made up of these E_1 components, plus the three D_{Rij} which may be estimated from the two observed mean cross-product matrices, as shown at the foot of the table. These component covariance matrices can also be rescaled, as shown, to generate genetic and environmental correlation matrices if required. The approach generalizes in an obvious fashion as more variables are added.

When other kinds of twins are included in the study, it is necessary to use a weighted estimation procedure and the matrix log likelihood χ^2 function used by Young et al. (1979), previously discussed in connection with pedigree analysis, is appropriate. Although lengthy in terms of computer time, a great advantage of this estimation procedure is that much more complex models may be written in place of the $D_{R_{ij}}$ and $E_{I_{ij}}$ without involving additional computational effort. Thus the genetic and environmental factor structures may be explored, for example, simply by replacing the $D_{R_{ij}}$ and $E_{I_{ij}}$ with appropriate combinations of factor loadings and specific variances. As a trivial example, but one which illustrates the point, the three $D_{R_{ij}}$ in Table 4.19 could be replaced by a model involving one genetic factor and two specific variances. The appropriate loadings would be (0.17, 0.17) and the specific variances (0.04, 0.03), which by means of the matrix multiplication below would regenerate the D_R matrix:

$$\text{Thus } D_{Rij} = \begin{bmatrix}0.17\\0.17\end{bmatrix}[0.17\ \ 0.17] + \begin{bmatrix}0.04 & 0\\0 & 0.03\end{bmatrix}$$

$$= \begin{bmatrix}0.03 & 0.03\\0.03 & 0.03\end{bmatrix} + \begin{bmatrix}0.04 & 0\\0 & 0.03\end{bmatrix}$$

$$= \begin{bmatrix}0.07 & 0.03\\0.03 & 0.06\end{bmatrix},$$

as given in Table 4.19.

The full analysis of Eaves et al.'s (1978) data was, of course, more complex than this simple example. Their study involved the four scales described above and the mean cross-product matrices corresponding to the five kinds of twins in the Twin Register. The genetical and environmental model chosen was the simple D_R, E_1 model. However, the factor model, chosen after some experimentation, was as follows. One general factor was specified, this having the same genetical and environmental loadings, apart from a multiplication factor (b), reflecting a difference in overall variation for these two components. The specific variances were divided into genetic and environmental components, with it being further necessary to subdivide the genetical specifics into different values for male and female twins.

Table 4.20. Maximum Likelihood estimates for parameters in a genetic and environmental factor model of impulsiveness (Eaves et al. 1977)

Trait	Factor loadings		Specific standard deviations		
	Genetical	Environmental	Genetical		Environmental
			Male	Female	
IMPN	0.16	0.14	0.19	0.18	0.18
RISK	0.16	0.14	0.19	0.00	0.20
NONP	0.12	0.11	0.14	0.12	0.15
LIVE	0.13	0.11	0.25	0.28	0.27

b 1.13 in ratio of genetic to environmental loading

$\chi^2_{83} = 88.50$ $p = 0.3$

The results of fitting this model are shown in Table 4.20, where specific variances are shown as their square roots, that is, as specific standard deviations, for more appropriate comparison with the factor loadings.

In the present context the most interesting finding is that after allowing for specific effects in both genetic and environmental variation, the same factor structure applies to the genetic and environmental covariance components. Since the genetic component necessarily reflects constitutional differences between individuals, this identity of structure also suggests that the causes of the E_1 structure might well be constitutional, too.

A very similar finding emerged from another study of measures related to extraversion, those of Zuckerman's (1974) sensation-seeking (SS) questionnaire (Fulker et al. 1979). The questionnaire involves four subscales. One, Disinhi-

bition (Dis), is concerned with seeking release through activities such as social drinking and party-going. Another, thrill- and adventure-seeking (TAS) is concerned with a liking for dangerous and exciting sports. Experience-seeking (ES) involves seeking novel sensation and unconventional experience mainly in a social context, while boredom susceptibility (BS) is concerned with a dislike of routine activities and dull, predictable people. The study involved 422 pairs of twins drawn from the Institute of Psychiatry Twin Register. Analysis of total scores showed that the simple $\frac{1}{2}D_R$, E_1 model was adequate to explain variation in sensation-seeking (($\chi^2_8 = 8.59$, $p = 0.4$). In the analysis of trait profiles, however, it was necessary to allow the additive variation to interact with sex in order to obtain a satisfactory fit. The problem with the simple model was a very low DZos twin resemblance, the correlation being essentially zero so far as the profile patterns were concerned. This interaction with sex was more complex than that found in the previous study, in that it could only be detected in the opposite sex DZ pairs taking the form of a reversal of loadings for males and females of the same genotype. Fortunately, however, the form of this sex interaction was such that same-sexed pairs would be expected to have identical covariance structures.

Applying the above multivariate approach (Fulker 1978), the analysis of the MZ and DZ male and female between and within mean cross-product matrices, omitting the DZos pairs, showed that the $\frac{1}{2}D_R$, E_1 model fitted well ($\chi^2_{60} = 61.61$, $p = 0.5$). Applying the additional restriction that the genetical environmental covariance structures be the same, apart from specific variation, also gave a good fit ($\chi^2_{62} = 64.63$, $p = 0.3$), with no significant deterioration between the two χ^2s (the difference χ^2_2 being 3.02, $p = 0.2$). Thus, as in the previous example, genetic and environmental covariance structures appeared to be identical, apart from a scalar factor, again indicating a common constitutional basis for D_R and E_1 variation.

EPQ scores were also available for the same subjects, and an attempt was made to relate extraversion and neuroticism to the sensation-seeking scales using a model assuming the same genetic and environmental covariance structures for the uncorrelated factors identified with extraversion and neuroticism. The model was fitted to the mean cross-product matrices, again omitting those due to DZos, with the result shown in Table 4.21 (Fulker 1979).

Table 4.21. Maximum Likelihood estimation of parameters in a genetic and environmental factor model of sensation-seeking, extraversion (Ex) and neuroticism (N) (Fulker 1979)

Trait	Factor loadings				Specific standard deviations	
	Genetical		Environmental			
	1	2	1	2	Genetical	Environmental
Ex	1.37	–	0.54	–	1.88	1.89
N	–	1.52	–	0.32	1.54	2.04
Dis	4.33	2.25	1.70	0.47	0.00	1.47
TAS	2.76	−3.20	1.08	−0.68	0.68	2.37
ES	2.12	−0.65	0.83	−0.14	1.91	1.52
BS	1.92	0.78	0.75	0.17	1.44	1.87
b	2.55	4.74				
	$\chi^2_{145} = 172.46$			$p = 0.06$		

All parameters highly significant except b_2 ($\chi^2_1 = 1.70$ N.S.)

The analysis resulted in a poorer fit once more variables had been included. However, the power of the χ^2 goodness-of-fit test has now increased and the model does appear to account for a substantial amount of the phenotypic variation according to criteria suggested by Martin et al. to be published. Two points of interest emerge from the analysis. First, the assumption of equal genetic and environmental factor structure is reasonable, since dropping the assumption only reduces the χ^2_{134} to 159.33, resulting in a difference χ^2_5 of 14.13 ($p = 0.1$). Again it seems likely that the bulk of phenotypic variation for these traits has a constitutional basis. A second point of interest is that the factor structure is identified much more with extraversion than neuroticism, judging from the small environmental loading for N and the non-significant b_2 indicating the absence of a significant genetic factor for N. Thus we see that genetic and environmental factor analysis represent a

powerful extension of the biometrical method, allowing us to explore the genetic and environmental architecture determining the complex relationships among traits. As we have seen, this information can provide additional insight into the causes of variation in human personality.

4.4 Conclusion

The aim of this chapter was to discuss the genetic and environmental architecture of the major personality dimensions, psychoticism, extraversion and neuroticism from the point of view of biometrical genetics, at the same time introducing the methods to the differential psychologist. The main finding both from the older literature employing related tests and from the more recent literature employing the Eysenck scales overwhelmingly supports the view that the bulk of variation for these traits is controlled by the simplest of all possible genetic and environmental systems, involving only additive genetic variation arising from random mating and environmental variation specific to the individual.

The genetic picture suggests an intermediate adaptive optimum from an evolutionary point of view and the absence of common family environment suggests the social recognition of the importance of this optimum. The high heritability, once unreliability is removed, the absence of common family environment and the similarity of genetic and environmental covariance structures suggests a high, if not total, degree of constitutional determination of reliable individual variation and provides strong support for a biological theory of the origin of these traits.

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Chapter 5

Personality and Conditioning

A.B. Levey and I. Martin

5.1 Introduction

"Considering their dense ignorance of psychology" (J.B. Watson announced to his 1925 audience), "the signers of that document (the American Declaration of Independence) were nearer right than one might expect. They would have been strictly right had the clause *at birth* been inserted after the word equal". Thus, in a lecture entitled, rather quaintly 'Presenting the Thesis that Our Personality Is But the Outgrowth of Our Habits' (Watson 1925), he laid the foundation for several decades of neglect of individual differences in the investigation of human conditioning. If you believe that 'every healthy individual starts out equal' (p. 217) and subsequently develops from 'each unit of unlearned behaviour' 'an ever expanding system of habits' (p. 218) which eventually constitutes his personality, then there is very little point in the investigation of personality variables in conditioning. In short, if personality is entirely the *result* of habit formation, it cannot also *influence* habit formation in any interesting way. Today, we are well aware that behaviour can be studied objectively without recourse to the pejorative attitudes of the early Behaviourists, and individual differences are a respectable, if still neglected, source of variance in the study of conditioning.

In this chapter we examine the fact that individual variation is one of the most characteristic features of human conditioning. We explore the implications of that fact for personality theory and the understanding of conditioning itself, and we argue that the concept of personality offers the most appropriate tool for its investigation. Individual differences are frequently reported in conditioning studies, though their systematic study remains an area of neglect. If a given response parameter, for example orienting behaviour, alertness, level of awareness or reflex sensitivity is found to influence conditioning differentially, the finding is of interest. If in turn that parameter is one aspect of a more global construct or dimension of individual differences, its interest is surely enhanced. The role of personality studies in the conditioning field is to offer a higher order explanation of the effects of these isolated parameters and to remove some components of the extensive array of individual variation from the pool of error variance. Interestingly, the usual subject variables of age, sex and IQ, within the normal range, usually contribute relatively little to conditioning performance. At the extremes of infancy, mental deficiency and senility major sources of variation are found which are not immediately relevant to the concepts of personality. On the whole, however, the concept of personality, that is of a relatively consistent patterning of individual behaviour, appears to offer the most promising approach to the study of individual differences in conditioning. It should be noted in this context that it is not logically necessary for a personality theory to assume the primacy of innate determinants, though the influential theories in the field have tended to do so. It was the use made of the tabula rasa formula by Watson that discouraged research on individual variation rather than the formula itself.

A swift reconnaissance of the field will show its main contours. Pavlovian psychobiology showed an early interest in the typology of individual differences which is still very active but which was largely ignored in the West until fairly recently. Interest in personality dimensions began for Western psychologists with the

work of Spence on the Taylor Manifest Anxiety Scale and of Eysenck on the dimension of extraversion. From the early 50s to the mid-60s this interest centred on a controversy between the two schools which was eventually resolved. Subsequently, but probably not as a result of that resolution, the problem has again been relatively neglected. Recent work on conditioning has tended to involve a growing awareness of the complexity of conditioning phenomena with an attendant interest in fine-grained analysis of its mechanisms, including particularly the role of brain mechanisms. Research has become more empirical, with less interest in all-embracing systems. Recent emphasis on qualitative aspects of responding, including response topography, has partially replaced the older preoccupation with a mere count of responses, and more attention has been paid to cognitive and affective variables in conditioning. Complex paradigms have been introduced and novel theoretical approaches have been developed to explain them. Finally, the interaction of classical and instrumental processes has occupied the interest of a number of investigators and is a promising field in its own right. All these factors have tended on the one hand to distract attention from the study of personality and on the other to make that study considerably more formidable. Needless to say, it is the expectation of students of personality that newer research strategies will lead to the integration of these new findings with personality theory, and some of the current research to be reviewed here will exemplify that trend.

In the following pages influential theories are first outlined in historical perspective and the issues to which they gave rise are then reviewed. We next turn our attention to newer developments, selecting those which exemplify the essential concepts of personality theory and some of the methodological problems involved. In spite of the rhetorical tone of our opening remarks on the neglect of this field, a considerable literature has accumulated which we feel on would not be useful to review in detail. A separate bibliography is appended in addition to the list of text references, which, while not exhaustive, will serve as a guide to the literature. In the final section the issues and concepts are summarized and the argument for the usefulness of personality in conditioning studies is considered in the light of the material reviewed. Our aim is primarily to defend the thesis that personality concepts are useful and perhaps essential in the study of conditioning, rather than to defend a particular theory or present an exhaustive dossier of the evidence in favour of this view.

5.2 Basic Issues: The Major Theories

5.2.1 Pavlovian Typology

Any consideration of individual differences in conditioning must start with the work of Pavlov, and with the observation that the literature on Pavlovian typology is remarkably confusing. Pavlov's work on individual differences extended over 3 decades and inevitably involved changes in terminology and conceptualization. As early as 1910, Nikforovsky produced a systematic study of individual differences in dogs in relation to conditioning performance. Pavlov published his own systematic typology in 1925, which was modified in relatively final form in 1935, though it did not become generally available to Western readers until 1957, with the publication of a further collection of his papers (Pavlov 1957). No attempt will be made to review this development in detail, and we shall concentrate on the final system. An interesting and informative review of the philosophical basis of Pavlov's thinking and development has been presented by Corson and Corson (1976), which includes a description of the typology and draws on original Russian references. The fact that only a small proportion of Pavlov's voluminous writing has been translated into English obviously confounds the difficulty of understanding (Pavlov 1927, 1941, 1955, 1957).

Legend has it that the basic notion of a typology of canine temperament came to Pavlov during the Leningrad flood, when the dogs in his laboratory were exposed to extreme stress. Be that as it may, we know that the basis for the typology was derived from conditioning ex-

periments, from the experimental neurosis paradigm in which the dog is placed in conflict, from observations of behaviour and from response to administration of caffeine. The basic observation was that some dogs were temperamentally excitable, while others tended to become inert under stress. This basic difference was ascribed to the property of excitability of nervous tissue in the cerebral cortex and was conceptualized as the 'strength' of the nervous system. Pavlov's model of the cortex, consistent with that of his contemporaries, was of the highest regulating centre containing specific localization of function. The weak dog was described as timid or 'cowardly' and was noted to be slow in establishing conditioned reflexes. The strong dog, described as 'bold', developed conditioned reflexes rapidly and stabilized them easily under a variety of stimulus situations. The system invoked physiological concepts of excitation and inhibition and their balance to explain these differences. By the term strength was meant the 'working capacity of the cerebral cells' (Pavlov 1957), the amount of 'excitatory substance' available to the nervous tissue and the capacity to react to repeated stimulation without going into a state of inhibition. The term inhibition referred in part to inhibition of behaviour, that is, of motor activity, and its locus was assumed to be the inhibition of activity in the cells of the cortex. Thus two fundamental types were a strong animal, active and excitable and resistant to inhibition, and a weak animal, prone to inhibition and low in excitatory processes. Weak animals for example became inhibited and inert in the situation of the experimental neuroses, while strong animals became excited.

A series of diagnostic tests were used to classify the animals and of these the most important was the resistance of the strong animal to transmarginal inhibition, that is, to inhibition as a consequence of continuous or very strong stimulation above an optimum level. Pavlov believed in the prolonged observation of the intact animal and the classification of a single individual could occupy from $1^1/_2$ to 2 years of periodic testing. Another legend tells us that on a visit to Cambridge in the 1920s the Soviet psychologist Luria was able to classify dogs according to their personality by playing with them in an open field. Whether this story is apocryphal or not, the classification based on behaviour was eventually dropped in the face of mounting evidence that timidity was not associated with speed of conditioning. Interestingly, what must have been one of the first studies of puppies reared in isolation was undertaken in Pavlov's laboratories in 1933 by Vyrzhikhovsky and Maiorov (Pavlov 1955, p 317), who separated the eight members of a single litter into two groups, one of which was reared in complete isolation for 2 years. At the end of this time the puppies reared in isolation showed the behavioural characteristic of timidity but did not differ from their litter-mates in the strength of conditioned-reflex formation.

The final typology envisaged three fundamental properties of the nervous system. Strength, already described, was indexed by the speed of formation of conditioned reflexes, reflecting the availability of excitatory processes. Equilibrium reflected the relative ease of formation of excitatory as opposed to inhibitory conditioned reflexes, such that the balanced individual formed positive and negative conditioned responses (CRs) with equal ease, while the unbalanced formed excitatory CRs more readily than the inhibitory ones. The third property was named mobility and represented the ease with which a given individual switched from positive to negative CR formation. Thus the combination of three basic properties could give rise to eight types, identified by the appropriate conditioning indices. In practice, Pavlov's interest centred on four types, named from the Hippocratic temperaments: sanguine, phlegmatic, choleric and melancholic. Table 5.1 summarises the system of classification. It will be seen that the melancholic type represented the 'weak' dog, regardless of the other two properties. The sanguine type was strong, balanced and mobile and represented the animal most capable of forming conditioned reflexes, of forming positive and negative conditioned reflexes with equal ease and switching readily between them.

It is important to notice that this typology was derived from conditioning studies and referred back to those studies. Pavlov believed that the three properties of the nervous system

Table 5.1. Pavlovian typology

S	B	M	Sanguine (IV)	Extreme excitatory
S	B	I	Phlegmatic (III)	
S	U	M	Choleric (II)	
S	U	I		
W	B	M		
W	B	I	Melancholic (I)	Extreme inhibitory
W	U	M		
W	U	I		

S: strong B: balanced M: mobile
W: weak U: unbalanced I: immobile

were innate determinants of temperament, though his system allowed for modification by the environment. Western psychologists have sometimes equated Pavlovian psychobiology with a stimulus-response behaviourism, and this has led to considerable misunderstanding. He was interested in what we would now call the transaction between the organism and its environment, and his study of conditioning was a study of a self-regulating holistic organism in which the formation of conditioned reflexes secured a progressively finer correlation with the environment. No brief review of the Pavlovian system can do it justice and the foregoing account merely outlines the essentials. The important features were the concepts of inhibition and excitation and of the interaction between them, reflected in the speed, stability and flexibility of conditioned reflex formation. Thus a significant part of Pavlov's contribution to the theory of conditioning was his identification of individual differences and their investigation within a systematic framework.

5.2.2 Modifications of the Pavlovian System

It is evident that a system based on the neurophysiological concepts of the earlier part of the century would eventually stand in need of revision, particularly in view of the discovery of the function of the reticular formation and the role of arousal in CNS functioning. While minor modifications were introduced by Pavlov's followers, the important developments are due to Teplov and Nebylitsyn, working at the Moscow Institute of Psychology, each in turn as director of that institute. The work of Teplov has been translated by Gray (1964) and is available to Western readers. The same author has collaborated with Nebylitsyn (Nebylitsyn and Gray 1972), and in addition some work is available in English (Nebylitsyn 1972a, b). The following outline does not attempt to be complete and merely summarizes the essential features of this work.

Pavlov tended to write of the cortex as a single unit and to speak of sensory stimulation as though each sensory modality obeyed generally the same laws. The work of these two authors extended the Pavlovian concept of functional properties of the nervous system to include differences in the sensory modalities, as represented in the cortex. These were divided into a group of special or partial functions and the more general function attributed to the overall regulatory activity of the CNS (Nebylitsyn 1972). Particular interest was attached to the function of the frontal cortex, of subcortical regions, particularly septal and hippocampal, and the functioning of the reticular activating system (RAS). In addition, the concept was extended to include the regulation of function between the two hemispheres. These developments may be seen in part as a response to the concepts of modern neurophysiology and in part as a result of the extension of Pavlovian concepts to experimental studies of human subjects.

Nebylitsyn also added a dimension of considerable importance and revised the basic concept of the original system. In his experimental work it was noted that some of the Pavlovian predictions did not suffice to explain experimental results. For example, the response to caffeine was not regularly predictive of differences in CR formation. More importantly, the concept of the weak nervous system as being more sensitive to low intensities of stimulation proved to be independent of the speed of formation of conditioned responses under some experimental conditions. Nebylitsyn has reserved the term 'strength' for the former function, that is, the sensitivity to stimulation, and has treated it separately from the formation of new responses.

An example of this approach is the study of simple reaction time to stimuli of varying intensity. It is a general finding in the work of this group that individuals characterized as weak in strength of the nervous system are more sensitive at low intensities, as measured by the speed of reaction to discrete stimuli. Thus for stimuli of 30 dB there is a marked separation between the strong and weak responders. As stimulus intensity is increased, the function relating reaction time to intensity tends to approximate the two groups until they are nearly equal. Figure 5.1 illustrates such as result.

One interesting implication is that while strength of response to stimulation may interact with speed of CR formation, experimental situations can be devised in which this factor is equated, that is, in which the respective groups of subjects are functioning at the same level with respect to strength. Nebylitsyn has used the term 'dynamism' to refer to the factor of speed of CR formation and has presented evidence that it is an independent 'property' of the nervous system. For Pavlov, the intensity of the excitatory process was high in the weak nervous system and this led to the excitability of the weak animal, while the susceptibility to inhibition was regarded as the observe of the same phenomenon. For Nebylitsyn, the dynamism of the excitatory processes, that is, those involved in the formation of response to positively reinforced CSs, is kept independent of the dynamism of the inhibitory process, that is, the formation of responses to negative or inhibitory reinforcement.

In addition, the equilibrium between the two dynamisms has been studied in a manner analogous to the Pavlovian concepts of mobility and equilibrium, that is, to the balance of processes and the capacity to change between them. This is an important conception for the study of conditioning in the context of personality differences, since it allows for separate identification of what we might crudely refer to as 'input' and 'output' components of the organism's behaviour. One consequence of Pavlov's conceptualization of strength was to associate strong excitation with weak inhibition and vice versa. While it is apparent that this formulation is not logically necessary, the assumptions underlying it determine a large part of Pavlovian theory. The logical possibility that strong inhibition and strong excitation may exist in the same individual is in one sense opened by Nebylitsyn's formulation. Again, it is possible to arrange experimental situations in which the equilibrium of dynamism is equated, and the strength function studied in isolation from it. While again this brief outline cannot do justice to an important theoretical framework, it serves to identify the essential features. These include the separate identification of sensitivity levels to stimulus input in various sense modalities, the independent identification of excitatory components of responsivity, an interest in the differentiation of brain function, particularly in relations between RAS, frontal cortex and subcortical structures, and the extension of Pavlovian concepts to human performance.

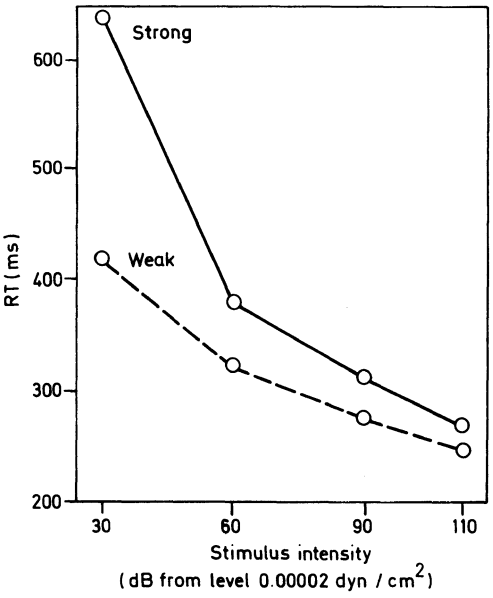


Fig. 5.1. Reaction times as a function of stimulus intensity, comparing strong and weak subjects. (After Nebylitsyn 1972b)

5.2.3 The Personality Theory of Eysenck

The most influential theory relating personality to conditioning in the West is that of Eysenck (1957, 1967), which is described in detail elsewhere in this volume. In the present context,

that is, the study of conditioning, it will suffice to offer one or two comments on the specific features involved in conditioning. It is a common misunderstanding among both critics and supporters of the theory to refer to it as a theory of conditioning which states that introverts condition 'better' than extraverts. In fact, it is a theory of individual differences based initially on the factor analytic study of objective tests and later of questionnaires which yielded two orthogonal personality dimensions, extraversion and neuroticism (Eysenck 1953). Following on the identification of these independent dimensions, deductions were drawn from various theoretical sources to predict the behaviour of introverts and extraverts and of stable and neurotic subjects in a variety of situations. In the case of conditioning (Eysenck 1957), the Pavlovian concept of the balance of inhibition-excitation was invoked, retaining the formula that strong excitation is associated with reduced susceptibility to inhibition and vice versa. The Pavlovian concept was combined with the Hullian notion of reactive inhibition in a formulation which predicted that extraverts would condition less well, as a consequence of their accumulation of inhibition and their greater susceptibility to that inhibition. This formulation, referred to the concept of cortical inhibition, predicted differences in the performance level of introverts and extraverts.

This theoretical framework regarded the cortex as having a mainly inhibitory function, so that the effect of accumulated inhibition was to weaken the inhibitory function of the cortex itself. Thus the introvert functions under stronger inhibitory control by the cortex, while the extravert shows in his uninhibited and impulsive behaviour a relative reduction of cortical control. Conceptual difficulties with the concept of reactive inhibition, together with the theoretical impact of the discovery of the arousal properties of the reticular activating system, led to a change in the description of the basic mechanisms (Eysenck 1967). In behavioural terms, the reduction of activity, susceptibility to boredom and distractibility attributed to cortical inhibition can as well be regarded as a function of under-arousal. Thus, in the new formulation, it was postulated that extraverts have a chronically low level of cortical arousal and are more susceptible to arousal decrement than introverts, who have a relatively high level of cortical arousal, associated with a lesser susceptibility to arousal decrements. This formulation has been applied in a number of substantive areas, both to the processing of stimulus input and the responsivity and availability of motor output.

The typology to which the original factor analytic studies gave rise, and which was incorporated in the explanatory framework, produced four basic types, stable vs. unstable introverts and stable vs. unstable extraverts. Unstable introverts were identified with the dysthymic group of neuroses, while unstable extraverts were identified with the clinical groups of hysteria and psychopathy, and the first of the conditioning studies were done on these two groups.

In summary, the theory of Eysenck proposed a typology of four categories very similar but not identically related to the four major categories of the Pavlovian system. Differences in the introversion and extraversion dimension are attributed to the activity of arousal mechanisms, while those in the continuum from stable to unstable, or neurotic, are referred to the activity of the autonomic nervous system.

5.2.4 The Drive Theory of Spence

The most influential learning theory of the postwar decades was the hypothetico-deductive system of Hull, and his student Spence was the first to give serious attention to individual differences in conditioning. The Hullian system, while paying lip service to Pavlovian concepts, was an essentially different framework, within which two major components contributed to the determination of performance. The first of these, an associative component, Habit Strength, referred to the strength and stability of newly formed associations, and was derived from the habit construct of James and the inhibition concept of Dodge. Hull's achievement was to attempt a rigorous modification of the notion of habit, but it is important to note that where Pavlov's quantification was concerned equally with response amplitude and response

frequency, the Hullian system tended to regard all response parameters as alternative indices of habit strength, with a consequent neglect of the qualitative features of responding which interested Pavlov. The second component of the Hullian theory was a motivational component, Drive, and Hull's well-known formula identified performance level as a multiplicative function of drive and habit strength.

It was to the motivational component that Spence's theory of individual differences was directed. To the concept of drive was added the concept of incentive (Spence 1958), as a refinement of the theory directed particularly at human performance. Within this system he suggested that anxiety, defined essentially as a sympathetic autonomic arousal, is itself a drive. It postulated that drive and incentive components summate in the multiplicative formula, and this carried the implication that drives irrelevant to the task would influence performance. In these terms he predicted that anxious persons would condition more rapidly than non-anxious, a proposition which had previously been demonstrated for simple forms of learning and which was now extended to the conditioning paradigm.

The influence of Spence's theory has waned in recent years, but he is to be firmly credited with drawing the attention of investigators to the relevance and more importantly to the experimental feasibility of using concepts of individual differences in the study of learning. This theory was not intended to be a theory of personality, but was a theory of conditioning performance linked to a single trait, that is, susceptibility to anxiety. A part of the function of Spence's theory was to generate a considerable quantity of research chiefly using the simple paradigms which were current in the era in which he worked, particularly the eyelid-conditioning paradigm. As noted earlier, the interest of a new generation of learning theorists has been directed to more complicated paradigms and to a more empirical analysis of learning behaviour, and Spence's formulation of conditioning as a simple task no longer suffices. Another result of his work was to stimulate a long-lasting controversy between his students and the proponents of Eysenck's theory. Before turning to an overview of this controversy, however, it is necessary to look at an important theoretical contribution which combines elements from both theories.

5.2.5 Gray's Reformulation of the Eysenck Theory

Students of personality and conditioning are indebted to Gray for his considerable service in making available the work of Teplov and Nebylitsyn to Western readers and for clarifying Western understanding of Pavlovian concepts. In addition to this he has also provided his own formulation of the issues, partly in an attempt to encourage research on the integration of Russian and Western theory. The casual similarities between the systems of Eysenck and Pavlov must necessarily tempt speculation as to their degree of overlap, but attempts to map one system onto the other must inevitably raise complexities, which are partly due to the rather different historical and philosophical milieu of each of the theories. Some authors have proposed that Pavlov's sanguine dog, that is, the strong, balanced and mobile individual, corresponds to Eysenck's stable extravert, but a moment's reflection will show that this comparison is not tenable. Gray has performed a more useful exercise in comparing the Eysenck dimension of extraversion with the theoretical formulations of Nebylitsyn, which are more in tune with contemporary notions of nervous system functioning. Wisely, he does not cast his vote but raises two interesting hypotheses. The first hypothesis is that the introvert corresponds to the weak nervous system, while the extravert corresponds to the strong nervous system, using these terms in the sense intended by Nebylitsyn. The second hypothesis is that the introvert displays the predominance of excitation in dynamism and the extravert the predominance of inhibition in dynamism, drawing on the second of Nebylitsyn's fundamental properties. The arguments are presented in full in the original paper (Gray 1967).

The division of the properties of the nervous system into separate components of strength and dynamism by Nebylitsyn makes it apparent that the two are combined in Eysenck's theory.

Obviously, the issues will not be resolved until identical experiments are undertaken in the laboratories of both theorists, but the difficulties in the way of such a project in terms of differences of technique, language, experimental philosophy and laboratory tradition are formidable indeed. An interesting suggestion by Brebner and Cooper (1974) may foreshadow a possible line of development, though the experimental evidence to date is not overwhelming. These authors suggest that excitation be divided into S-responsivity, the processing of stimuli on the input side, and R-responsivity, a function of motor output. They present evidence (Brebner and Cooper 1978) that, under conditions in which inhibition is minimized, the extraverts show higher levels of R-responsivity and the introverts show higher levels of S-responsivity, as monitored by the number of responses given on the one hand and the time taken to inspect stimuli on the other. In their engaging terminology, the introvert is described as 'geared to inspect' while the extravert is described as 'geared to respond'. It is too early to evaluate this line of research, but it is to be hoped that it will be continued.

Returning to the work of Gray, an alternative formulation to that of Eysenck has been offered which incorporates both the drive component of Spence, indexed by the dimension of neuroticism, and the extraversion component. This worker's major interest is in active and passive avoidance learning and susceptibility to punishment and frustrative non-reward, and his theory is addressed in part to these research areas. He suggests that Nebylitsyn's concept of strength is best understood in Western terms by the term arousability. Thus, part of the arousal component in Eysenck's theory is referred to the dimension of sensitivity to stimulation. It describes the introverted personality as 'amplifying' stimulation, and this is consistent with Eysenck's view. He further suggests that the introvert is sensitive in particular to aversive stimulation, that is, to punishment and frustrative non-reward. So far, this leads to predictions which are identical with those of Eysenck. The second assumption is that the neuroticism factor involves sensitivity both to reward and punishment, on the basis of autonomic responsivity. This formulation leads to the prediction that in aversive learning situations, which tend to include most of the common conditioning situations, the individuals who are most susceptible to conditioning will be those who are neurotic and introverted. Our purpose at this point is not to examine data but to outline the major theories, and this prediction will be referred to again in a more appropriate place.

5.2.6 Summary

The foregoing material has presented in outline form the essential features of those theories of personality which are relevant to conditioning in contemporary research. They offer tantalizing similarities and are clearly pointed in the same direction. The conditioning theories of Pavlov and the typology to which they gave rise are perhaps best represented in the more modern versions of Teplov and Nebylitsyn. The theory of Eysenck, derived not from conditioning studies but from the factor analysis of objective tests and questionnaire items, has been used to generate predictions about learning and performance in the conditioning situation. The theory of Spence has been largely subsumed within the formulation of Gray, for reasons which will become apparent. In the following section we describe some of the experimental work, and in this we are confined almost entirely to the Western literature. Although it is to be hoped that the account offered by Nebylitsyn will be tested by Western psychologists, there has been very little work reported to date. At this point it should be noted that while each of the theories refers to conditioning, for practical reasons the evidence is virtually limited to conditioning of the skin resistance response (SRR) and to the eyelid-conditioning paradigm, partly because these have been the principal focus of experimental work in the West.

5.3 Basic Issues: The Period of Aufklärung

It has already been mentioned that one of the effects of the two opposing theories of Spence

and Eysenck was to generate a controversy which lasted well over a decade. At the same time the controversy itself resulted in a clarification of issues, a settling out of the evidence and eventually a resolution of the opposing points of view. It may be useful to review the controversy briefly and to describe some of the findings of this era.

It is probably not unfair to say that the early period of this controversy was marked by polemical tendencies in both the 'London' and the 'Iowa' schools. This is not to be regretted, since it gave rise to a considerable body of information and eventually led to a better understanding of the issues. However, the extreme positions were extreme indeed. Spence claimed that anxiety, and anxiety alone, could account for performance differences between groups of subjects in the eyelid-conditioning situation and that extraversion had nothing whatsoever to do with the case. To buttress this claim, he pointed to the fact that the Maudsley Personality Inventory (MPI), used at that time to index extraversion, tended to yield a correlation between neuroticism and extraversion in the direction of higher introversion scores associated with neuroticism. Eysenck countered by pointing out that the Taylor MAS scale used to index anxiety was in turn correlated with introversion and suggested that this would account for the results relating MAS anxiety to conditioning performance. In the course of a decade, more than 100 studies using eyelid and SRR-conditioning were directed at these issues, and when the polemics had died down the theories were not finally in conflict.

An early study by Spence and Taylor (1951) offers a prototype of the experiments run in defence of the anxiety hypothesis. Undergraduate volunteers were divided into high and low anxiety groups and were conditioned at two levels of unconditioned stimulus (UCS) intensity. The results showed that UCS intensity interacted with anxiety, and the authors argued that this supported the drive interpretation, drawing on the formulations of Hull. The first study investigating the Eysenck hypothesis (Franks 1956) used hysterics, defined as unstable extraverts, and dysthymics, defined as unstable introverts, to demonstrate superior conditioning in the latter group. In the ensuing decade a number of conflicting results were reported, and eventually the theoretical issues were clarified and the evidence reviewed by both theorists (Spence 1964; Eysenck 1965). At this time each of the authors offered score sheets for their theories, together with explanations for failures of confirmation. For Spence, 64% of studies favoured the hypothesis that anxious subjects condition more rapidly than non-anxious, and he pointed out that the failures included situations in which no attempt had been made to induce emotionality. Spence listed the following as characteristics of his experimental situation which were conducive to the production of anxiety: an impersonal manner on the part of the experimenter; the 'impressive' array of electronic equipment visible to the subject; isolation in an experimental cubicle; the use of a dental chair with associated anxiety cues in a conditioning laboratory; the reduction of illumination to a low level; and a generally 'strange situation' for a student subject. This was a genuine clarification of theory, based on the review of studies, since the earlier version had not considered the induction of emotionality as being an essential feature of the experiment.

Spence and Spence (1964) conducted a large-scale study employing 100 undergraduates in which they found, in addition to the predicted effect for anxiety, a positive, but non-significant, relationship between the MPI extraversion scale and the conditioning level in the direction predicted by Eysenck. The pointed out that conditions in their laboratory were not such as to lead to he summation of inhibition and offered this as a partial explanation of their failure to demonstrate the extraversion effect. A further study by the same authors (Spence and Spence 1966) embodied modifications of procedure, which included the use of a distracting task. This study failed to demonstrate the effect of drive on performance. As a matter of historical interest, experiments conducted at Duke University had uniformly failed to fulfil the Spence predictions. Workers at this laboratory (Ominsky and Kimble 1966) responded to Spence's challenge by altering the experimental conditions, reporting that "intuitively the new situation seems more threatening than the old

one". The results dramatically confirmed the Spence prediction, and though subsequent studies (e.g. Hobson 1968) showed that the *deliberate* induction of emotionality in the conditioning situation is not necessarily a prerequisite for the demonstration of drive effects, the weight of evidence was in the direction that superior conditioning of anxious subjects is best predicted in situations in which they are acutely anxious or threatened. This type of evidence is used by Gray (1967) in support of his contention that the highest level of conditioning is obtained with neurotic introverted subjects in a situation of threat.

For Eysenck, the score worked out at 55% in favour of the prediction that introverts would condition at a higher level than extraverts. He noted that in general the favourable studies had been those in which inhibition was likely to be generated. The summary reviews by each of the protagonists specified for each theory the conditions under which it could be accurately tested. For Eysenck these were the provision of experimental conditions such that differential inhibition would be allowed to accumulate, but excluding conditions conducive to massive inhibitory effects which would be inappropriate, since the ceiling effect of maximal inhibition would be expected to obscure the predicted differences. Among the conditions expected to give rise to inhibition when controlled at the appropriate level are weak CS intensity, weak UCS intensity, short CS/UCS interval, use of a discrimination paradigm and of partial reinforcement. In terms of the more recent arousal model these are all conditions under which arousal might be expected to be minimal. The possible exception is the use of partial reinforcement, which in fact leads to high levels of orienting. Spence, as noted earlier, specified the provision of emotion-inducing situations and the elimination of voluntary responders. With regard to the latter point, the American conditioning studies of this era had identified a group of subjects who produced responses of a distinctive topography closely resembling those produced by subjects instructed to blink voluntarily. At this time the policy in many laboratories was to exclude such subjects, though this is no longer the case and the issues will be referred to again. Both theorists agreed that in view of the very large variance in human conditioning, any critical experiment should use appropriately large groups of subjects.

An experiment conducted in the laboratories of the London group (Eysenck and Levey 1967) fulfilled all these requirements and will be described as an illustration of the methods and results to which the clarification of theory by both authors gave rise. Some reference will be made to aspects of the data not previously reported. 144 paid volunteers were tested on the Eysenck Personality Inventory (EPI), the scale which succeeded the older MPI and in which the correlation between introversion and neuroticism had been removed by item analysis. Subjects also filled in the Taylor MAS. Nine experimental groups, consisting of the crossed combination of three levels of extraversion and of neuroticism, constituted the sampling model. The subjects in these groups were assigned at random to one of the eight combinations of three experimental conditions, viz. high (6 psi) UCS air-puff intensity vs. low (3 psi), continuous as opposed to two-thirds partial reinforcement and short (400 ms) vs. long (800 ms) interstimulus interval. The laboratory in which the subjects were tested fulfilled in all respects the description quoted earlier with regard to illumination and facilities, but no attempt was made to deliberately induce anxiety. With regard to voluntary responding, the subjects conditioned in the London laboratories routinely failed to demonstrate the type of voluntary responding in which Spence was interested, i.e. production of a blink whose topography resembled an instructed voluntary blink. This issue has subsequently been refined, and will be referred to again, though the reasons for this failure of voluntary responding to appear in our samples remain obscure. The most persuasive speculation, which in its nature cannot be tested, is that the American studies are almost invariably conducted on undergraduate volunteers, who may have had some inkling of the purpose of the conditioning experiment, while the London group routinely used volunteers drawn from a working situation (e.g. Post Office employees), who were genuinely naive with respect to the purpose of the conditioning experiment. What-

ever the cause, it meant that the proposal that voluntary responders be excluded was unnecessary. SRR responding was monitored throughout the acquisition and extinction series in recognition of Spence's claim that this measure accurately indexes emotionality. Controls included the determination of a threshold to the air-puff intensity, prior to the acquisition series, in order to separately identify the factor of stimulus sensitivity. Results for the main effects are shown in Figs. 5.2, 5.3 and 5.4. The partial reinforcement effect (Fig. 5.2) showed a difference in the expected direction which was not statistically significant. For UCS intensity (Fig. 5.3) and for inter-stimulus interval (Fig. 5.4) the results were in the predicted direction and statistically significant. The chief interest of the data lay in the comparison of the combined effects of the three experimental conditions conducive to inhibition as contrasted with those which were relatively free of inhibition. Figure 5.5 indicates that the difference between groups was dramatic. For the inhibition-producing conditions the extravert group virtually failed to condition, while the acquisition curves for introverts and ambiverts were suppressed, as would be expected of inhibition or under-arousal. The fact that extraverts far exceeded introverts in the number of responses under conditions of low inhibition (Fig. 5.6) had not been predicted and this fact merits some discussion.

Clearly, the results of the experiment favoured the Eysenckian prediction that experimental conditions conducive to inhibition would interact differentially with the personality levels. These results have been quoted from time to time both by supporters and critics of Eysenck's theory. For the latter, the results tend to be reported in the form "Eysenck's theory holds true, *but only under certain conditions*" (italics usual). Supporters tend to confine themselves to the observation "the predictions of Eysenck for the effect of inhibition were upheld". These formulae seem to represent a misunderstanding of the data. Within the confines of the Eysenck theory, and drawing on Pavlovian concepts, the superiority of the extraverts in response frequency relative to the introverts is adequately handled by the concept of transmarginal (supra-optimal) inhibition. At some critical point above optimal stimulation the 'weak' nervous system becomes susceptible to protective inhibition, and this phenomenon was repeatedly demonstrated in Pavlov's laboratory, where it was the principal diagnostic index of the weak nervous system. Nevertheless, it is interesting to reconsider the formulations of Nebylitsyn discussed earlier. It can be seen that the interaction between the property of strength, that is, the susceptibility to stimulus intensity and the property of dynamism, that is, the speed of formation of conditioned reflexes, may both be represented in these figures. It was noted earlier that experimental conditions can be arranged such that either one is held constant, but this would require preliminary investigation of appropriate levels of stimulation using the Russian techniques, and this was, of course, not fulfilled. In the absence of this provision we can nevertheless speculate that the findings for the stimulus conditions associated with under-arousal may have tapped the dynamism component, while those for arousing conditions may have tapped the strength component. While only a very elaborate experiment would confirm or disconfirm this speculation, it again gives rise to the interesting possibility that the Eysenckian dimension of introversion and extraversion contains both components.

A further possibility of interest is raised by the related suggestion of Brebner and his coworkers, mentioned earlier (Brebner and Cooper 1974), that introverts and extraverts can be reclassified in terms of S-responsivity (sensitivity to stimuli) and R-responsivity (tendency to increased motor activity). The authors further suggest that S-excitation and S-inhibition are related states independent of R-excitation and R-inhibition. They propose that the introvert generates higher S-excitation but is more prone to R-inhibition, while the converse is true for the extravert. Brebner and his associates in two experiments involving simple reaction time (Brebner and Cooper 1974; Brebner and Flavel 1978) show greater motor responsivity for extraverts than introverts. In a further study, Brebner and Cooper (1978) report the interesting finding that in a situation in which subjects are given the opportunity of inspecting visual materials which they change by pressing a but-

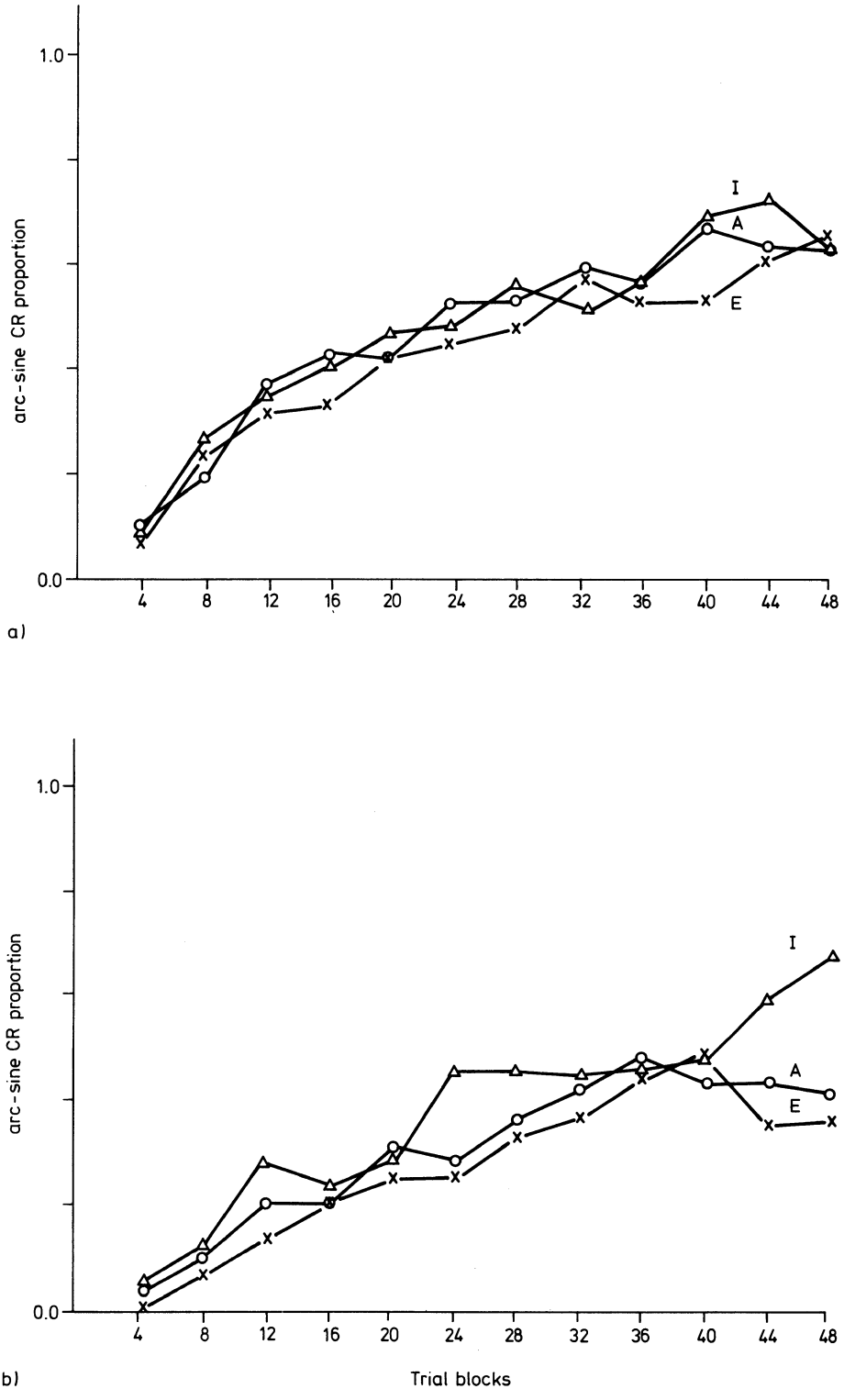


Fig. 5.2. The effect of reinforcement on acquisition: continuous (upper figure) vs. partial (lower figure). Response frequency is expressed as arc-sine proportion for blocks of four trials in this and Figs. 5.3 to 5.6

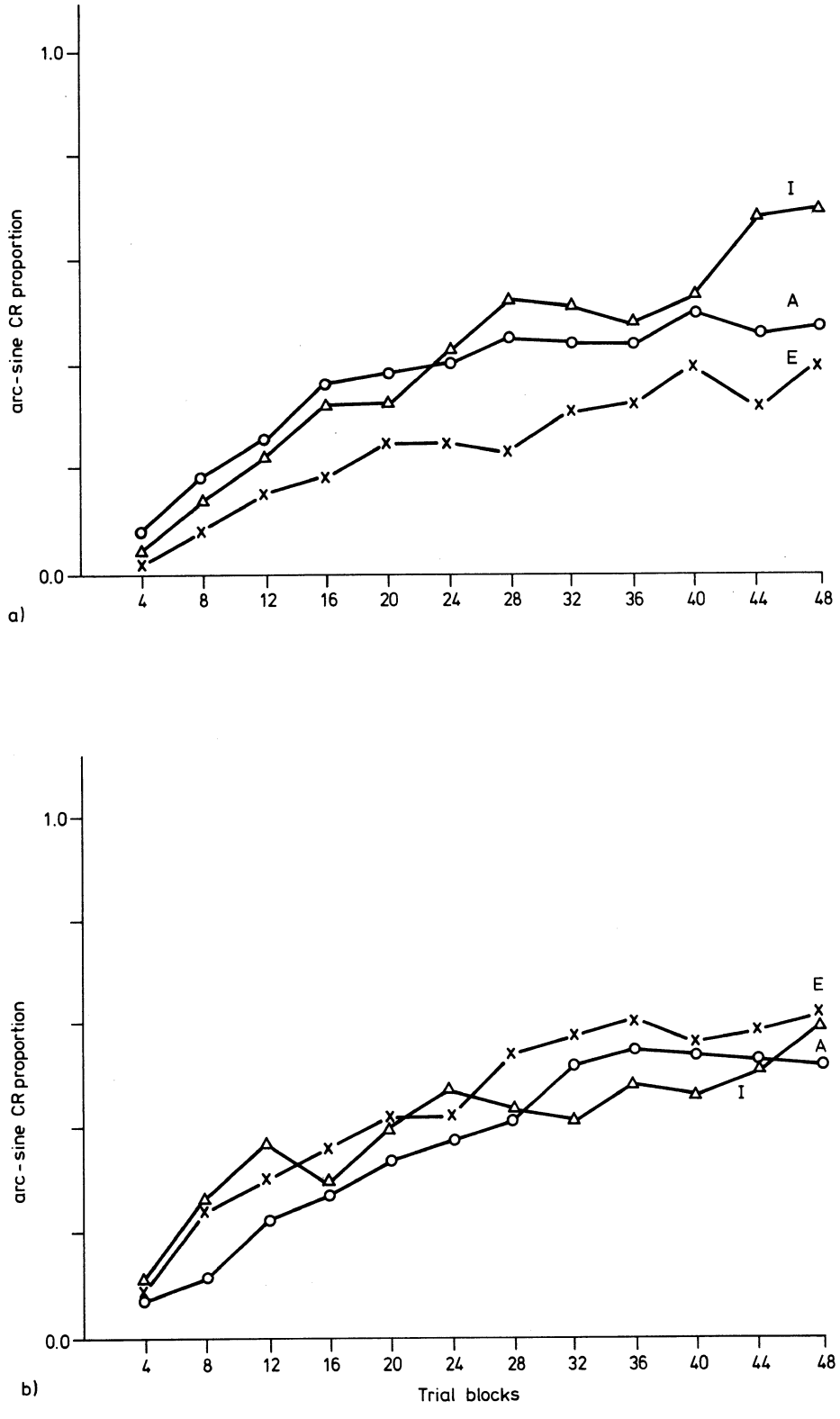


Fig. 5.3. The effect of UCS intensity on acquisition: 3 psi (upper figure) vs. 6 psi (lower figure)

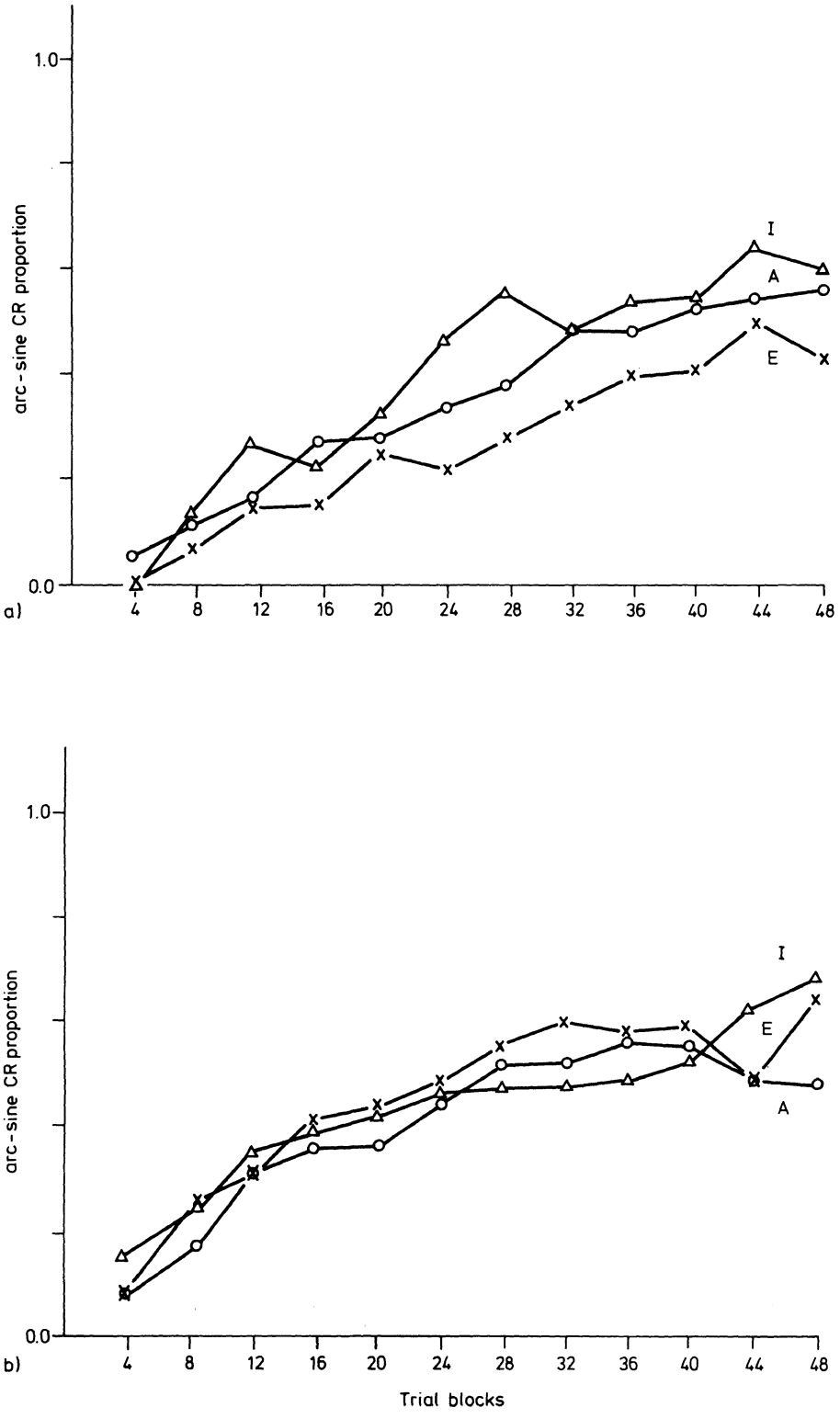


Fig. 5.4. The effect of interstimulus interval on acquisition: 400 ms (upper figure) vs. 800 ms (lower figure)

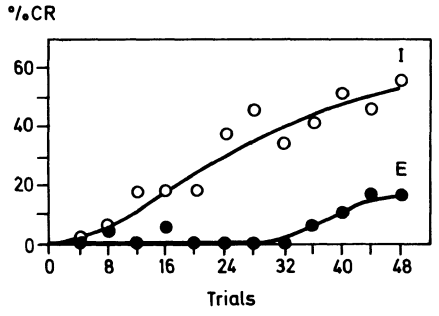


Fig. 5.5. Acquisition curve for weak stimulus conditions (partial reinforcement, short interstimulus interval, weak UCS). (After Eysenck and Levey 1967)

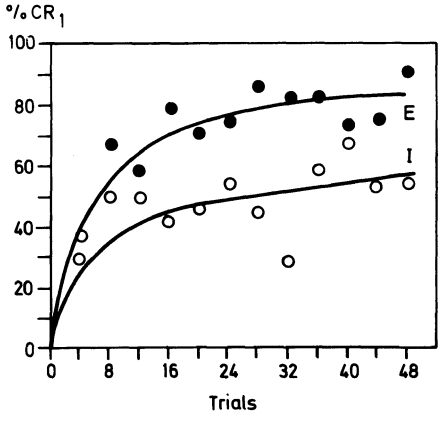
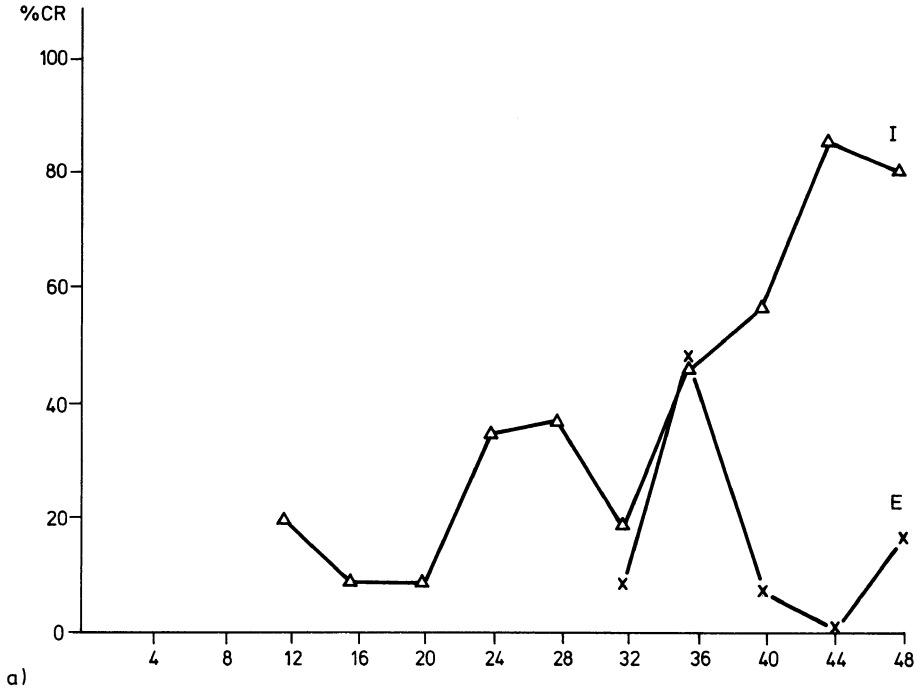


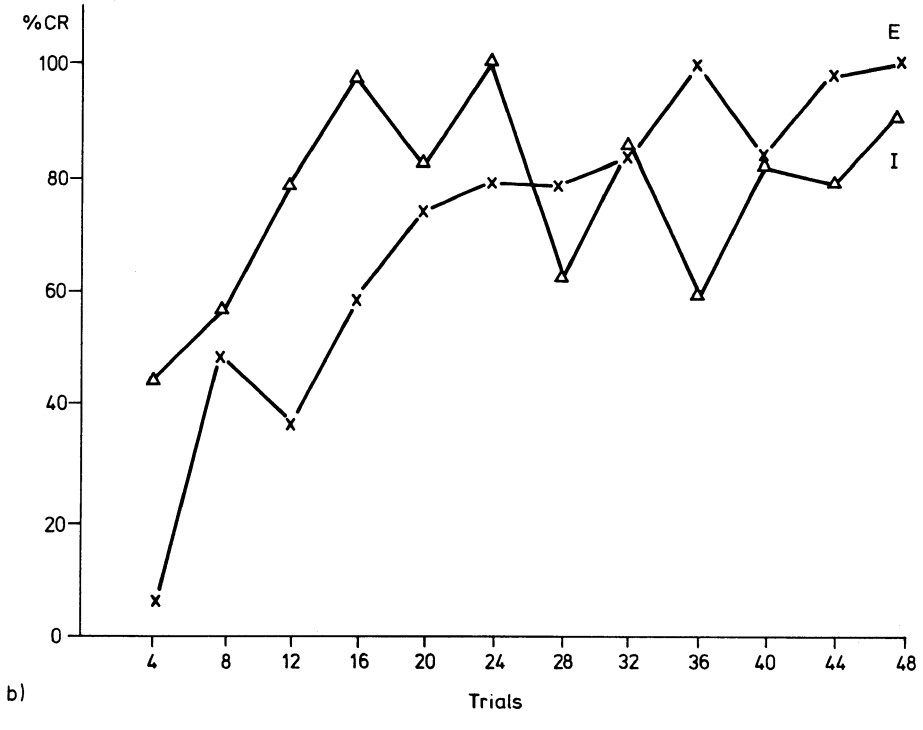
Fig. 5.6. Acquisition curve for strong stimulus conditions (continuous reinforcement, long interstimulus interval, strong UCS). (After Eysenck and Levey 1967)

ton, extraverts produce more button presses, while introverts spend more time inspecting the materials. These findings are not entirely unexpected, but their significance in the context of the neo-Pavlovian formulation as discussed earlier is of some interest. It is known that extraverts will alternate more rapidly between visual stimuli (Eysenck and Levey 1965), an effect usually attributed to satiation, but one which would necessarily imply a greater degree of motor activity. Similarly, the impulsive behaviour of extraverts in giving errors of commission is well-known in such tests as the spiral maze, and this type of error is one of the measures used by the reaction time studies cited earlier. These formulations resemble, as noted earlier, the separate dimensions of strength and dynamism postulated by Nebylitsyn, and indeed an earlier study of Mangan and Farmer (1967) convincingly showed the effect of the dimension of 'strength' to be related to extraversion. In short, it is possible that under conditions of relatively higher arousal extraverts would characteristically produce more responses, and this proposition has not been adequately tested in conditioning theory. This supposition is consistent with the phenotypically impulsive behaviour of the extraverts. In passing, it is of some interest that the data of the present study, illustrated in the preceding figures, were reanalysed in terms of an ad hoc impulsivity score derived from the EPI, and the effects of interest were shown to be entirely due to this component (Eysenck and Levey 1972).

Before leaving this topic, one further line of evidence, though indirect, is of potential interest. A series of studies by the present authors (Levey and Martin 1968; Martin and Levey 1969) showed that the efficiency of responding during the acquisition of conditioned eyelid responses increases independent of response frequency. Response efficiency was defined either as effective avoidance of the UCS or as effective integration of the CR and UCR, and these were indexed by objective ratio measures based on response topography. The data under review were reanalysed and the results are shown in Fig. 5.7 for each of the extreme sets of experimental conditions. The magnitude of the response represents efficiency ratios ranging from zero to 100%. Under conditions of low arousal it can be seen that the introverts progressed fairly steadily to higher levels of efficiency, while the extraverts failed to do so. The initial increase in efficiency in the extravert curve can be attributed to random response placement, which generates a spuriously high, but unsystematic, ratio. Under conditions of high arousal, in which it can be assumed that attention was reasonably secured, the introverts began at a higher level of efficiency and maintained it throughout acquisition, while the extraverts began at a low level, which was increased throughout acquisition. Our tentative explanation of these results is consistent with the formulation of Brebner and his co-workers and also with



a)



b)

Fig. 5.7. Curves of response efficiency under weak stimulus conditions (upper figure) and strong stimulus conditions (lower figure) averaging blocks of four trials.

the Eysenckian position. We suppose that introverts process stimuli in more detail, with greater selective amplication, and that they are better able to use the information gained in the acquisition series to place an effective response. The extraverts, by contrast, are inclined to respond impulsively and to produce responses regardless of whether they are efficient or not. These suggestions are tentative but they again raise the possibility that the dimension of introversion–extraversion contains complex determinants which have not yet been completely explored but which are consistent with general neo-Pavlovian formulations and not inconsistent with those of Eysenck.

Turning to the findings for drive, they are less easy to summarize. Both the reinforcement and UCS intensity variables appeared to produce drive effects in themselves, and the acquisition scores tended to display U-shaped distributions under each of the experimental conditions, with the exception of low UCS intensity and partial reinforcement, which showed a linear increase in responding from low through medium to high drive levels, as indexed by neuroticism and Taylor MAS. Under the remaining experimental conditions, the neuroticism and MAS scores tended to produce different results. As an overall summary it can be said that drive effects were manifested but tended to be highly specific to stimulus conditions. These results seem to dictate the conclusion that drive effects based on neuroticism (N) should be indexed not from the stable end of the dimension but from its mid-point. Something happens to both the hyper-labile and the hyper-stable in the middle range of induced drive level.

Since the original predictions for drive were based on the MAS rather than on the N scores, separate analyses were made for these two scales. While they were fairly highly correlated, 46% of the variance between them was unshared. The analyses were re-run, partialling out the MAS scores, and yielded the surprising finding that none of the drive effects was attentuated. This means that the rather complex effects of drive in relation to the interaction of stimulus-induced drive and personality could be referred to the N scale and not to anxiety as such. In other words, the residual variance in N not accounted for by MAS was sufficient to account for the drive results. N scores showed no relationship to the threshold measure, based on the weakest air-puff intensity found to elicit a blink. When these data were reanalysed, partialling out the MAS scores, the residual component was related to threshold at a significance level of 0.003 and accounted nearly completely for the threshold effect. This suggests that the pure N factor, that is, what is measured by N independent of manifest anxiety, is indeed measuring reactivity as a subject variable.

It remains to examine briefly the interaction of extraversion and drive, and the interest here centres both on the interaction of N with manifest anxiety and on the level of conditioning reflected. Table 5.2 shows the MAS scores for each of the nine cells of the sampling design, that is, three levels of extraversion–introversion crossed with three levels of anxiety. The means show that the anxiety level was comparable for each of the low drive levels within the three levels of extraversion, reading across the table, but that the slope of the increase was markedly higher within the introvert group. This seems rather consistent with Gray's (1970) suggestion that the introvert is more susceptible to punitive stimulation and to threat. This adds a certain fillip of interest to the examination of the conditioning scores in Table 5.3 for the same nine cells. It is apparent that for low-drive subjects the response level increased with increasing ex-

Table 5.2. Mean MAS scores of subjects within the extraversion × neuroticism sampling model

	Low N	Medium N	High N
Introvert	6.81	11.19	21.75
Ambivert	5.81	8.81	18.69
Extravert	6.56	10.62	15.69

Table 5.3. Mean CR frequency in acquisition for each cell of the extraversion × neuroticism sampling model

	Low N	Medium N	High N
Introvert	0.298	0.201	0.522
Ambivert	0.319	0.373	0.330
Extravert	0.406	0.290	0.304

traversion, while the converse was true for high-drive subjects. This finding is also consistent with Gray's prediction that the highest level of conditioning should be found for neurotic introverts and that level of performance is a joint function of introversion and neuroticism. It is also suggestive that without the facilitating effect of drive the extraverts were more responsive than the introverts, in line with the speculations discussed earlier. Within the overall design this interaction of E and N for CR frequency in acquisition was significant at a p value of 0.05.

Jones (1975) undertook a partial replication of this study using UCSs of very low intensity (3 and 1 psi) and adding as experimental factors two levels of programmed rest pause (after 25 and 50 trials), in order to manipulate inhibition, and the presence or absence of a warning signal on each trial, in order to assess the influence of this widely used methodological refinement. Subjects were 104 female volunteers, who received a total of 75 conditioning trials, divided between two successive days. The data were analysed in terms of a variety of measures, in addition to CR frequency, including onset latency and peak latency of both CRs and UCRs, amplitude at several critical points, e.g. peak, UCS impact, UCR onset, and a number of the measures of response efficiency mentioned earlier.

Not surprisingly, the results were complex. Significant effects on the measures of response topography and/or frequency were produced by each of the experiment variables. This was the first major study to have examined the influence of rest pause and warning signal on measures other than CR frequency, and the results showed that both manipulations profoundly influence response topography and efficiency. Analysis of personality effects was confined to half the sample who demonstrated extreme scores on the introversion scale. Under the low arousal conditions of the experiment, the introverts showed significantly higher levels of responding. Analysis of the two levels of UCS intensity showed, however, that the effect was entirely due to the lower intensity in interaction with extraverted scores as shown in Fig. 5.8. On several other measures the 3-psi extravert groups tended to resemble the 1-psi introvert groups, and the author notes the important point that conditions may be arranged either to favour or to cancel out personality differences between groups.

Differences between the personality groups were observed for several of the topographical measures, including latency and amplitude, the extraverts showing lower amplitudes of responding and shorter *peak* latencies, consistent with an inhibited or under-aroused performance. Interestingly, at the low UCS intensity, appreciable adaptation to the UCS occurred and was more marked in the extraverts, as would be expected. Finally, as in the experiment just reported, the introverts showed appreciably greater response efficiency on all measures of this factor. In summarizing his results, the author states: "It is apparent ... that introverts are generally superior to extraverts in frequency of responding, magnitude of CRs, efficiency, and avoidance."

The two studies just described afford an interesting comparison of methods. The first study examined the proposition that personality effects due to extraversion are linked to the level of arousal determined by the experimental conditions. The second study examined in finer grain the effects of under-arousal on extreme

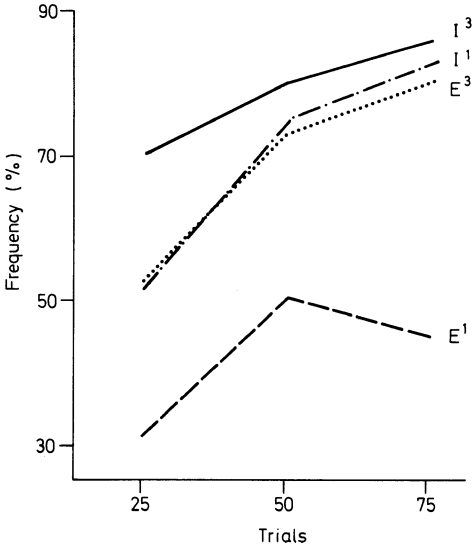


Fig. 5.8. Acquisition curves for introverts and extraverts under two conditions of UCS intensity. (After Jones 1975)

groups of introverts and extraverts. Results from each study are comparable and argue for the robustness of personality effects. More importantly, the studies show that statements about these effects *must* be couched in terms of the stimulus conditions employed to test them.

5.3.1 Summary

The period from the early 1950s through most of the 1960s produced a considerable amount of research activity on personality and conditioning, largely centred on the controversy between two theorists whose views at first seemed opposed but which were eventually resolved in the manner described. Both anxiety as a trait and neuroticism as a dimension act to influence the level of conditioning and interact with extraversion in a complex manner. The foregoing description of an experiment which met the assumptions of both theorists has been presented in some detail because it gives rise to interesting speculations about the relevance of the neo-Pavlovian factors discussed earlier. This era produced a clarification of issues and resolution of controversies and has been labelled the period of 'Aufklärung'. The end of this settling-out period unfortunately coincided with a decline of interest in the study of personality and conditioning, probably for reasons outlined in the introduction, that is, that experimental issues within conditioning theory became considerably more complex and the attention of younger investigators tended to centre on new theoretical problems. Nevertheless, it was said at the outset that our purpose was to suggest that the study of personality can contribute richly to the study of conditioning, and in the next two sections we describe more recent studies and their relevance to personality theory.

5.4 Newer Perspectives: Determinants of Responding

During the period when issues were being resolved, the experimental studies tended, with few exceptions, to be fairly simple. The basic strategy was to take a group of subjects, divide them on the basis of personality scores, either into extreme groups discarding the middle range, or at the median. The subjects were then run on a conditioning schedule to determine whether the personality factor influenced frequency of responding.

At the end of the era a curious study by Piers and Kirchner (1969) reversed the process and introduced the novelty of post hoc personality testing on high and low-conditioning groups, using personality questionnaires sent and returned by post. This modification is not trivial, since in the general experimental situation subjects fill in a personality questionnaire whose content may lead them to make inferences about the nature of the subsequent conditioning experiment. This issue of awareness is one which has never been satisfactorily resolved and it assumes new interest in the light of current scepticism from cognitive theorists (e.g. Brewer 1974). The study met the criteria of each of the theories with regard to the induction of emotionality and the provision of stimulus conditions conducive to inhibition, and the results produced positive findings for both theories, thus drawing the era to a satisfactory close.

While the present decade has seen fewer studies, those that have been done have usually been addressed to more specific issues and have in general been more interesting. In this section we shall first describe three studies which illustrate these trends. We shall then describe three studies of comparable interest and competence in which similar variables were measured but the personality dimensions not included.

5.4.1 Studies Including Personality

In the study described earlier (Eysenck and Levey 1967), it was noted that an ad hoc impulsivity scale drawn from the EPI was ale to account for all of the difference between introverts and extraverts under extreme stimulus conditions. As is well known, the introversion scale contained two sub-factors, or components, one of impulsivity and the other of sociability. In earlier forms of the personality questionnaire

the items reflecting social aspects of introversion tended to tap a facet of social shyness which was related to neuroticism. The EPI used in the study described was a revision which eliminated this problem. At a simple level it is possible to think of the impulsivity sub-factor as reflecting the basic component of responsivity or excitability, while the social factor may reasonably be regarded as a phenotypical expression of this factor, which may include social learning.

In the formulation of Eysenck's theory, the proposals for testing were always aimed at the inhibition or under-arousal of extraverts rather than at differences in excitability. This was largely because of real experimental difficulties in establishing stimulus situations in which excitation could be differentially expressed. As noted in considering the Brebner studies, neglect of the excitatory side of the equation may have produced misleading results. Barrat (1971) undertook a study to assess the effect of impulsivity independently, using a questionnaire of his own design which correlates 0.60 with extraversion. Sixty subjects were divided into four groups on the basis of scores on this measure of impulsivity and on the Taylor MAS to produce equal numbers of high impulsive, high anxious (Hi Ha), high impulsive, low anxious (Hi La), low impulse, high anxious (Li Ha) and low impulsive, low anxious (Li La). Clearly, the Li Ha group corresponds to the neurotic introverts of interest to the formulation of Gray. Subjects from each of the personality groups were randomly assigned to one of three experimental conditions, using a differential paradigm adopted from an earlier version of Grant's (1972) model of information processing in conditioning. This model involves the presentation of verbal material as CS, under two conditions. The verbal material may either conform to the subject's expectation or not. That is, one set of stimuli involves greater information processing, and Grant has argued that this borrows time and information-processing capacity from an assumed central processing unit. In the present study the conforming CS was a correct arithmetic sum, while the non-conforming was an incorrect sum. For one group, the CS+ was the incorrect sum, for the second group, CS+ was the correct sum. For a third control group, the CS+ was randomly associated with either correct or incorrect sums. Sixty trials were run, balancing the CS+ and CS− in ten trial blocks.

The EEG was recorded for the 4 s preceding each CS presentation. Four other physiological variables were included which did not discriminate among conditions, and the results for these were not reported. Consistent with expectation, the Hi La group showed significantly more alpha abundance in the epoch immediately preceding CS presentation. In other words, the low anxious impulsive subjects (extraverts) were less aroused at the moment of stimulus presentation than were the subjects of the other groups. Hi La subjects gave significantly fewer CRs in acquisition, while the greatest number were given by Li Ha subjects, in accordance with expectation for introverts. Figures 5.9 and 5.10 show the results. For correct sums the Hi La group gave more CRs, and this is the condition under which it is expected that there is less need to process information. For the CS+ presentation involving incorrect sums the Li Ha group gave significantly more CRs, and this is the situation in which it is expected that information processing is at a maximum. In short, introverted subjects responded with what appeared to be greater capacity for detailed information processing, while the extraverted subjects tended to respond less systematically and, in fact, gave more responses to CS−. This result is reminiscent of Brebner's observation of the introverts as 'geared to inspect' and the extraverts as 'geared to respond'.

Two succinct morals can be drawn from this experiment. If the personality measures had not been taken, the difference between correct CS+ and incorrect CS+, used as the excitatory or inhibitory stimulus, would not have been identified. In other words, a parameter of the experiment would have disappeared into error variance. Similarly, had the EEG measures been taken without the measurement of personality, the finding of interest would have been the observation that subjects who are less alert conditioned less well. The integration of this finding into a higher-order concept, that is, the concept of personality, yields considerably more information. In passing, it can be noted that the

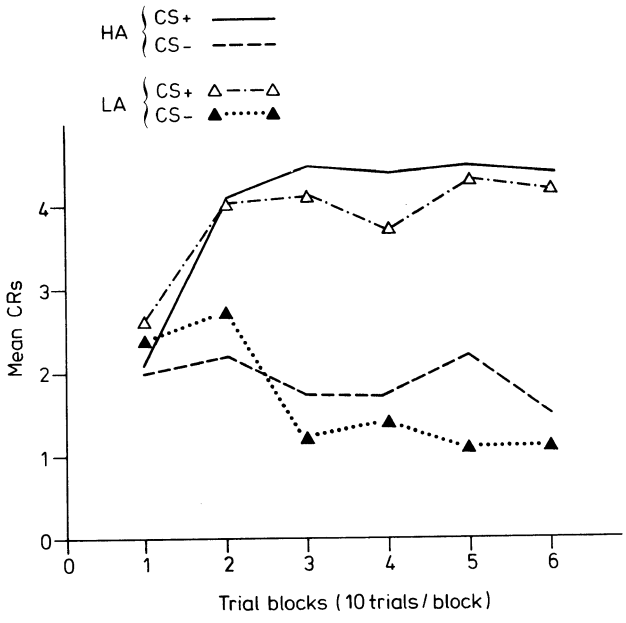


Fig. 5.9. Acquisition of CRs for high anxiety (HA) vs. low anxiety (LA) subjects. (After Barrat 1971)

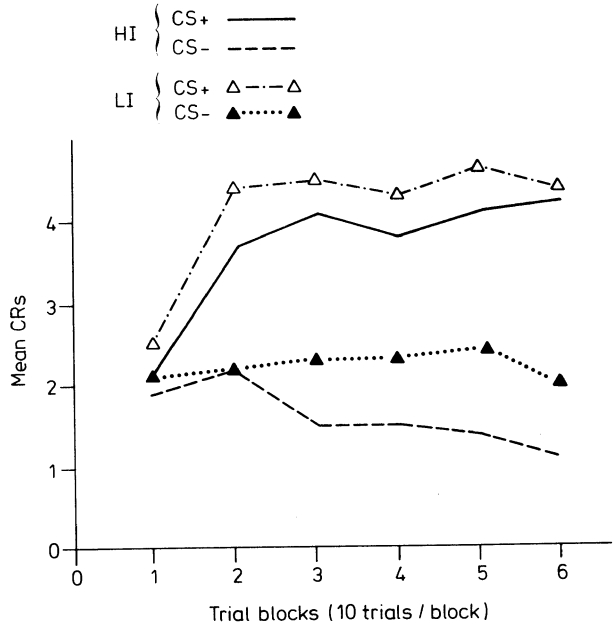


Fig. 5.10. Acquisition of CRs for high impulsive (HI) vs. low impulsive (LI) subjects. (After Barrat 1971)

EEG evidence of under-arousal is consistent with the predictions of Eysenck, though this has not always been the case. An experiment of this design could well be used to look at the neo-Pavlovian constructs discussed earlier, and this would require very careful setting of stimulus intensities. In other word, the excitatory balance between CS+ and CS− could be examined in more detail, and a fourth group could be included who were required to switch from CS+ to CS−. This type of experiment reflects the trend, noted earlier, of bringing the concept of personality to bear on more fine-grained issues in the conditioning field.

The next study to be reviewed is of interest because it directly attempts to test the formulations of Nebylitsyn and comes from the laboratory of an investigator who has done an appreciable amount of work on neo-Pavlovian concepts. Mangan (1974) tested 25 unselected male volunteers aged between 18 and 22 on a series of physiological measures selected to reflect the parameters of interest to the Nebylitsyn formulation. The aim was to examine cognition, psychophysiology and personality in a single experiment – a formidable task, as noted earlier. The measures included visual and tactile imagery, orienting to conditioned stimuli and to other test stimuli, measures of tactile sensitivity and so on. The 21 measures were factor analysed and yielded four factors, of which the first, loading on measures of inhibition, approximated reasonably well to Nebylitsyn's nervous system property of strength. The EPI measure of extraversion failed to load on this factor. Extraversion showed only two significant correlations, namely with initial amplitude of the orienting response to tactile stimulation (0.63) and the Spiral After-effect (-0.39), a result expected from theory.

The conditioning paradigm was unusual, in that it involved conditioning to appetitive (sexual) stimuli. The dearth of conditioning studies on appetitive stimuli is one of the serious limitations of work both on conditioning and personality. It is in fact difficult to find appropriate positive stimuli for use in conditioning studies for obvious reasons. We cannot deprive subjects of food and water to the level required in animal experiments, and other forms of appetitive stimulation tend to be richly involved with cognition. The recent change in our mores allows investigators to use sexual stimuli, and these are unarguably appetitive, though they probably also contain cognitive components. In the present study the paradigm was a differential SRR model, using photographs of attractive nudes.

One criticism of this study might be that the number of subjects is small relative to the number of variables, and the use of factor analysis might raise the eyebrows of the statistical purist. More importantly, it has been noted already that the questionnaire measures of personality usually require large numbers of subjects if the personality effects are to be expressed over a sufficient range and if the systematic effects are to be separated from the very wide range of individual variation. However, this observation is made in the full knowledge of the formidable difficulty of this kind of intensive testing schedule, and we might note in passing that a part of the Pavlovian and neo-Pavlovian tradition is the study of the 'chronic experiment', that is, the observation of subjects over a long period of time. It is to be hoped that studies of this type will be undertaken with greater frequency by Western psychologists.

The next study is of interest because it extends the domain of conditioning and personality to a practical area and was aimed at clinical populations in the context of behaviour therapy using reconditioning techniques. Martin et al. (1969) studied a group of 62 patients who had undergone controlled trials of systematic desensitization for the treatment of phobias. The patients fell into three symptom categories, exhibiting specific phobias, e.g. animals and birds ($n=19$), social phobias ($n=15$) and agoraphobia ($n=28$). EPI extraversion and neuroticism scores were obtained, as well as ratings of anxiety and of response to treatment. The aim was to examine the influence of these factors on conditioning performance, in relation to both the Spence and Eysenck hypotheses. The eyelid-conditioning procedure involved a UCS intensity of 6 psi, that is, strong UCS, and an interstimulus interval of 500 ms. Frequency of CRs in acquisition was significantly higher for younger subjects, for those more extraverted and, surprisingly, for those scoring lower on neuroticism. It is of interest that extraverts showed high levels of responding, comparable to those reported in the study of Eysenck and Levey (1967) in response to a strong UCS intensity. Ratings of anxiety were positively correlated with conditioning performance in the group showing specific phobias but were uncorrelated in the other two groups. Rate of extinction was examined independently of acquisition frequency, using covariance analysis. Extraversion was positively related to extinction in this analysis for the agoraphobic group but showed no relationship in the remaining groups, although

the effect of partialling out acquisition frequency was to equate the three groups in terms of extinction.

Ratings of improvement differed among the three groups, the agoraphobics showing least improvement. Good improvement was associated with earlier age of symptom onset. Interestingly, the improvement ratings across all symptom groups were positively correlated with CR frequency in acquisition. In other words, subjects who conditioned readily were more susceptible to treatment manipulations based on conditioning techniques. Clearly, the relationship between conditioning and personality in the clinical context is by no means cut and dried. The relationship between acquisition and extinction differed in the three groups independent of personality. The specific phobias, for example, extinguished more slowly than the remaining groups, suggesting the possibility that their phobic symptoms represent failures of extinction rather than increased susceptibility to the initial conditioning of anxiety. The authors wisely caution that these results should not be taken too literally, but the study suggests a promising lead for the investigation of interrelations between conditioning, symptom formation and personality.

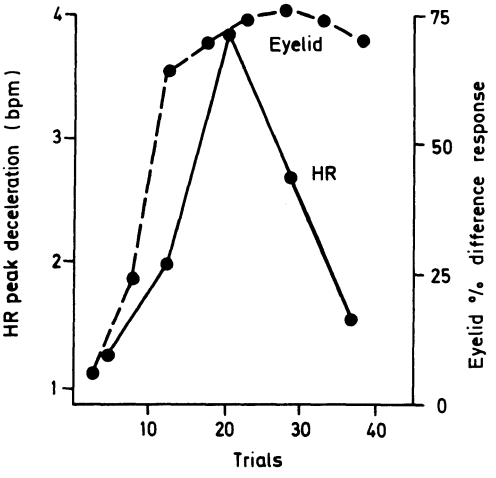


Fig. 5.11. Change over trials in the relation between differential eyelid responding of good conditioners and their peak heart rate (HR) decelerations to CS+. (After Putnam et al. 1974)

5.4.2 Studies Excluding Personality

Pavlov, in his early work, studied the orienting reflex in dogs and concluded that it interfered with conditioning. In his later work he came to the conclusion that conditioning is facilitated by an optimum level of orienting. By contrast, the defensive reflex was found to interfere with conditioning. Work in his laboratories showed that the orienting reflex tends to stabilize as conditioning progresses and then to habituate out. The reflex returns with any stimulus change involving alteration in the conditioning parameters. These principles were examined by Putnam et al. (1974) in an experiment designed to study the influence of orienting responses on conditioning, using a differential paradigm. Heart rate decrement was selected as the measure of orienting and heart rate increment as a measure of the defensive response, a selection based on Graham's previous work in this area. Figure 5.11 shows the results of the experiment. The subjects were divided into good conditioners and poor conditioners, and the figure shows that the Pavlovian prediction was upheld, in that the orienting reflex dropped out for the good conditioners as conditioning progressed. The association with low frequency of orienting responses has been observed in previous studies for poor conditoners (e.g. Maltzman and Mandell 1968) and the present study extends this observation to the course of development of orienting behavior across the acquisition series. The authors suggest that orienting interference is involved in the poorer performance of the relevant conditioning group and that this is consistent with the differential paradigm. In other words, the differential procedure, for some subjects, results in increased orientation, due to the relative novelty of randomly distributed reinforced and unreinforced signals.

McDonald and Johnson (1975), working in the context of sleep research and specifically interested in the possibility of sleep learning, conducted a well-controlled study which has implications for personality theory. Physiological measures included SRR, finger plethysmograph (FP), heart rate (HR) and EEG. The latter was recorded to monitor drowsiness, using a highly

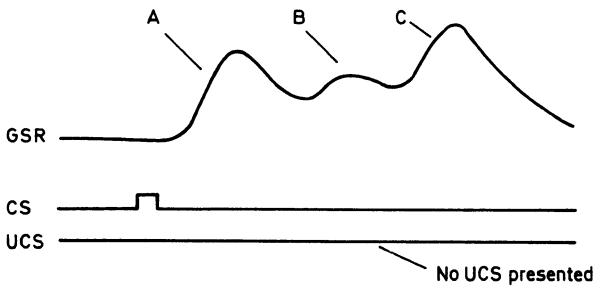


Fig. 5.12. Varieties of SRRs occurring during conditioning. A: the orienting response to CS onset; B: the anticipatory or pre-UCS response; C: post-UCS omission response or CR 2. (After Lockhart 1966)

reliable index developed in the laboratories of the investigators. Drowsiness was intermittent and was defined as the appearance of a criterion number of alpha bursts, and subjects were divided into those who were drowsy and those who were not. SRR was used as a dependent measure for both orienting and conditioning and yielded three measures, illustrated in Fig. 5.12: the orienting response (OR) to CS presentation, the anticipatory response (AR) to the CS presentation and the UCR omission response (CR2) observed on test trials. The same three indices were yielded by the FP measures, and HR was used as a measure of orienting. Of these measures the FP and SRR both showed reduced conditioning in drowsy subjects. For the SRR measure both the AR and CR2 showed the same relationship, while for the FP measure the CR2 showed evidence of conditioning, but no relationship with drowsiness.

An extremely interesting feature of this study was that continuous monitoring of the EEG enabled a description of the course of changes in alpha abundance and showed that for the drowsy subjects alpha enhancement increased in anticipation of the CS presentation, that is, it actually preceded the UCS. It was frequently noted in Pavlov's laboratories that dogs active in the environment tended to become rapidly drowsy during conditioning. In our own laboratories we have frequently encountered drowsiness in subjects in the routine conditioning procedure, and this problem is not an easy one in conducting studies of human conditioning. The stimulus conditions described in the experiment reported earlier (Eysenck and Levey 1967), that is, low illumination, sound-proof room, etc., are intrinsically under-arousing and in human terms extremely boring for the subject. However, any attempt to introduce novelty interferes with conditioning, and it will be recalled that the original studies of conditioning in Pavlov's laboratory only produced successful results when the properly isolated conditioning chamber was used. Pavlov attributed this fact to inhibition, and the modern view would attribute it to under-arousal. The implication that the under-arousal becomes actively associated with CS presentation has often been suggested, and this study provides some evidence, in that the subjects were not, as it were, chronically drowsy but became more drowsy on presentation of the stimuli. In our own laboratories we routinely use post-test questionnaires which include items concerning boredom and drowsiness, and these in general relate to the level of conditioning and to personality.

The last study in this section is again concerned with orienting and again follows from the work of Maltzman and his co-workers, mentioned earlier. The study was undertaken by Öhman and Bohlin (1973) in order to investigate Maltzman's suggestion that poor orienters are also poor conditioners. The theoretical issue was whether OR magnitude or OR habituation or both are better predictors of this effect. Seventeen male and 38 female undergraduate students were recruited as paid volunteers and were tested in a differential SRR paradigm, consisting of 20 presentations of CS+ and CS− in randomized order, that is, a total of 40 presentations. The CS+ was itself not 100% reinforced, and five trials were reserved for CS+ alone presentation, in order to test the UCS omission response. Eight presentations of each of the two CSs unpaired constituted the extinction procedure. Responses were scored in the

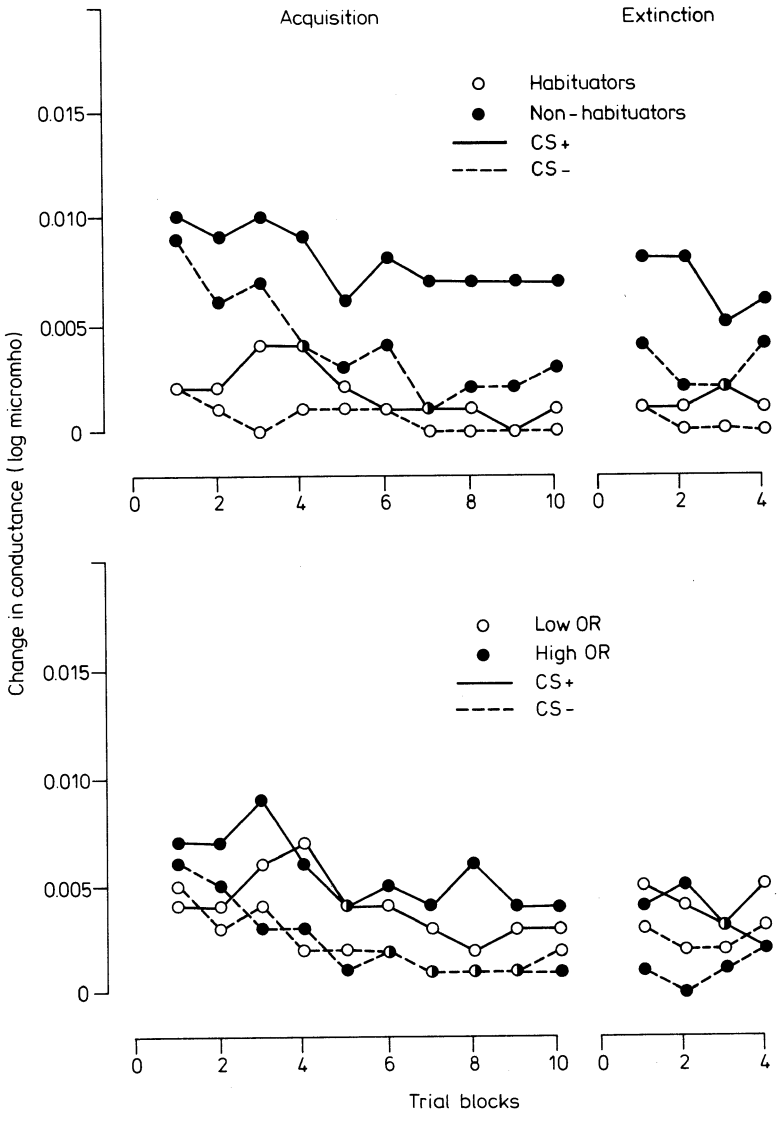


Fig. 5.13. Mean pre-UCS response to CS+ and CS−, as a function of trial blocks during acquisition and extinction. The upper part of the diagram shows means over the two OR magnitudes for the two habituation conditions, and the lower part shows the mean over the habituation conditions for the two OR magnitude groups. (After Öhman and Bohlin 1973)

usual manner, distinguishing the CS orienting response, the pre-UCS response or anticipatory response and the post-UCS, CR2 or UCS omission response (Fig. 5.12). Prior to the experiment, subjects were exposed to six presentations of CS+ (200 Hz tone of 8-s duration) and CS− (3,000 Hz of the same intensity and duration) in a habituation phase. This was followed by the acquisition series, using electric shock as UCS following immediately on the offset of CS+. Subjects were then divided into four equal groups on the basis of OR magnitude and habituation derived from the twelve-trial habituation series. Response to the first stimulus in the habituation phase was taken as an indicator of initial OR magnitude and the measure of habituation was the number of trials to a criterion of three successive zero responses, irrespective of CS+ or CS−. These two variables were correlated +0.38.

The plot of scores was divided into four quadrants, to yield a group of habituators and a group of non-habituators, each in turn divided into a group of low OR magnitude and a group of high OR magnitude. Results for the four groups are shown in Figs. 5.13 and 5.14 for

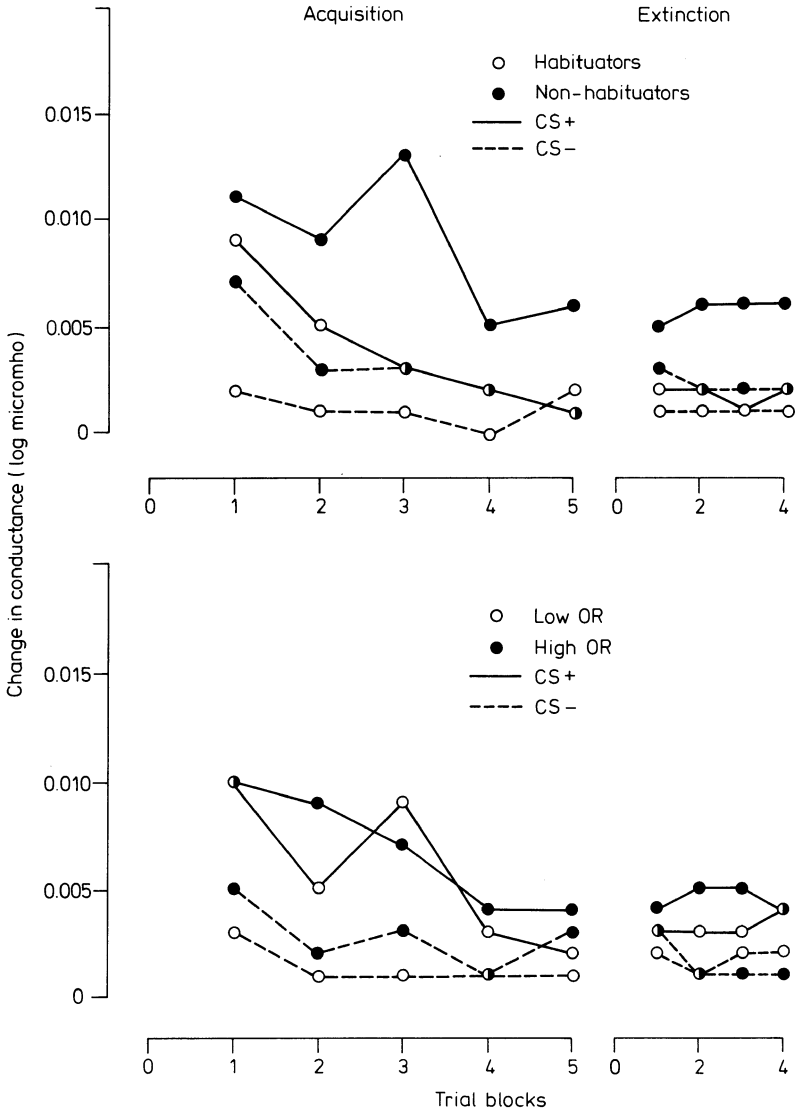


Fig. 5.14. Mean post-UCS response to CS+ and CS− as a function of trial blocks during acquisition and extinction. See text to Fig. 13. (After Öhman and Bohlin 1973)

pre-UCS and post-UCS responses respectively. Data were presented separately for habituators and non-habituators under CS+ and CS− and low OR and high OR for CS+ and CS−. As the figure shows, the habituators responded at a much lower level than the non-habituators, and the latter group also showed better conditioning, as measured by a significant habituation × conditioning interaction. By contrast, the division into high and low magnitude of initial OR did not influence conditioning. Similar results were found for the post-UCS response, and for both measures the extinction trials showed the same significant trend. Orienting responses to the CS were also compared between groups and CS type and showed similar trends, in that the habituators habituated rapidly during acquisition, as would be expected, while the non-habituators continued to respond during this phase. The non-habituators also showed greater discrimination between CS+ and CS−. OR magnitude to the initial presentation did not produce significant results for OR frequency in acquisition. Thus the orienting response parameter which determined conditioning was the rate of habituation, rather than

the magnitude of the OR, and the authors note that investigators who use the average OR magnitude from a series of habituation trials to index conditionability or reactivity are probably confounding magnitude with habituation.

5.4.3 Summary

The two sets of studies just reviewed, comparable in complexity, in subject matter, in experimental competence and in direction of results, require little comment. We need only say that the student of personality wonders whether the results for the last three studies could have been predicted from personality or whether the subgroups differed in personality scores. Clearly, the content and direction of the variables shown to influence conditioning were those of interest to personality theory, but it would be a waste of time to indulge in speculation, and the answer is that in the absence of personality measures we do not know.

5.5 New Perspectives: Recent Extensions

In this section we consider a number of issues which are relevant to personality theory and to conditioning, though they are not in every case directly related to personality studies. Both personality theory and conditioning theory have produced substantive issues and methodological problems of interest, together with theoretical questions, some of them as yet unanswered. The topics are presented in pseudo-random order, beginning with the substantive areas.

5.5.1 Substantive Areas

5.5.1.1 Conditioning in Infancy

It was noted earlier that the asumption that personality dimensions are innate is not a logically necessary requirement of personality theory, though the theories reviewed all make this assumption. Attempts were made in Pavlov's laboratories to separate the effects of environment from innate predisposition, partly prompted by the observation that behavioural measures, such as timidity, failed to relate to the conditioning measures. It is historically interesting that Pavlov speculated on the effects of intra-uterine environment and was careful to refer to his properties of the nervous system as innate rather than inherited in deference to this possibility. He concluded, however, that the methods of observation available were too crude to permit a meaningful resolution of the issue.

A considerable amount of interest has been shown in the conditioning of infants during the past decade by both Western and Eastern psychologists. The Institute for the Care of Mother and Child in Prague has been particularly active, originally under the direction of Papusek, who devised a number of conditioning tests suitable to infant behaviour. This is a surprisingly neglected area in conditioning theory and has produced a literature of its own which has in general not filtered through to the general body of conditioning theory. Studies in the West have included conditioning of heart rate, SRR, skin potential response (SPR), eyelid response, foot withdrawal, sucking and plantar reflex, using a wide variety of stimuli.

The difference noted earlier between Western and Eastern psychologists, that is, that the group following the Pavlovian tradition tend to use the 'chronic experiment', results in longitudinal studies of infant behaviour, which are lacking in the West. References to individual differences in conditioning are not infrequent and generally relate either to differences in the organismic state, for example degree of activity, or to orienting behaviour. A fairly consistent finding is that there are marked differences in the degree of 'preparedness' of differing CS – CR combinations, and these probably reflect the rapidly changing differentiation of the infant CNS. There is some consensus that the earliest conditioned responses to be formed readily are autonomic responses, using a temporal conditioning paradigm, and this is consistent with the well-known plasticity of infants in forming stable diurnal rhythms. One or two studies will be described briefly.

Ingram and FitzGerald (1974) studied 12 infants, using differential conditioning of skin potential responses in a design which included reversal of the discrimination. Subjects were habituated to CS+ and CS−, and the magnitude of the OR to the first habituation stimulus was used as an index of orienting. Subjects were divided at the median into a high-OR group and a low-OR group, and the high-OR group showed significantly greater conditioning. The effect of habituation was measured by correlation, and for the reversal of discrimination those infants who showed the most rapid habituation also showed the greatest facilitation of discrimination reversal. This study meets the suggestion of Öhman that the first OR magnitude is the appropriate measure of orienting and is not confounded with habituation. Interestingly the result differed from the study of adults. In this instance we cannot complain that personality questionnaires were not administered, and we must content ourselves with the reflection that the main parameters of adult conditioning in terms of individual differences were evidenced in the 4th month of life.

Among the ingenious conditioning procedures developed at the Institute for the Care of Mother and Child in Prague are conditioning of head-turning, using as UCS either the tonic neck reflex or alimentary reflex, a procedure which is simple and practical with infants. Krulisova (1975), working at this Institute, has used the alimentary reflex to study speed of conditioning in infants aged 1 and 4 months. The UCS was a milk bottle nipple with milk source and the response measured was head-turning towards this source. She reports individual differences in the rate of acquisition, but not in the rate of extinction. She also reports that acquisition was faster in the older infants, while extinction was faster in the younger. This raises the point that the Eastern workers have fairly consistently reported differences due to age, while Western investigators have rarely found them. The most probable explanation, though it does not apply to the present study, is that the Eastern workers tend to study the 'chronic experiment', whereas the Western workers tend to use a single experimental session, as is the case in animal and adult human studies. Tauremannova et al. (1978) have shown that the speed of conditioned response formation can be predicted from the infant's activity level during the waking state, the more active infants conditioning more readily. A later study of Krulisova (1978) studied the extinction of the conditioned alimentary response in 1- and 4-month-old infants, and also showed that the behavioural state, e.g. 'negative emotional state', was related to speed of extinction in younger infants.

An excellent review of conditioning in infancy, including consideration of individual differences, has been published recently by two workers who are themselves active in the field, FitzGerald and Brackbill (1976). They note that simple conditioning procedures are more likely to demonstrate individual differences. They also provide a stimulating conceptual organization of the field in terms of a set of working hypotheses concerned with the interaction of sensory modality and response system, prior behavioural state, individual differences in orienting and the degree of complexity required by the experimental conditions.

The obvious interest of conditioning in infancy for the student of personality is that it clearly exhibits marked individual differences, some of which appear to be related to temperament. The fact that differences in acquisition, in performance and in extinction are observed in the first few months of life seems to offer strong support for the view that individual variation is one of the characteristic features of human conditioning.

5.5.1.2 Response Topography

A number of investigators have recently shown an interest in response topography, and while there are no personality data to report, other than those discussed earlier, the status of response topography as a substantive issue deserves some mention. During the Hullian era, when it was assumed that all measures of responsivity index habit strength and are transformations of one another, interest in response topography was minimal. As early as 1956, Spence noticed that some responses in the con-

ventional eyelid-conditioning experiment were unscorable, because the CR had merged with the UCR. The phenomenon of blending of the CR and UCR is now well acknowledged (Gormezano 1966, Kimmel 1966, Martin and Levey 1969, Prokasy 1965) and was a standard observation in the Pavlovian laboratories, where it was attributed to inhibition of delay.

Three general viewpoints have emerged. Kimmel (1966) espouses the explanation based on inhibition of delay and has offered evidence in support of this view. Prokasy (1965) and others regard the phenomenon as a case of correlated reinforcement, such that partial avoidance of the UCS results in reinforcement of the specific appropriate latencies, and the developing conditioned response thus comes to approximate to the UCS onset. Gormezano (1966) has challenged the reinforcement or Law-of-Effect interpretation and prefers the notion that merging of the CR and UCR is a characteristic of classical conditioning. He regards this as a mechanism of classical conditioning and prefers to avoid purposive interpretations. One test of the law of effect is to administer the UCS as para-orbital or infra-orbital shock, with the assumption that closure of the eyelid has no effect in attentuating the UCS. Where this has been done the integration of CR and UCR has been observed.

In our own work (Martin and Levey 1969) we have observed that the exact location of the response can be predicted from UCS intensity. Under a high level of UCS intensity, the response tends to be located in apposition to UCS onset. Under low levels of UCS intensity, the response tends to integrate with the UCR. The measures on which these observations are based are the efficiency ratios briefly mentioned earlier. Figure 5.15 illustrates the use of these measures. For example, the amplitude of the developing CR at the point of impact of the UCS (c′ − c) is taken as a ratio of UCR amplitude (f′ − f), on the assumption that the latter represents in some sense the available response energy for that trial. Alternatively, we may define a measure which is based on the amplitude of the developing response at the point of emergence of the UCR (e′ − e) and reflects the extent to which the 'work' of the UCR has been accomplished by the CR before the UCS arrives.

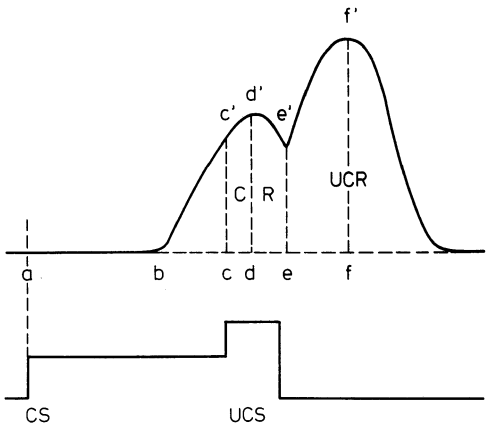


Fig. 5.15. The major parameters of the conditioned and unconditioned response: a, CS onset; b, CR onset; c, UCS onset; d, point of maximum CR amplitude; e, UCR onset; f, point of maximum UCR amplitude. (After Martin and Levey 1969)

It is obvious that the difference between these two measures in terms of time is a matter of a few milliseconds, and yet the finding noted earlier was significant for differentiating the two levels of UCS intensity. This reflects the remarkably finely adjusted timing mechanisms involved in the formation of the eyelid CR, mechanisms which lie well outside the conscious control of the subject. Subjects asked to replicate this phenomenon by blinking voluntarily produce 'learning' curves comparable to those for signalled reaction time and ascending steeply to approximate placement of the response. The tests were made after conditioning, that is, after the stimulus information was fully available to the subject. However the placement varies by up to 100 ms from the accurate placement of the response by the same subjects under standard acquisition procedures. The purpose of this section is primarily to suggest that the study of the topography of the conditioned response may well hold considerable promise for the study of individual differences, in that it reflects the factor of stimulus sensitivity as well as a factor of response equilibration, both at very fine levels of differentiation.

5.5.1.3 Extinction

For the same reasons that response topography tended to be ignored during the Hullian era, that is, because all indices of responding were assumed to be measures of response strength and hence redundant, the rate of extinction tended to receive relatively little attention as a separate subject. Frequently, although not always in the personality studies, the extinction measures tended to parallel the acquisition measures, that is, introverts and anxious subjects in general conditioned more rapidly in acquisition and extinguished less rapidly. However, extinction raises theoretical problems of interest in its own right, which cannot be discussed in detail here but which must raise issues for the study of personality.

It is of some interest that the phenomena of extinction have never been satisfactorily explained. They have been attributed variously to inhibition, to habituation, to negative conditioning, to the learning of a new response 'not to respond', and there is no sure answer to the question: where is the response when it has been extinguished? The fact that it is in some sense still there is indicated by the phenomenon of spontaneous recovery, and this phenomenon has often been cited as evidence for an inhibition mechanism. Spence (1966) was probably the first SR theorist to demonstrate a cognitive component in human extinction in the eyelid-conditioning experiment, and rapid extinction of SRR conditioned responses is a well-known phenomenon, probably involved with cognition. Öhman and Bohlin (1973), in the study mentioned earlier, found higher levels of responding for pre-UCS and post-UCS responses in non-habituators, but the CS response, which they attributed to conditioning of the orienting response, showed the most rapid decline in both habituators and non-habituators. Given that subjects who condition well begin extinction at a higher level of responding, it would seem that the slope or rate of decline is the more appropriate measure than the number of responses in extinction, but this has not been closely examined in personality research.

Eysenck (1976a) has recently proposed a reformulation of conditioning theory in relation to neuroses, suggesting that the factor of response enhancement leading to failure of extinction is the necessary component of a theory which attempts to account for neurotic symptom formation. In our own laboratories we have often observed informally that some subjects are remarkably resistant to extinction. We have often wondered who these 'inextinguishables' might be. The potential relevance of extinction not only to clinical neuroses but also to psychosomatic conditions (Levey and Martin to be published) has recently led us to reanalyse the data of the personality study discussed in some detail earlier (Eysenck and Levey 1967). Subjects in that study were extinguished to a criterion of five non-response trials, with a limit of twenty trials. In order to examine the extinction level, the trial number of the last response was tallied for each subject. Figure 16 shows the distribution, which is clearly bimodal, producing a substantial group who effectively failed to extinguish. Subjects from the non-extinguishing sub-group were matched in terms of CR frequency with the subjects who extinguished rapidly, to produce two groups of 32 subjects each. These groups did not differ in frequency in regard to the cells representing the experimental conditions described earlier. For each of the groups the personality scores were averaged to determine whether personality variables contribute to differences in extinction, when acquisition rates are equated. The extraversion scores for the two groups were nearly alike, the neuroticism scores showed a non-significant trend for individuals high in neuroticism to be included in the non-extinguishable group. Rather strikingly, the Taylor MAS scores significantly discriminated the two groups, the respective means being 6.62 and 13.50 for extinguishers and non-extinguishers. This substantial difference suggested that at least part of the phenomena of the inextinguishables can be attributed to 'manifest anxiety', with some reservations as to what this may be. It is hoped that an item analysis of the personality and anxiety questionnaires will throw some light on this problem.

Several studies which have considered extinction in a clinical context have tended to support the Eysenckian prediction that extraverts

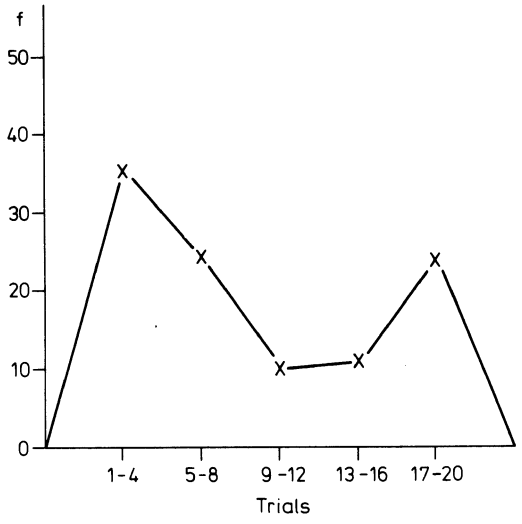


Fig. 5.16. Distribution of 144 subjects by trial number of last response in extinction. (Excluding 38 poor conditioners who gave no responses in extinction).

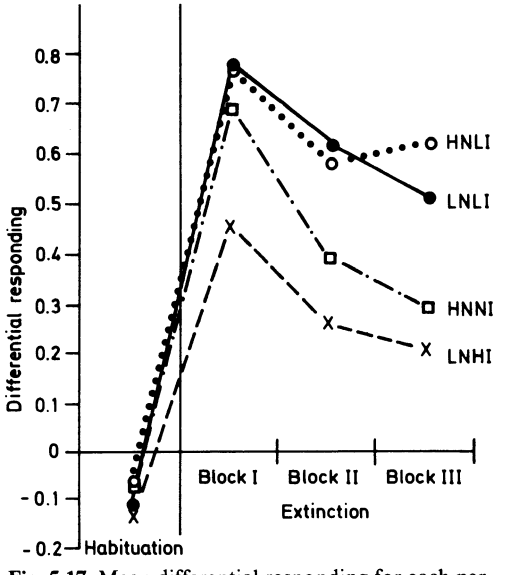


Fig. 5.17. Mean differential responding for each personality quadrant. Means calculated from individual differential responding. Measures by each subject's mean CR+ minus mean CR− for each block. CRs calculated as log change in conductance expressed as a proportion of change in conductance of maximum response. (After Hemming 1979)

should extinguish more rapidly, but these studies have not been concerned to eliminate the influence of acquisition. Marks et al. (1971) found EPI extraversion to be positively related to the rate of reduction in phobic symptoms following treatment by flooding, but not by desensitization. Mathews et al. (1974) found a similar result for both flooding and desensitization following fifteen treatment sessions, though the level of improvement differed between the groups. Extraversion in this study was measured by the Cattell 16PF scales, of which only the 'outgoing' scale reflected the treatment effect at a 6-month follow up. A study which used the newer Eysenck Personality Questionnaire (EPQ, Eysenck and Eysenck 1975) as the measure of extraversion (Hallam 1976) found that extraverts required fewer treatment sessions to reach a criterion of 'no noticeable further improvement', though introversion was not related to the final outcome.

It may be argued that the attribution of extinction processes to the outcome of behavioural therapy requires assumptions which are difficult to justify empirically. However, a recent and careful study by Hemming (1979) used a differential paradigm to condition SRR to light stimuli, preceded by a habituation series and followed by extinction. An ingenious masking procedure involving a simulated driving task served to control attention and expectancy, while awareness of CS+ contingency was controlled by informing all subjects. The subjects were 51 student volunteers, selected to provide four quadrants of high and low impulsivity, with high and low neuroticism on the basis of EPI scores. Figure 5.17 shows the results for the four personality quadrants in habituation and extinction. The second point in the graph represents the level following acquisition. The significant difference between high neuroticism – low impulsivity, and low neuroticism – high impulsivity, is in the direction predicted by Gray's (1970) extension of Eysenck's theory, discussed earlier.

5.5.1.4 Attitude and Evaluative Conditioning

Studies in human conditioning have extended to variables other than somatic and autonomic

responding, to include subjective affect and attitude. Noteworthy in this development has been the work of Staats, using a paradigm in which emotive words are paired with neutral nonsense syllables which are subsequently rated on attitude scales (Staats and Staats 1957). A number of studies have illustrated that this type of conditioning shows the basic phenomena of classical conditioning, and the results of these studies have been applied to Staats' theory of social behaviour (Staats 1975). The usual experimental procedure involves testing groups of subjects with slide presentation of a neutral nonsense syllable, followed by the emotive UCS word, which the subjects are required to repeat aloud. There has been some controversy over the role of cognitive awareness in these studies (Page 1974), the details of which are not appropriate to the present context.

In our own work (Levey and Martin 1975; Martin and Levey 1978), we have demonstrated a similar form of classical conditioning, which we refer to as evaluative conditioning. In this procedure subjects are asked to rate a variety of visual materials, for example picture postcards, and to identify the most and least preferred in terms of the dimension of like – dislike, leaving a pool of neutral items. The criterion experiment involves presentation in a Latin Square design of neutral pictures paired in either forward or backward direction, with the liked or disliked stimuli together with a neutral control pair. We have shown that this experiment produces both forward and backward conditioning, and that aversive conditioning is a stronger phenomenon than positive conditioning. A series of parametric studies of the factors involved has recently been presented, together with an extensive account of the theoretical underpinnings of the experiment (Martin and Levey 1978). This procedure tends to be independent of subjective awareness, possibly because the experiment contains five within-subject pairings which are difficult for most subjects to keep separate.

The evaluative conditioning and attitude conditioning paradigms, specific to human conditioning, would seem to provide an appropriate framework for the examination of personality differences. Costello (1967) followed the Staats paradigm exactly, but divided his subjects into high and low-extraversion groups. Under the conditions of the experiment the introverts showed superior conditioning. In one of our parametric studies (Martin and Levey 1978), we examined the effect of personality and found the opposite result, that is, extraverts demonstrated superior conditioning. Subjects were rated by questionnaire as to whether their responses had been primarily in terms of thinking or feeling, and the result tended to overlap the results for personality, that is, the introverts were more likely to be among the group whose judgement of the picture materials was cognitive, while the extraverts reported judgements of feeling. This is again reminiscent of Brebner's identification of introverts as geared to inspect while extraverts are geared to respond. The basis of the evaluative conditioning study is the subjects' rating of the picture materials, and while subjects are asked to rate on the basis of their own subjective response, that is, in a non-mediating manner, some subjects find this difficult and tend to apply judgemental standards, which must be regarded as cognitive. In this connection, naive subjects condition better than sophisticated subjects, in terms of knowledge of the art materials used in some of the studies. By contrast, the attitude-conditioning experiment of Staats requires no explicit judgement of attitude, and subjects are in effect set to respond rather than to inspect by the procedure of repeating the words aloud. It is possible that this difference in procedure accounts for the opposite results of the two studies. The precise parameters of this type of experiment have not been specified in the same way as those of somatic and autonomic conditioning, and predictions of the role of personality are probably premature. Nevertheless, this type of human oriented conditioning seems to offer an interesting area for the exploration of personality variables.

5.5.2. Theoretical Issues

5.5.2.1 V-Form and C-Form Responding

It was mentioned earlier that eyelid-conditioning studies undertaken in the United States ear-

ly showed a category of voluntary form responses which resembled the responses of subjects asked to blink voluntarily to the CS. There is not enough space here to review the theoretical issues which grew out of this finding, but a brief historical summary is in order. Difficulties in the identification of the voluntary form responses by visual inspection led to several attempts to introduce objective methods of classification. Of these a response onset latency measure (Spence and Ross 1959) was widely used but failed to transfer across varying laboratory conditions. An alternative criterion, based on the slopes of conditioned responses compared with the criterion slope of the first set of unconditioned responses, was suggested by Hartman and Ross (1961) and became the standard means of identifying voluntary responses in most laboratories. Using this criterion, the responses identified did not necessarily resemble instructed voluntary responses, and the term V-form was adopted for these responses. The original strategy of discarding voluntary responders was dropped, and a number of studies appeared in which the data for V-form and C-form responders, identified by the proportion of each response type given, were analysed separately.

Much of the work on V-form responding has been done by Grant and his associates at the University of Wisconsin, and this aspect of response topography has yielded an interesting array of findings (Cody and Grant 1978; Hellige and Grant 1974; Kadlac and Grant 1977; Zajano and Grant 1974). From these and other studies the following characteristics of V-form responders can be summarized: (1) resistant to extinction (2) more resistant to changes in CS-UCS contingencies (3) influenced more by semantic features of the verbal CS presentation (4) higher level of responding (5) superior UCS avoidance (6) respond less frequently to CS− in differential eyelid conditioning and (7) less susceptible to partial reinforcement effects. Evidence has been presented that the V-form response is not a voluntary response, principally on two grounds, that the V-form subjects do not report responding voluntarily, and they do not identify contingent UCS omission relationships in avoidance conditioning more readily than C-form responders. Grant (1972) has suggested that the V-form responders have an excitatory response bias, while the C-form responders have a negative or inhibitory bias. He has also suggested that V-form responders have a different cognitive style, either in the processing or reception of stimuli. Finally he has suggested that response processing, as measured by the type of studies described earlier, in which contradictory verbal CSs are used in the differential conditioning paradigm, is more difficult for C-form than for V-form responders. In Grant's terminology, a greater part of 'central processing capacity' of the V-form responder is free for detailed stimulus processing than for a C-form responder.

The pattern of these differences presents a tantalizing resemblance to some of the differences associated with personality in both the Eysenckian and neo-Pavlovian systems. It is tempting to suggest that the basis of differentiation seems to include components both of the strength and dynamism factors of Nebylitsyn and that the V-form responders seem to resemble in some aspects the performance of introverts. While this is idle speculation at the moment, it appears to offer a promising area for research. The use of personality questionnaires has not been part of the experimental tradition of the Wisconsin group, partly because their emphasis on tight experimental design precludes the use of subject samples which cannot be clearly dichotomized into explicit subgroups (Grant 1977 personal communication).

5.5.2.2 Psychoticism as a Dimension of Personality

Recently the Eysenck personality model has been reformulated to include the dimension of psychoticism (P). The early studies of Eysenck, using objective tests, identified a psychoticism factor orthogonal to extraversion and neuroticism (Eysenck 1953). The original studies were done using criterion groups of psychotic patients, who are notoriously difficult to test, and this line of research was not pursued until the development of the psychoticism questionnaire, appropriate to identification of psychoticism

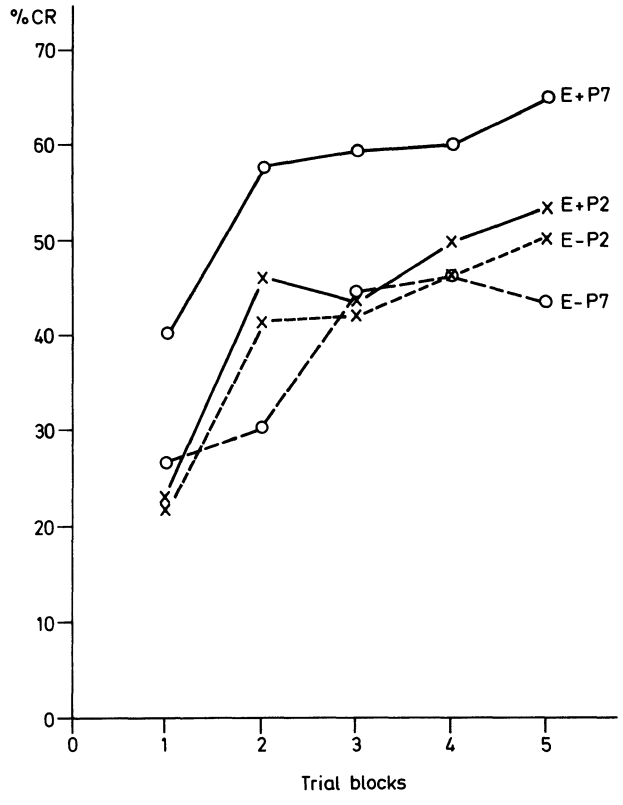


Fig. 5.18. Response frequency in acquisition for extraverts and introverts at two levels of UCS intensity (2 and 7 psi)

factors in accessible subjects (Eysenck and Eysenck 1976). A considerable amount of work has shown this factor to be related to clinical psychosis, to psychopathy, to criminality and to other categories. The essential components of the dimension appear to be a tendency to deviant behaviour, together with an affective component related to something like callousness. The P factor also contains an impulsivity component which is not identical to that found in extraversion and includes two subcategories of impulsivity and venturesomeness. The reformulation of the personality framework has involved some modification in the factor structure of the extraversion scale, and some degree of impulsivity now loads on each of the personality dimensions. To date, no work has been done on the effect of psychoticism on conditioning, although a large-scale study is currently being undertaken by Gertrude Frcka in our laboratories, and we have been unable to resist the temptation to make a preliminary analysis of the data for the sake of their current interest.

The study involves selection of subjects for high and low P, crossed with high and low E leaving N uncontrolled. Inevitably, the design of this study reverts to the early personality studies, in that measures of interest are simple acquisition and extinction levels in a standard eyelid-conditioning procedure, using two levels of UCS air-puff intensity (7 psi and 2 psi). It is emphasized that the experiment is still in process and that differential rates of recruitment to the four cells of the sampling design means that there are substantially differing numbers in each quadrant. With this reservation, Fig. 5.18 shows that the high-E subjects under strong UCS give more responses than the other three groups, and this is comparable to findings discussed earlier. In the preliminary data, high-P scorers tend to accelerate initially at the same rate as low-P scorers but to increase the rate of acquisition in the second half of the series, though this effect is probably not statistically significant. In fact, statistical analyses have not been undertaken, in view of the incom-

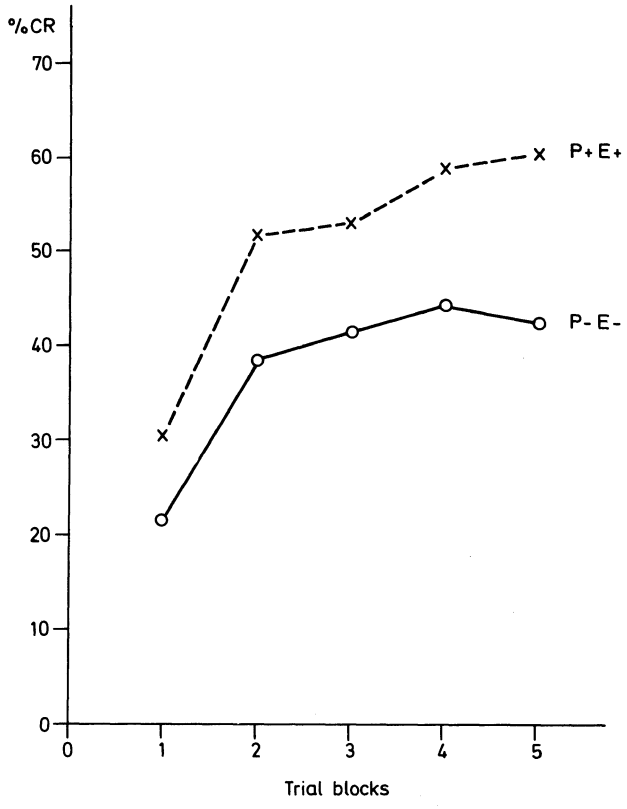


Fig. 5.19. Response frequency in acquisition for subjects high on both psychoticism and extraversion compared with subjects low on those factors

plete design, and we are reporting descriptive results.

Unlike the dimensions of extraversion and neuroticism there is no firm basis for predicting to the conditioning situation from the dimension of psychoticism. However, a preliminary hypothesis can be framed on the basis of the higher impulsivity of the high-P scorers, and it might be expected that they would tend in the direction of the extraversion dimension. Figure 5.19 shows the acquisition curves for high-P, high-E scorers, compared with low-P, low-E, for the data collected to date. In the absence of statistical analysis there would appear to be a difference in rate of acquisition favouring the high-P, high-E groups and suggesting that the extraversion and psychoticism dimensions interact, since P shows no overall relationship. In an attempt to elucidate the hypothesis that impulsivity is involved, we have calculated the impulsivity and venturesome scores for an equal number of subjects in each of the four quadrants, limited by the quadrant containing the fewest number (n = 11) but balanced for sex. The high-P scorers show significantly higher impulsivity and venturesomeness scores than the low-P, and this is to be expected. Less expected is the failure of the two impulsivity scales to differentiate between low and high-extraversion scorers, and any explanation of this must await the final outcome of the study. In the interim, these tentative results are offered for their interest value and in the hope that others will be prompted to undertake conditioning studies of the psychoticism factor.

5.5.2.3 Cognition

The recent swing of the pendulum towards a renewed interest in cognitive explanations of behaviour, both in the theoretical literature and within the behaviour-therapy movement, makes it inevitable that students of personality and conditioning will wish to examine the role of cognitive variables in the conditioning perfor-

mance of human subjects. Attitudes range from the complete scepticism of Brewer (1974), who defends the claim that classical and operant conditioning are not human phenomena, to those of Grant (1972), who believes that cognitive mechanisms are involved, and to those such as Gormezano and Kehoe (1975), who find no place for volitional antecedents of performance in the conditioning field. The study of Mangan (1974), described earlier, used cognitive measures which loaded on his factor II, and it seems probable that cognition is involved in conditioning performance. Human subjects are in general less susceptible to partial reinforcement effects, and this may have a cognitive basis in the subjective probability of CS-UCS pairing, given the occurrence of a criterion run of unreinforced trials.

A recent study by Furedy and Schiffmann (1974) which controlled for stimulus awareness has provided suggestive evidence that cognition need not be involved in human classical differential electrodermal conditioning, but this finding will inevitably meet with some scepticism. Differences between retarded and normal subjects have been invoked in an attempt to sort out the cognitive factors, assuming that they are absent or less critical in the performance of profound retardates, and these studies have in general shown relatively little difference between the criterion groups. Albin and Lobb (1976), for example, have compared severely retarded adults with age-matched college students in a design which included both air-puff and infra-orbital shock as UCS, trace versus delayed-conditioning paradigms at equal interstimulus intervals, crossed with strong and weak UCS levels, equated between the infra-orbital and air-puff modes of UCS presentation. Retardates failed to condition only under the weak air-puff trace paradigm procedure, but conditioned, though at a lower level, for all other cells of the experimental design. This study was not aimed at the cognitive issues, but the results are interpreted in what might be regarded as an essentially cognitive framework, that is, Kamin's (1969) proposal that the surprisingness of the UCS is a major determinant of conditioning. In these terms, having found that shock was a more effective stimulus for both normals and retardates, the authors suggest that it may have been more surprising.

This illustrates two difficulties in the study of cognitive variables, particularly in paradigms which were originally designed for use with animals. For better or for worse, conditioning theorists have subjected their human subjects to a wide range of procedures which were designed in the animal laboratory, and many students of behaviour feel that paradigms which are more appropriate to human responding, for example evaluative and attitude conditioning, may offer a preferable route to the understanding of human performance factors. The difficulty is that of defining what is meant by cognitive, and inferring when it has happened. One definition, though not a satisfactory one, is the questionnaire identification of subject awareness of CS-UCS contingencies. A very considerable literature surrounds this subject, and while the issues are not relevant in the present context, it is apparent that awareness plays some role in conditioning, particularly in SRR conditioning. The issues have been reviewed (Lockhart 1973) and need not detain us further. It suffices to say that personality theory must come to terms with the variables of cognitive style, of stimulus awareness and of attribution, and include these variables in their study.

5.5.2.4 Conditionability

An important theoretical problem has been ignored in the preceding presentation. It is probably fair to say that it has been largely ignored in the field, and we end our review with a consideration of the problem of conditionability. If a personality theory states that one group of subjects defined by a personality variable or variables conditions 'better' than another group similarly defined, the implication is clear that an assumption is made that the theory is talking not about eyeblinks or SRR or finger withdrawal but about conditionability. Indeed, this assumption underlies most of the conditioning literature until fairly recently when it has been questioned more rigorously by proponents of the biological limits of conditioning. There is a small but contradictory literature in this

field, and we shall not attempt to review it exhaustively but shall confine our attention to a handful of studies and to the main issues involved.

The assessment of conditionability is no easy matter and the obstacles cannot be underestimated. Conditioning for any particular sensory modality and for any somatic or autonomic response system is likely to have its own temporal parameters in terms of optimum interstimulus and intertrial intervals. For example, the SRR conditioning paradigm requires an interval of 5 s on average to establish the CS onset, pre-UCS and post-UCS response of interest. By contrast, the eyelid-conditioning experiment is unlikely to succeed at interstimulus intervals beyond 1 s, and this means that concomitant conditioning of these two response systems is difficult or impossible. Levels of stimulation which are arousing in one CS-UCS modality may not be the same in another, and the difficulty of equating this factor has never been satisfactorily resolved. Subjects run in successive sessions on alternative CS-UCS pairs learn something about the general experimental condition in the first session which transfers to subsequent sessions. The neo-Pavlovian school assumes that conditionability is related both to general and partial properties of the nervous system, and their work includes extensive use of successive test sessions, which enable some judgement to be made about conditionability. One of the designs used in Pavlov's laboratory was the 'stereotypy of conditioning', in which subjects were trained on a CS which was in fact a series of stimuli conditioned either to the same response or to alternative responses. This procedure bears some resemblance to the chaining of responses in operant conditioning in the Western literature, but its application to classical conditioning has been virtually ignored. The method would seem to be appropriate at one level for the examination of differences in conditionability, but the reservation noted above, that the common factors in the conditioning situation may produce a spurious effect for conditionability, cannot be ignored.

Turning briefly to the experimental literature, Campbell in 1938 attempted simultaneous conditioning of the eyelid response and the patellar reflex and found no relationship between them. Problems of methodology, including response identification and instrumentation, make these early studies suspect, and it is likely that no firm conclusion can be drawn. However, a more recent study by Van Bunt and Barendregt (1961), in which subjects were conditioned in separate sessions using eyeblink, SRR and EEG alpha desynchronization similarly showed no relationship in CR frequency among the three response systems.

The study of McDonald and Johnson (1975) offered, in the words of the authors, 'a unique opportunity to study concurrent conditioning'. They analysed their data from this standpoint, with the following results. Skin resistance and finger plethysmography proved to be independent measures across subjects, but both responses were conditioned and both were affected by the variable of drowsiness, with which the study was concerned. Within the finger plethysmography measure the anticipatory response and the UCS omission response were independent but similarly both conditioned.

The authors suggest that the fact that the independent skin resistance and finger plethysmography measures both conditioned in the same direction is inferential confirmation for a factor of individual conditionability. With regard to the results within finger plethysmography, both measures conditioned, but evidence was lacking for conditioning in the same direction, and they point out that the study was not designed to test this. They conclude that there was 'substantial evidence of concurrent conditioning of each measure' (p. 111), but they do not report the evidence in detail.

An interesting study by O'Brien and coworkers (1977) was not directed to the issue of conditionability but contains implications of interest for the methodological problem. These authors were concerned with the practical problem of the reappearance of symptoms of drug withdrawal in discharged addicts in remission when returned to the setting of their previous addiction. These patients frequently report an immediate return of craving on exposure to familiar situations, and this is expressed in terms

of actually experienced withdrawal symptoms, though these have long since been eliminated in the treatment procedure. A conditioning hypothesis has been offered in explanation for this very serious clinical problem but has been without empirical support. In passing, it might be noted that the study of contexts and situations as CSs has often been assumed within the behaviour-therapy movement but rarely examined explicitly. It was a frequent finding in Pavlov's laboratories that dogs transferred from one conditioning chamber to another tended to reintroduce the behaviour appropriate to the first chamber, regardless of the altered stimulus conditions, if the investigator who had previously worked with the dog appeared in the second chamber. This anecdotal observation has been studied systematically by Wyrwicka (1972), who has defined situations in which the contextual or situational cues override the specific CS parameters.

The experiment of O'Brien and his colleagues was not concerned with contextual cues specifically but rather used an ingenious design to test the reinstatement of withdrawal symptoms to a specific and manipulable CS. Eight volunteer subjects, themselves heroine addicts in remission, were screened for freedom from 'street drugs', using blood samples, and were maintained on methadone. Two subjects acted as controls and received a placebo, while the experimental subjects received the drug naloxone, which produces the symptoms of drug withdrawal, viz. increased respiration, decreased skin temperature, increased heart rate, increased motor activity and pupillary dilatation, together with report of subjective malaise. A compound CS was used consisting of tone (700 Hz) and a prepared CS presentation of a distinctive odour (oil of peppermint), and the time relations were controlled to correspond to the time of onset and duration of the withdrawal symptoms.

Conditioning of each of the physiological variables, with the exception of pupillary dilatation and subjective report of malaise, was evidenced by the majority of subjects, with one or two exceptions for individual subjects and measures. The individual results are presented by the authors. The evidence for concomitant conditioning of each of the measures is clear but raises an interesting problem of what exactly is conditioned. Investigators are used to thinking of conditioning heart rate, conditioning skin resistance and so on, and tend to forget that in every conditioning experiment there are probably a number of somatic and autonomic responses which are not measured but which are part of the complex pattern of responding which is conditioned. Examples are the conditioning of the eosinophil response to stress, which occurs as a conditioned response, regardless of whether it is measured or not. We must ask the question: if a syndrome of physiological and subjective responses is conditioned as an entity, does this mean that the individual response components reflect a factor of general conditionability? Clearly, the arguments are complex and need not be pursued in the absence of evidence. They serve to illustrate, however, the difficulty of determining the issue of conditionability.

A further study of interest also undertaken in a clinical context by Kantorowitz (1978a) is relevant to the problem of personality and conditionability, but in the opposite direction. This investigation was addressed to a series of recent findings, based on questionnaire surveys and retrospective data which demonstrate marked differences in the subjective report of sexual experience between introverts and extraverts (e.g. Eysenck 1976b). As in the previous study, this investigator was interested in experimental testing of the retrospective anecdotal findings. The study combined the techniques of pre-orgasmic and post-orgasmic conditioning to erotic cues which are part of the repertoire of behavioural sex therapy. Conditioning of tumescence, in the pre-orgasmic state, has been used to induce positive erotic feelings towards appropriate objects, while post-orgasmic deconditioning of detumescence has been used to attentuate erotic responses to inappropriate objects. The method is reported in detail in a separate publication (Kantorowitz 1978b). It was hypothesized that extraverts would show greater positive conditioning (tumescence) and introverts greater negative conditioning (detumescence). Eight cooperative subjects, young men aged between 18 and 23, were tested indi-

vidually, using slides of attractive female nudes, which for each subject were selected in a mid-range of erotic intensity, using penile amplitude as a criterion. The use of stimuli in the mid-range enabled identification of increments as well as decrements required for the comparison of positive and negative conditioning. The three slides were randomly assigned to CS+, CS− and neutral control stimulus. A control for attention was included which required subjects to signal the onset of a momentary light stimulus of low intensity.

The dependent variable was the difference between the magnitude of penile circumference to CS+ or CS− prior to the conditioning session and the magnitude of response during the post-conditioning test presentation. The data were treated by correlation, and for subjects overall a correlation of -0.82 ($p<0.025$, two-tailed) was found between the magnitude of CS+ and CS− conditioning. Extraversion scores were positively correlated with CS+ conditioning (0.88, $p<0.01$) and negatively correlated with CS− conditioning (-0.76, $p<0.05$); in other words, the extraverts responded with larger increases in amplitude to the CS+ stimuli as a consequence of treatment and with smaller decreases in amplitude in response to CS− stimuli.

The study has several interesting implications. In spite of the small numbers, the results were statistically significant at a satisfactory level and indicated that the abilities to 'turn on' (to stimuli associated with sexual arousal) and to 'turn off' (to stimuli associated with detumescence) are largely incompatible. The argument is put forward that the pre-orgasmic state is one of nonoptimal over-arousal, which could be expected to lead to conditioning superiority in extraverts, while the refractory phase of the post-orgasmic state could represent a nonoptimal under-arousal state, which would tend to favour the conditioning of introverts. The authors discuss their results in terms of Gray's extension of the Eysenck theory as an alternative model and point out that the pre-orgasmic phase is appetitive, while the post-orgasmic phase represents a condition of satiation and frustrative non-reward. These explanations are complementary, and they again raise the suggestion that the extraversion dimension contains components both of strength and dynamism, as defined by Nebylitsyn.

In the present context, it is of interest to ask what are the implications of findings of this sort for the concept of conditionability. The study is of obvious interest, in that conditioning in opposite directions was obtained in the same subjects differentiated by personality. Where a single response is used, the demonstration that one personality group is superior to another under specified conditions cannot be addressed to the problem of conditionability. However, the negative correlation of positive and negative conditioning in the same subjects places the matter in a different context. It suggests among other things that the definition of conditionability should not be allowed to hinge on the direction of conditioning, and this is consistent with the neo-Pavlovian formulation separating positive and negative conditioning. In this study it can be said that the introverts conditioned 'poorly' under positive conditioning but 'well' under negative conditioning. It can be argued that this represents a single dimension of 'conditionability'.

Finally, we turn to the one study which has been directly concerned with the problem of conditionability and has found positive results. Barr and McConaghy (1972) compared a number of conditioning measures of penile volume response (PVR) and skin conductance response (SCR) under two experimental paradigms, appetitive and aversive. The study was conducted on 62 male student volunteers, who were tested in separate occasions on the same day, one half being assigned first to each of the experimental conditions. For the appetitive paradigm, film presentations of attractive female nudes paired with a visual CS+ (red circle) alternated with male nudes paired with CS− (green triangle). For the aversive paradigm, unpleasant shock was paired with CS+ (500-Hz tone) alternated with CS− (1500 Hz tone). Heart rate (HR), SCR and PVR were monitored continuously. For the aversive stimulus condition, penile responding was measured as a decrease in volume. Of the total group of subjects, 8 failed to show significant differentiation of penile responding to the male and

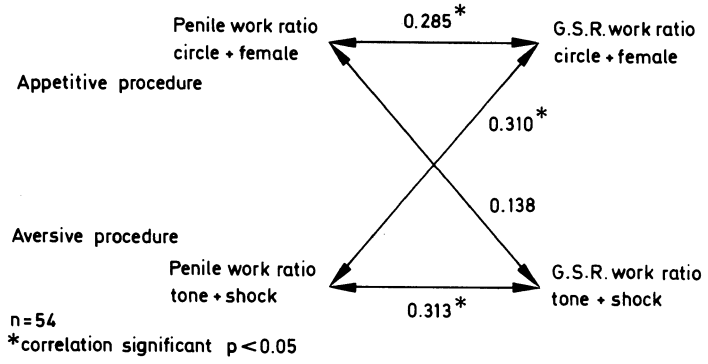


Fig. 5.20. Correlations among work ratio measures of conditioning across response systems and experimental paradigms. (After Barr and McConaghy 1972)

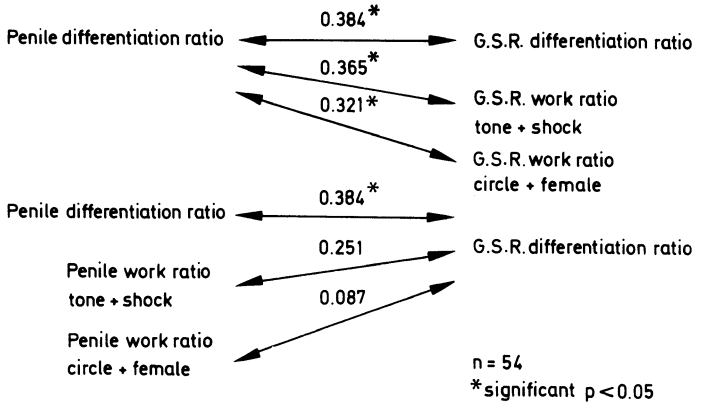


Fig. 5.21. Correlation among differentiation ratios and work ratios across response systems. (After Barr and McConaghy 1972)

female nude sequences and the overall analyses were confined to the remaining 54.

Results for response amplitude and rate of acquisition were comparable to those found in earlier studies. That is, the measures were correlated within response systems but showed no significant relationships across systems. It is well known that response amplitude and frequency are related to the responsivity of the system conditioned. The authors argue that this fact may obscure the measurement of conditionability, since individual differences in responsivity in the systems measured may not be comparable between subjects. To test this supposition they used two measures which control for differences in responsivity: the work ratio measure of response efficiency described earlier, and a measure of differentiation, taking the difference between total responses to CS+ and CS− as a ratio of total response to the CS-UCS complex. The relationships among the work ratio measures are diagrammed in Fig. 5.20.

Of the four correlations, three were significant, while the fourth was in the predicted direction. In other words, the level of conditioning indexed by the efficiency measure was consistent within response systems and across experimental procedures. A further test of the conditionability hypothesis is to compare the differentiation ratios which measure the degree of discrimination of CS+ and CS− within each response system with the comparable differentiation and work ratios of the other systems under each of the procedures. The results of this comparison, involving six correlations, are diagrammed in Fig. 5.21. Again, the results appear to favour a factor of conditionability. The authors remark, with commendable restraint, "It is concluded that when the influence of reflex sensitivity is minimized, conditioning of penile volume changes is significantly related to conditioning of galvanic skin responses" (p. 226). In a less cautious vein it can be suggested that this study, the first to provide support

for a general factor of conditionability, opens the door to a considerable methodological advance with the notion of controlling for a variety of individual differences which may mask the underlying factor of conditionability.

Clearly, however, the issue of conditionability is complex and the theoretical problems are in need of further refinement and redefinition. One further approach to the problem, given the methodological and theoretical problems discussed above, would be to apply the method of twin studies to carefully selected conditioning measures. A study that adequately controlled and compared differences in peripheral responding and demonstrated heritability of conditioning could be taken as reasonably strong evidence for the existence of a factor of conditionability.

5.6 Conditioning and Personality

More than half a century has passed since Watson decreed that personality is 'but the outcome of our habits'. New personality theories have recently appeared which rely on the concepts of learning theory to explain individuality, yet avoid the proposition that individual differences cannot therefore interact with learning (e.g. Staats 1975). The established theories of personality have drawn on learning theory to elucidate and enrich the description of personality on the one hand and to qualify and enlarge the description of learning on the other (e.g. Eysenck 1967; Gray 1970). Meanwhile, the neo-Pavlovian group have used a fine-grained analysis of human performance in learning situations to evolve new models of the interaction of factors which govern individual differences in that performance (e.g. Nebylitsyn 1972a). The case for including individual differences within the scientific study of learned behaviour would appear to have been established.

Against this encouraging background, it remains to restate the main arguments of this chapter and to indicate the directions of future research which seem promising or inevitable. Three main points are worth re-examining in the interests of conceptual clarity, away from the bustle of detail surrounding their initial presentation. Firstly, it has been argued that the higher-order constructs of personality theory offer more general and more powerful conceptual tools for the understanding of behaviour, and of conditioning phenomena in particular, than are offered by the study of isolated aspects of individual performance. Individual traits such as anxiety, impulsivity or sensation seeking, individual differences in orienting behaviour, attention or arousal are interesting in their own right, and it is important to understand their influence on conditioning and learning. It is much more important to integrate these individual behaviours into a general predictive model of individual behaviour, that is, into a theory of personality. At the same time it should be recognized that the onus is on the personality theorists to do the integrating, when new dimensions of individual differences are established by investigators not primarily committed to personality theory.

Secondly, it is important to recognize the conceptual and methodological differences which distinguish two quite separate approaches to the study of individual differences in conditioning. In spite of the close parellels, for example, between the school of Eysenck and that of Nebylitsyn, they arise from entirely distinct theoretical bases. The former approach takes a general theory of personality and uses it to predict to individual differences in conditioning. The latter approach starts from closely observed differences in conditioning performance and uses these to abstract general properties of the nervous system, from which to predict further important differences in behaviour. There is no room here for rhetorical arguments in favour of one approach or against the other. There is room for both. Indeed they represent alternative strategies for approaching the same goal: a better understanding of individual behaviour in the context of specific stimulus situations. In these terms the two approaches can be seen as mutually advantageous and it is hoped that the work of collation begun by Gray (1969) and others will be extended.

Finally, the question of the role of stimulus conditions in determining individual differences

in conditioning performance, just mentioned *sotto voce*, deserves separate restatement. The principal outcome of the decade of controversy described earlier was the constructive reformulation of each of the theories in terms of the conditions under which they should hold true. This is an inevitable step in the development of any science and represents a necessary refinement of the theoretical bases of scientific understanding. In the case of personality theory, and of conditioning theory, their application to wider fields and particularly to practical problems requires a careful examination of the stimulus milieu. For example, does the learning of social behaviours in the home or in the classroom occur under conditions of low or high arousal? Ultimately, this type of question can only be answered by empirical examination, and the time is probably ripe for finding the ways and means for conducting objective investigations in this area.

The literature suggests that interest in conditioning and personality is due for a healthy growth spurt after a period of relative quiescence. Inevitably, this renewal of interest must lead to a close integration of studies derived initially from personality theory and those derived primarily from conditioning theory. Hopefully, it will also include consideration, on the one hand, of the rapidly increasing understanding of brain mechanisms in behaviour, and on the other, of the renewed appreciation of the role of cognitive determinants in human performance. With regard to the former, newer concepts of arousal and stimulus registration in both the limbic system and the hippocampus, together with developing knowledge of hemisphere differences, may be expected to contribute to theoretical refinement. As to the latter, the current cognitive backlash against the domination of the S-R establishment in behaviour theory will undoubtedly direct the attention of serious students to the role of cognition (properly defined and anchored in observation) in stimulus processing in the conditioning laboratory.

It is to be hoped that the study of personality will be extended to encompass the new findings and concepts which are emerging from the study of conditioning and learning. These include the effects of pre- and post-presentation of both the conditioned and the unconditioned stimuli and the role of compound stimuli, including contextual stimuli, in modifying conditioned behaviour. Perhaps the most important movement in this area is the rapidly developing integration of the methods and concepts of classical and operant conditioning to produce important models of their interaction. Taken together, each of these developments, the study of brain mechanisms, of cognitive determinants and of learning situations should eventually contribute to an increased understanding of conditioning and personality and to a closer approximation to a working model of human behaviour.

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Chapter 6

Learning, Memory and Personality

M.W. Eysenck

6.1 Introduction

There is a tremendous volume of research concerned with the effects of individual differences on learning and memory (see M.W. Eysenck 1977 for a review), and the individual-difference variables investigated include cognitive factors (e.g. intelligence), motivational-emotional factors (e.g. anxiety) and purely motivational factors (e.g. need for achievement). The emphasis in this chapter will be on personality factors of the motivational-emotional kind, especially those that appear to constitute major, consistently replicable, personality dimensions. There is very substantial evidence (e.g. H.J. Eysenck 1967) that the orthogonal personality factors of neuroticism and introversion–extraversion fulfil these criteria, as does anxiety. It is reasonable to assume that the anxiety dimension, as measured by tests such as the Manifest Anxiety Scale (Taylor 1953), lies within the two-dimensional space defined by introversion–extraversion and neuroticism, correlating approximately +0.3 to +0.4 with the introversion end of the introversion–extraversion dimension and +0.6 to +0.7 with the neuroticism end of the neuroticism–stability dimension (Eysenck 1973).

An explicit limitation of the coverage of this chapter is that it deals primarily with verbal learning and memory rather than, for example, perceptual-motor learning. However, the latter topic is dealt with in considerable detail by H.J. Eysenck and Frith (1977). While it is as yet unclear whether the effects of personality on different kinds of learning are comparable, it is probably wisest to resist the temptation to extrapolate the findings on verbal learning to other areas of research.

The chapter is divided into three main sections. The first section is devoted to an analysis of some of the major theoretical information-processing constructs that have potential relevance for work on individual differences; the second section deals with selected highlights of the voluminous literature on the effects of anxiety on learning and memory; and the third section evaluates work on the personality dimension of introversion–extraversion.

6.2 Basic Theoretical Constructs

6.2.1 Attention: Selectivity and Intensity

As will be seen later, many theorists have assumed that personality effects on learning and memory are mediated by the effects of individual differences in arousal on attentional mechanisms. At the most general level, it is possible that there are important individual differences either in the *selectivity* of attention or in the *intensity* of attention, or both. An extremely influential theory of the former kind, which related arousal to attentional processes, was put forward by Easterbrook (1959), who argued that states of high emotionality, arousal and anxiety produce restrictions in the range of cue utilization. More specifically, Easterbrook argued that the progressive reduction in the range of cues used as arousal increases, "will reduce the proportion of irrelevant cues employed, and so improve performance. When all irrelevant cues have been excluded, however,... further reduction in the number of cues employed can only affect relevant cues, and proficiency will fall" (p. 193). It is worth noting at this point

that this theory appears to make some curious predictions; for example, the assumption is that performance decrements under high anxiety are attributable to great concentration on only certain task elements, whereas much evidence indicates that anxiety leads to increased distractibility. A second bizarre prediction is that low arousal (e.g. boredom) is associated with an excessive openness to experience.

In spite of these difficulties, there is reasonable empirical support for Easterbrook's hypothesis, which has usually been tested by observing performance on concurrent primary and secondary tasks. The most natural prediction is that high arousal will increase the attentional bias towards the primary task at the expense of the secondary task. This result was obtained in several of the studies reviewed by Easterbrook (1959). One ambiguity in the data from many of the studies is whether the reduced responsivity to subsidiary-task stimuli under high arousal reflects an actual attenuation of sensitivity or whether it is due to an increase in the subjective response criterion. Bacon (1974) investigated this issue in a study in which subjects concurrently performed a pursuit-rotor tracking task and an auditory signal-detection task, with arousal being induced by painful electric shocks. As implied by Easterbrook's hypothesis, the stimulus loss under high arousal resulted from a reduction in sensitivity to the signal rather than from a change in the response criterion.

Walley and Weiden (1973) have extended some of Easterbrook's ideas and proposed a speculative mechanism to explain the reduction in cue utilization under high arousal. They argued that pattern recognition involves a hierarchical network of feature analysers, and that encoding represents the function of the highest level of the pattern-recognition network. Encoding can, at least potentially, be a parallel process, but the encoding of one input tends to interfere with the concurrent encoding of further inputs, the degree of interference being directly related to the similarity relations existing among the encodings. They used the term 'cognitive masking' to refer to this interference effect, and claimed that it was due to inhibitory interactions among cortical neurons at the highest level of the hierarchical pattern recognizers. An assumption of most relevance in the present context was that the degree of cortical lateral inhibition is positively correlated with arousal, so that increases in arousal produce increased cognitive masking. As a result of this increment in cognitive masking, parallel or shared processing becomes decreasingly possible as the level of arousal increases.

In spite of the fact that there is no direct physiological evidence supporting Walley and Weiden's (1973) hypotheses, it certainly appears to be the case that states of arousal produced by reticular formation activity exert both inhibitory and excitatory influences on cortical responsiveness (e.g. Demetrescu et al. 1965). Their data suggested that, in addition to the reticular activating system, there was also a diffuse ascending inhibitory system originating in the ventral pontine reticular formation. This particular inhibitory system apparently activates an intra-cortical inhibitory network which is excited by specific input to the cortex and may be the source of cognitive masking.

A major inadequacy of the Easterbrook formulation is that it assumes that very variegated methods of producing arousal (e.g. white noise, incentives, anxiety and threat of shock) all have comparable effects on attentional processes. In essence, the evidence indicates that several different stressors or arousing agents do increase attentional selectivity, but there is much more variability in terms of the effects of stressors on attentional capacity. M.W. Eysenck (in preparation) has reconsidered the data from studies investigating the effects of arousal on concurrent main and subsidiary tasks. While there are fairly consistent detrimental effects of most arousing agents, interesting differences are evident when main-task performance is evaluated. Anxiety, whether measured by questionnaire or threat of electric shock, typically had no effect on main-task performance, whereas other arousing agents such as incentives or white noise often enhanced main-task performance. Anxiety improved performance significantly on the main task only once in 18 comparisons, whereas non-anxiety stressors improved main-task performance to a significant extent in 10 out of 19 comparisons. This difference between

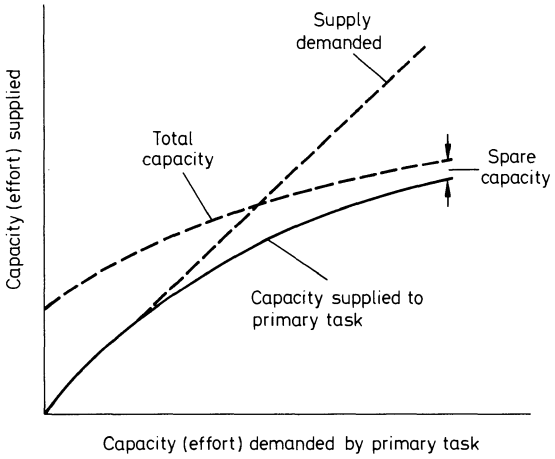

Fig. 6.1. The relationship between task demands and capacity supplied. (After Kahneman 1973)

the effects of anxiety and of other stressors on main-task performance is highly significant. The implication is that high anxiety leads to increased attentional selectivity and reduced attentional capacity with respect to the performance tasks, whereas white noise and incentives increase attentional selectivity without necessarily reducing attentional capacity.

A general theory of attention and arousal that obviates some of the problems with Easterbrook's hypothesis and that has considerable relevance to the position taken in this chapter was proposed by Kahneman (1973). He argued that attention involves not only selection of certain inputs rather than others, but also has an intensive component which 'corresponds to effort rather than to mere wakefulness' (p. 4). If a task imposes severe processing demands, then the subject will respond with increased effort or attentional capacity. Kahneman claimed that the amount of effort invested in a task was mainly determined by the intrinsic demands of the task, although he recognized that there might additionally be some effects of incentives and voluntary control on mental effort.

One of the main hypotheses put forward by Kahneman (1973) is illustrated in Fig. 6.1. As the processing demands of the primary or main task increase, there are three important consequences: (1) capacity or effort increases; (2) there is an increasing discrepancy between demand and supply of effort; and (3) spare processing capacity varies inversely with the amount of effort invested in the main task.

A crucial problem concerns the measurement of mental effort. Kahneman (1973) suggested that increased effort would be associated with heightened arousal and indicated that pupillary dilation or skin conductance might prove to be appropriate indices. The major difficulty is that high arousal may either reflect the subject's active processing activities and mental effort, or it may reflect what is happening to him or the stress to which he is exposed.

So far, we have seen that the evaluation of demands on the limited capacity and external stressors can produce both increased arousal and increased attentional capacity. The ways in which the available capacity is used depend on the allocation policy, which is controlled by four factors: (1) enduring dispositions based on the rules of involuntary attention (e.g. allocate attention to a novel stimulus); (2) momentary intenions (e.g. detect the letter 'e' in a visual display); (3) the evaluation of demands (i.e. if two or more activities demand more capacity than is available, one is completed); and (4) high arousal causes attention to be concentrated on the dominant and most obvious situational features, it reduces the ability to discriminate relevant from irrelevant sources of information, and it increases the lability of the allocation policy.

The basic notion that the available attentional or processing capacity is affected systematically by motivational factors, by the cognitive evaluation of task demands and by external stressors is an appealing extension of earlier

limited capacity theories. As will be argued subsequently, the effects of individual differences in anxiety and in introversion–extraversion on performance can be clarified in terms of Kahneman's conceptual analysis.

6.2.2 *Working Memory*

An impressive attempt to integrate thinking on problems of attention and short-term memory was made by Baddeley and Hitch (1974). They proposed the notion of 'working memory', comprising the two separable components of a modality-free central processor and an articulatory loop. While the central processor is necessarily involved in a wide range of cognitive tasks, use of the articulatory loop is largely optional; its function is primarily to allow temporary storage of a limited amount of information in a phonemic code at relatively low 'cost' to the processing system.

Some evidential support for this conceptualization was obtained by Baddeley and Hitch (1974). They used a pre-loading paradigm in which subjects were asked to remember up to six random digits in the correct order, followed by a verbal task (comprehension, reasoning or free recall), followed by serial recall of the digit string. As expected, there was a performance decrement on the interpolated verbal task when subjects had to retain as many as six digits. However, a more interesting finding was that the verbal task was performed equally well whether the subject was retaining three digits or no digits (control condition). These data can be explained by assuming that the articulatory loop has a capacity of approximately three items and that all the verbal tasks rely mainly on the central processor. Since the central processor would thus only be substantially involved in the digit-string task if more than three digits had to be retained, it was only with such digit strings that the two tasks could not be combined.

Baddeley and Hitch (1974) argued that their data suggested that the central processor and the articulatory loop could be used concurrently and in parallel. However, a methodological problem with the pre-load technique was identified by Hitch and Baddeley (1976), who pointed out that the subjects in their earlier study were not necessarily actively processing the digits at all during the performance of the interpolated verbal task. Accordingly, they used a concurrent-load technique in which subjects had to repeat a random sequence of six digits out loud at high speed throughout the performance of a verbal reasoning task. While this retention task produced a significant decrement in performance on the task of verbal reasoning, other forms of interpolated activity (e.g. cyclical repetition of 'one-two-three-four-five-six') had little effect on verbal reasoning. The most natural interpretation of these data is that the concurrent load of six digits impaired verbal-reasoning performance because of the involvement of the central processor, rather than simply due to use of the articulatory loop.

There is some available evidence on the effects of arousal on working-memory capacity, using a letter-transformation task in which subjects have to add up to four letters to each of between one and four letters. For example, the problem 'D+2' requires the answer 'F', and the answer to 'FAJR+4' is 'JENV'. This task has the advantage of permitting systematic changes in the relative weighting of processing speed and 'holding' capacity across the various conditions. On this task, arousal produced by white noise appears to increase processing speed but to reduce capacity (Hamilton et al. 1977), as can be seen in Fig. 6.2. This result is broadly consistent with Easterbrook's (1959) hypothesis, according to which increased arousal leads to a narrowing of attention. However, in recent work by M.W. Eysenck (unpublished) it was found that arousal induced by monetary incentives led to a considerable improvement in both processing speed and working-memory capacity (see Fig. 6.2). The enhancement effect of the incentives was of the order of 30–40% in all conditions.

This differential effect of incentives and white noise on working-memory capacity may well be of considerable theoretical importance. Since incentives appear to affect performance by leading to the setting of more difficult goals and increased effort, it may be the case that effort is positively related to the effective capacity of

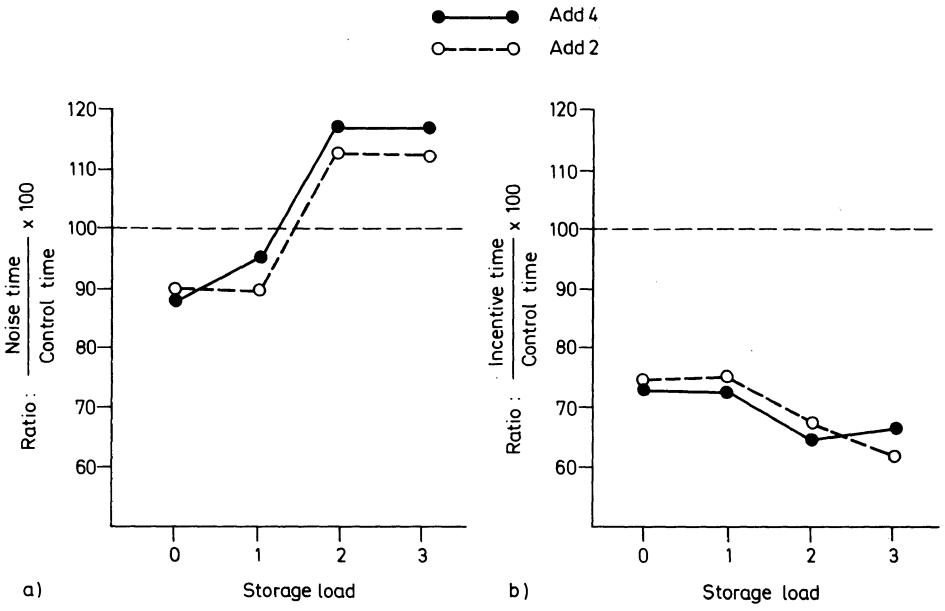

Fig. 6.2. Performance on the letter-transformation task as a function (**a**) of white noise and (**b**) of monetary incentive. (**a**) is after Hamilton et al. (1977)

working memory capacity (cf. Kahneman 1973). The effects of white noise on performance are probably mediated by several mechanisms. However, one important factor was identified by Thayer (1978), who discussed data on the effects of white noise on self-report measures of activation and tension or anxiety. The most consistent finding was that white noise increased tension or anxiety, and it may be that the detrimental effects of white noise on working-memory capacity are due to anxiety. In anticipation of later discussion of more of the evidence, we tentatively conclude that arousal deriving from increased effort increases working-memory capacity, whereas arousal that is associated with anxiety reduces working-memory capacity.

6.2.3 Summary

Some of the major theoretical constructs in the field of attention and short-term memory have been outlined. It will be argued subsequently that personality effects on learning and memory are importantly determined by individual differences in attentional mechanisms. Already we have seen that arousal has substantial and widespread effects on the selectivity and intensity of attention and on the capacity of working memory. The relevance of such evidence is that individual differences in arousal are frequently thought to underlie the personality dimensions of introversion–extraversion and of anxiety.

6.3 Effects of Anxiety on Learning and Memory

6.3.1 Spence and Spence (1966)

The most highly developed theory of the effects of anxiety on learning was put forward by Spence and Spence (1966) and discussed by M.W. Eysenck (1977). Their theory can appropriately be considered in the context of the Yerkes-Dodson Law (Yerkes and Dodson 1908). This 'law' postulated that there is an inverted-U relationship between arousal and performance and that the optimal level of arousal is inversely related to task difficulty. The basic problem with the Yerkes-Dodson Law is that it merely describes predicted relationships among arousal, task difficulty and

performance without providing any insights into the factors producing these relationships.

Spence and Spence's (1966) theory appeared inter alia, to provide a potential underpinning for the Yerkes-Dodson Law. Their theory started with the basic Hullian notion that habit strength (an index of the degree of learning) multiplied by drive (a motivational concept) produced excitatory potential. In any situation, the habit of greatest strength will manifest itself in performance, provided that the level of excitatory potential produced by that habit when multiplied by drive exceeds the response threshold. In addition, it was argued that the level of drive was a function of a persistent emotional response aroused by aversive stimuli. Among the various factors producing this emotional response are ego-involving instructions, electric shock and the subject's emotional responsiveness, usually indexed by the Manifest Anxiety Scale. Finally, the emotional response not only partially determines the amount of drive, but also produces drive stimuli which lead to task-irrelevant behaviour.

This theory predicts the interaction between anxiety and task difficulty in the following way. Since there is a multiplicative relationship between drive and habit strength, an increase in drive produced by anxiety will increase the difference in probability of two responses differing in habit strength. Anxiety would thus cause the stronger of two competing responses to become still stronger, leading to the prediction that anxiety would increase the efficiency of performance on tasks such as simple conditioning, where the correct response has no effective competitor. Spence (1964) reported that this prediction had been confirmed in 21 of 25 independent comparisons of high- and low-anxiety subjects, as measured by the Manifest Anxiety Scale.

The position is rather different with more complex tasks, in which the correct response has to be discriminated from other strongly competing responses. In this case, an increase in anxiety would make the incorrect responses still stronger relative to the correct response, thus leading to a decrement in performance. The evidence is generally supportive of this prediction (M.W. Eysenck 1977), with the work of Spence et al. (1956a, b) being of especial interest. In both studies, paired-associate tasks involving non-competitive and competitive pairs were used. Non-competitive pairs were formed by selecting words possessing an initial associative connection (e.g. 'tranquil-placid'), whereas competitive pairs were formed by selecting words with minimal associative connection and a stimulus term that was similar in meaning to the stimulus word in one of the non-competitive pairs (e.g. 'serene-headstrong'). As predicted, high anxiety improved the speed of acquisition of the non-competitive pairs, but reduced the rate of learning of the competitive pairs. There is an important qualification associated with the prediction that anxiety will reduce the acquisition speed of competitive pairs. As learning progresses and the correct responses start to become stronger than the incorrect response, high-anxiety subjects should surpass low-anxiety subjects. That prediction was confirmed by Standish and Champion (1960).

One difficulty with the theoretical approach of Spence and Spence (1966) is that there are situations in which the theory makes exactly the opposite predictions from those discussed so far. For example, consider a task in which there is a dominant response and a weak competing response. Under conditions of low anxiety, only the dominant response exceeds the response threshold, thus guaranteeing perfect performance. Under conditions of high anxiety, on the other hand, the excitatory potential of the weak competing response now exceeds the threshold (because anxiety increases the excitatory potential of all responses) and thus has some finite probability of being elicited. Therefore, the prediction must be that anxiety will reduce performance on this easy task.

It is also relatively straightforward to derive the opposite predictions from those of Spence and Spence (1966) on a difficult task where the correct response is not dominant. For example, high-anxiety subjects will outperform low-anxiety subjects on a difficult task if the correct response is below the threshold for the high-anxiety group. Thus the Spence and Spence approach only generates unambiguous predictions provided that detailed information is available about the number and relative strengths of all

the relevant responses and about the positioning of the response threshold. Since there is usually no suitable method of obtaining this information, the theory is weaker in its predictive power than has often been assumed.

While Spence and Spence (1966) argued that the effects of anxiety on learning depended on the extent of intra-task response competition, most studies have failed to distinguish between response competition and task difficulty, thus confounding two conceptually distinct variables and rendering any interpretation equivocal. Saltz and Hoehn (1957) attempted to unconfound the two factors in two separate experiments. In the first, subjects learned a serial list of nonsense syllables. Two lists were constructed, one consisting of familiar syllables with a high level of intra-list competitiveness produced by letter duplication and the other consisting of less familiar syllables with little intra-list competitiveness. The two lists proved to be of equal difficulty for low-anxiety subjects, as identified by the Manifest Anxiety Scale. Since the theory put forward by Spence and Spence (1966) argued that high anxiety would augment the difference in response probability of two competing responses, it follows that high-anxiety subjects should take longer to learn the competitive list than the non-competitive list. In fact, the difference was non-significantly in the opposite direction.

Saltz and Hoehn (1957) used the same general approach in their second experiment to produce a non-competing list that was more difficult than a competing list. There was no effect of anxiety on the easy competitive list, but anxiety was inversely related to learning speed on the hard non-competitive list. The results from this study thus suggest that anxiety interacts with task difficulty rather than degree of response competition per se.

Since the Spence-Spence approach has not been successful in explaining the interaction between anxiety and task difficulty, other interpretations must be considered. One plausible factor is that feelings of failure are likely to be more prevalent when attempting an extremely demanding task than when attempting a very straightforward task. Tennyson and Wooley (1971) gave their subjects concept acquisition self-instruction tasks with both difficult and easy materials. They found that the mean state-anxiety score on the State Trait Anxiety Inventory following the difficult task was significantly higher than the mean obtained after the easy task. Spielberger et al. (1972) also found that difficult learning tasks increased state anxiety much more than easy tasks, especially during the initial stages of learning.

The greatest inadequacy with the theory proposed by Spence and Spence (1966) is that it assumes that the effects of anxiety are centred exclusively on retrieval processes and that anxiety does not affect other aspects of information processing (e.g. attentional and encoding processes and the response threshold). Recent research has indicated the inadequacy of assuming that anxiety does not affect the response threshold. Since high drive in the form of anxiety should theoretically have the effect of raising additional items above the response threshold, it follows that high-anxiety subjects should recall more items than low-anxiety subjects on a free-recall test. However, Rogers and Battig (1972) and Mueller (1976) failed to obtain any differences in free recall as a function of subject anxiety, and Mueller (1977) actually found that low-anxiety subjects had significantly higher levels of free recall than high-anxiety subjects.

Work more directly relevant to the effects of anxiety on response thresholds was carried out by Goulet and Mazzei (1969), uding a paired-associate task. They calculated a 'confidence threshold' for each paired associate, defined as the trial on which a response was given for the first time to the stimulus member of the pair, irrespective of the correctness of the response. Low-anxiety subjects had a lower confidence or response threshold than high-anxiety subjects, leading Goulet and Mazzei to conclude as follows: "The high-anxiety subjects may withhold responding until fairly confident of the stimulus-response pairings, whereas low-anxiety subjects may require a lower degree of confidence, and thus respond earlier in practice where a lower degree of associative strength exists between stimuli and responses" (p. 251).

Apparently discrepant data were obtained by Clark and Greenberg (1971), who used one learning trial followed by three recognition-test

trials. The recognition data were analysed in terms of signal-detection theory measures, and there was an interaction between instruction-induced stress and trials with respect to the response criterion. In this interaction, the response criterion decreased over trials with stress and increased without stress, with the consequence that the response criterion was more stringent for unstressed than for stressed subjects by the third test trial.

The various findings indicate that Spence and Spence (1966) were wrong to assume that anxiety does not affect the response threshold or criterion, in spite of some inconsistencies in the empirical evidence. In fact, a fairly direct and obvious theoretical prediction seems to follow from the signal-detection theory analysis of the determinants of the placement of the response criterion, which emphasizes the role played by the gains associated with correct responding and the costs associated with incorrect responding. It is plausible that anxiety induced by fear of failure and punishment should augment the subjective costs associated with inaccurate responding (i.e. false alarms) and thus lead to the adoption of a relatively stringent response criterion.

In retrospect, one of the most surprising aspects of the enormous literature investigating the theories of Spence (1958) and Spence and Spence (1966) is the almost complete failure to examine at an empirical level the crucial theoretical assumption that anxiety does not affect learning and storage, but does affect retrieval. Most of the studies failed to separate out the effects of anxiety on storage and on retrieval, so that the data obtained are entirely equivocal. There has been an increasing tendency in recent years to argue that anxiety has important effects on attentional processes, over and above any effects on retrieval (e.g. M.W. Eysenck 1977; Mueller 1976). An appropriate paradigm for investigating this issue was used by Straughan and Dufort (1969). Their subjects were of high- or low-anxiety, and they received relaxation instructions either prior to list study or prior to recall. There was a significant interaction between anxiety and relaxation in terms of speed of correct responding, with subjects who were low in anxiety being slowed by relaxation instructions, whereas the performance of subjects high in anxiety was facilitated. The most important result was that relaxation prior to acquisition was more effective than relaxation prior to recall in making the high-anxiety group faster and the low-anxiety group slower, suggesting that the effects of anxiety manipulations on learning are as strong as, or stronger than, those on retrieval.

Research into the Spence (1958) and Spence and Spence (1966) formulations has, until fairly recently, tended to obtain groups of high- and low-anxiety groups on the basis of scores on the Manifest Anxiety Scale, a measure of trait anxiety. Some doubts have been raised about the validity of the Manifest Anxiety Scale. For example, Kimble and Posnick (1967) re-wrote the test, preserving the formal elements of each question, but eliminating the anxiety-related content, and found that this new measure correlated substantially with the original Manifest Anxiety Scale. This finding raises the question of whether the Manifest Anxiety Scale might be measuring something other than anxiety. At the theoretical level, the test is only an appropriate measuring instrument within the Spence-Spence formulation provided that individuals scoring high on the Manifest Anxiety Scale are chronically higher in emotional responsiveness than those scoring low. However, Spielberger (1972) reviewed the relevant evidence and concluded that high and low Manifest Anxiety Scale scorers only differ in anxiety under conditions of ego threat. Accordingly, the Manifest Anxiety Scale is obviously not an entirely suitable measure of emotional responsiveness or drive. Spielberger (1966, 1972) has usefully extended the Spence-Spence approach by resurrecting the distinction (originated by Cicero) between state anxiety and trait anxiety. He and his associates (Spielberger et al. 1970) have devised an appropriate instrument, the State Trait Anxiety Inventory, to measure these two kinds of anxiety. They proposed that state anxiety is 'characterized by subjective, consciously perceived feelings of tension and apprehension, and heightened autonomic nervous system activity' (p. 3), whereas trait anxiety 'refers to relatively stable individual differences in anxiety proneness' (p. 3).

As Spielberger et al. (1972) pointed out, it is reasonable to argue that state anxiety is more closely related to the Hull-Spence concept of drive than is trait anxiety. In a series of experiments, they related anxiety as measured by the State Trait Anxiety Inventory to performance on computer-assisted learning. In three separate experiments, they failed to obtain significant effects of trait anxiety on performance, whereas state anxiety was significantly negatively related to performance under difficult task conditions. However, in some of their comparisons, the direction of causality is in doubt: state anxiety may produce poor performance, and/or poor performance may produce state anxiety.

The overall conclusion on the Spence and Spence (1966) theory is that it is seriously deficient as a general approach to the effects of anxiety on learning and memory. While recent research is increasingly demonstrating a wide range of effects of anxiety on information processing, Spence and Spence focused only on the retrieval process. In spite of the fact that the theory apparently makes clear-cut predictions about performance, this is usually not the case, for the following reasons: (1) predictions can only safely be made when complete knowledge of the response strengths of the correct response and all competing responses is available; and (2) in conditions of low response competition, the prediction appears to be that high anxiety will enhance performance; however, anxiety also produces drive stimuli which generate task-irrelevant behaviour, so that detrimental effects of anxiety on performance can also be 'explained'. Furthermore, it is not clear that response competition rather than task difficulty is the key factor in determining the effects of anxiety on task performance. Finally, the evidence suggests that trait anxiety, which was the variable investigated by Spence and Spence, is less importantly related to performance than is state anxiety.

6.3.2 Anxiety: Cognitive Factors

A plausible theoretical framework for the interpretation of experienced emotional states such as anxiety was proposed in a classic study by Schachter and Singer (1962). Their basic hypothesis, for which they obtained supporting evidence, was that experienced emotion is multiplicatively determined by the two factors of level of arousal and cognitions about the arousing situation.

In broad agreement with the Schachter-Singer approach, Russell and Mehrabian (1977) factor-analysed data from 42 verbal-report emotion scales and found that the three independent and bipolar dimensions of pleasure-displeasure, degree of arousal and dominance-submissiveness were both necessary and sufficient to define emotional states. Six scales measuring fear or anxiety were considered by Russell and Mehrabian, including the state-anxiety scale of the State Trait Anxiety Inventory. Regression analyses produced a consistent pattern of results over all six scales, in which easily the major component of anxiety was displeasure, followed by high arousal and submissiveness. If, as Schachter and Singer's (1962) theory suggests, the displeasure component of anxiety is based on cognitive appraisal, then theories of anxiety must consider cognitive factors. The fact that self-reported state anxiety is far more closely associated with displeasure than with high arousal is presumably of relevance in explaining the relatively weak evidence of greater physiological arousal in anxious individuals (reviewed by Eysenck 1977).

A plausible application of this general approach to anxiety is discussed by Morris et al. (1977). Factor analyses of Mandler and Sarason's (1952) Test Anxiety Questionnaire indicated that test anxiety comprises the two separable components of worry and emotionality. Worry is the cognitive aspect of anxiety, involving conscious concern regarding one's performance and its consequences, negative task expectations and negative self-evaluations, whereas emotionality involves changes in physiological functioning and directly accompanying unpleasant feeling states of uneasiness, tension and nervousness.

One of the several pieces of evidence supporting the proposed distinction between the cognitive and physiological components of anxiety was obtained by Spiegler et al. (1968). Five days before an important examination for graduate students, worry scores were already elevated,

whereas emotionality scores were not. In addition, emotionality decreased significantly from immediately before to immediately after the examination, whereas worry scores remained unchanged.

It has frequently been suggested in the literature (e.g. Morris et al. 1977; Sarason 1975; Spence and Spence 1966; Wine 1971) that a major reason for detrimental effects of anxiety on performance is the presence of task-irrelevant cognitive activities associated with high anxiety. This viewpoint was expressed in the following way by Sarason (1975): "The highly test anxious individual is one who is prone to emit self-centred interfering responses when confronted with evaluative conditions. Two response components have been emphasized by writers who espouse this view. One is emotional and autonomic – sweating, accelerated heart rate, etc. The other concerns cognitive events – e.g., saying to onself while taking a test, 'I am stupid', 'Maybe I won't pass'." (p. 175)

Direct supportive evidence was obtained by Ganzer (1968), who studied the effects of audience presence and test anxiety on serial learning. A record was kept of the subjects' task-irrelevant comments while they were working on the task. His main finding was that, "high TAS (Test Anxiety Scale) scorers, especially in the Observed condition, emitted more than any other group. Content analysis revealed that the comments were mostly of a self-evaluative or apologetic nature" (p. 194).

Further evidence of the effects of anxiety on task-irrelevant behaviour was obtained by Nottelman and Hill (1977), who compared children who had high and low scores on the Test Anxiety Scale for Children on an anagram task. Task-irrelevant behaviour was indexed by the frequency and direction of off-task glancing. High-anxiety children had significantly inferior anagram performance than low-anxiety children and produced substantially more off-task glancing.

Theorists who have drawn the distinction between the emotionality and worry components of anxiety have almost invariably (and very plausibly) assumed that worry will consistently impair the quality of performance. At the empirical level, several studies have indicated that performance decrements are due more to worry than to emotionality (see Morris et al. 1977 for a review). For example, Doctor and Altman (1969) asked their subjects to answer worry and emotionality questions from the Test Anxiety Questionnaire in terms of their feelings immediately prior to an important examination. While both emotionality and worry were negatively correlated with performance, worry was the stronger determinant of poor performance. Similarly, Morris and Liebert (1970) found that correlations between worry scores and final examination grades, with emotionality partialled out, were negative and significant. On the other hand, correlations between emotionality scores and grades, with worry partialled out, were non-significant.

It is probable that the task-irrelevant cognitive activities associated with high anxiety play a major part in producing impaired performance. However, theories that emphasize the role played by worry seem to predict that anxiety will always reduce the quality of performance, and it is by no means obvious how they could handle facilitatory effects of anxiety. In addition, there has been an unfortunate failure to discriminate among the various types of task-irrelevant cognitive activities. It is presumably the case that mutual interference between task performance and task-irrelevant activities depends in large part upon the similarity of the two processing mechanisms. For example, if worry and related forms of verbal processing constitute the dominant task-irrelevant cognitive activity, then main tasks involving verbal processing should be more impaired than those dependent upon non-verbal processing.

6.3.3 Working-Memory Capacity

It is of obvious importance to investigate the relationship between anxiety and the capacity limitations of working memory. The reason is that working memory is substantially involved in the processing and temporary 'holding' of information, so that any reduction in its processing capacity would have wide-ranging effects on the performance of many tasks. In the light of the earlier discussion of the concept

of working memory, it would clearly be desirable to have information about the respective effects of anxiety on the articulatory loop and central modality-free components of working memory. However, the necessary research has not yet been carried out. In practice, investigators have shown an overwhelming preference for digit-span measures rather than any other index.

Since it is generally assumed that state anxiety is more closely related to performance than trait anxiety, it is appropriate to consider separately the effects of these two anxiety measures on performance. With respect to trait anxiety, the modal finding is that there is no effect of anxiety on digit span (Dunn 1968; Finch et al. 1976; Hodges and Spielberger 1969; Mueller 1976, 1978; Stewart and Davis 1974; Walker and Spence 1964). While Finch et al. (1976) found no overall digit-span difference between high and low scorers on the Children's Manifest Anxiety Scale, they also considered the separate effects on performance of the three constituent factors of that test (i.e. worry, physiology and concentration). Those subjects scoring low on concentration had highly significantly inferior digit-span performance in comparison with high scorers. Jurjevich (1963) obtained a significant positive relationship between Manifest Anxiety Scale scores and digit span among teenage delinquent girls, and Haynes and Gormly (1977) found a highly significant positive correlation of $+0.73$ between trait anxiety and digit span. On the other hand, Calvin et al. (1955) obtained a significant negative correlation between Manifest Anxiety Scale score and digit span, Mueller and Overcast (1976) also found that digit span was inversely related to anxiety, and Mueller (1977) discovered that both forward and backward digit span were significantly reduced among high scorers on the Test Anxiety Scale. In contrast, Mueller (unpublished work) found that anxiety, as measured by the Test Anxiety Scale, was negatively related to backward digit span but was unrelated to forward digit span.

Hodges and Durham (1972) obtained an interaction between trait anxiety and intelligence on a digit-span task. In this interaction, high anxiety was associated with better performance than low anxiety in subjects of high intelligence, but the opposite occurred among those of less intelligence. Finally, Knox and Grippaldi (1970) found that span performance was better among those of intermediate trait anxiety than among those of either high or low anxiety.

As yet, it cannot be claimed that any very consistent effects of trait anxiety on digit span have been demonstrated. However, several other studies have considered the effects of situational stress or state anxiety on digit-span performance, and a rather more coherent set of findings has emerged. The typical finding is that stress is negatively related to performance (Capretta and Berkun 1962; Dunn 1968; Griffiths 1958; Moldawsky and Moldawsky 1952; Pyke and Agnew 1963). There is also additional evidence that state anxiety reduces span performance (Firetto and Davey 1971; Hodges and Spielberger 1969; Walker et al. 1970; Walker and Spence 1964), although Knox and Grippaldi (1970) found that an intermediate level of state anxiety produced the best performance. An unfortunate aspect of the Firetto studies is that their subjects were asked after testing whether they had felt anxious during testing, thus raising the possibility that poor performance may have caused anxiety, rather than the reverse.

An alternative method of measuring short-term capacity in free recall was proposed by Tulving and Colotla (1970), an item being classified as recalled from short-term store if fewer than a given number of input-output events (e.g. seven) intervene between presentation and recall. Mueller and Overcast (1976) found that high-anxiety subjects had greater recall from short-term store than low-anxiety subjects according to this measure, but other studies failed to obtain anxiety effects (Brower and Mueller 1978; Mueller 1976). An interpretative problem with the Tulving-Colotla index is that it is affected by the extent to which subjects adopt the strategy of recall priority for last-presented items.

Overall, nine of eleven studies reporting a significant effect of state anxiety or situational stress on working memory capacity found that high anxiety reduced its capacity. This is one of the most reliable effects in the literature and may be of great import. However, as was

pointed out earlier, the major unresolved question is whether this detrimental effect is attributable to reduced efficiency of the modality-free central processor or of the articulatory loop. Clearly, if anxiety is only affecting the articulatory loop, then the findings are of limited generality and consequence, whereas the implications for information processing are considerable if anxiety has a detrimental effect on the central processor.

6.3.4 Levels of Processing and Elaboration of Encoding

The evidence reviewed in the previous section is consistent with the notion that anxiety tends to reduce available processing capacity. The probable consequence is that anxiety should lead to reduced encoding or processing of to-be-remembered material. In contemporary terminology, the expectation is that anxiety would reduce the elaboration of encoding by reducing the number and variety of stimulus attributes or features encoded, an hypothesis discussed in detail by Mueller (1979). Before evaluating the evidence for this hypothesis, a related theoretical notion will be discussed. Schwartz (1975) claimed, with supporting evidence, that, "arousal facilitates recall based on the actual physical properties of verbal stimuli but adversely affects memory for semantic features" (p. 2). It should be noted *ab initio* that in the two experiments which he reported, Schwartz assigned his subjects to different arousal groups on the basis of their neuroticism and extraversion scores on the Eysenck Personality Inventory. Since he regarded neurotic introverts as the highest arousal group and stable extraverts as the lowest arousal group, one might equally well refer to the dimension investigated as one of anxiety, since neurotic introverts are high in anxiety and stable extraverts are low in anxiety (Gray 1973).

In Schwartz's (1975) first experiment, the task involved paired-associate learning, with the response words being either all phonemically or all semantically similar. The evidence in the literature indicates that response similarity has an interfering or detrimental effect on learning.

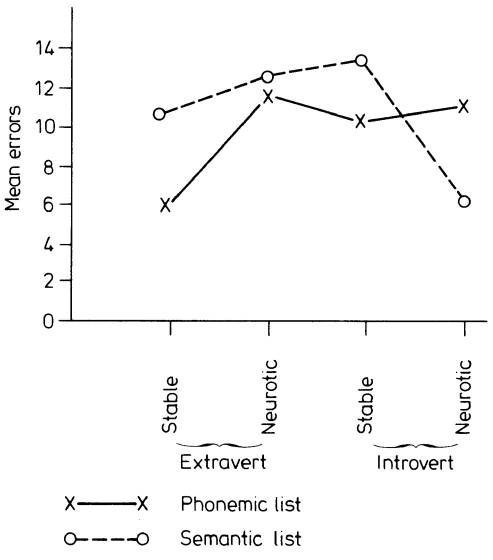

Fig. 6.3. Errors on a learning task as a function of personality and depth of processing. (After Schwartz 1975)

However, semantic response similarity will presumably only impair performance provided that semantic encodings are formed, and similarly, phonemic response similarity will only interfere if phonemic encoding occurs. In fact, there was a highly significant three-way interaction involving neuroticism, extraversion and list type (see Fig. 6.3). High-arousal (or high-anxiety) subjects were not adversely affected by semantic similarity, presumably because they were concentrating on the physical attributes of the presented material, and low-arousal (or low-anxiety) subjects, who focused on the semantic characteristics, were not detrimentally affected by phonemic similarity.

In the second experiment, Schwartz (1975) presented his subjects with a categorized word list in a random order, followed by free recall. Analysis of the recall data indicated that high-arousal, or high-anxiety, subjects recalled the list in a less semantically organized way than did subjects with lower levels of arousal or anxiety and also exhibited a tendency to recall the words in the order in which they were presented. The results indicate that high arousal or anxiety reduces semantic processing but enhances physical processing.

The hypothesis put forward by Schwartz (1975) is clearly related to the levels of processing approach of Craik and Lockhart (1972). In essence, they argued that stimulus encodings differ in terms of the amount of meaningfulness extracted from the stimulus, or, in their terminology, the 'depth' of processing. They regarded semantic processing as deep and phonemic or physical processing as shallow, and argued that retention was positively related to the depth of processing. Within the context of this approach, negative effects of anxiety might be due to a relative inability to process deeply or semantically under high levels of anxiety. While the Craik-Lockhart approach has proved stimulating, there are several unresolved problems with it (Eysenck 1978a, b, c). For example, since there is no independent measures of processing depth, there is, as M.W. Eysenck (1978c) pointed out, 'the danger of using retention-test performance to provide information about the depth of processing, and then using the putative depth of processing to 'explain' the retention-test performance, a self-defeating exercise in circularity' (p. 36).

Edmunson and Nelson (1976) compared paired-associate learning under either an effective deep processing strategy (i.e. interactive imagery) or an ineffective shallow processing strategy (i.e., repetition rehearsal). It was hypothesized that if high-anxiety subjects are normally less likely to utilize deep imaginal attributes in their learning, then the imaginal processing strategy should be of more assistance to high- than to low-anxiety subjects. Subjects were assigned to groups on the basis of trait anxiety scores on the State Trait Anxiety Inventory. The results were in line with expectation (see Fig. 6.4).

In spite of the existence of empirical support for Schwartz's (1975) hypothesis, it has the limitation of merely describing an observed finding without any clear indication of why there should be these effects of arousal or anxiety on depth of processing. Furthermore, depth of processing is typically confounded with the elaboration and distinctiveness of encoding (Eysenck 1979). Semantic encodings will tend to be more elaborate or extensive than phonemic encodings, simply because the number of potentially encodable semantic features or attributes of a word is vastly greater than the potential phonemic word attributes. Similarly, as Jacoby (1974) and M.W. Eysenck (1979) have found, the semantic encoding of a word tends to be more distinctive or unique than the phonemic encoding. The reason is that the semantic encoding of a given word in a given context is different from the semantic encoding of the same word used in different contexts, with the result that one semantic encoding of a word is discriminable from prior semantic encodings of the same word. This trace discriminability may be absent in the case of phonemic encodings.

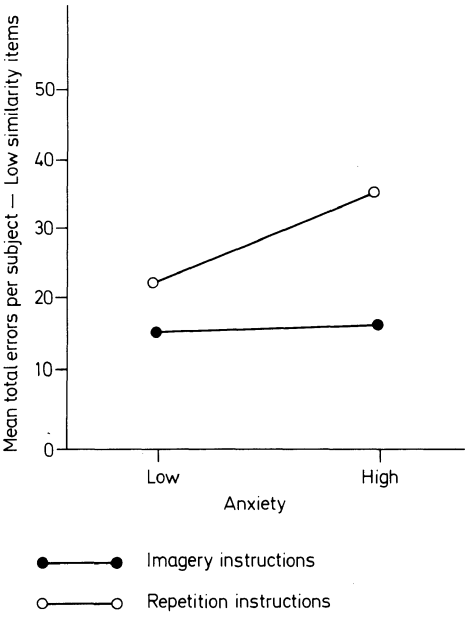

Fig. 6.4. Learning errors as a function of anxiety and of instructional conditions. (After Edmunson and Nelson 1976)

As was mentioned earlier, the fact that state anxiety reduces the available processing capacity of working memory is highly relevant to an explanation of some of the effects of anxiety on processing observed by Schwartz (1975) and others. Indeed, the hypothesis that anxiety reduces the elaboration of encoding because of a reduction in working-memory capacity should be seen in the context of Schwartz's (1975) hypothesis about the effects of arousal or anxiety on processing: since there is an immense variety

of potential semantic encodings of a word, but only a single phonemic encoding, reduced elaboration of encoding might tend to affect semantic processing more than phonemic processing.

Some evidence in favour of the hypothesis that high anxiety reduces elaboration of encoding was obtained by M.C. Eysenck and M.W. Eysenck (1979). High-anxiety subjects were those scoring in the neurotic introvert quadrant on the Eysenck Personality Inventory, whereas low-anxiety subjects were stable extraverts. Subjects were presented with a list of to-be-remembered words, followed by a test of cued recall. The retrieval cues were either phonemic (e.g. 'HARE rhymes with–') or semantic (e.g. 'TABLE associated with–'), with the subject being asked to fill in each blank with an appropriate list word. Another factor included in the design was the strength of the relationship between the cue word and the list word, as determined by association norms. It was argued that more extensive or elaborative encoding would be necessary for successful recall if the relationship between the cue and the list word was weak than if it was strong.

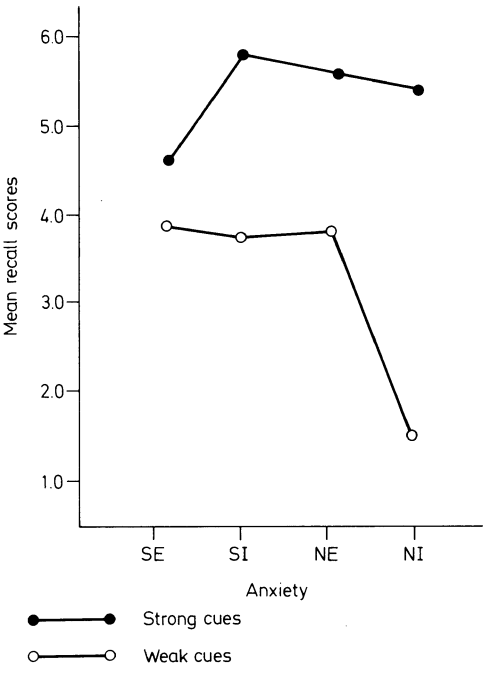

Fig. 6.5. Recall performance as a function of personality and retrieval cue strength. (After M.C. Eysenck and M.W. Eysenck 1979) SE, stable extraverts; SI, stable introverts; NE, neurotic extraverts; NI, neurotic introverts

The main findings are shown in Fig. 6.5. The most important result was that there was a highly significant interaction between anxiety and cue strength. While high anxiety had little effect on recall with the strong retrieval cues, there was a substantial detrimental effect of high anxiety on recall with the weak retrieval cues. This is, of course, precisely the pattern of results expected by the elaboration hypothesis. Some of the list words were homographs (i.e. words with two or more quite separate meanings). When these words were cued for either the dominant or the non-dominant sense, anxiety impaired retention-test performance for the non-dominant sense but not the dominant sense. This result also indicates that anxiety reduces elaboration of encoding.

There was also support for Schwartz's (1975) hypothesis, with the interaction between anxiety and cue depth being significant. In this interaction, high-anxiety subjects recalled fewer words than subjects in the other personality groups with the semantic cues, but there were no effects of anxiety with the phonemic retrieval cues. In this study, as is common in the literature, it is unclear whether the effects of anxiety on processing occur primarily at input or at output, or both. Theoretically, it would be expected that reduced working memory capacity would restrict processing both at storage and at retrieval. However, the relevant data have not as yet been obtained.

The effects of anxiety on shallow and deep processing have been considered in a series of articles by Mueller and his associates (Miller et al. 1978; Mueller 1976, 1977, 1978, unpublished work; Mueller et al. 1977, 1978). In all the experiments, except for some of those reported by Mueller (1976), subjects were assigned to high- and low-anxiety groups on the basis of scores on the Test Anxiety Scale. Nearly all the studies used a free-recall paradigm in which the list words could be organized in shallow or semantic fashion; the exceptions are the studies by Miller et al. (1978) and Mueller et al. (1978), in which picture-recognition memory was investigated. Two different dependent vari-

ables were considered in the free-recall studies: number of items recalled and clustering or organization of recall in terms of either deep or shallow attributes.

Across these studies, anxiety had a significantly detrimental effect on retention-test performance in six of eleven comparisons, and there was no effect on performance in the remaining five cases. In nine experiments, information was obtained about the interactive effects of anxiety and depth on retention-test performance. In one experiment (Mueller unpublished work), there was a significant interaction, with high anxiety producing a significant decrement for phonemic attributes but not for semantic attributes. The difference, of course, is in the opposite direction to that predicted by Schwartz (1975). In the other eight experiments, there was no interactive effect of anxiety and depth on retention-test performance. It seems that anxiety impairs retention for both deep and shallow attributes, rather than having the specifically detrimental effect on deep or semantic attributes anticipated by Schwartz.

In seven experiments, the effects of anxiety on the organization of free recall at the semantic and shallow levels were assessed. Unfortunately, the data do not form any consistent pattern: in two experiments, anxiety only reduced shallow clustering; in two other experiments, anxiety only reduced shallow clustering; and in the remaining three experiments, anxiety either reduced both deep and shallow clustering (twice) or had no effect on clustering. The most important conclusion to be drawn from these clustering data is that clustering or organizational decrements in free recall as a function of anxiety are equally likely to occur at both deep and shallow levels of processing. Thus the data for correct recall and organization of recall are both more in line with the hypothesis that anxiety reduces elaboration of encoding than with the hypothesis that anxiety only interferes with relatively deep levels of processing.

However, an alternative interpretation of the consistent impairment of retention-test performance under high anxiety needs to be considered. Walker's (1958) action-decrement theory stated that any psychological event establishes a perseverative trace lasting for some period of time. During this time, long-term memory is laid down by a process of consolidation, but there is a temporary inhibition of retrieval (termed 'action decrement'), which preserves the trace and protects it from disruption. Of immediate relevance, Walker also assumed that high arousal produces a longer-lasting active trace, with the consequence that high levels of arousal will improve memory at long retention intervals, but impair retention at short retention intervals. Since high-anxiety subjects tend to be more aroused than low-anxiety subjects (Eysenck 1977), the implication from this theory is that the inferior retention-test performance of high-anxiety subjects to low-anxiety subjects at the short retention intervals usually used might be reversed at longer retention intervals. On the other hand, the elaboration hypothesis predicts inferior recall from high-anxiety subjects at all retention intervals.

In fact, the evidence does not suggest that high-anxiety subjects have superior long-term retention to low-anxiety subjects. In one of the better studies, Pagano and Katahn (1967) compared the retention of subjects scoring low and high on the Test Anxiety Questionnaire. They controlled for original learning and discovered that there were no effects of anxiety on retention at either 24 h or 7 days. Similar results have been obtained a number of times by Mueller (e.g. 1978, unpublished work). In another study, Ray et al. (1971) investigated retention of a complex verbal task 2 days after acquisition. They found that high-anxiety subjects showed a greater retention loss than low-anxiety subjects.

In sum, the data indicate that high-anxiety subjects show inferior retention-test performance to low-anxiety subjects on both deep and shallow attributes and at all retention intervals. These findings are plausibly explained in terms of reduced elaboration of encoding under high anxiety, and are not consistent with the predictions from either Walker's (1958) action-decrement theory or Schwartz's (1975) hypothesis.

6.3.5 Towards a Theory of Anxiety

An attempt is made in this section to provide a new synthesis at the theoretical level, based

partly on some of the theories already discussed and partly on the work of contemporary theorists in the area of information processing, especially Kahneman (1973). The initial assumptions are that there is a valid distinction between state anxiety and trait anxiety, and that state anxiety is the result of the dynamic interaction of trait anxiety and situational stress or threat. Since state anxiety is responsive to situational factors, whereas trait anxiety is not, state anxiety should be more predictive than trait anxiety of task performance. Evidence supporting this prediction was obtained by Spielberger et al. (1972) in three separate experiments, described earlier; in addition, studies using the digit-span task have consistently found that performance is inversely related to state anxiety, but only marginally related to trait anxiety.

Most of the theories that have been put forward to explain the effects of anxiety on learning and memory have emphasized either the physiological/motivational component of anxiety (e.g. Malmo 1966; Spence and Spence 1966) or the cognitive component of worry and self-oriented responses (e.g. Sarason 1975; Wine 1971), although there are some exceptions (e.g. Eysenck 1973). In essence, the proposed theory of anxiety starts with the assumption that anxiety has both motivational and cognitive components, and that accurate predictions of the effects of anxiety on behaviour must necessarily consider both components.

There is accumulating evidence (reviewed by Morris et al. 1977) that the task-irrelevant cognitive activities associated with high anxiety have consistently detrimental effects on performance. The reason for this is that decisions about the threat posed by external stimuli, the retrieval of anxiety-related information and the formulation of appropriate cognitive coping strategies all make demands on the information-processing system. Thus the task-irrelevant information involved in worry and cognitive self-concern competes for space in the processing system with task-relevant information. In other words, the anxious subject is effectively in a dual-task situation, whereas the non-anxious subject is in a single-task situation. The most plausible assumption is that working memory is that part of the system most directly involved both in processing task-relevant data and in attending to task-irrelevant information. Since it is known that working memory is of limited capacity (Baddeley and Hitch 1974), it seems inevitable that task-irrelevant cognitive activities should impair the quality of performance.

The findings from digit-span and other tasks designed to measure the capacity of working memory are, of course, highly relevant. As we have seen, one of the most consistent and reliable effects in the literature is that state anxiety reduces digit span and so, presumably, the capacity of working memory. While it would clearly be desirable to have relevant data from experimental paradigms other than digit span, such evidence as is currently available is broadly consistent with the notion that the processing demands on the system made by the task-irrelevant cognitive processing associated with anxiety pre-empt processing capacity in working memory.

One of the most interesting implications of this theoretical approach is its potential relevance to the Yerkes-Dodson Law (Yerkes and Dodson 1908), according to which there is a curvilinear relationship between arousal and performance, with moderate levels of arousal being optimal for performance. Yerkes and Dodson also argued that this optimal level of arousal varied inversely with task difficulty. While relatively few attempts have been made to delineate the crucial determinants of task difficulty, it is reasonable to assume that 'difficult' tasks make greater demands on working-memory capacity than do 'easy' tasks. It follows from this assumption, combined with the hypothesis that anxiety produces attention-demanding, task-irrelevant cognitive activity, that detrimental effects of anxiety should be more pronounced on complex tasks than on simple ones. A partial search through the literature on the effects of anxiety on easy and difficult learning taks produced 12 studies in which the predicted interaction between anxiety and task difficulty was obtained (Berkey and Hoppe 1972; Farber and Spence 1953; Katahn and Koplin 1966; L'Abate 1956, for men only; Lee 1961; Lucas 1952; Montague 1953; Sarason and Palola 1960; Solso 1968; Spence et al. 1956b; Standish and Champion 1960; Tenny-

son and Wooley 1971). In all 12 studies, high anxiety improved learning performance on the easy task, and this was reported as being significant in five cases. In ten of the studies, high anxiety impaired performance on the difficult task, and this was significant in two cases. While several other studies failed to obtain a significant interaction between anxiety and learning-task difficulty, it is noteworthy that no studies reported the 'reverse' interaction, i.e. high anxiety facilitating performance on difficult tasks but impairing it on easy tasks.

At this juncture, it is important to note that, in spite of the fact that the task-irrelevant cognitive activities associated with anxiety always reduce the subject's task performance, it does not inevitably mean that high-anxiety subjects will invariably perform tasks worse than low-anxiety subjects. The reason for this can be seen if one draws a distnction between performance effectiveness and performance efficiency. Efficiency is a measure of the quality of performance, whereas effectiveness is the relation between the quality of performance and the effort invested in it. In other words, we have the following formula: performance effectiveness = efficiency/effort. The theoretical position argued here is that anxiety always produces a reduction in effectiveness, because it leads to task-irrelevant cognitive processes. However, the extent to which anxiety impairs (or even enhances) performance efficiency depends on the extent to which highly anxious subjects compensate for reduced efficiency by exerting increased effort.

In terms of the theory presented here, it is crucially important to distinguish between the effects of anxiety on efficiency and on effectiveness, although this has usually not been done in the past. In the great majority of published studies on the effects of anxiety on performance, the dependent variable or variables selected only make available information about the efficiency of performance. The important point to note is that comparisons of performance efficiency under low and high anxiety are only indicative of performance effectiveness provided that one makes the often-erroneous assumption that anxiety does not affect effort.

It is assumed that effort is positively related to performance efficiency, and, more specifically, that increased effort enhances attentional capacity, as Kahneman (1973) and others have assumed. The literature on incentive effects and on knowledge of results is relevant here, since it is commonly supposed that both variables operate largely through their effects on effort. M.W. Eysenck (in preparation) has reviewed this literature and concluded that incentives and knowledge of results both have a predominantly facilitatory effect on most tasks. Of greatest relevance, M.W. Eysenck (in preparation), in a study discussed earlier, found that monetary incentives produced a considerable increase in the capacity of working memory.

The question of the effect that anxiety is likely to have on mental effort has no simple and obvious answer. However, it seems probable that high-anxiety subjects who find that task-irrelevant cognitive activities are reducing processing effectiveness, and thus performance efficiency, will typically *compensate* for this by increased effort. A related way of considering this issue is suggested by Kahneman's (1973) theory of attention and effort. He contended that the amount of effort expended by a subject performing a task was mainly determined by task demands. Since we have already argued that the effective processing demands on high-anxiety subjects are greater than those on low-anxiety subjects, because they have to process both task-relevant and task-irrelevant information, it follows that high-anxiety subjects will usually exert more effort than low-anxiety subjects. Additional complicating factors will be discussed after an examination of some of the relevant evidence.

Dornic (1977) has investigated the effects of anxiety on effort, measuring effort by means of self-report questionnaires. In one study, the difficulty level of a high-load task involving time stress and storage load was manipulated. The subjects used in the experiment were neurotics and normals. While the two groups manifested comparable levels of performance efficiency, there was indirect evidence that the neurotic subjects were expending greater effort than the normal subjects on the more difficult tasks. Subjects were asked to rate the perceived difficulty of the tasks, and the neurotics increasingly rated the tasks as more difficult than did the

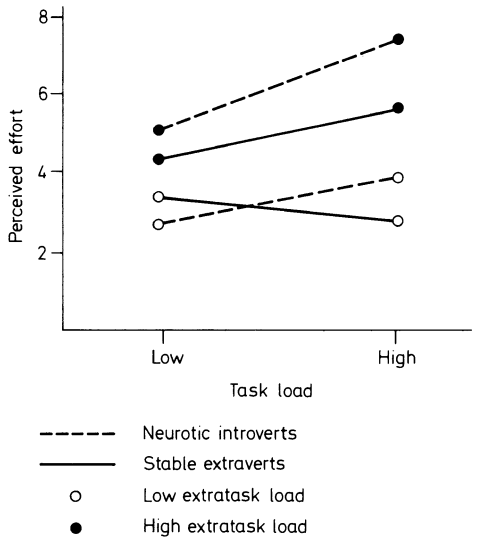

Fig. 6.6. Effects of task and extra-task load on perceived effort as a function of personality. (After Dornic 1977)

normal controls, as task difficulty became greater.

In a second study, Dornic (1977) compared stable extraverts and neurotic introverts, who can plausibly be regarded as being at opposite ends of the anxiety continuum. Task difficulty was minipulated by changing the number of information sources within a closed-system-thinking task (task load), and by altering the distraction power of semi-verbal, extra-task stimulation (environmental load). There were no significant differences in performance efficiency between the two personality groups. However, and more importantly, there were pronounced differences between the two anxiety groups with respect to perceived effort. As can be seen in Fig. 6.6, there was a significant triple interaction involving task load, environmental load and anxiety. The neurotic introverts expended more effort than stable extraverts, especially in the more demanding conditions (i.e. high task load and/or high environmental load).

An alternative method of measuring the amount of effort being employed during task performance as a function of anxiety is suggested by Kahneman's (1973) theoretical analysis. He argued that spare processing capacity is inversely related to the amount of effort involved in the main task. This 'spare capacity' can be measured by observing performance on a secondary task. The basic prediction is that, if anxious subjects devote more effort to the main task plus task-irrelevant processing than do non-anxious subjects, then anxiety should be negatively related to spare processing capacity. As a consequence, secondary-task performance should be worse for those subjects high in anxiety than for those low in anxiety. It should be noted that the studies that are relevant in this context have generally failed to utilize methodologically sound designs (see Brown 1978 for a discussion of the problems).

An examination of the literature on the effects of anxiety in paradigms incorporating both a main or primary task and a secondary or incidental task produced 19 relevant experimental findings, in which anxiety was usually investigated either by using threat versus no threat of electric shock or by dividing subjects into anxiety groups on the basis of questionnaire data. In many of these studies, the main task was intentional learning and the subsidiary task was incidental learning. There was a non-significant effect of anxiety on main-task performance in 15 of the 19 experimental comparisons (Bacon 1974 (2); Eysenck and Eysenck 1979; Kohn 1954; McNamara and Fisch 1964; Markowitz 1969; Miller and Dost 1964; Reeves and Bergum 1972; Silverman and Blitz 1956 (2); Tecce and Tarnell 1965; Wachtel 1968 (2); Weltman and Egstrom 1966; Weltman et al. 1971).

According to the theory presented here, anxiety invariably impairs processing effectiveness, because it generates task-irrelevant cognitive activities. It follows that comparable performance efficiency on the primary task as a function of anxiety can usually only be achieved if the high-anxiety groups of subjects exert additional compensatory effort. In other words, we may have examples here of non-significant effects of anxiety on performance masking real effects of anxiety and effort on processing effectiveness. The subsidiary-task data are consistent with the expectation from Kahneman's (1973) theory that subsidiary-task performance should be inversely related to the effort and resources invested in the primary task. Of the 15 subsidi-

ary-task comparisons, five revealed a non-significant effect of anxiety on performance, none indicated an enhancement effect of anxiety and ten produced the predicted significant adverse effect of anxiety on the subsidiary task. Anxiety is thus far more likely to have a detrimental effect than a facilitatory effect on subsidiary-task performance ($p=0.002$).

An especially interesting use of the subsidiary-task approach was reported by Hamilton (1978). The main task was to retain up to seven digits for a few seconds, with a subsidiary visual reaction-time task being interpolated between input and output of the digit string. Performance on the reaction-time task was only considered on those trials on which the digit string was correctly recalled. According to the theory presented here, which argues that anxiety always reduces processing effectiveness, comparable efficiency of performance on the digit-retention task by subjects high and low in anxiety can only be achieved by increased effort on the part of the high-anxiety subjects, especially as processing demands on working-memory capacity mount. As predicted, high-anxiety subjects had significantly longer reaction times than low-anxiety subjects only when seven digits had to be retained. This difference occurred in spite of the fact that there was no difference in skin conductance as a function of anxiety, suggesting that the results are not directly attributable to anxiety-induced effects on arousal level.

It has been assumed that high-anxiety subjects will usually exert more effort on performance tasks than low-anxiety subjects, primarily in an attempt to compensate for the reduced processing effectiveness under high levels of anxiety. However, other considerations are relevant. For example, there is considerable evidence that anxiety reduction is positively reinforcing (Miller 1948), implying that anxious subjects would exert more effort and be more motivated. However, an important factor is likely to be the *perceived instrumentality* of high levels of effort and performance efficiency in leading to anxiety reduction. If the source of the anxiety is task-intrinsic, i.e. caused by fear of the consequences of inadequate task performance, then anxiety should lead to increased effort. On the other hand, if anxiety is task-extrinsic, i.e. caused by events and stimuli unrelated to the task, then successful performance of the task would not be instrumental in reducing anxiety, and so anxiety would probably fail to produce greater effort. In similar fashion, if an organism experiencing anxiety believes that the probability of reducing anxiety through successful task performance is extremely low, then it is likely that anxiety will produce a motivational state but a very low investment of effort in the task. This combination of high anxiety and little effort may be seen in experimental demonstrations of learned helplessness. Maier et al. (1969) gave one group of dogs inescapable shocks in a classical aversive conditioning paradigm and found that when the dogs were subsequently placed in an instrumental avoidance paradigm in which they could terminate the shock, they did not engage in appropriate shock-termination instrumental behaviour.

A paradigm that seems relevant to the distinction between task-intrinsic and task-extrinsic sources of anxiety is one in which electric shocks are either contingent or non-contingent on the efficiency of performance on a learning task. It would certainly be expected that high-anxiety subjects would learn better with performance-contingent shock than with non-contingent shock, and Deese et al. (1953) found that, relative to control conditions, high-anxiety subjects showed an 18% improvement in learning with contingent shock and a 12% decrement with non-contingent shock, whereas low-anxiety subjects had a learning decrement of almost 30% under both contingent and non-contingent shock. However, results in this area have been somewhat inconsistent, and Lazarus et al. (1954) found no effects of either shock contingency or anxiety on serial learning.

Any theoretical consideration of the effort expended by high- and low-anxiety subjects should include goal-setting behaviour. There is reasonable evidence (reviewed by Locke 1968) that the effort exerted by subjects in task performance is positively related to the difficulty of the goal which they have set themselves. The most illuminating measure may well be that of goal discrepancy, which refers to the difference between the level of past performance and the current level of aspiration. Goal discrepancy

may be either positive or negative, depending on whether the level of aspiration exceeds or falls short of the previous level of performance. Sears (1941) divided children into groups on the basis of the size of their goal-discrepancy scores for experimental school-type tasks. Those with a high goal-discrepancy pattern were poor in school achievement and rated as showing low self-confidence accompanied by admission of incompetence (high anxiety?). On the other hand, those children with low positive discrepancy scores were rated as highly confident and successful (low anxiety?)

D.R. Miller (1951) investigated the goal-discrepancy scores of various psychiatric groups on a three-dimensional form-board test. Neurasthenics (introverted neurotics) had the largest positive goal-discrepancy scores and hysterics (ambiverted neurotics) had the lowest positive or negative goal-discrepancy scores, with normal controls intermediate. Similar results were obtained by H.J. Eysenck and Himmelweit (1946).

Schwartz (1974) looked at the effects of depression (which is related to anxiety) on goal discrepancy. He asked students to indicate what final numerical grade they expected to obtain on the day of a final examination, and obtained a correlation of $+0.56$ between depression and goal discrepancy (the difference between predicted and actual grade). Those high in depression were more inclined to over-estimate.

In sum, the evidence suggests that those high in anxiety (especially if introverted) tend to set higher, and less realistic, goals than those low in anxiety, although on some occasions they set rather lower goals. While the reasons for unrealistic goal-setting under high anxiety are obscure, a speculation will be offered: low-anxiety people tend to set goals with respect to their own level of achievement, whereas those high in anxiety feel that goals should be set with respect to some real or imagined level of achievement of others, rather than with respect to their own putatively inadequate performance level. Be that as it may, the evidence from studies of level of aspiration is broadly in line with other findings in indicating a positive relationship between anxiety and motivation.

The theory presented here makes some additional predictions that will now be discussed. We have agreed that high-anxiety subjects engaged in a learning task will concurrently encode the to-be-remembered material, contextual information and anxiety-related information, whereas low-anxiety subjects process primarily the to-be-remembered material together with the encoding context. Since retention appears to depend on a matching of the information in the study-trial encoding and the test-trial encoding (Tulving 1976), it is reasonable to expect that retention-test performance should be better when the level of anxiety at study and at test is comparable than when it is not. The general phenomenon of retention being affected by the similarity of the subject's internal state at input and at output is known as state-dependent retention.

Macht et al. (1977) investigated the issue of whether state-dependent retention could be demonstrated with the anxiety state. In their first experiment, subjects learned a list of nouns either in the presence or absence of mild electric shocks, and then attempted recall with or without shocks. As predicted, those subjects most likely to be in comparable states of anxiety at input and at output (shock either consistently present or absent) recalled a higher proportion of the words than subjects switching from shock to no shock or vice versa (0.55 versus 0.38 respectively). Unfortunately, they obtained more equivocal data in two further experiments, so that the precise conditions required to demonstrate state-dependent retention for the anxiety state are not known.

There have been several studies carried out in an attempt to demonstrate 'experimental repression' or the motivated inability to recall stored information. In fact, the usual paradigm essentially involves investigating the effects of anxiety on retrieval, followed by retrieval after the removal of anxiety in order to demonstrate the 'return of the repressed'.

In a representative study by Penn subjects learned a list of paired associates and then performed a tapping task under failure or no failure conditions. The first retention test was then given, followed by more tapping associated with no information, success or failure. Finally, there was a second retention test. Subjects given failure feedback before the first retention test and

success feedback before the second test had a very low level of recall on the first test and a high level of recall on the second test. Several other studies have reported similar findings (see Holmes 1974 for a review).

The implication is that anxiety reduces the efficiency of recall, although whether the Freudian notion of 'repression' is required to explain the data is doubtful, unless one is prepared to argue that the consequences of failure on a tapping task resemble crippling neurotic conditions studied by Freud. Two possibilities are worth considering. The first is based on state-dependent forgetting. The basic argument is that acquisition occurs in a low-anxiety state in these studies and that retention is better when retention occurs in a similar low-anxiety state rather than in a dissimilar high-anxiety state. This hypothesis predicts a reversal of the usual results if anxiety at input is high.

The second hypothesis would explain the results in terms of the detrimental effects on retrieval of the task-irrelevant cognitive activities associated with high anxiety. Evidence in favour of this hypothesis was obtained by D'Zurilla (1965), who did a study similar to that of Penn (1964). After the experiment, he asked the subjects what they had thought about immediately after the failure or success information had been given, and found that the failure subjects reported more thoughts than the success subjects that were totally irrelevant to the subsequent task of word recall. He concluded that the reduced recall efficiency after failure might be due to the increased number of conflicting cognitive events, which is very much in line with our general viewpoint on the reasons for performance decrements under high anxiety.

We have argued that a major consequence of high anxiety is a reduction in the working-memory capacity that is available for task processing. The most general expectation from this theory as regards the effects of anxiety on learning and memory is that there will be a reduction in the extensiveness or elaboration of encoding, and we have already seen that Mueller and others have obtained supporting evidence. It will be noted that we are making the plausible assumption that the more elaborate an encoding is, the more demands are made on processing effort or working-memory capacity. M.W. Eysenck and M.C. Eysenck (1979) investigated the effects of elaboration of processing on effort by using a subsidiary reaction-time task. They found a greater slowing of reaction time when subjects encoded more elaborately (e.g. when they decided whether a word was a European country versus simply a country), and concluded that processing effort was positively related to elaboration of encoding.

The theory presented here has a family resemblance to the theory of H.J. Eysenck (1973), who argued that the effects of anxiety on performance were meidated by a combination of drive (a motivational construct) and by drive stimuli (related to task-irrelevant cognitive activities). The theory also has a number of similarities with the views of Hamilton (1975), who regarded the main effect of anxiety as involving complex cognitive processing: "Anxiety can be defined as internally generated cycles of connotative signals elicited by external stimuli, which a central interpreting or appraisal process codes as requiring avoidance, and as indicating physical danger, injury to self-esteem, rejection, and loss of affection in valued social settings" (p. 50). Hamilton went on to argue that task-irrelevant processing interferes by competing with relevant task-induced forms of processing in a processing system with limited capacity. Finally, Hamilton claimed that the precise effects of anxiety on task performance depend on the processing capacity available at that time. These ideas were expressed in terms of a formula stating that successful performance occurs when average processing capacity plus spare processing capacity are equal to or greater than internally-generated, task-irrelevant information (produced by anxiety) plus internally-generated, task-relevant information plus externally-generated, task-relevant information. Since high anxiety increases the probability that the 'anxiety problem' will be regarded as the primary or main task, it is inevitable that any tendency for the available information to exceed the available processing resources will produce a decrement in main-task performance. Under those circumstances in which total information-processing demands cannot be handled by the available processing capacity, according to

Hamilton, a likely strategy is to restrict attention voluntarily to a small number of information sources. This suggestion is related to the hypothesis put forward by Easterbrook (1959), according to which heightened arousal reduces the range of cue utilization.

One of the main differences between the views of Hamilton and the theoretical position taken in this chapter is that we have argued that effort determines the available processing capacity and that anxiety is complexly related to effort. Among the relevant factors determining whether anxiety leads to increased or reduced effort are the evaluation of task demands, the subjective probability of performing the task successfully, the level of aspiration and the perceived instrumentality of enhanced effort in leading to anxiety reduction.

6.3.6 Success and Failure

The effects of anxiety on learning and memory are importantly affected by success and failure feedback. Much more work has been done on failure feedback, and, as we shall see, failure seems to affect differentially effort expenditure by high-anxiety and low-anxiety subjects. Some of the most interesting work in this area was discussed by Weiner (1972), who argued that there are four major perceived causes of failure on a performance task: bad luck; lack of effort; high task difficulty and lack of ability. He further claimed that these four factors could be considered within a two-dimensional framework, with the two dimensions being the locus of control (internal versus external) and stability (stable versus unstable):

1) Bad luck: unstable; external locus of control;
2) Lack of effort; unstable; internal locus of control;
3) Task difficulty: stable; external locus of control;
4) Lack of ability: stable; internal locus of control.

Weiner (1972) discussed evidence indicating that failure (and success) produce a greater effective or emotional response when attributions are made to the internal causal factors of effort or ability than when they are made to the external causal factors of luck or task difficulty. In terms of the effort expended on a task following a failure experience, Meyer (1970) found that attribution of failure to the unstable and variable factors (i.e. bad luck and lack of effort) was far more positively related to subsequent effort than was attribution of failure to the stable and constant factors (i.e. low ability and high task difficulty).

The relevance of the above findings to work on anxiety was clarified by other work discussed by Weiner (1972). There is indirect evidence in several studies (e.g. Kukla 1972; Weiner and Potepan 1970) that high-anxiety subjects are more inclined than low-anxiety subjects to attribute failure to lack of ability, whereas low-anxiety subjects are more likely to ascribe failure to lack of effort. It thus follows that high-anxiety subjects should experience the negative affect of anxiety after failure, but should not increase their effort, because they ascribe failure to the relatively unchangeable factor of lack of ability.

On the other hand, low-anxiety subjects attribute failure to the unstable and variable factor of lack of effort and thus believe that increased effort would substantially reduce the probability of future failure. Accordingly, while anxiety produces some anxiety, the major effect of a failure experience on low-anxiety subjects is to increase effort.

Unfortunately, Weiner (1972) did not provide any detailed explanation of the reasons for these different causal attributions, nor was he directly concerned with the effects of these attributions on task performance. We would argue that the different causal attributions are reasonable in view of the fact that, prior to failure feedback, high-anxiety subjects are typically much nearer maximum effort expenditure than low-anxiety subjects. Accordingly, it is actually the case that low-anxiety subjects can realistically ascribe failure to lack of effort, because they have by no means maximized effort expenditure, whereas high-anxiety subjects have been investing considerable effort in the task and thus can only ascribe their failure to factors such as lack of ability.

Fairly straightforward predictions about task performance follow from our theoretical assumptions that anxiety states reduce processing capacity, whereas effort increases capacity. Failure should tend to *improve* the performance of low-anxiety subjects via enhanced effort but to *impair* the performance of high-anxiety subjects through increased anxiety. The anticipated interaction between anxiety and feedback (neutral versus failure feedback) has been obtained using several different learning tasks (Gordon and Berlyne 1954; Krugman 1958; Lucas 1952; Sarason 1957a). In these studies, high-anxiety subjects consistently showed reduced performance as a consequence of failure feedback, whereas low-anxiety subjects either improved their performance or showed no effect of failure.

A related series of studies has been concerned with the effects of different levels of involvement in the task on learning and retention. The instructions given to the subjects have either been designed to be ego-involving (e.g. "This is a test of intelligence") or to be task-involving (e.g. "I want to see how well this apparatus works"). Ego-involving instructions are likely to have motivational consequences, in the sense of increasing the subject's level of aspiration, but they may also produce anxiety through fear of failure. If low-anxiety subjects respond to ego-involving instructions by increased motivation, whereas high-anxiety subjects become more anxious, then anxiety level should interact with type of instructions (ego-involving versus task-involving). A number of learning studies have obtained this interaction (e.g. Nicholson 1958; Sarason 1956, 1957b). In addition, also as predicted, ego-involving instructions have consistently been found to lead to a decrement in performance for high-anxiety subjects and to improved performance for low-anxiety subjects.

Some studies have considered the effects of success feedback on performance. Not surprisingly, one of the most reliable effects of success is to reduce the level of state anxiety (Gaudry 1977). According to the theory presented in this chapter, there are two requirements for high-anxiety subjects to outperform low-anxiety subjects: (1) the experimental conditions must minimize ego threat; and (2) high-anxiety subjects must exert more effort than low-anxiety subjects. The second requirement will usually be met if high-anxiety subjects perceive that increased effort will be instrumental in reducing anxiety. Both of these conditions are likely to be fulfilled when success feedback is provided during the performance of the task.

There is some evidence that task difficulty may frequently be confounded with feelings of success or failure. Tennyson and Wooley (1971) and Spielberger et al. (1972) found that difficult learning tasks increased state anxiety much more than easy tasks. The reason may be that performance on a difficult task results in a feeling of failure, due to the relatively slow rate of learning, whereas performance on an easy task produces success feelings, because of the speed of mastery. Weiner (1966a) and Weiner and Schneider (1971) unconfounded the two variables of subjective feelings of success or failure and task difficulty by giving their subjects false social norms, indicating that they were succeeding at a difficult verbal learning task or failing at an easy learning task. Under these conditions, subjects high in anxiety performed better on the difficult task and worse on the easy task than subjects low in anxiety. These results are inconsistent with the notion that high trait anxiety necessarily impairs performance on demanding tasks, but are in accord with the position taken here. The key finding that success feedback enabled high-anxiety subjects to outperform low-anxiety subjects on a difficult learning task suggests that high-anxiety subjects will exert more effort than low-anxiety subjects when effort is seen to be instrumental in reducing the level of anxiety.

Related findings were obtained by Sarason (1972), who compared the performance of high and low scorers on the Test Anxiety Scale on a set of difficult serial learning items. Several different instructional conditions were used: control; achievement-orientation; reassurance; motivating task orientation and task-orientation. While the low-anxiety subjects were significantly superior to be high-anxiety subjects under achievement orientation, the high-anxiety group was somewhat better than the low-anxiety group under reassurance and task-orientation conditions.

6.4 Effects of Introversion–Extraversion on Learning and Memory

6.4.1 Interrelationship Between Introversion–Extraversion and Anxiety

H.J. Eysenck (1967) argued that the orthogonal dimensions of introversion–extraversion and neuroticism–stability had identifiable physiological bases. Differences in introversion–extraversion were seen as being related to cortical arousal stemming from the ascending reticular activating system, whereas differences in neuroticism–stability stemmed from activation of the visceral brain, i.e. the hippocampus, amygdala, cingulum, septum and hypothalamus. Within this framework, anxiety is regarded as combining aspects of neuroticism and introversion (see Fig. 6.7). It follows from this position that the effects of anxiety on learning and memory can be expected to depend on these two separable components of anxiety, and a specific hypothesis of this type was proposed by H.J. Eysenck (1973): "It is introversion (characterized by high cortical arousal) that is responsible for the drive-properties of the MAS+ (high Manifest Anxiety Scale scoring) subjects, rather than neuroticism (which when aroused through ego-involving instructions, or some other manipulation of the situation, produces the drive stimuli that interfere with performance)" (p. 401). It would be predicted from this theory that neuroticism would typically impair performance, and Willoughby (1967) found that high scorers on a scale of emotionality or neuroticism that was essentially unrelated to introversion–extraversion were significantly inferior to low scorers on the learning of both easy, non-competitional paired associates and difficult, competitional pairs.

A related, but conceptually distinct, theory was proposed by Gray (1970, 1972, 1973), parts of which are shown in Fig. 6.7. He argued that the two orthogonal dimensions of neuroticism and introversion–extraversion lie within a theoretically important two-dimensional space. Moreover, he provided persuasive evidence that two orthogonal dimensions of anxiety and im-

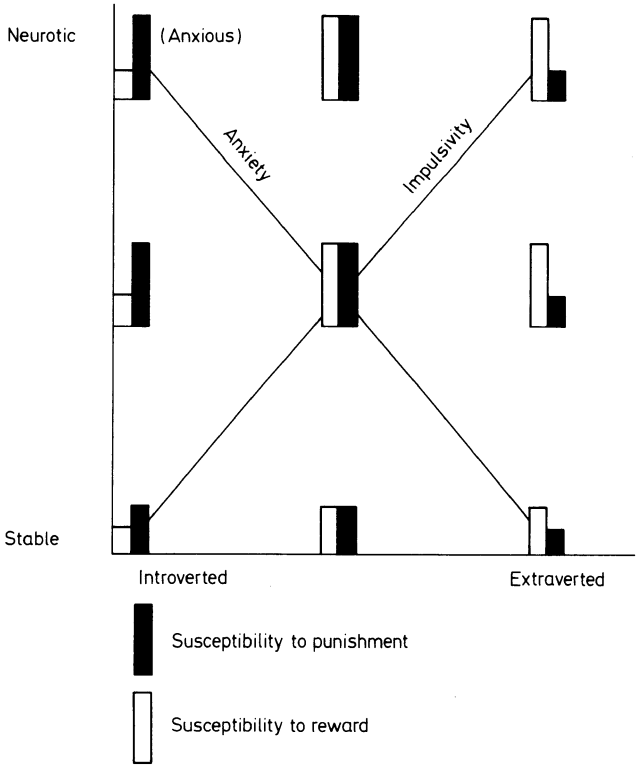

Fig. 6.7. Gray's theory of the inter-relationships among anxiety, impulsivity, neuroticism and extraversion. (After Gray 1973)

pulsivity can be identified within this space. The anxiety dimension runs from stable extraversion to neurotic introversion and represents a dimension of susceptibility to punishment. The impulsivity dimension runs from stable introversion to neurotic extraversion and is a dimension of susceptibility to reward. One of the consequences of this theoretical framework was stated in the following terms by Gray (1972): "We may regard the dimension of introversion-extraversion as a dimension of susceptibility to punishment and non-reward: the greater the degree of introversion, the greater is this susceptibility" (p. 194). Some of the evidence relevant to this hypothesis, and also to the hypothesis that differences in cortical arousal underlie the introversion–extraversion will now be considered.

6.4.2 Introversion–Extraversion: Reward and Punishment

There is strong evidence from studies of knowledge of results and of incentive effects that extraverts are more susceptible than introverts to reward. Several relevant findings were reported by Corcoran (1962, 1972). He reanalyzed some data obtained by Wilkinson on the five-choice serial reaction time task and found that the increment in speed of performance, when shifting to knowledge of results from no knowledge, correlated +0.76 with extraversion. When subjects were run after a night's loss of sleep, the correlation was +0.64. An interpretative problem with these data is that while a major effect of knowledge of results is incentive-motivational in nature (Broadbent 1971), it also provides information that can be used to improve performance.

Corcoran (1962) provided some clarification of the processes involved by investigating the effects of a non-informative incentive manipulation on performance of the five-choice serial reaction task. The incentive was that subjects were told they could stop performing the task after attaining a given number of correct responses. Extraverts responded more than introverts to the incentive, especially under sleep-deprived conditions, where there was a correlation of +0.59 between extraversion and the incentive effect.

Other data also indicate a differential susceptibility of introverts and extraverts to reward or incentive manipulations. For example, Corcoran (1962) gave subjects the task of cancelling all the occurrences of the letter 'e' in a passage of prose. In the low motivation condition, the subjects were ignored during task performance, whereas fake feedback was given in the high motivation condition. The results were dramatically affected by the motivational manipulation. Under the low motivation condition, there was an extremely high correlation of +0.90 between introversion and the number of lines completed. In contrast, the correlation was actually reversed in the high motivation condition. Introverts were almost completely unaffected by the introduction of fake feedback, whereas extraverts showed an improvement of approximately 80%.

A few studies have considered the applicability of Gray's conceptualization to individual differences in conditioning and learning. Gupta (1976) investigated verbal operant conditioning under various conditions of positive and negative reinforcement. Introverts consistently showed more conditioning than extraverts with negative reinforcement, suggesting that introverts are more affected by punishing stimuli. There was in addition some evidence that extraverts were more affected than introverts by the provision of reward. These findings were replicated by Gupta and Nagpal (1978), who obtained additional information by performing separate analyses on the impulsivity and sociability components of extraversion. While Gray's theory seems to predict that the effects of positive reinforcement should be greater for impulsivity than for sociability, whereas the effects of negative reinforcement should be greater for sociability than for impulsivity, the data indicated that the two components of extraversion were each comparably affected by the various reinforcement conditions.

Seunath (1975) compared the performance of introverts and extraverts on a pursuit rotor learning task under conditions of positive or negative reinforcement. He obtained a signifi-

cant interaction between introversion–extraversion and reinforcement conditions: extraverts outperformed introverts when positive reinforcement was used, whereas introverts performed better than extraverts when negative reinforcement was used.

Some of the theoretical ideas put forward by Gray (1970, 1973) seem of potential relevance to effects of introversion–extraversion on the signal-detection theory measure of the response criterion. In essence, signal-detection theory (e.g. Green and Swets 1966) makes use of the two parameters of d′ and beta to describe a subject's performance. D′ is a measure of the subject's sensitivity, whereas beta is an index of the amount of information required for response emission, i.e. a measure of the cautiousness of responding. Of most immediate relevance, the setting of the response criterion is determined by the subjective gains associated with correct responding and the subjective costs associated with incorrect responding (i.e. false alarms). Since introverts are more susceptible to punishment than extraverts but less susceptible to reward, one may assume that introverts will attach relatively more importance than extraverts to the potential costs of false alarms, whereas extraverts will attach relatively more importance than introverts to the potential gains of correct responding. As a consequence, introverts should tend to set a higher criterion point for their responses than extraverts, in line with the greater cautiousness of introverts (e.g. Cameron and Myers 1966).

In the literature on the effects of introversion–extraversion on vigilance tasks, a number of studies have considered the false alarm rate. If introverts adopt a more stringent response criterion than extraverts, then the general expectation is that introverts should produce fewer false alarms. Krupski et al. (1971) found that the positive correlation between false alarms and extraversion just failed to attain statistical significance. Carr (1971) found that introverts not only made fewer false alarms but also detected more signals than extraverts on an auditory vigilance task. Of most interest, Harkins and Geen (1975) analysed the data from a visual vigilance task in signal-detection terms, and found that introverts set a significantly higher criterion point for their responses than did extraverts.

There is some evidence that similar differences in the response criterion as a function of introversion–extraversion obtain in memory paradigms. McLaughlin and Kary (1972) found that extraverts made more correct responses and more errors or false alarms than introverts on a recognition test. Forrest (1963) asked subjects to recall a series of drawings and described those who tended to produce exaggerated descriptions of the drawings as 'sharpeners'. The sharpeners were much more extraverted than the 'levelers' (i.e. those who did not produce exaggerated descriptions).

Gillespie and M.W. Eysenck (1980) did a signal-detection theory analysis of performance in a continuous recognition-memory task in which subjects were allocated to groups on the basis of their scores on the extraversion scale of the Eysenck Personality Inventory and the general activation scale of Thayer's Activation-Deactivation Adjective Check List. The latter test is an objective self-report measure of transient levels of activation, requiring the subject to indicate the extent to which several adjectives (e.g. energetic, active, vigorous, peppy) apply to him at that moment. In a number of experiments, Thayer (1967, 1970) has found that the general activation scale correlates approximately +0.6 to +0.7 with an index representing a pooled amalgam of various physiological measures. In terms of recognition performance, activated extraverts had the greatest d′ or sensitivity, but the most relevant finding was that introverts adopted more stringent response criteria than extraverts, particularly during the initial period of the continuous recognition task.

6.4.3 Cortical Arousal

Most investigators of the effects of introversion–extraversion on learning and memory have assumed that introverts are chronically more cortically aroused than extraverts. For example, H.J. Eysenck (1967) has suggested that, "skin conductance and alpha activity are measures of extraversion" (p. 170). The physiological evidence is mixed. Gale (1973) reviewed the EEG

findings, noting that most investigators have claimed that high amplitude and low frequency of alpha are indicative of low arousal, whereas low amplitude and high frequency reflect high arousal. Of the 16 studies discussed by Gale, seven supported the hypothesis that introverts are more aroused than extraverts, three supported the opposite hypothesis, and six found non-significant EEG differences between introverts and extraverts. In addition, a subsequent study by Travis et al. (1974) found no difference in alpha between introverts and extraverts. One methodological problem with several of the studies is that subjects were instructed to "do nothing", an instruction that fails to control adequately the subjects' behaviour.

The results from studies investigating other physiological measures in introverts and extraverts were discussed in detail by M.W. Eysenck (1977). In general, there is modest support for the hypothesis that introverts are more physiologically aroused than extraverts, but the evidence is not compelling. One potentially important complicating factor was identified by Blake (1967). He noted that the body temperature of introverts was higher than that of extraverts during the morning and early afternoon, whereas the body temperature of extraverts was higher than that of introverts during the evening. The implication is that there is a phase difference in the circadian rhythm of arousal as a function of introversion–extraversion, with introverts reaching peak arousal earlier in the day than extraverts. There are other physiological and behavioural data suggestive of a phase shift, and it has been claimed that the phase difference is primarily a function of the impulsivity component of extraversion, rather than the sociability component. Some of the relevant issues are discussed by M.W. Eysenck and Folkard (1979).

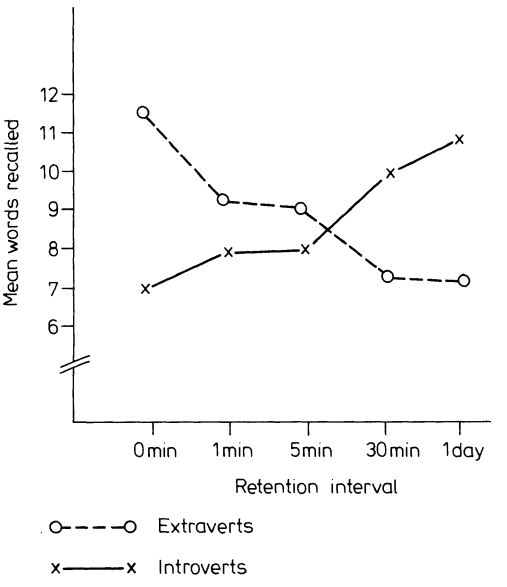

Fig. 6.8. Differential effects of retention interval on recall of introverts and extraverts. (After Howarth and Eysenck 1968)

6.4.4 Retention Interval

One of the most interesting findings in the literature on introversion–extraversion and memory is the fairly consistent evidence for an interaction between introversion–extraversion and the length of the retention interval (Howarth and Eysenck 1968; McLean 1968; Opollot 1970; Skanthakumari 1965). The findings from the Howarth and H.J. Eysenck study are shown in Fig. 6.8. As can be seen, extraverts showed better short-term recall than introverts, but this was reversed with long-term recall. It must, however, be pointed out that there have been a number of studies in which the anticipated interaction between introversion–extraversion and retention interval failed to materialize (Berlyne and Carey 1968; McLaughlin 1968; McLaughlin and Kary 1972; Schneller and Garske 1976).

H.J. Eysenck (1973) accounted for this interaction by extending the action decrement theory of Walker (1958). According to Walker's theory, high arousal produces a longer-lasting active trace, leading to enhanced consolidation and long-term memory. However, during the consolidation period there is a temporary inhibition of retrieval (called 'action decrement') which protects the trace from disruption. Since there is more pronounced action decrement at high levels of arousal, short-term retention is inversely related to the level of arousal. It is important to note that studies in this area have typically employed the term 'short retention in-

terval' to refer to retention up to approximately 20 min after stimulus presentation, so that it would be extremely misleading to identify the term 'short retention interval' with primary memory or short-term store, which characteristically has a much shorter duration. On the assumption that introverts are more cortically aroused than extraverts (Eysenck 1973), it follows that extraverts should outperform introverts at short retention intervals, but that there should be a reversal at longer retention intervals. This is, of course, the standard result.

While Walker's (1958) theory has usually been investigated in memory paradigms, there is every reason to suppose that it should be potentially applicable to multi-trial learning. For example, Opollot (1970) found that introverts took rather more trials than extraverts to learn a paired-associate list to criterion. Of most interest, a more detailed analysis of performance indicated that introverts were greatly inferior to extraverts in terms of the difference between the trial on which responses were first given correctly and the trial on which they were last given incorrectly. This result suggests that short-term forgetting by introverts impaired their ability to acquire the list.

One of the problematical aspects of Walker's (1958) theory is that it has been supported by an extremely narrow range of empirical findings, of which the main one is the interaction between arousal conditions and retention interval. An interesting attempt to provide alternative indices of the hypothetical process of consolidation was made by Amelang et al. (1977). In their first experiment, subjects retained a string of eight consonants for a period of 13–25 s. A subsidiary visual reaction time task was used during the retention interval, the assumption being that the memory task would only produce lengthened reaction times while the consolidation process was occurring. As predicted, introverts had slower reaction times than extraverts early in the retention interval, with the difference between introverts and extraverts being eliminated later in the retention interval. Performance on the memory task was rather similar, with introverts showing poorer memory performance only at the shorter retention intervals.

In their second experiment, Amelang et al. (1977) used alpha suppression during the retention interval as their measure of the extent to which consolidation was occurring. They obtained an interaction between introversion–extraversion and time after stimulus input: introverts showed less alpha than extraverts for the first 4 or 5 s of the retention interval, but the reverse was the case thereafter.

While Amelang et al. (1977) interpreted their data as providing evidence about the time course of the consolidation process, it should be borne in mind that their study used retention intervals that were markedly shorter than those typically used in the literature based on Walker's (1958) theory. Furthermore, the most obvious interpretation of the data is that long reaction times and alpha suppression both reflect the use of active rehearsal strategies on the part of the subjects and thus may be totally unrelated to the putative process of consolidation.

Some of the difficulties with Walker's (1958) hypothesis have been dealt with at some length by M.W. Eysenck (1976a; 1977), but a brief analysis is in order at this point. The most damaging factor is that several studies have reported findings arguing strongly against the generality of the hypothesis. For instance, while high arousal typically impairs immediate retention in paired-associate learning, other findings indicate that high arousal *enhances* immediate free recall and recognition. Even within the confines of paired-associate learning, it appears that matters are more complex than had originally been envisaged. Hamilton et al. (1972) looked at the effects of an arousing stimulus (white noise) on paired-associate learning under two different conditions. They either used the customary technique in which the order of the pairs was changed from one learning trial to the next, or they kept the order of items constant. There was no effect of differences in white noise intensity on initial learning when item order was changed, but high arousal (intense white noise) produced much better learning than low arousal when the order of items was kept constant from study to test trials. The latter result is, of course, exactly the opposite of that predicted on Walker's theory. The implication is that an

arousing agent such as noise leads to increased processing of sequential information. When this is of use, as in the fixed-order case, then learning performance is enhanced; however, with variable ordering of the pairs, such information will tend to interfere with learning by producing competing responses. One plausible way in which this might occur is if arousal led to increased use of the articulatory loop (Baddeley and Hitch 1974).

It seems a promising line of approach to look for processing differences between introverts and extraverts in order to explain the retention-interval effects. For example, Bellezza and Walker (1974) distinguished between storage and coding processing strategies, a storage strategy involving filling short-term store or working memory to capacity with stimulus information which is then rehearsed, whereas a coding strategy involves adding appropriately chosen old information from long-term store to stimulus information in short-term store and then rehearsing the entire ensemble. As predicted, the storage processing strategy produced better short-term retention than the coding strategy, but the opposite happened at a long retention interval (see Fig. 6.9). If extraverts tend to adopt the coding strategy, whereas introverts tend to adopt the storage strategy, then the obtained interaction between introversion–extraversion and retention interval would result. While this account is entirely speculative, it is worth noting that alternative interpretations of the interaction between introversion–extraversion and retention interval are possible.

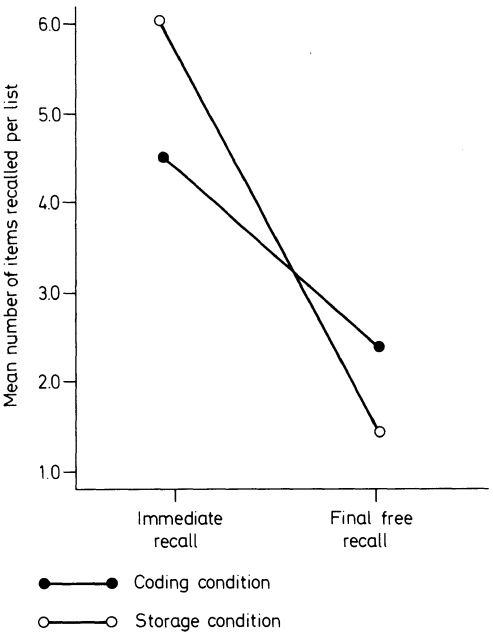

Fig. 6.9. Differential effects of processing strategies on immediate and final free recall. (After Bellezza and Walker 1974)

6.4.5 Distraction

An issue of some theoretical consequence concerns the effects that irrelevant and distracting stimulation have on performance with the to-be-remembered material. According to Easterbrook (1959), "the number of cues utilized in any situation tends to become smaller with increase in emotion" (p. 197), and, in addition, "as the total number of cues in use is reduced, task-irrelevant cues are excluded before task-relevant cues" (p. 193). Since high arousal increases the extent to which attention is concentrated on the main task, it follows that introverts should be less affected than extraverts by extraneous stimulation.

On the other hand, Gray (1970, 1972, 1973) argued that introverts were more susceptible than extraverts to punishment, with the consequence that introverts are more likely than extraverts to experience anxiety. Since anxiety always decreases processing effectiveness, it follows that introverts must exert additional effort in order to perform the main learning task under non-distracting conditions as efficiently as extraverts. If, therefore, introverts have less spare capacity available than extraverts, they will be less able to cope with the rejection demands posed by the irrelevant environmental stimulation (cf. Dornic 1977). In other words, introverts should be more affected than extraverts by distracting stimuli.

In an early study on this issue, Shanmugan and Santhanam (1964) found that extraverts were less detrimentally affected than introverts in a learning task when exposed to extraneous visual interference or distraction. Subsequently, Howarth (1969a) asked subjects to learn serial

lists of number groups, with distraction being provided by additional irrelevant numbers. Although there was no effect of introversion–extraversion on learning when no distraction was present, extraverts outperformed introverts under the distraction conditions. Finally, in a study by Morgenstern et al. (1974), subjects learned auditorily presented words for recall, and distraction consisted of additional auditorily presented words, a German prose passage or an English prose passage. There was no difference in retention as a function of introversion–extraversion in the absence of distraction, but introverts were significantly more detrimentally affected than extraverts by distraction.

In sum, the available data consistently indicate that distraction impairs learning performance more for introverts than for extraverts. This suggests that a straightforward application of Easterbrook's (1959) hypothesis is not warranted and that introverts typically have less spare capacity available to reject distracting stimulation. Further confirmatory evidence is available from performance on the Stroop test, in which subjects are shown cards on which the names of colours are printed in inks of different colours, and the subjects are required to report the colour of the ink in which each word is printed. The distraction is present in the tendency to read the word itself, and its effect is measured by the reduction in speed of performance compared to control conditions. The usual finding is that extraverts are less affected than introverts by the irrelevant information presented on the Stroop test (Davies 1967; Gulian 1972).

It follows from Easterbrook's hypothesis, as well as from the notion that introverts tend to have less spare capacity than extraverts, that introverts should show less incidental learning of extraneous information than extraverts. Imam (1974) presented subjects with nonsense syllables enclosed in geometrical figures, having instructed them to learn the syllables. The test of incidental learning required the subjects to match each nonsense syllable with the geometrical shape in which it had been enclosed during learning. There was no difference in incidental learning between introverts and extraverts, but the experiment was methodologically inadequate, in that there was no guarantee that the subjects actually perceived the incidental stimuli. It is clear that there is a need for further research on this problem.

6.4.6 Task Difficulty

If one combines the assumption that introverts are more cortically aroused than extraverts with the theory (Yerkes and Dodson 1908) that the optimal level of arousal is inversely related to task difficulty, then the prediction follows that introversion–extraversion should interact with task difficulty and that extraverts should cope relatively better than introverts with highly demanding tasks. A number of studies have investigated this hypothesis with generally supportive evidence. Jensen (1964) carried out a large-scale factor-analytical study of the effects of introversion–extraversion on learning and came to the following conclusion: "Extraverts perform better than introverts, especially in serial learning, and extraversion had a loading of 0.41 on the general learning ability factor. Extraversion seemed to correlate mainly with resistance to response competition" (pp. 7–8). Since tasks involving response competition (e.g. certain retroactive-interference paradigms) are more difficult than those not involving response competition, this conclusion is consistent with the Yerkes-Dodson Law.

Howarth (1969b) investigated the hypothesis that extraverts show greater resistance to response competition than introverts, using a paired-associate task. Subjects learned an initial list to criterion, and then a second list, which consisted of a re-paring of the stimuli and responses from the first list. Finally, they mastered a third paired-associate list, which comprised a further re-pairing of the same stimuli and responses. Extraverts took non-significantly fewer trials than introverts to learn the first two lists to criterion and significantly outperformed the introverts on the final list, in which response competition was hypothesized to be at a maximum.

Bone (1971) looked at the learning to criterion of two paired-associate lists, in one of which (the interference list) each stimulus word had

a strong associate as the response member of a different pair; in the other list, all the words were unrelated. Performance was much worse on the interference list, and there was a significant interaction between list difficulty and introversion–extraversion, in which extraverts outperformed introverts only on the interference list.

M.W. Eysenck (1975a) considered the effects of introversion–extraversion on the learning of two paired-associate lists. On the easy list, the response words were strong semantic associated of their respective stimuli; the difficult list consisted of a scrambled version of the first list. All subjects learned the easy list first and then the difficult list. While there were no effects of introversion–extraversion or general activation on the number of failures to produce the correct response during the 10 s allowed for recall, the data for speed of recall were more consistent with expectation. On the easy list, the most aroused subjects (highly activated introverts) had the fastest recall speed, whereas, on the difficult list, subjects of intermediate levels of arousal (high-activation extraverts and low-activation introverts) responded faster than highly activated introverts.

While there have been some inconsistent findings reported in the literature (e.g. Purohit 1966), the general indication is that extraverts are less affected by response competition than introverts. It is not as yet clear how this finding can most appropriately be explained. One possibility is that, since introverts are more susceptible to punishment than extraverts, their performance is impaired on difficult learning tasks because anxiety reduces the available processing capacity of working memory. The plausibility of this suggestion is increased by other studies (Allsopp and Eysenck 1974; McLaughlin and Eysenck 1967) in which paired-associate lists verying in difficulty were used. The interaction between task difficulty and extraversion was not significant in either study, but the triple interaction of extraversion, neuroticism and task difficulty was significant in both studies. However, a surprising aspect of the data from both studies is that stable introverts performed better than neurotic introverts on the easy list, but the reverse was the case for the difficult list. When the subjects of their scores on the Manifest Anxiety Scale, the medium-anxiety subjects made considerably more errors on the difficult learning task than the high-anxiety subjects. It is possible that the situation was insufficiently threatening, since there were only modest correlations between scores on the Manifest Anxiety Scale and on a measure of state anxiety.

6.4.7 Retrieval: Speed and Power

One of the problematical features of most studies concerned with the effects of introversion–extraversion on memory is that any obtained differences may be due to effects at any or all of the following stages of information processing: initial perception, attention, rehearsal, encoding, consolidation, retrieval or response criterion. Further understanding of the salient differences in information processing between introverts and extraverts is only likely to occur provided that paradigms are used that permit investigation of a single aspect of processing. This approach is feasible if one is interested in the effects of introversion–extraversion on the retrieval of well-learned information from permanent storage. Vocabulary tests can be used to ensure comparable storage of relevant information by introverts and extraverts, thus permitting the conclusion that performance differences reflect individual differences in the retrieval process.

Tests of verbal or word fluency have been used in several studies, with the subjects being asked to write down as many words as possible within a given time period that fulfil some criterion (e.g. words starting with the letter R or male first names). Cattell (1934) asked his subjects to produce as many two-syllable words as possible over a period of $2^1/_2$ min and found that surgency (similar to extraversion) correlated +0.30 with fluency. Although some subsequent studies found no systematic relationship between extraversion and verbal fluency (Hofstaetter et al. 1957; Rim 1954), several investigators have found that extraverts recall more words than introverts (DiScipio 1971; Gewirtz 1948; White 1968).

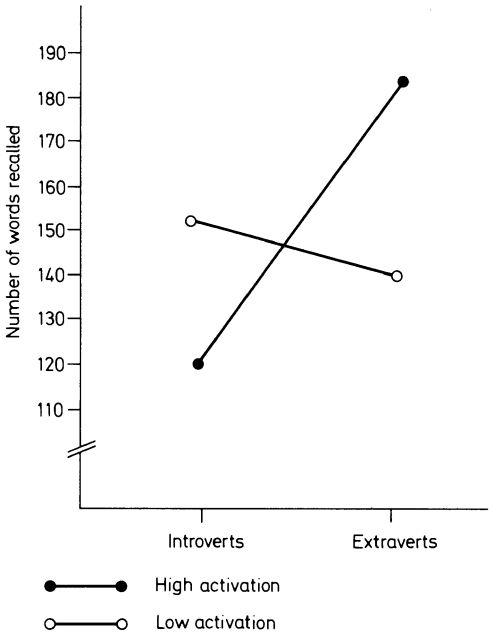

Fig. 6.10. Retrieval from permanent storage as a function of activation and extraversion. (After Eysenck 1974a)

While these studies clearly suggest that extraverts have a more efficient retrieval process from permanent storage than introverts, little attempt was made in these studies to provide a theoretical account of the findings. A more systematic investigation of the effects of introversion–extraversion on retrieval from permanent storage was attempted by M.W. Eysenck (1974a). The task was to retrieve words from each of five semantic categories, and subjects were allowed to shift category at any time. The subjects were assigned to four groups on the basis of their scores on the extraversion scale of the Eysenck Personality Inventory and the general activation scale of Thayer's Activation Deactivation Adjective Check List. Extraverts recalled significantly more words than introverts, but the most crucial finding was a highly significant interaction between extraversion and general activation (see Fig. 6.10). There was no difference in recall between introverts and extraverts who reported themselves to be low in activation, but highly activated introverts were considerably inferior to highly activated extraverts. Since highly activated introverts recalled fewer words than any of the other groups, it seems that high arousal has a detrimental effect on retrieval from permanent storage.

Further analysis of the data indicated that the extent to which recall was organized or clustered along the lines of the five semantic categories was strongly related to recall performance. Since introverts, and especially highly activated introverts, had less clustering in their recall than extraverts, it is possible that some of the individual differences in retrieval may be attributable to differences in organization.

One of the most noticeable characteristics of spoken recall from semantic memory is the existence of response bursts, i.e. recall of several words with very short inter-response times preceded and followed by a longer interval of time. It is of some interest whether the recall difference between introverts and extraverts is due more to the number of bursts retrieved or to the size of each burst. Accordingly, the author has reanalyzed the data of M.W. Eysenck (1974a), defining a burst as two or more successive responses where all the inter-response times were among the fastest 25% produced by each subject. Highly activated introverts produced many fewer bursts than extraverts, but actually averaged more words per burst. If one assumes that burst size is limited by the capacity of working memory, then it appears that the impaired retrieval efficiency of activated introverts is not due to reduced working memory capacity.

A final finding from the study of M.W. Eysenck (1974a) was that differences in recall between introverts and extraverts only became apparent after some time on the task. Since more common category exemplars tend to be emitted initially, the retrieval difficulty of introverts seemed to centre on retrieval of relatively rare and non-dominant items. This finding was investigated further by M.W. Eysenck (1974b), who argued that there was a possible methodological inadequacy in studies of individual differences in verbal fluency, due to the fact that subjects tend to search through their previous responses in order to avoid repetitions. The greater cautiousness of introverts (Cameron and Myers 1966) may lead them to engage in more re-checking than extraverts, with the result that

they have less effective time available for retrieval of new items from permanent storage.

The above problem was obviated by using a task requiring only a single response on any given trial. On recall trials, the task was to produce a word belonging to a specified category starting with a designated letter (e.g. 'four-footed animal – Y'), and on recognition trials the task was to decide whether or not a word belonged to a specified category (e.g. 'four-footed animal – yak'). A further factor incorporated into the design was item dominance, based on the normative data of Battig and Montague (1969). Thus, there is an appropriate high-dominance response to 'four-footed animal – D', namely 'dog', but only a low-dominance response to 'four-footed animal – Y', namely 'yak'. As can be seen in Fig. 6.11, extraverts responded faster than introverts on recall trials, but there was no effect of introversion–extraversion on recognition trials. The dominance factor was relevant, since the advantage of low-arousal subjects over high-arousal subjects was more marked on low-dominance recall trials than on high-dominance recall trials. In this study, arousal was assessed both by scores for introversion–extraversion and by the general activation scale of the Activation Deactivation Adjective Check List (Thayer 1967).

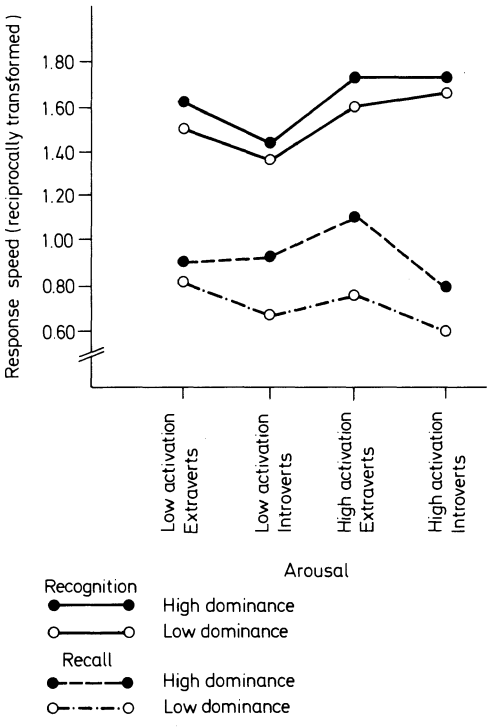

Fig. 6.11. Speed of recall and recognition from permanent storage as a function of activation, extraversion and item dominance. (After Eysenck 1974b)

In a subsequent study, using the same recall and recognition tasks, but manipulating arousal by means of white noise and general activation, M.W. Eysenck (1975c) obtained similar results. Further studies investigated the generality of the effects of introversion–extraversion on retrieval speed on tasks not involving permanent storage. In one study, M.W. Eysenck (1975a) compared the recall latencies of introverts and extraverts during the learning of easy and difficult lists of paired associates. There were four groups of subjects, representing the various combinations of high or low extraversion and high or low general activation. Those subjects of intermediate arousal level (highly activated extraverts and lowly activated introverts) had generally greater response speed than those of low or high arousal (low-activation extraverts and high-activation introverts, respectively). However, there was an exception to this finding on the easy list, in which the correct responses were presumably becoming more accessible over trials. Here the response-speed advantage of high-arousal subjects increased over trials. In contrast to the latency data, there were no effects of either extraversion or general activation on probability of recall. This suggests that introversion–extraversion affects the ease of access to stored information rather than the nature of the stored information itself.

In a further study, an attempt was made to investigate the effects of introversion–extraversion on retrieval unconfounded by storage differences by equating the degree of learning of introverts and of extraverts. In this study (Eysenck 1975b), subjects learned each of two semantically categorized word lists to a criterion of two consecutive errorless recalls of the members of each category in their correct serial order. Speed of retrieval was measured by probing each subject with a category name and an item-specific cue, with the subject required to respond as rapidly as possible. The data were similar to those of M.W. Eysenck (1975a), with

extraverts responding more rapidly under high general activation, whereas introverts performed better under low general activation.

Some clarification of the effects of introversion–extraversion on retrieval was obtained by M.W. Eysenck and M.C. Eysenck (1979). They used a modified version of the Sternberg paradigm in which subjects were presented with a short set of between one and four category labels, followed by a probe item. In the physical identity condition, subjects had to decide whether the probe word was identical to one of the memorized set. In the semantic category condition, subjects had to decide whether the probe was a member of one of the categories comprising the memorized set. Since performance of this task is nearly error-free, the dependent variable of experimental interest is the speed of response. More specifically, if Sternberg (1975) is right that subjects perform a serial exhaustive scan through the memorized set, then the increase in reaction time with increases in memorized set size presumably reflects the time to compare the probe with each memorized item. In other words, interest centres on the *slope* of the function relating set size and reaction time, rather than the *intercept,* which reflects an amalgam of time to perceive the probe, and the programming and emission of the response.

On the basis of Schwartz's (1975) hypothesis, according to which high arousal facilitates physical processing but impairs semantic processing, it was predicted that extraverts would perform the scan faster than introverts when only physical matching was required, whereas the opposite would be the case when semantic matching was required (semantic category condition). There was a significant interaction between introversion–extraversion and conditions (see. Fig. 6.12). It is clear from these data that there was no difference in scanning speed between introverts and extraverts in physical matching performance, but extraverts scanned faster than introverts on semantic matching trials.

Schwartz (unpublished work) presented subjects with two words simultaneously under one of three instructional conditions: physical identity, requiring a decision based on whether the two words were identical; homophone identity,

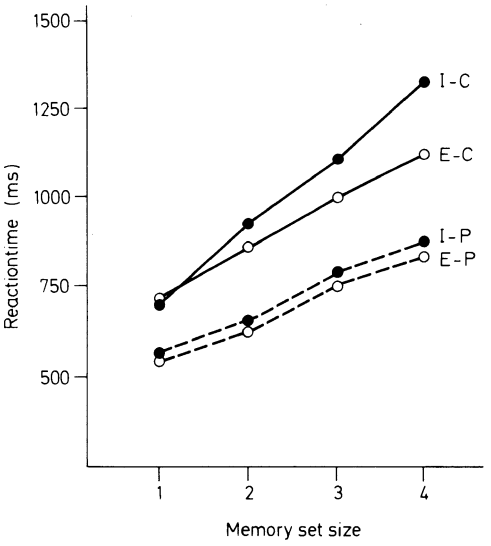

Fig. 6.12. Speed of performance on a Sternberg-type task as a function of memory set size, depth of processing and extraversion (After M.W. Eysenck and M.C. Eysenck 1979). I, introverts; E, extraverts; P, physical match; C, category match

requiring a decision based on whether the two words are pronounced in the same way (e.g. 'deer – dear') and taxonomic category identity, requiring a decision as to whether the two words belonged to the same semantic category. There was a very significant interaction between introversion–extraversion and instructional condition: introverts and extraverts responded at comparable speed on physical identity trials (801 versus 780 ms, respectively), but introverts were much slower than extraverts on homophone (1,095 versus 953 ms) and taxonomic category trials (1,205 versus 1,058 ms).

The data obtained by M.W. Eysenck and M.C. Eysenck (1979) and by Schwartz (unpublished work) are largely consistent with the hypothesis that introverts experience greater difficulty than extraverts in retrieving deep or semantic information from long-term store, but that there is no effect of introversion–extraversion on the retrieval of shallow, physical information. However, the finding that introverts were slower than extraverts at making phonemic decisions in the homophone condition of Schwartz's study is inconsistent with that hypothesis. It may be more correct to argue that introverts take longer than extraverts to access

information in long-term store, whether that information is semantic or phonemic in nature; on that argument, there was no effect of introversion–extraversion in the physical identity conditions used by M.W. Eysenck and M.C. Eysenck (1979) and by Schwartz (unpublished work), because the required processing did not necessitate accessing information from long-term store.

M.W. Eysenck (1976b) argued that many of the above findings were consistent with the following hypothesis: "High arousal has the effect of biasing the subject's search process towards readily accessible, or functionally dominant, stored information more than is the case with lower levels of arousal" (p. 401). Part of the reason for this may be that high arousal causes cognitive masking and reduces parallel or shared processing (Walley and Weiden 1973). If introverts are more aroused than extraverts and thus less able to process in parallel, this would imply that they would be at a disadvantage in any task (such as recall of low-dominance items) involving the processing of several different items of information.

However, other evidence suggests that the effects of introversion–extraversion on retrieval are more complex than had originally been envisaged, that such effects may depend on factors in addition to arousal. For example, monetary incentives are usually thought to increase physiological arousal, and there are data to support that contention (e.g. Wilkinson et al. 1972). The consistent finding in the literature is that incentives do *not* lead to impaired efficiency of retrieval; instead, incentives typically have no effect on retrieval (Nelson 1976; Wasserman et al. 1968; Weiner 1966b; Wickens and Simpson 1968). It is tempting to argue that the greater susceptibility of introverts than extraverts to punishment means that introverts will tend to be more anxious than extraverts and that anxiety reduces the available processing capacity of working memory. This would explain why introverts take longer than extraverts to access information, and especially non-dominant information, from long-term memory or permanent storage.

Alternative interpretations of the retrieval findings should also be considered. One obvious possibility is that introverts access information as rapidly as extraverts, but that they take longer to arrive at a decision as to whether the retrieved information is appropriate, perhaps because of their greater cautiousness. However, two kinds of findings are inconsistent with such a notion. Firstly, under some conditions (e.g. the recognition task used by Eysenck 1974b) there were no effects of introversion–extraversion on response latencies, and under others (e.g. the later trials on the easy paired-associate task used by M.W. Eysenck 1975a) introverts actually responded faster than extraverts. Such results clearly indicate that the putative caution of introverts does not always slow the responding of introverts. Secondly, analyses of the error scores on the recall and recognition tasks used by M.W. Eysenck (1974b) revealed non-significant error-rate differences between introverts and extraverts, although introverts did tend to make fewer errors than extraverts. It remains possible, however, that differences in response criterion are partially responsible for the obtained data.

A different interpretation is suggested by Bieri's (1970) contention that cognitive complexity is greater in introverts than in extraverts and that this difference is reflected in the organization of permanent storage. One possibility is that introverts organize information in permanent storage more complexly than extraverts and that ease of access to the contents of permanent storage is inversely related to complexity of organization. While storage differences may be of some consequence, it is unlikely that all the findings are interpretable along such lines, since the effects of general activation on performance in tasks requiring retrieval from permanent storage are presumably on search and retrieval processes.

6.4.8 Summary and Conclusions

In spite of the relatively small volume of research on the effects of introversion–extraversion on learning and memory, there appear to be a number of fairly robust findings. Some of the more important of these have been discussed earlier and will now be listed:

1) Reward enhances the performance of extraverts more than introverts, whereas punishment impairs the performance of introverts more than extraverts.
2) Introverts are more susceptible than extraverts to distraction.
3) Introverts are more affected than extraverts by response competition.
4) Introverts take longer than extraverts to retrieve information from long-term or permanent storage, especially non-dominant information.
5) Introverts have higher response criteria than extraverts.
6) Extraverts show better retention-test performance than introverts at short retention intervals, but the opposite happens at long retention intervals.

While it is probably premature to attempt any theoretical integration of these various findings, it is nevertheless tempting to argue that introverts are characteristically better motivated on performance tasks than extraverts, with the consequence that their normal expenditure of effort and utilization of working memory capacity is closer to the maximum. Since introverts, as it were, start from a high motivational baseline, it follows that they are less able than extraverts to utilize extra processing resources to handle increasing processing demands (e.g. from distracting stimulation, from response competition or from difficult retrieval tasks).

One of the major reasons for regarding differences between introverts and extraverts in motivational terms is that the performance of introverts is remarkably little affected by incentives or by positive reinforcement. In addition, H.J. Eysenck (1967) has discussed evidence indicating that introverts typically have higher levels of aspiration than extraverts. On the other hand, the application of negative reinforcement or punishment will often be ineffective in improving the performance of introverts, because they respond to negative reinforcement with substantially greater anxiety than extraverts (Fremont et al. 1970). This differential susceptibility to reward and punishment also provides a potential explanation of response-criterion differences between introverts and extraverts, assuming that extraverts tend to focus on the gains associated with correct responding, whereas introverts focus on the costs associated with incorrect responding.

The preferred interpretation of the data in terms of motivational differences between introverts and extraverts is obviously related to accounts based on arousal. However, since arousal occurs both as a result of what is happening to an organism and as a result of active processing effort, theories based on effort and motivation are more specific than those based on arousal.

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Chapter 7

Personality and Social Behaviour

G.D. Wilson

7.1 Introduction

The model of personality described in previous chapters assumes stable traits which are genetically and constitutionally determined to some extent and which have a wide range of applications to behaviour in social situations. It has been the interest of previous authors to demonstrate that (and investigate the extent to which) major personality dimensions are rooted in individual biology; the purpose of this chapter is to give some indication of the areas of social psychology in which personality constructs have proved to be relevant and important. In no way does this exercise deny that there are culturally determined patterns of social behaviour or that the social behaviour manifested by an individual at any given occasion is also influenced by the situation pertaining at the time. These factors are clearly also important in social behaviour, but for the present purpose these sources of variation will be largely set aside (i.e. treated as constant or as random error sources) in favour of a study of the role of indiviudal differences in temperament in the determination of significant social behaviours.

To some extent what follows may be considered as a kind of validation of the questionnaires used for measuring personality, in that the social behaviour associated with each trait is usually what would be expected on the basis of a description of that trait. Thus it will be seen that the extravert is more sociable, active and impulsive in his social behaviour than the introvert, who functions best in conditions of isolation and quiet. The high N scorer is inclined to find himself in social difficulties of various kinds, because he is emotionally unstable. The high P scorer also finds himself in trouble, because of his aggressive, sensation-seeking instincts, though by the same token he may also register certain achievements that are missed by the more careful and considerate low P scorer.

The main purpose of this chapter is to document the extent of the applications of Eysenck's three-dimensional personality model to the different areas of social psychology. At the same time we shall consider ways in which the results of this research may feed back information relevant to the biological substrates of these personality dimensions. We end with a summary of some of the main conclusions to emerge from all this literature.

7.2 Affiliation and Personal Space

Since sociability is one of the major defining characteristics of extraversion it is not surprising to find a number of studies reporting greater affiliative tendencies in extraverts than introverts. For example, Leipold (1963) and Patterson and Holmes (1966) found that extraverts would approach an interviewer more closely and talk longer in response to questions than introverts. Although Williams (1963) found no such difference in approach tendencies, he did observe that extraverts would allow other people to approach them more closely. Subjects high on the exhibition and impulsivity scales of the Personality Research Form (who are presumably extraverts) were found to position their chair in closer proximity to the interviewer when requested to "pull up a chair" (Sewell and Heisler 1973). Despite some negative or inconclusive results (Porter et al. 1970; Meisels and Canter 1970; Williams 1971; Tolor 1975),

it is fair to conclude that there is in general a positive relationsip between extraversion and physical proximity preferences.

The neuroticism dimension also relates to preferred social distance. De Julio and Duffy (1977) had male and female students select seats in a classroom, where they also completed the EPI neuroticism scale. High N scorers were found to have located themselves further away from the experimenter than low N scorers. This was interpreted as consistent with a number of previous studies showing that people who are under stress of any kind, whether externally imposed or an intrinsic aspect of their personality, seek greater social distances. Although extraversion was probably measured in this study, the authors do not mention whether it related to proxemic behaviour; either it was not analysed, or it showed no significant relationship.

An interaction between extraversion and neuroticism in determining preferred personal distances is suggested in a study by Shapiro and Alexander (1969). They used Schachter's paradigm for the study of anxiety and affiliation, manipulating subjects' anxiety with threats of electric shock of various degree and then giving them the choice of waiting for the experiment alone or in the company of others in the same plight. As the situational stress was increased, the extraverts became more affiliative while the introverts became less so. It seems, then, that if extraverts are made anxious they tend to gravitate towards other people, but introverts, when anxious, prefer all the more to be alone.

While there do not appear to be any studies directly investigating the relationship between psychoticism and social distance preferences, some clues are available as to what that relationship may be. Horowitz et al. (1964) found that, consistent with their tendency towards interpersonal withdrawal, schizophrenics were more reluctant than non-schizophrenics to approach a target person. Kinzel (1970) found that violent prisoners preferred social distances of up to four times greater than non-violent prisoners. Also relevant, perhaps, is the finding that females approach others and allow them to approach more closely than do males (Horowitz and Rothschild 1970; Lott and Sommer 1967; Pellegrini and Empey 1970), although this finding was not replicated by De Julio and Duffy (1977). Taking these findings together, it would appear that high P individuals are likely to adopt greater interpersonal distances because their orientation towards others is basically suspicious or hostile. At the less extreme end of this dimension, men tend to regard others in a competitive spirit, compared with women, who are generally more kindly and caring towards others.

7.3 Birth Order

The classic studies of Schachter (1959) showed an association between affiliative behaviour and birth order, first-born and only children being more affiliative than later-born children. This might lead us to expect that first-born children would be more extravert than later borns, although if the affiliative tendency were due to insecurity it might also imply higher neuroticism in first-borns. In any case, research has not really supported either hypothesis; some studies have produced totally negative results (e.g. Farley 1975), while in others the findings have been complex and inconsistent. McCormick and Baer (1975) obtained EPI scores from 120 college students from two-child families in which the inter-sibling age difference was less than 6 years. They found an interaction between sex and birth-order in determining E scores. First-born males were more extravert than second-born males, but for females it was the other way about – first-born girls being more introvert. Another interesting finding from this study was that opposite-sex siblings were higher in neuroticism than same-sexed siblings. No easy explanation for these results offers itself. The best conclusion at the present time is that birth-order effects on personality are complex, weak and inconsistent, which of course is what we would expect if a high proportion of variance in personality is genetic.

7.4 Group Interaction and Social Skills

The tendency for extraverts to be more interested in initiating and maintaining social contact

than introverts has been confirmed in a number of studies of non-verbal communication. Thus Mobbs (1968) engaged subjects in conversation, during which he stared continuously at them; he found that extraverts were more likely than introverts to stare back at him. Similarly, Kendon and Cook (1969) found a positive correlation between extraversion and the frequency of looking while talking, and Rutter et al. (1972) found that extraverts engaged in more periods of eye contact while speaking and initiated more looks and speech bursts in the course of a 4-min conversation with the experimenter. Carment et al. (1965) and Leipold (1963) showed that extraverts talk more and sooner than introverts, and Cook (1968, unpublished work) found that extraverts would prefer to sit directly opposite another person in a variety of social situations, whereas introverts would more often choose a right-angle arrangement.

There are various ways of viewing this finding that extraverts are more interested in making contact with other people. It may be taken as validation of the questionnaire measure of extraversion – since sociability is one of the major defining characteristics of the trait. It could also be interpreted in relation to Eysenck's theory that extraverts are arousal-seekers, if it can be assumed (quite reasonably) that social contact is arousing. Evidence that social contact is in fact arousing has been reviewed by Zajonc (1965), and an arousal model of interpersonal intimacy has been given by Patterson (1976). A third possibility is that the relationship between extraversion and social interest is mediated by another personality trait such as confidence or assertiveness (both of which are higher in extraverts than introverts). Finally, perhaps there is truth in all three of these propositions, since they are by no means mutually exclusive.

One of the most comprehensive studies of personality in relation to patterns of bodily communication is that of Campbell and Rushton (1978). They had 46 female occupational therapy students complete measures of extraversion, neuroticism and intelligence. The students were also rated on these traits by a lecturer who knew them well. They were later asked to help in a study of 'social interaction', in which they were videotaped having a conversation with a female experimenter concerning their plans for the summer vacation. The most striking finding was that the extraverts talked more than the introverts. And since people who listen tend to nod their head more often, it was not surprising that the introverts were found to have done more looking and head nodding. The latter finding is interesting, because it might explain why there are some inconsistent findings concerning the gaze of introverts and extraverts. Extraverts were not more gesturally expressive than introverts, only more verbal – as listeners the introverts were quite socially responsive.

Neuroticism as rated by the teacher was associated with self-touching behaviour and an absence of talking, but using the probably more valid questionnaire measures of neuroticism neither of these relationships was sustained. Instead, the most consistent indicator of neuroticism was gaze aversion (turning the eyes away from contact with another) – a finding that has previously been reported by Kendon and Cook (1969), Rutter and Stephenson (1972) and Williams (1974).

This tendency for anxious (high N) subjects to spend less time in eye contact with an experimenter has been examined more closely by Daly (1978). Using a sample of 213 high-school students classified into low-, medium- and high-anxiety groups on the Watson-Friend Scale of Social Anxiety, she found that highly anxious people looked less while they were talking to the experimenter. When they were *listening*, however, there was some suggestion of bimodality among the anxious subjects. This implies that highly anxious people adopt one of two opposite strategies – either long-term fixation or glancing away very quickly. Another finding to emerge was that anxious subjects were less likely to talk spontaneously while being given instructions for the experiment, but the hypothesis that they would show more irrelevant arm and hand movements was not supported.

If the relationship of social skills with E and N is complex, the connection with P is almost certainly more so. Although the relevant studies do not appear to have been done, we might suppose that where high P is due to schizoid tendencies, the individual would display shyness

and sensitivity; where high P is due to psychopathic tendencies, we would observe assertive gesturing and steady eye contact. The clinical pictures of schizophrenia and psychopathy at least would point to such an outcome.

7.5 Speech Patterns

A number of studies have focussed in greater detail on the relationship between personality and non-verbal aspects of speech. Ramsay (1968) recorded subjects' verbalizations by means of a throat microphone and then analysed the patterns of speech and silence by means of computer. Verbal tasks ranged from reading passages of prose to describing pictures and conversing with the experimenter. Although no significant differences were found between extraverts and introverts as regards the length of utterances, the introverts tended to use longer silences between utterances. This supports the idea that the introvert is more thoughtful than the extravert, taking more heed of the maxim that one should "be sure brain is engaged before putting mouth into gear". Such a conclusion is further supported by the fact that the difference was greater for complex verbal tasks, compared with simple tasks. For the well-practised habit of reading, the pauses were short for extraverts and introverts alike, but as the verbal task became more complex, requiring a greater amount of cognitive processing, the introverts' pauses before speaking became relatively longer than those of extraverts. The effect of neuroticism on speech patterns was found to be small and inconsistent.

Rim (1977a) had Israeli students complete the EPI and then engage in conversation about a topic of current interest within a randomly assigned three-person group. Trained observers counted the number of times each individual interrupted or talked simultaneously with another participant. Subjects high on neuroticism and extraversion were found to interrupt most often. Extraverts were also more inclined to speak on top of another person than introverts. This finding rather supports Ramsay's conclusion that extraverts are less controlled in their speech patterns than introverts.

Another characteristic of speech that seems to be connected with personality is stuttering. Raj and Rao (1970) compared the personalities of 100 male stutterers at the Mysore Institute of Speech and Hearing with 100 non-stuttering male controls. The stutterers were significantly higher in neuroticism and more introvert than the non-stutterers, placing them in the 'dysthymic' quadrant of the Eysenck personality classification system, along with the majority of clinical neurotics. There is a problem of cause and effect here, of course. Stutterers may become anxious and avoid social contact because of the embarrassment occasioned by their disability rather than the stuttering being a function of their personality.

7.6 Expressive Behaviour and Person Perception

People vary in the extent to which they transmit emotional messages to others in their facial and bodily movements and in their tone of voice. There are also differences in people's ability to read or 'decode' these non-verbal messages originating from other people. Cunningham (1977) has studied the role of extraversion and neuroticism in the ability to transmit and receive such non-verbal emotional messages. He found some consistency between different non-verbal channels (i.e. face, body and tone of voice) as regards transmission ability, regardless of whether or not the individual was consciously trying to signal his emotions. However, there was a negative relationship between ability to transmit and ability to receive emotional messages. That is, people who were best at passing non-verbal messages to others were the least effective at decoding messages sent to them by others. Whereas stable extraverts were best able to *communicate* emotion to others, neurotic introverts were most successful at *receiving* the messages of others. There were also some sex differences; males were slightly better at sending messages, while females were significantly better at interpreting them.

Cunningham's interpretation of these findings is mainly in terms of relative social power. Males and stable extraverts, he suggests, are less frightened or inhibited concerning the expression of emotion and therefore emit less ambiguous signals. Females and neurotic introverts are in a relatively weak social position and are therefore more guarded with respect to open emotional displays. The latter groups are, however, more dependent upon accurate readings of the emotional disposition of dominant groups and therefore acquire greater sensitivity and skill in this area. On the other hand, Cunningham does consider the possibility that excessive sensitivity to the emotions of other people could contribute causally to high levels of neuroticism and might lead to the kind of social withdrawal seen in some introverts.

Another study implicating neuroticism in the prerception of other people's feelings is that of Duckworth (1975). He divided 36 married couples into experimental and control groups and had them attend a session in which each partner tried to identify the feelings expressed by the other through vocal (but non-verbal) communication. Emotions such as boredom, disgust and tenderness, had to be conveyed through tone of voice and expression, while reciting the standard phrase "What are you doing?" The experimental group underwent emotionally provocative disagreements before trying to identify the feelings of their partner, while the control group did not. The effect of these disagreements was to increase the ability of stable introvert males to identify the spouses' feelings, while this capacity was decreased in neurotic introvert males. Although a complex interaction, this finding suggests that the differential susceptibility of various personality types to arousal and stress may be implicated in the ability to accurately perceive the emotional state of other people. High N might help with emotional readings, but apparently not if it is compounded by situational stress.

A rather subtle form of expressiveness that shows consistent differences between extraverts and introverts is that of humour enjoyment. Overall, extraverts are more receptive to sexual and aggressive forms of humour while introverts prefer relatively 'safe', intellectual jokes, i.e. those based on cognitive factors such as surprise and incongruity (Eysenck and Wilson 1976).

Is this difference founded on the introvert's greater sensitivity to threat? Some research by Verenis (1970) would seem to suggest that it is. He had subjects rate the funniness of a number of cartoons classified as neutral, sexual and aggressive. Some subjects were asked to rate the cartoons on the basis of their first impression, while others (matched groups) were asked to analyse the point of the cartoon before judging its funniness. Results showed higher correlations between extraversion and enjoyment of the cartoons in the 'analysing condition' than in the 'non-analysing condition', and this was particularly true for the cartoons with sexual and aggressive content. It appears that the task of explaining the point of the joke that was given to subjects in the analysing condition had the effect of mobilizing their inhibitions against expressing enjoyment of the cartoons, and the introverts were particularly sensitive to this mobilization.

How does personality relate to ability to judge the personality characteristics of others? At first sight a study by Vingoe and Antonoff (1968) seems to suggest that introverts are more accurate at judging other people. Women stdents living in the same dormitory were given the EPI and California Personality Inventory and rated their peers on dimensions from these same tests. 'Good' judges were relatively introverted (E scores averaging 10.8 as against 14.8 for the 'bad' judges) and less neurotic (mean scores of 9.1 and 12.3, respectively). Good judges were also more tolerant of other people and tended to 'fake good' according to their Lie Scale scores. But perhaps the peer ratings made by the 'good' judges corresponded more to the peers' self-descriptions, because they were relatively flattering. It is hard to know how to interpret this study.

Brown and Hendrick (1971) found that extraverts were more 'visible' than introverts, in the sense that they were more accurately perceived by other people. This applied to the perceptions of both extraverts and introverts; both personality types perceived extraverts more accurately. An examination of discrepancies between the

perceived selves and ideal selves of the two personality types revealed that while the extraverts were reasonably happy being extravert, the introverts would have liked to be less introverted than they were. In other words, both types saw extraversion as a more ideal type of personality. Introverts pretending to be more extravert than they actually were (because this is what they would prefer) might explain why they were less accurately perceived than extraverts.

7.7 Expressive Control

Suppose people are motivated to conceal their true personality by acting the role of somebody with a different personality. How easily can they do so? Professional actors and actresses are of course fairly proficient at this, but to what extent are the rest of us able to suppress the expressive clues to our personality? Lippa (1976) assessed subjects in terms of Eysenck's extraversion scale and then videotaped them as they role-played being maths teachers first 'as themselves,' then pretending to be introverts, and again as extraverts. A number of expressive behaviours such as the length of their stride in front of the blackboard, the expansiveness of their writing and the amount of eye-contact and talking were recorded. Naive raters then had to judge how 'extraverted' subjects appeared in their three trials. Results showed that although there were considerable individual differences in acting ability there was also significant 'leakage' of people's dispositional (real) extraversion. In other words, judges were fairly good at estimating peoples' extraversion, regardless of which personality type they were trying to simulate.

It was particularly interesting to note that although subjects changed their expressive behaviours in accordance with the stereotypic picture of extraverts and introverts (e.g. extraverts taking longer strides, using bigger writing, more talking and more eye-contact), the extent to which the role-players altered these behaviours did not affect the ability of the judges to see through their act and identify their true personality. This probably means that personality is expressed through a great many subtle gestures, in addition to the more obvious indicators that were monitored in this study.

In a replication and extension of this study, Lippa (1978) considered the trait of neuroticism as well as that of extraversion. The most surprising aspect of the results of this study was a failure to find any correlation between the same expressive behaviours as those described above and either extraversion or neuroticism (in the previous study extraversion was reliably associated with the expressive behaviours in the direction hypothesized). However, a significant interaction between extraversion and neuroticism in determining graphic expansiveness was found. That is, extraversion went with large writing for subjects who were low on neuroticism, but the reverse was the case for subjects high on neuroticism. This is interesting, because it is consistent with previous findings with respect to personality and handwriting (e.g. Taft 1967). The usual interpretation of this interaction is that extraverts will show expansive handwriting unless they have reason to be defensive, in which case they will react decisively and unambiguously in the opposite direction. High N is presumably one basis for such defensiveness.

Lippa included in his experiments a questionnaire measure of expressive control called the Snyder Self-Monitoring Scale. This consists of 25 true-false items asking respondents how good they are at controlling their expressive behaviour and how motivated they are to modify their behaviour according to social demands. Some evidence was found for the validity of this scale as a measure of expressive control; for example, in the first experiment subjects who were high on the self-monitoring scale changed their expressive behaviour more strongly in accordance with experimental role-playing requirements. In connection with personality, it was found that differences in degree of self-monitoring affected neuroticism displays but were not related to the expression of extraversion. In particular, the high self-monitors succeeded in suppressing facial and vocal expressions of anxiety, but they were less successful in covering bodily expressions of anxiety.

Lippa maintains that these results go some way towards reconciling the dispute between the exponents of trait theory such as Eysenck and personality theorists such as Mischel who think that traits are either non-existent or unimportant because they are so unstable from one situation to another. Lippa suggests that there are stable personality dispositions but that their manifestation is strongly affected by moderator variables such as expressive control and approval-seeking. In support of this idea he argues that the suppression of neurotic behaviour, but not extraversion, is consistent with a social desirability hypothesis. It is a pity that these studies of Lippa did not include the P scale, since the extent to which psychopaths are capable of deceiving people with respect to the true nature of their personality is of particular interest to forensic psychologists. The classic picture of a psychopathic criminal is that of a man who is friendly and charming up until the point when he commits his unfeeling and vicious crimes. Confidence men also appear to have warm personalities, which they use deliberately in the service of their crime.

7.8 Field Dependence

A perceptual phenomenon that is thought to be prototypic of some aspects of social perception is that of field dependence. Essentially, this means the extent to which judgements of an object are perturbed by its context. A number of studies have shown that extraverts are more field dependent than introverts on a number of measures of the phenomenon (e.g. Loo 1976; Fine and Danforth 1975). Cegalis and Leen (1977), however, found no differences between extraverts and introverts as measured by the rod-and-frame test until inverting spectacles were worn by the subjects. Under these conditions of induced perceptual conflict, the extraverts showed greater field dependence. They suggest that this could be because the complexity of the visual cues became so great at this point that the introverts found it necessary to dis-attend vision in order to control escalating arousal.

Loo and Townsend (1977) looked at the relationship of various components of extraversion to field dependence as measured by the Group Embedded Figures Test. They found that impulsiveness and quick decision times were related to field dependence, whereas sociability and sensation-seeking were not consistently related. This leads to a slightly different interpretation from that suggested by Cegalis and Leen; extraverts may show greater field dependence because they are less likely to inhibit their first response impulse. The introverts would appear to gain greater field independence, not by blocking out the environment, but by evaluating it cognitively in a more careful way. This connection between impulsiveness and field dependence has been supported by a number of other studies (see the review by Messer 1976), although at least one failure to replicate has been reported (Davidson and House 1978).

There seems to be little consistent difference between neurotic and stable subjects with respect to field dependence. Arora and Murthy (1975) found a suggestion that clinically neurotic subjects were more field dependent than normals, but Loo (1976, 1978) was unable to find any correlation with the Eysenck N Scale. Other studies have indicated that a combination of introversion and field dependence (which, according to the above findings, is statistically rather unusual) is associated with high levels of personal conflict and neurosis (Doyle 1976a). It is perhaps worth noting in this connection that under the high level of stress that was presumably induced by the inverting spectacles used by Cegalis and Leen, all subjects became more field dependent in their judgements.

In summary, field dependence seems to be associated with extraversion and situational stress. Results are equivocal with respect to trait neuroticism, and the relevant research does not appear to have been conducted for psychoticism.

7.9 Suggestibility

There is a sense in which field dependence could be thought of as a form of suggestibility. Can

we, then, generalize from this purely perceptual situation and say that extraverts are more susceptible to all forms of social influence? Unfortunately, it is not quite as simple as that. It appears that, as Eysenck and Furneaux (1945) have argued, suggestibility is not a unitary concept.

In some circumstances extraverts do seem more open to social influence. They have been shown to be more inclined to change their judgements under the influence of prestige suggestions – a form of compliance with authority (Sinha and Ojha 1963) – and to change their evaluation of paintings after learning the name (and thus reputation) of the artist (Mohan and Mohan 1965). The Mohans also found in their early studies that high neuroticism scores were associated with this kind of suggestibility. Unfortunately, their more recent research, which uses more sophisticated techniques, has failed to replicate either of these personality associations (Mohan and Kumar 1973). Another study showing extraverts to be more suggestible is that of Rim and Seidencross (1971), which found that extravert children were more responsive to peer influences with respect to anti-social behaviour.

In other situations it seems that introverts may be more susceptible to social influence than extraverts. Thus introverts are apparently more given to reporting autokinetic effects – that is, the movement of a stationary light in a dark room following suggestions that the light is moving (Panek 1962). Introverts are also more likely to show autokinetic word effects (seeing a fixed pin-point of light as writing words under appropriate suggestion), although this difference was noted only among stable subjects and not those high in neuroticism (Frigon 1977, unpublished work). In what could be viewed as a similar experimental situation, Wilson and Gregson (1967) found that introverts were more likely to vary their judgements of taste intensity in parallel with the intensity of ambient light in the tasting booth. Finally, introverts seem more responsive to operant reinforcement schemes in the industrial setting. Organ (1975) gave bonus points to business school students for performance on quizzes that tested daily preparation. Introverts obtained more points throughout and maintained a steadier performance record over time; extreme extraverts actually showed a decrement with time, which suggests that they were progressively losing interest.

When an introvert meets an extravert with different views on a controversial topic, the introvert is more likely to change his mind as a result of the encounter (Carment et al. 1965). However, it is not clear whether this is because extraverts are more stubborn, more assertive, more plausible or simply more talkative (they were observed to do most of the talking during the meeting). Introverts are also more likely to change their attitudes as a result of writing an essay that is counter to their own viewpoint (Norman and Watson 1976). This study also showed that introverts found interpersonal disagreement more aversive than extraverts.

One might think that the most suggestible persons would be those in the neurotic-extravert (hysteric) quadrant of the E/N System. One well-known study that would point to such an idea is that of Moss and McEvedy (1966). They investigated what was described as 'an epidemic of overbreathing' that occurred amongst girls in a North of England school. It began with one or two girls feeling dizzy and fainting, and by late morning 'they were going down like ninepins'. Eighty-five girls were taken to hospital by ambulance, and the school was closed. Twice it was re-opened, and the same thing happened again. After various physical hypotheses had been checked out, the researchers looked at the role of personality in susceptibility to this 'hysteria'. As predicted, the girls who were affected by this contagion were significantly more extraverted and neurotic than those who were not so affected. Moss and McEvedy concluded that the phenomenon was psychosomatic and that the stage had been set by a prior outbreak of polio in the area which had aroused widespread fears of epidemic.

This finding is only partly supported by Schmidt (1975) in a study of the personality characteristics of men and women who fainted during blood donation in Milwaukee. The fainters were higher in neuroticism than a control sample of non-fainters, but there was no significant difference with respect to extraver-

sion – the fainters were actually a little more introverted. Of course, from the social reinforcement point of view, fainting in the playground may be attention-getting, while fainting during blood donation could be construed as cowardly and inappropriate. It is therefore not entirely surprising that the correlation of fainting with extraversion should be different in these two situations. Neuroticism, though, is a consistent correlate of psychosomatic symptoms such as fainting. Similarly, Claridge (1970) has noted that subjects who respond to placebo therapy are neither consistently extraverted or introverted, but they do tend to be high in neuroticism.

Hilgard and Bentler (1963) found that subjects in the 'hysteric' quadrant of the EPI. were most susceptible to hypnotic induction. However, recent studies by H.B. Gibson and associates have fairly consistently shown neurotic introverts and stable extraverts to be most readily hypnotizable (Gibson and Curran 1974; Gibson and Corcoran 1975). In another study (Gibson et al. 1977), it was found that the tranquillizing drug diazepam appeared to act in such a way as to decrease the operative level of neuroticism, raising the level of suceptibility of neurotic extraverts and lowering it in neurotic introverts. While the explanation for his complex connection between personality and hypnotizability is not yet clear, Gibson has suggested the possibility that the type of hypnotic induction procedure might be critical – neurotic introverts might be more susceptible to an authoritarian procedure which emphasizes 'task-motivating' instructions, while stable extraverts might respond more to permissive procedures that maximize interpersonal reinforcement. This is perhaps another way of saying that the two personality groups have different motives for complying with hypnotic suggestions – the stable extraverts allow themselves to be hypnotized because they have nothing to fear and are again seeking an approving audience, while the neurotic introverts are afraid *not* to comply.

A possible parallel is seen in an experiment on verbal operant conditioning by Gupta (1976). When positive reinforcement was used (e.g. 'good' rather than 'bad'), and when the experimenter was an attractive female, extraverts acquired the conditioned response faster than introverts. Under the threat of punishment, and with a male experimenter, introverts were more readily conditioned.

Altogether, these findings point to the conclusion that "either extraverts or introverts may appear as more open or susceptible to social influence depending upon the particular conditions of motivation. When social rewards and excitement are offered as incentives, extraverts appear more likely to comply and cooperate. In conscience arousing situations, and those that are relatively impersonal, introverts are often more motivated to comply" (Wilson 1977). Neuroticism seems in general to be a predisposing factor towards social influence, particularly those manifestations which are irrational or maladaptive. No studies have been reviewed concerning the role of psychoticism in social influence and suggestibility, though it is reasonable to suppose that because of its association with dominance and 'machiavellianism' (Eysenck and Wilson 1976), the individual scoring high on the P dimension would be fairly immune to persuasion by others.

7.10 Conflict Handling

Kilmann and Thomas (1975) describe five modes of handling interpersonal conflict as follows: *competing* (forcing behaviour and win-lose arguing), *collaborating* (confronting disagreements and problem-solving to find solutions), *avoiding* (withdrawal and failure to take a position), *accommodation* (attempting to soothe the other person and seek harmony) and *compromising* (the proposal of middle-ground solutions). These five categories of conflict handling can be theoretically organized in relation to two dimensions according to the extent to which the individual tries to satisfy his own concern and the extent to which he tries to satisfy the interests of the other party (Fig. 7.1).

Kilmann and Thomas go on to investigate the relationship of personality to this scheme.

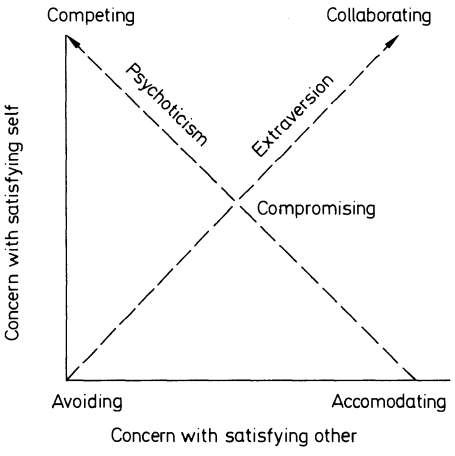

Fig. 7.1. Five modes of handling conflict and their probable relationship with extraversion and psychoticism. (Adapted from Kilmann and Thomas 1975)

Unfortunately, they used the Jungian personality classification system (operationalized by the Myer-Briggs Indicator); nevertheless, it is possible to suggest on the basis of these results how the Eysenck personality system would fit in. The dimension running from avoidance to collaboration seems to relate to extraversion (a pattern of correlations being found with the Jungian measure that is compatible with this idea). Likewise, the dimension running from accommodation to competition would seem to connect with psychoticism in the Eysenck system (since in the Jungian system the compassionate, empathetic 'feeling type' was found to be particularly accommodating). These suggested relationships are also shown in Fig. 1. At the moment they must be treated as hypothetical; the Kilmann and Thomas finding needs to be replicated using the Eysenck Personality Questionnaire and preferably some more objective measures of conflict handling (the authors admit that their use of self-report measures of response to conflict were not entirely satisfactory). At the least, these results suggest that personality is involved in the modes of conflict resolution adopted by people – an observation that is of obvious importance to politicians and diplomats dealing with other people at all levels of dispute up to the international.

7.11 Attraction

There is a slight tendency for extraverts to be more popular than introverts, the correlation averaging 0.10 according to a review of several studies by Mann (1959). The correlation of extraversion with leadership is slightly higher on average (0.15), though it varies a great deal from one study to another. Presumably, extraverts tend to emerge as informal group leaders because they are relatively assertive, more interactive with others and slightly more popular. No doubt P will also prove to correlate with leadership, though not necessarily popularity.

Hendrick and Brown (1971) split popularity into several different aspects and considered the question of whether introverts and extraverts prefer their own kind. The EPI was used to classify 205 students into introvert and extravert groups, and these subjects were required to evaluate bogus strangers who conformed to the stereotype of introvert and extravert respectively. On four out of six attributes the extravert stranger was preferred by both extravert and introvert subjects (especially the extraverts). These were: 'liking', 'interesting at party', 'ideal personality' and 'prefer as leader'. On two other measures, however, ('reliable as friend', 'honest and ethical') the introvert stranger was preferred by the introvert subjects, and there was no strong preference expressed by extravert subjects. These results suggest the interplay of three generalizations:

1) Other things being equal, people like others who are similar to themselves.
2) Extraverts are generally more popular than introverts, in that they are seen as more likeable, interesting and influential.
3) Introverts are perceived as having the qualities of honesty, stability and reliability more than extraverts.

Stern and Grosz (1966) observed the behavior of patients in group therapy in relation to their personality scores. In line with the affiliation studies, extraverts interacted more with other patients. It was also found that patients tended to interact with others who were like themselves in terms of introversion–extraversion. That is,

extraverts like to talk with other extraverts, and introverts also preferred their own kind. This supports the idea that people prefer their own personality type. However, contrasting results were found for the internal-external control dimension; in this case patients tended to interact with dissimilar others. All we can say, then, is that in a group situation, interaction patterns are partly determined by the personalities of the individuals concerned, one of the important dimensions being extraversion.

Studies of partner choice indicate that people select mates and marry almost at random as far as P, E and N are concerned. If anything, there is a very slight tendency for people to mate with others similar to themselves on these dimensions, and they become progressively similar over the course of their marriage (Nias 1977a). There is also some evidence that couples who are similar in personality at the time of marriage are less likely to suffer a breakdown of their marriage (Bentler and Newcomb 1979). Altogether, the similarity theory of matching is better supported than the rival complementation theory. As regards sexual preferences, extravert men tend to prefer large-breasted women, and extravert women tend to prefer sporty, muscular men (Wilson and Nias 1976; Eysenck and Wilson 1979).

7.12 Sexual Behaviour

Eysenck (1976) reported on a series of studies concerned with connections between personality and sexual attitudes and behaviour. A questionnaire was constructed containing over 100 items, some concerned with the variety of sexual acts that the respondent had participated in, and others referring to attitudes in the area, e.g. "I think about sex almost every other day", "women who get raped are often partly responsible themselves". "There should be no censorship on sexual grounds of plays and films". "Would you accept an invitation to take part in an orgy?" The attitude items were factor analysed to reveal 14 factors at the primary level. These factors are listed in Table 7.1, to-

Table 7.1. Sexual attitude areas identified by factor analysis.

Factor	Example of items
Satisfaction	I have not been deprived sexually
	My love life has not been disappointing
Excitement	It doesn't take much to get me excited sexually
	I get very excited when touching a woman's breasts
Experimentation	A person should learn about sex gradually by experimenting with it
	Young people should be allowed out at night without being too closely checked
Curiosity	Sex jokes don't disgust me
	I would agree to see a 'blue' film
Premarital sex	Virginity is a girl's most valuable possession
	One should not experiment with sex before marriage
Promiscuity	Sex without love ('impersonal sex') is not highly unsatisfactory
	I have been involved in more than one sex affair at the same time
Homosexuality	I understand homosexuals
	People of my own sex frequently attract me
Hostility	I have felt like humiliating my sex partner
	I have felt hostile to my sex partner
Prudishness	I don't enjoy petting
	The thought of a sex orgy is disgusting to me
Censorship	There are too many immoral plays on T.V.
	Prostitution should not be legally permitted
Repression	Children should not be taught about sex
	I think only rarely about sex
Inhibition	My parents' influence has inhibited me sexually
	Conditions have to be just right to get me excited sexually
Nervousness	I don't have many friends of the opposite sex
	I feel nervous with the opposite sex
Guilt	At times I have been afraid of myself for what I might do sexually
	My conscience bothers me too much

Table 7.2.[a] Sexual attitudes related to personality

Factors	E	N	P
Satisfaction	+	− − −	−
Excitement	+	+ +	+
Nervousness	− − −	+ +	0
Curiosity	0	+	+ +
Premarital sex	+	0	+ +
Repression	0	0	−
Prudishness	−	+	+
Experimentation	+	0	0
Homosexuality	0	+	+
Censorship	−	0	−
Promiscuity	+ +	0	+ + +
Hostility	0	+ + +	+ + +
Guilt	0	+ + +	0
Inhibition	0	+ + +	+
Summary factors			
Sexual pathology (vs satisfaction)	−	+ + +	+ +
Libido	+	+	+ + +

[a] +, 0, and − signs indicate positive, zero and negative relationships respectively. The strength of the relationship is indicated by the number of signs. (Data of Eysenck 1976)

gether with items representative of each. These 14 factors were also collapsed into two broader 'superfactors', which were called *libido* and *satisfaction*. The first concerned various aspects of permissiveness and active sexuality, apparently reflecting a general sex drive; the latter included various sexual difficulties and claims of being deprived. Later work with twins revealed these two factors to have a considerable genetic loading – about half to two-thirds of variance being due to heredity.

When the two main factors were correlated with personality, it was found that libido was associated with high P and to a lesser extent high E and N. Satisfaction was most strongly linked with N (high N people, of course, being dissatisfied) and to a lesser extent with P and E (the most satisfied individuals being low on P and high on E). For a more detailed look at these relationships, consult Table 7.2, which shows how the 14 more specialized factors are correlated with P, E and N. High E subjects are particularly high on promiscuity and low on nervousness and prudishness. High N scorers are high on excitement, nervousness, hostility, guilt and inhibition factors and low on satisfaction. High P scorers are high on curiosity, premarital sex, promiscuity and hostility.

Analysis of self-reported sexual behaviour in relation to personality revealed that high E subjects have intercourse earlier in life, more frequently and with more different partners. They experiment more with petting and with different positions for intercourse and generally indulge in more varied sexual behaviour. They kiss more and pet to orgasm more than introverts and engage in longer pre-coital love-play. They masturbate less than introverts, perhaps because of the availability of other sexual 'outlets'. Female extraverts have orgasms more often during intercourse than female introverts (29% of female extraverts 'nearly always' experience orgasm during intercourse, compared to only 17% of introverts). Altogether, then, extraverts emerge as hedonists and happy philanderers; they epitomize the permissive approach to sex, with frequent changes of sex partner and a 'healthy' appetite for frequent sex contacts. By contrast, introverts are characteristically puritanical, putting a stress on virginity and fidelity and playing down their animal passions. Such a pattern of differences is of course compatible with the idea that extraverts seek social and physical stimulation in order to maintain an arousal level that constantly threatens to fall below that which is comfortable.

High N subjects pet less than low Ns, have less intercourse and engage in fewer oral-genital acts. High N males, however, express a strong sex drive and desire for coitus. Although less experienced sexually, they do seek sexual outlets, masturbate more frequently and report more spontaneous erections. High N females have orgasm less frequently during intercourse, in spite of feeling a strong sex drive, but are no less experienced in terms of sexual behaviours engaged in. Apparently, the 'nervous' male has difficulty in initiating sexual meetings and practices; he feels a high degree of desire and excitement but this is accompanied by anxiety, which prevents him from consummating his desires. High N females, however, are no less experienced, perhaps because a dominant man can persuade a 'nervous' girl more easily than a calm and independent one. It is not diffi-

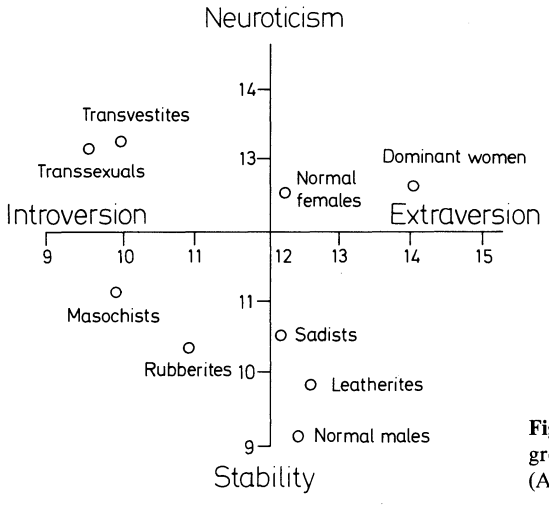

Fig. 7.2. The position of various sexual preference groups in relation to extraversion and neuroticism. (After Gosselin and Wilson 1980)

cult for a nervous female to simply acquiesce – indeed it may be more difficult for her to say 'no'.

High P scorers are distinguished mainly by a liking for oral sex. They are very experienced, having participated in a wide variety of sexual practices, but again they are unhappy with their sex lives. It seems that the high P individual can 'never get enough'. Thus high N and high P scorers both show a 'pathological' or 'non-conforming' pattern of sexual reactions. Both report strong sex drives, but whereas high Ps tend to act out their libidinous, promiscuous and oral desires, high Ns do not. It seems that the inhibitions, worries and guilt feelings of neurotics prevent them from consummating their desires. Both high Ns and high Ps report dissatisfaction, but for different reasons, neurotics because they are repressed and psychotics because they are insatiable. This is a nice illustration of the need to separate the N and P dimensions in research and clinical practice.

Certain groups with mildly deviant sexual proclivities have also been studied with respect to personality. Gosselin and Wilson (1980) had groups of (mostly male) sexual variants complete the EPQ and compared their scores with various normative and clinical groups. The arrangement of these groups in relation to the E and N factors is shown in Fig. 7.2. Generally speaking, men with deviant sexual proclivities tended towards the dysthymic quadrant. The transsexuals, transvestites and masochists, in particular, were on the introvert side, and all were slightly higher than control males on the N dimension. But it is interesting to note that the transvestites and transsexuals (all males who dressed or identified as females) were little higher on N than normal females, whom they, at least, would probably regard as the more appropriate control group. Actually, many of the transsexuals claimed to have two different personalities, according to whether they were in male or female mode at the time, and this was confirmed by differences in their responses to the EPQ completed once as a man and then again as a woman. When dressed as a woman the E scores tended to be higher than when in the male mode, while P and N were reduced.

With respect to the E scale, there were two male groups that were just as extravert as normal males – these were the sadists and leather fetishists. The most extravert group of all, however, was a sample of dominant women, calling themselves 'superbitches', who specialized in catering to the interests of the introvert and submissive males. Some of these women were professionals, but others seemed to enjoy adopting the 'male' role in satisfying the needs of masochists and fetishists. These dominant women were also the only group to be significantly higher than normal males on the P scale, thus confirming the idea that a kind of male-female

Table 7.3. Responses of variant and normal groups to certain selected items from the Eysenck Personality Questionnaire

Theoretical factor	Item	Percentage 'yes' responses					
		Sado-masochists	Leather-ites	Trans-vestites	Trans-sexuals	Domi-nant women	Male con-trols
Shyness	"Do you tend to keep in the background on social occasions?"	70	11	64	69	20	51
Sensitivity	"Are your feelings easily hurt?"	73	11	78	88	16	43
Loneliness	"Do you often feel lonely?"	54	45	51	57	76	29
Depression	"Have you ever wished that you were dead?"	30	31	43	63	52	18
Guilt	"Are you often troubled by feelings of guilt?"	48	29	62	44	52	44
Obsessionality	"Do good manners and cleanliness matter much to you?"	92	63	94	88	72	95
Concern with looks	"Do you worry a lot about your looks?"	36	42	57	82	48	37
Sense of humour	"Do you like telling jokes and funny stories to your friends?"	60	21	58	25	44	80
Relationship with mother	"Is (or was) your mother a good woman?"	83	37	83	100	48	95

[a] Note: Control data are taken from EPQ standardization. Although matched for mean age, there is slightly less variance in the ages of the control males.

role reversal is taking place between these male and female variant groups.

Gosselin and Wilson also looked at the way in which certain individual items from the EPQ of special theoretical significance discriminated the various groups of men (Table 7.3). Since it has often been supposed that sexual deviates are lacking in the social skills necessary for normal sexual contact, items referring to shyness and social difficulty were specifically examined. It appears that the sado-masochists, transvestites and transsexuals are particularly shy and sensitive, while the leather fetishists and dominant women deny such tendencies. All the deviant groups are, however, susceptible to loneliness and depression. In line with the authors' impression that these people take life and themselves quite seriously, all of the deviant groups were significantly less likely to enjoy telling jokes to their friends. That the leather fetishists shared an element of toughness with the dominant women is indicated by their dislike for their mothers. (Over half of these groups denied that their mother was a 'good woman', compared to only 5% of control men). Such detailed analysis rounds out the picture given by the broader scales of the personality of sexually deviant men and women and the social difficulties that might underlie their behaviour.

7.13 Attitudes and Values

Of the two major dimensions of social attitudes identified by Eysenck (1954) – conservatism versus radicalism and tough-mindedness versus

tender-mindness – it is the latter that seems more firmly rooted in personality. At first it was supposed that extraversion would be the prime correlate of tough-mindedness, and a number of studies have shown correlations of greater or lesser degree. A recent heritability study of this issue even went as far as to demonstrate that while extraversion and tough-minded attitudes were individually about half determined by genetic factors, the overlap between them was almost entirely due to heredity (Eaves and Eysenck 1974).

However, since the third dimension of psychoticism has been added to the Eysenck system of personality classification, it has become clear that it is an even stronger determinant of tough-minded attitudes – so much so, in fact, that tough-mindedness has been entertained as a better description of the dimension, particularly where non-clinical samples are concerned.

The radicalism dimension does not seem to connect very strongly with personality measures, although some studies have indicated a slight tendency for introversion to go with conservatism (Wilson and Brazendale 1973). Neuroticism is not a strong correlate of social attitudes, except for the finding that has sometimes been reported that high N predisposes to ethnocentrism and intolerance of outgroups. For a more complete analysis of the relationship between personality and social attitudes see Eysenck and Wilson (1978).

The correlations between personality and social attitudes can be detected fairly early in life. Powell and Stewart (1978) had more than 800 children aged 8 to 15 complete a Junior EPQ and a Children's Scale of Social Attitudes. Children scoring high on psychoticism were more likely to endorse ethnocentric and punitive themes and show opposition to religious values (i.e. a generally tough-minded attitude pattern). Extraversion went with sexual permissiveness and sociability and opposition to ethnocentrism, while neuroticism was not consistently related to social attitudes. These relationships are very much the same as those found for adults.

Studies of personality in relation to values show a similar picture. Simmons (1976) correlated EPI extraversion and neuroticism with his own value survey and found that extraversion correlated positively with ratings of the importance of 22 of the 100 value statements. These were categorized as being concerned with 'open, warm and loyal social relations' (e.g. entertaining others, pleasure in being with others, living in a world at peace, being open and receptive to others, being charitable, loving parents), 'vibrancy, vitality, vigor and viability in daily living' (e.g. seeking adventure and excitement, a sense of aliveness, being the one who brings about changes, the joy of experiencing, playfulness) and 'the search for individuality with integrity' (being myself, developing into a more satisfying person, leading a meaningful life, being unique). Only three items correlated beyond the 0.05 level of significance with neuroticism, which is less than would be expected by chance. A criticism that could be made of this study, as well as that of Watkins (1976), which found similar results, is that the value surveys used were non-ipsative in design. Since all the values in the test were positive or socially desirable to the majority of the population, it might make more sense to have subjects choose priorities among them (e.g. by a ranking technique), rather than have them assign absolute values. As it is, the tendency for extraverts to rate most of the items more highly than introverts could partly reflect approval-seeking (a value in itself). It is no accident that the best-known measure of values, the Allport-Vernon 'Study of Values', is an ipsative instrument.

In the study of Watkins, it was observed that the correlations between personality and values were not always the same for men and women. For example, extraversion was associated with economic values in women, but not in men, and with religious values in men, but not women. A rather interesting case study of sex differences in relation to personality-value correlates is provided by Borden and Francis (1978), who were concerned with the question of individual differences in environmental attitudes – issues such as energy consumption and pollution. They found that while extraversion was conducive to environmental 'concern' in females, introverted males were more likely to be concerned about environmental issues. This led the authors to suppose that men and women

have different motives for involving themselves with the ecological movement – women are apparently seeking to further their social and leadership ambitions, while the males appear to be more oriented towards the rational aspects of the issues themselves (they have plenty of other opportunities to satisfy their needs for social stimulation and recognition). Whatever the interpretation, it is clear that personality does not translate into values in the same way for men and women.

Rim (1977b) studied the relationship between personality and work values – those aspects of work which are seen as most important and rewarding. Men who were high on neuroticism and extraversion perceived the following areas as most significant: economic, social contact, patterning of time and power. These patterns were the same for women also, with the exception that the economic value was not correlated with the NE personality type. Unfortunately, this study also suffered from the shortcoming that the value measure was non-ipsative, so that no value preferences for stable introverts could be established.

7.14 Recreational Interests

Nias (1977b) has studied the relationship between personality and choice of leisure-time activities, using a sample of 1,270 adults. He found that extraversion was associated with social entertainments such as talking to friends and drinking, the liking for adventure films, romantic films and crime films. For men, but not women, extraversion was negatively associated with academic interests but positively with an interest in encouraging their children (perhaps socially rather than educationally). Neuroticism was not strongly correlated with interest patterns, although there was a slight tendency for high N scores to go with a dislike of the home environment, and for academic interests in the parents to be associated with neuroticism in the children (failure anxiety induced by parental expectations perhaps?).

A rather similar study of recreational interests in over 1,000 school children aged 12 to 16 was conducted by Nias (1979). As with the adult sample, the correlations between personality and interest were fairly low but consistent with expectation. Extraversion was associated with sporting interests and enjoyment of pop music and neuroticism had a negative association with scientific interest. Psychoticism was associated with sport and with a liking for entertainment involving crime, horror and war.

In some other unpublished work, Nias has found a fairly consistent connection between extraversion and specialization in sport. When a group of 118 male physical education students were tested with the EPI, the mean E score was 14.53, which is higher than that given for any occupational group in the test manual. He also tested 39 members of the Great Britain athletics team and found that they too were fairly extraverted on average. Neither of these groups was particularly distinguished in terms of neuroticism. Unfortunately, the version of the EPI used in these studies did not incorporate a psychoticism scale, but it is possible to infer that the athletes were fairly high an P on the basis of their high scores on need for achievement, heterosexuality and aggression, registered on the Edwards Personal Preference Schedule.

When the student groups were classified according to their extent of participation in sport and their degree of success, little difference was found with respect to personality. All physical education students seem to be fairly high in extraversion, regardless of their dedication and ability. However, when sports ability was studied among school children, there was a positive correlation between extraversion and teacher ratings of ability for both boys and girls. In addition, among boys extraversion was correlated with self-rated past performance in sport. Finally, Nias and Hardy (1971) found a correlation of 0.67 between extraversion and learning to swim among a group of 29 children aged 10–12 who were not previously able to swim. Indeed, extraversion was the only characteristic among about a dozen mental and physical traits studied that was found to predict the ease with which the children would learn to swim. However, the authors caution that this finding cannot be generlized to high level ability at sport, for the less extraverted individual may be better

suited to persisting with long-term training schedules.

It is interesting to note that sporting activity is also claimed to promote favourable changes in personality. A number of studies reviewed by Young and Ismail (1978) led them to the conclusion that as people become physically fit through exercise such as jogging, their level of neuroticism is lowered. While physically fit people tend to be extraverted, the extraversion clearly precedes the fitness rather than arising as a result of it; neuroticism, however, appears to be affected by exercise more than providing an impetus for it. This has led some theorists to campaign for jogging and sport as a treatment for neurotic conditions, especially depression. Unfortunately, it may yet turn out that the relationship is spurious, in that the motivation to engage in exercise may increase as one aspect of an ongoing (possibly spontaneous) recovery process. The studies described by Young and Ismail do not adequately control against this alternative interpretation.

7.15 Occupational Choice and Aptitude

Eysenck (1971) reviews work showing that extraversion is linked with vocational preferences and various aspects of industrial performance. In general, extraverts display greater 'social intelligence' than introverts, i.e. ability to relate to other people, to take a personal interest in them and their problems and to anticipate their reactions. Thus they tend to gravitate towards, and perform best in, jobs that involve dealing with other people (e.g. sales and personnel work, nursing and teaching). The ability of the introvert to resist boredom and persist with a task for a long period of time is also valuable in other occupational contexts. Introverts are more reliable and conscientious, they are more punctual, absent less often and stay longer at a job (having less need for novelty). While on the job, the extraverts appear to waste more time talking to their work mates, drinking coffee and generally seeking diversion from the routine.

However, personality tests are of limited usefulness as selection devices in education and industry, because they are readily faked. If extraversion is to be a useful basis of selection it would often have to be measured covertly, and the ethical acceptibility of this is doubtful. In any case, the power of personality tests to predict occupational success is open to question (Turnbull 1976).

A study using the Strong Vocational Interest Blank (Bendig 1963) confirms that introverts tend to prefer theoretical and scientific occupations such as architecture, journalism and the teaching of mathematics, while extraverts are more suited to people orientated jobs such as social work and selling life insurance. Neuroticism was generally less important as a predictor of occupational interest; though high N seemed to be antithetical to business-type occupations such as accountancy, banking and office management. Again, while this is of some theoretical interest, it has little practical application, because specialized interest scales are more directly useful for vocational guidance purposes.

Sometimes there appears to be an interaction between extraversion and neuroticism such that 'quadrant analysis' is the best way of investigating occupational aptitude. For example, Jessup and Jessup (1971) tested a group of trainee pilots with the EPI early in their course and compared the scores with their eventual success or failure. Failure rates for the four personality combinations were: stable introverts 14%, stable extraverts 32%, neurotic extraverts 37% and neurotic introverts 60%. Thus, in this case, it seems that introverts are much more affected by high levels of emotionality than are extraverts; the extraverts experienced moderate success regardless of their level of neuroticism. This finding is not just of theoretical interest, the relationships in this case are sufficiently powerful for personality to be considered a potentially useful selection device.

Studies of the personality of students following different degree courses have revealed a number of consistent differences. For example, Kokosh (1976) using the MMPI found that physics and zoology majors were more introverted than those following social sciences such as sociology and history. The sociologists and

historians were also higher on the 'Psychopathic Deviate' scale than the physical and biological scientists, which both supports the extraversion correlation and hints at a correlation with psychoticism (the Pd scale of the MMPI correlates with both E and P in the Eysenck classification). Perhaps we have here a clue as to the reason why sociologists have gained such a reputation for policital radicalism and activism. We shall return to this question in a later section.

Studies of entrepreneurs (Lynn 1969) and successful businessmen (Eysenck 1967) show them generally to be stable introverts, although there is some indication that this might not hold true for the United States, where extraversion is a more positively valued attribute compared with the United Kingdom. Henney (1975) found his sample of production superintendents to be mostly stable extraverts and suggested that factors such as the size of the organization and the different management functions, such as production, sales and personnel areas, might be important in the link between personality and management aptitude. Blunt (1978) produced some support for such a hypothesis in a study of 263 white South African managers. Personnel managers scored higher on N than finance and production managers. Sales and marketing managers were the most extraverted group, while technical production and transport managers were the most introverted. Blunt concluded that the extraversion dimension in particular might be useful in placement for job satisfaction, if not actually for selection purposes. Neuroticism, he thinks, is more reactive than predictive; although his group of personnel managers was very small in size, he speculates that the stress involved in the mediating function of these men might have led to the significantly high N score that they displayed. Again, it is a pity that P scores do not appear to have been examined in relation to success in business and management.

The personality characteristics of young men who are unable (or perhaps unwilling) to find work have been studied by MacLean (1977). Subjects were married men between the ages of 25 and 40, with at least average intelligence and education and without any obvious physical disability. Recipients of welfare payments over varying lengths of time were compared with controls on Cattell's 16 Personality Factor Questionnaire. The findings could be summarized by saying that the welfare recipients were more neurotic, psychotic and introverted than matched controls, and the longer they had been unemployed, the more marked these characteristics (particularly neuroticism) became. It was unclear to what extent these personality characteristics were the cause of the unemployment; possibly the inability to find work could have led to personality changes in these directions. Most likely a kind of vicious spiral was operating – men with inadequate personalities finding themselves out of work and their personality deteriorating further as a result of that experience.

7.16 Industrial Performance

The role of personality has been investigated in relation to a number of other areas of work performance. Hockey (1972), for example, reviews evidence showing that extraverts are likely to enjoy and benefit from a noisy environment as regards work performance, while introverts are likely to perform better in conditions of quiet. There is also evidence that extraverts are more influenced by environmental circumstances than introverts, who maintain relatively stable performance under a variety of environmental conditions. A very practical demonstration of these effects has been provided by Fagerstrom and Lisper (1977). These Swedish researchers investigated the question of whether deterioration in driving performance over a 4-h period could be offset by talking to the driver or having him listen to the car radio. As predicted from Eysenck's arousal theory of introversion, extraverts showed the strongest decrement in performance over time, but they also benefited most strikingly from stimulation (either talking or music).

Accident proneness also appears to be related to personality. In a study of South African bus drivers (Shaw and Sichel 1970), neurotic extra-

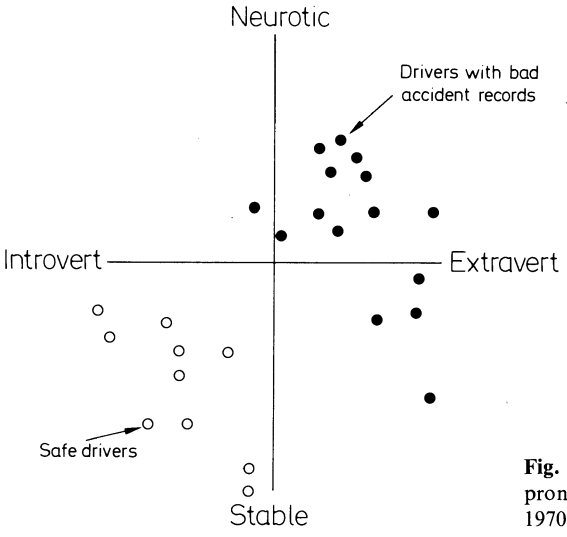

Fig. 7.3. Personality differences between accident-prone and safe bus drivers. (Data of Shaw and Sichel 1970)

verts were found to have the worst accident record, while safe drivers were clustered in the stable-introvert quadrant (Fig. 7.3). Drivers with intermediate safety records tended to be scattered in between these two groups in terms of personality.

The 'riskiness' of extraverts, which is no doubt a function of their inability to tolerate boredom, has been observed in other contexts. For example, Vestewig (1977) showed that extraverts preferred high levels of risk in a monetary gambling situation. On the other hand, introverts show at least an equal amount of curiosity – Farkas et al. (1978) were not able to detect any personality differences between men who volunteered for laboratory sex experiments and those who would not.

7.17 Academic Aptitude and Achievement

We have already noted certain personality differences with respect to preferred areas of educational specialization. Introverts gravitate towards the 'hard' sciences, while extraverts seem more at home in the arts and social sciences (Wankowski 1973). Horn et al. (1975) also report that, compared with engineering students, social scientists are higher on the N scale. This was apparently a result of the kind of people opting for the various subjects and was not affected by drop-outs or changes of major. It is perhaps significant that high N is a female characteristic, and the social sciences are more often chosen by women than men. (Sex was not a mediating factor, though, since the personality/specialization correlation held within each sex). Horn et al. also raise the question of whether the greater tendency towards radical political activity observed in social science students is attributable to their personality at intake rather than the content of their studies. The same personality differences were observed when high school seniors were asked to indicate a preference for either social science or engineering, so it seems that personality determines the choice of subject to some extent, at least, and the radical tendencies of sociologists cannot be attributed entirely to their training.

With respect to academic success and failure there is quite a lot of evidence that extraverted children perform better in school up until the age of 13 or 14, after which introverts seem to gain a progressive advantage (Eysenck and Cookson 1969). Anthony (1977) observed that this switchover could occur either because introverted children applied themselves better to solo studies or because the more able and intelligent children develop introverted behaviour as they grow older. Examination of some longitudinal data collected from a group of 266 children tested at ages 10–11 and again at 15–16 showed

that both kinds of change had occurred. Apparently introverts do get brighter as they get older, but brighter children also become more introverted.

A study by Mehryar et al. (1975), using self-rated academic success, at first sight seemed to suggest that in the American culture extraversion is more conducive to success in higher education than introversion. This sounds reasonable, since extraversion seems more normative and desirable in the United States than Britain. However, there is reason to doubt the validity of the self-ratings of academic prowess. Irfani (1978a) repeated the study with Iranian and Turkish students and again found the extraverts claiming greater academic success. But this was not borne out by their grade-point averages (an objective criterion of academic performance); this showed no difference between extraverts and introverts. Apparently, extraverts in these countries are just less modest about their achievements than introverts, and this might apply to United States subjects also.

There has been relatively little interest in N and P as correlates of academic success. Such evidence as is available suggests that high P is generally deleterious to academic performance, while N is detrimental at lower age levels but beneficial to some aspects of higher level achievement (Goh and Moore 1978; Savage 1972). Another interaction involving N was reported by Walsh and Walsh (1978). Among a group of Canadian high school students, high N was found to impair performance on English and Maths exams for those students above the median in intelligence; no such relationship between N and academic performance was found for students of lower intelligence. Perhaps this can be explained in terms of the Yerkes-Dodson Law, one clause of which states that high anxiety is more detrimental to the performance of complex tasks (of the kind no doubt being tackled by the brighter children) than simple tasks (occupying the lower IQ children).

Of course, it is probably misleading to talk about academic aptitude in general. There are, no doubt, particular areas in which each personality type excels. We have already seen that extraverts are quicker than introverts at learning to swim – a practical and sociable skill. There is also evidence that extraverts perform better in verbal fluency tasks such as generating word rhymes (Di Scipio 1971; Tapasak et al. 1978), and this might be expected to generalize to public speaking ability. For their part, high P scorers appear to come into their own where creative ('divergent') thinking is concerned – in line with the widespread idea that creativity bears some relationship with psychotic thinking (Woody and Claridge 1977).

The other important factor mediating the effect of personality on learning performance is the teaching strategy exployed. Thus stable school children seem to benefit from a teaching approach which is inductive, learner-centred and exploratory, whereas anxious (high N) children seem to need a more deductive, teacher-centred and supportive strategy (Trown and Leith 1975). A number of other studies have indicated that extraverts are likely to benefit from informal, unstructured teaching methods, while introverts do better with the more traditional, structured approaches (Leith 1973; Shadbolt 1978).

Other work has shown that extraverts perform better in a group situation, while introverts are better working on their own (e.g. Leith 1974). This could mean that extraverts are happier in a sociable situation and give of their best before an audience or that their improved performances are due to an increase in arousal, induced by the presence of other people. The fact that the stimulation need not be human company was demonstrated by Revelle et al. (1976), who found that caffeine and time pressure would improve the performance of extraverts on an advanced university examination but was more likely to impair the performance of introverts. A fairly detailed description of how personality might be taken into account in optimizing each individual child's chances of success in school is given by Wakefield (1979).

7.18 Mental Health

Eysenck (1970) has argued that his three-dimensional system of personality classification may

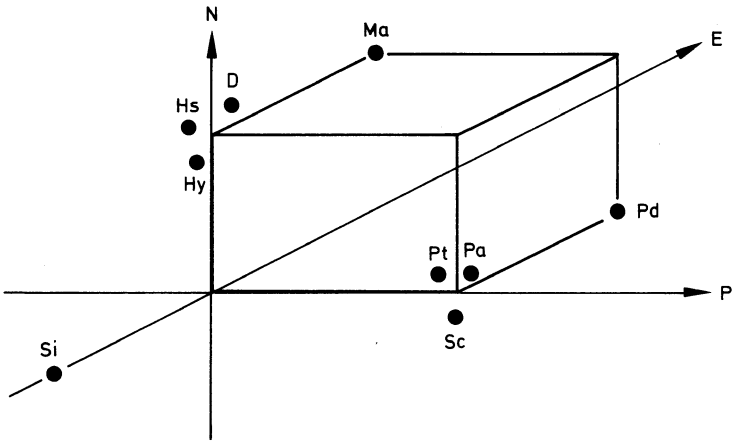

Fig. 7.4. The conceptual placement of MMPI scales within Eysenck's three main dimensions. (After Wakefield et al. 1974). *Abbreviations:* Ma, mania; D, depression; Hs, hypochondriasis; Hy, hysteria; Si, social introversion; Pt, psychathesenia; Pa, paranoia; Sc, schizophrenia; Pd, psychopathic deviate

be used to replace the traditional psychiatric diagnostic system. Indeed, it does seem to be possible to separate many of the traditional diagnostic labels by their coordinates on P, E and N. Thus, for example, schizophrenics and psychopaths are both characterized by high P, but the latter are higher on E.

Endogenous and reactive depression may be distinguished in that the latter has a higher premorbid N score (Paykel et al. 1976). That the classical scales of the MMPI (which correspond fairly closely to traditional psychiatric diagnoses) can be collapsed into Eysenck's three dimensions has been shown by Wakefield et al. 1974. The spatial model that was tested and confirmed by these workers is shown in Fig. 7.4.

The criticism of this approach is that there are many diagnoses such as alcoholism, fetishism and epilepsy that are too specific or 'medical' in nature to be covered by such a system. It is also possible to obtain the same scores on P, E and N as the average schizophrenic without manifesting any degree of thought disorder, and no doubt the same applies to the other diagnostic categories. Probably it is best to work with both the diagnostic and dimensional system at different times for different purposes, perhaps superimposing the former upon the latter. Another worthwile compromise that is recognized by Eysenck is to supplement the three personality dimensions with more specific dimensions derived from the factor analysis of clinical symptoms.

Although E and N are fairly independent dimensions, it often seems that extraversion rather than introversion is related to optimal adjustment. Doyle (1976b) correlated the EPI with Shostrom's Personal Orientation Inventory, which is intended as a measure of 'self-actualization' or positive mental health. All of Shostrom's 12 scales were negatively correlated with N (11 of them significantly) and positively correlated with E (10 of them significantly). It seems that in the North American culture at least, extraversion is a more healthy and desirable characteristic than introversion. It is a pity that this study did not include a measure of P.

Perhaps the ultimate form of maladjustment is suicide, and the inclination towards this seems to be associated with high P and N and again low E. (Irfani 1978b; Mehryar et al. 1977). However, the correlation of suicidal tendency with introversion is less strong than for the other two variables, and there is an indication that suicide attempts which involve low intention to die are associated with impulsivity, particularly in men (Pallis and Jenkins 1977).

Personality correlates of psychiatric disturbance have been shown to precede the onset of the illness. Thus Lewine et al. (1978) found that teachers had described children who be-

came schizophrenic in adulthood as insecure, introverted, submissive and lacking in consideration for others. Depressives, by contrast, were described as independent and mature as children. Thus personality testing of children holds promise for early detection of psychiatric difficulties.

7.19 Psychotherapy

Personality may also be used to predict the outcome of therapy. For example, Di Loreto (1971) found that in treating students for social and general anxiety, systematic desensitization was an effective therapy, regardless of personality type. Rogers' client-centred therapy was effective only for extraverted patients, while Ellis's rational-emotive therapy was effective only for introverts. This could be interpreted in terms of Gray's (1972) theory that extraverts respond mainly to rewards and introverts to punishment. Rogers' therapy is generally supportive and rewarding, whereas Ellis's might be seen as fairly punitive.

Sloane et al. (1976) found that while traditional psychotherapy and behaviour therapy were about equally effective overall in treating mixed neurotics, the personality predictors of outcome were not the same. Psychotherapy was most effective with patients who showed less psychopathology to begin with, especially those low on the hysteria and psychopathic deviate scales of the MMPI. The effectiveness of behaviour therapy did not depend on the initial level of pathology – in fact, it was most effective with patients who scored high on hysteria and mania. This pattern of results suggest that traditional psychotherapy might be best for stable introverts and behaviour therapy for neurotic extraverts.

Other studies of prognostic indicators in behaviour therapy have also indicated that extraverts are more responsive to this type of treatment than introverts (e.g. Gelder et al. 1967; Mathews et al. 1974). Hallam (1976) found only a marginal tendency for extraverts to respond to desensitization for their phobias more rapidly than introverts, but he did find that treatment led to a change in personality scores. Specifically, those patients who improved the most changed their EPQ scores in the direction of lower N and higher E. Such a result is consistent with that of Ingham (1966), who also found that as patients improved with psychiatric treatment over a 3-year period, their N scores dropped and their E scores increased. It appears, therefore, that to some extent personality scores may be reflecting the transient symptoms of the mental disorder, in addition to constitutional traits. The fact that successful therapy increases the E scores of patients could be taken as further support for the idea that extraversion is generally associated with optimal adjustment.

Also in support of this idea is the observation that long-term psychiatric patients are more introverted than those who have been hospitalized for a shorter period of time. Martin and Moltmann (1977) found a correlation of -0.67 between E and length of hospitalization. Of course, this finding in itself could mean that introverted patients were more severely ill, or that the doctors and staff just perceive them that way. Perhaps even more likely is the possibility that the introversion of long-stay patients is a function of their experience of hospitalization. Social withdrawal is one of the well-documented characteristics of the 'institutionalized' person.

7.20 Drug Use and Abuse

Probably the most widely abused drug in our culture is nicotine, taken in the form of cigarettes. On several grounds we would expect cigarette smokers to be more extraverted and neurotic than non-smokers – extraverted because smoking is risk-taking behaviour (at least as determined by the health authorites) and because the drug itself is a stimulant. Neurotics we might expect to smoke more, because the habit is supposed to indicate insecurity – some smokers at least claim they do so in order to 'calm their nerves'. Such correlations have been reported several times (e.g. see Coan 1973;

Reynolds and Nichols 1976). Generally speaking, smokers are more extraverted than non-smokers and are less well adjusted and anti-social in other aspects of their behaviour. Relationships between personality and smoking have tended to come out as stronger for women than men, although we might find in the future, as the sex difference in amount of smoking is reduced (more women smoking and perhaps less men), that the extent to which the behaviour is diagnostic of personality might also become more equivalent.

Apart from general cigarette consumption, some researchers have been concerned with the motives and occasions for smoking. Nicotine is a complex drug, in that it seems to act as a stimulant in small doses but it has a depressant effect in high doses. Eysenck therefore proposed that extraverts would be more likely to smoke for stimulation and would spread their smoking fairly evenly over the course of a day, while introverts would smoke for 'tranquillizing purposes', concentrating their smoking at periods of stress. Contrary to this hypothesis, Bartol (1975) found that female extraverts were more likely to smoke under stressful conditions while female introverts were more likely to smoke under non-stressful conditions. However, Bartol noted that other drugs such as caffeine and tranquillizers were frequently taken in combination with nicotine, so subjects were clearly manipulating their arousal in a complex way.

Smokers of cannabis (a depressant drug) are not distinguished on extraversion (Beaubrun and Knight 1973; Mendhiratta et al. 1978). The latter researchers found that chronic users were higher in N than controls, but it was not possible to tell whether this was a cause or effect of the cannabis smoking. Wells and Stacey (1976) found that young 'drug misusers' in Glasgow were distinguished from non-misusers by being high on N and P, whereas they were again not distinguishable in terms of extraversion. While the authors do not make clear what they mean by misuse, or even the particular drugs with which they were concerned, the impression is that they are talking mostly about cannabis. It appears, then, that extraverts are not more likely to take drugs of this (depressant) kind than introverts. Saviano et al. (1971) have similarly reported that heroin addicts are not distinguished on the basis of E. Perhaps the extravert tendency to seek new experiences is offset by the arousal-reducing effect of these drugs – which they have no need of. The connection with P might reflect sensation-seeking or the fact that these behaviours are illegal.

In a study of drug-dependent patients at the Maudsley Hospital, Gossop (1978) found greatly elevated scores on P and N and a significant degree of introversion. When oral users were compared with intravenous drug takers, the former group was especially high on P and N, which seemed to be contrary to some previous research showing intravenous addicts to be more hostile and dominant than oral users. Gossop resolved this by noting that previous studies were on detoxified patients, while in the current study the patients were still using drugs. He supposed that since the intravenous users were nearly all on narcotics (notably heroin), the effect of the drug might be to suppress their anxiety and hostility to some extent and thus lower their N and P scores (at least relative to the oral users, many of whom were using other drugs, such as amphetamine, which could well have the reverse effect). Gossop attributes the low E scores of the two drug-dependent groups to their lack of sociability and deficient social skills, but he admits the need for longitudinal studies to really sort out the problem of cause and effect with respect to the relation between drug addiction and personality development.

Elevated neuroticism scores have also been found in alcoholics, and in this case there is some evidence that the drinking causes anxiety rather than the other way about. Longitudinal studies (e.g. Kammeier et al. 1973) usually show attributes such as impulsivity and hostility as predisposing to alcoholism rather than neuroticism, and Orford (1976) reports that when alcoholics are successfully treated, their N scores are reduced to normal, along with their drinking behaviour. It was unfortunate that Orford's study did not include a measure of P, since there is some reason to expect high P scores might have been maintained despite cure (excessive drinking is a form of anti-social behaviour). Orford's group of alcoholic men were not distinguished from norms on the basis of extraver-

sion, though their wives were slightly introvert – perhaps because they avoided social situations for fear that their husband would disgrace them.

There is now some evidence that alcoholics may fit either into the dysthymic or the hysteric ('secondary psychopath') quadrants of the EPI, with the result that the overall correlation between E and alcoholism is largely cancelled. Early expectations that alcoholics would be extraverted were probably based on observations that behaviour becomes more extravert when the individual is given alcohol. But then, it can be predicted from arousal theory that the person whose behaviour is already extraverted would have less need of alcohol. He may occasionally get drunk to enjoy the novelty of the experience, but would not need continuous medication from alcohol. The dysthymic, on the other hand, might use alcohol as a method of reducing anxiety. For these reasons, the relationship between E and alcohol use, as reported in the literature, has been very ambiguous. N and P, however, are much more consistently associated with alcoholism, as with the use of other drugs of addition.

7.21 Crime and Delinquency

Eysenck's first theory of criminality focussed on the NE quadrant of the two-factor system. But as soon as the P dimension was added, it became clear that this too was associated with criminality. In fact, the P factor was developed partly to account for the finding of an overlap, descriptively and genetically, in the behaviour of psychotic patients and criminals. The bulk of evidence does support the revised 'three-factor' theory of the personality predisposition to criminality, although of the three, E is the least strongly and reliably implicated (Eysenck and Eysenck 1970; Wilson and MacLean 1974; Eysenck and Eysenck 1977; Eysenck 1977). A more detailed analysis of the particular items which differentiate criminals from non-criminals confirms the suspicion that insofar as E is associated with criminality, it is the impulsivity aspect, rather than sociability, which is responsible for this connection (Eysenck and Eysenck 1971). This item analysis enabled the Eysencks to produce a 'criminal propensity' (C) scale according to the method of empirical keying used to develop the MMPI. All items which significantly differentiated the criterion group of criminals from controls were included in the C scale and its validity cross-checked by reference to other samples.

More recent work on personality and crime has been concerned with subcategories of criminals. Thus Eysenck et al. (1977) investigated the personality profiles of five types of criminal: (1) violent, (2) thieves, (3) confidence men, (4) inadequates and (5) residual (mixed). The way in which these types of criminals were characterized in terms of personality is illustrated in Fig. 7.5. P separates the confidence men from all other groups – the conmen having low P scores (as well as high E and low N scores). The next division is in terms of N, with the 'residual' and 'inadequate' types having higher N scores than those whose crimes concerned violence and property. These groups are themselves divided on the basis of E, with the residual and violent types having higher E scores than the inadequate and property types. Once again, it is the P and N dimensions which show the most powerful associations. In fact, the differences on E failed to reach statistical significance at any point. The group which stands out by itself as very different from the other types of criminal is that of the confidence men, but since the authors do not report L scores for this group, one wonders if their low P and N scores are themselves a kind of fraud.

In any case, the above results support the idea that criminals in general, and different types of criminal, can be differentiated on the basis of personality scores. To what extent these personality scores are influenced by the criminal's environmental situation, for example, the fact that very often they are tested after incarceration, is an open question, but there is little doubt that some biological factors are involved. Woodman et al. (1977a, b) have demonstrated a high ratio of noradrenalin to adrenalin in the urine and blood of violent prisoners, compared with that of men convicted of sexual and property offences. This is in line with a consid-

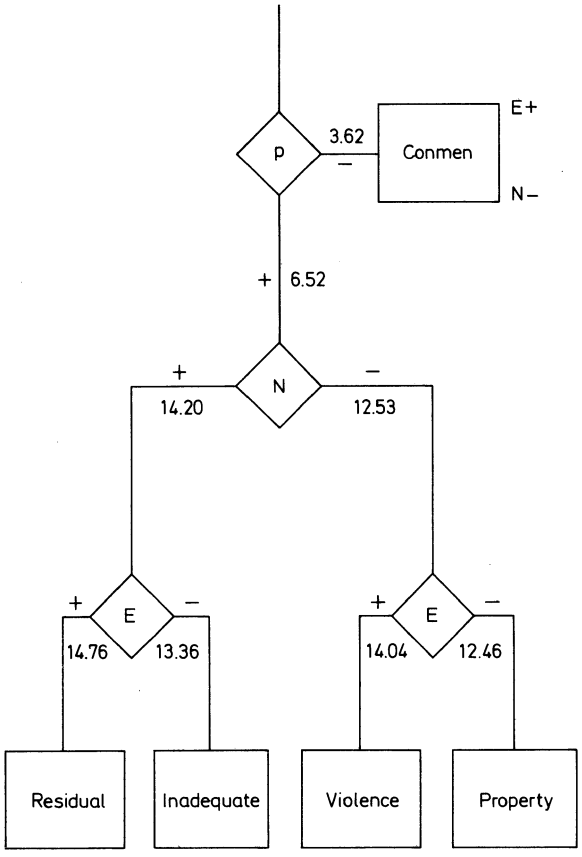

Fig. 7.5. Segregation of criminal groups according to personality scores. (After Eysenck et al. 1977)

erable amount of evidence associating anxiety with adrenalin secretion and aggression with noradrenalin. Other evidence supporting a biological factor is that showing a genetic link between schizophrenia and criminality, which has already been mentioned as one of the rationales underlying the development of the P scale in the EPQ (see studies discussed by Kirkegaard-Sorenson and Mednick 1975).

Eysenck's early theory that criminality and deviance could be attributed to a lack of conditionability in extraverts, with a resulting difficulty in acquiring social rules, has not found very strong support. For a start, sociability does not relate to criminality, only the impulsivity component, so that overall correlations with E are not very strong. Secondly, early expectations that criminals and psychopaths would show a general inability to profit from experience have not been confirmed. Particularly where positive reward is used for reinforcement rather than the threat of punishment, criminals and psychopaths have shown equal if not superior conditioning. However, learning in psychopaths and drug addicts does seem to be impaired if there is a considerable delay of reinforcement (Gullick et al. 1976), which rather suggests that impatience, the inability to tolerate a delay in gratification, is what characterizes the psychopathic type of person. Such a conclusion makes criminality readily interpretable in terms of Gray's (1972) rotation of the Eysenck's E and N dimensions, which identifies anxiety and impulsiveness as primary personality factors. The criminal type would, or course, correspond to the individual who is high on impulsiveness.

Another reason to doubt that criminality can be attributed to failure to acquire social rules is that prisoners come out as fairly conservative in their attitudes by comparison with appropriate control groups – possibly more conserva-

tive than the police (Wilson and Maclean 1974; Siddiqui et al. 1973). And when sex criminals are compared with other types of criminal they do not appear to be more liberated in their attitudes towards sex, rather they are sexually maladjusted and afraid of being unable to control their sex drive (Howells and Wright 1978). Of course, attitudes of this kind may be more cognitive than emotional and therefore are not identical with a Pavlovian-level conscience.

The three-factor theory has also been applied to juvenile delinquency. Allsopp and Feldman (1976) administered an anti-social behaviour questionnaire to around 300 teenage boys and also examined their record of punishment for infringements in the school. Results showed that children who had engaged in anti-social behaviour were higher in psychoticism, neuroticism and extraversion than well-behaved boys. However, the Lie Scale was an even more reliable discriminator of delinquent from non-delinquent children. Boys who produced low-L scores were more ready to admit to anti-social acts (and it appears were more likely to have been punished for them). Since lying is itself an anti-social act, it seems that in this case the L scale is acting more as a measure of good behaviour than faking good. These authors suggest that the failure of E to relate to adult criminality might be due to the effects of incarceration, which render many of the sociability items in the E scale inappropriate, even ludicrous, given the prisoner's situation. On the other hand, the relatively minor acts of delinquency, such as swearing, buying cigarettes and letting off fireworks in the street examined in the Allsopp and Feldman survey appear to be much less maladjusted forms of behaviour than full-blown adult criminality, and this non-equivalence of the two types of offence may be the main reason why E correlates with the one and not the other.

Somewhat contradictory results have been reported with New Zealand schoolboys by Saklofske (1977). He took criterion groups of 10- and 11-year old boys whose school conduct was regarded either as decisively good or problematic by their teachers. In this case, the badly behaved boys were significantly higher on P and lower on L (consistent with Allsopp and Feldman), but there was no difference on N and they were significantly *lower* on E. It appeared that at this young age at least, introverted children were more likely to show disrespect and defiance of their teachers and involve themselves in classroom disturbances.

Similarly, in a study of delinquent boys in Scotland, Forrest (1978 unpublished work) found that P was the only EPQ scale to distinguish delinquents from non-delinquents, although certain N scale items relating to mood variation and depression were significant discriminators when taken individually. With respect to the E scale, the delinquents were slightly higher than controls with respect to both sociability and impulsivity. However, those who had been committed to borstal or prison more than once (recidivists) were relatively introverted and particularly low with respect to sociability. This suggests either that they were more maladjusted and potentially hard-core in their criminality than other delinquents or that the experience of custody had the effect of reducing their sociability. In support of the latter idea, Heskin et al. (1973) have shown that long-term prisoners are significantly more introverted than short-term prisoners. (Recall also that long-term hospitalization is associated with introversion).

Other forms of social deviance that have been studied in school-age children are bullying, which appears to be associated with high N and unrelated to E (Lowenstein 1978) and school refusal/truancy, where N was found to be unrelated but the refusers were significantly more introverted than controls (Smith 1974). Neither of these studies can be said to support the idea that E is associated with childhood delinquency. It is a pity that neither included a measure of P.

As with adult criminality, it is bound to be profitable to divide delinquents into various subgroups, based on the nature, seriousness and motivation of their misdemeanours. One such classification that is gaining some currency categorizes delinquents into three types, as follows: (a) *psychopathic delinquents*, who are socially unresponsive and lacking in remorse, (b) *neurotic delinquents*, who suffer feelings of inferiority, guilt, anxiety and depression and who appear

to act in response to inner conflicts and (3) *subcultural delinquents*, who appear to be socialized into delinquent behaviour by their peers. These groups have been shown to be differentiated in terms of moral reasoning, cognitive development and social responsiveness (Jurkovic and Prentice 1977). Although they have not been tested for personality differences, it is reasonable to suppose that they might be characterized by high P, N and E respectively, thus perhaps resolving some of the discrepancies in the literature reported above.

7.22 Cross-National Differences

Can the Eysenck personality dimensions be extended to the analysis of 'national character'? The first requirement would be that the EPQ produce a similar factor structure from one culture to another. That it indeed performs very well in this respect has been shown Eysenck and Eysenck (1980), who present coefficients of factor similarity between a considerable variety of European and non-European countries (Table 7.4). These coefficients can be judged to be highly satisfactory if above 0.95 and reasonably healthy still if they exceed 0.90. Only in a few rare cases, such as the P scale in Nigerian versus English women, is there any reason to doubt that the scales are directly comparable from country to country. Such findings give confidence that EPQ scores generated in different cultural contexts can be meaningfully compared.

Lynn and Hampson (1975) investigated cross-national differences in personality, based on demographic and epidemiological data. The variables used were national rates of divorce, illegitimacy, accidents, crime, murder, suicide, alcoholism, chronic psychosis, coronary heart disease and the per capita consumption of calories, cigarettes and caffeine. The theoretical and empirical relationship of these variables to extraversion and neuroticism was used to set up a model for their relationship among nations. For example, cigarette smoking and divorce were presumed to be indices of extraversion; suicide and alcoholism were expected to fall on an axis of neuroticism, and accidents, crime, murder and

Table 7.4. Coefficient of factor similarity from one country to another for EPQ scales. (After Eysenck and Eysenck 1980)

	P	E	N	L
English standardization males v. English quota-sample males:	0.99	1.00	1.00	1.00
English standardization females v. English quota-sample females:	0.99	1.00	1.00	1.00
English males v. Yugoslav males:	0.97	0.97	1.00	0.98
English females v. Yugoslav females:	0.97	0.99	1.00	0.99
English males v. French males:	0.98	1.00	1.00	1.00
English females v. French females:	0.97	1.00	1.00	0.98
English males v. Indian males:	0.97	0.99	0.99	0.96
English females v. Indian females:	0.95	0.99	0.99	0.98
English males v. Greek males:	0.94	0.99	0.98	0.98
English females v. Greek females:	0.89	1.00	0.96	1.00
English males v. Nigerian males:	0.98	0.99	0.99	0.98
English females v. Nigerian females:	0.66	0.91	0.92	0.93
English males v. Portuguese males:	1.00	0.99	1.00	1.00
English females v. Portuguese females:	0.99	0.98	1.00	0.99
English males v. Australian males:	0.93	1.00	0.99	0.99
English females v. Australian females:	1.00	1.00	0.99	0.99
English schoolboys v. Japanese boys:	0.96	0.99	0.99	0.89
English schoolgirls v. Japanese girls:	0.97	1.00	0.94	0.98
English boys v. New Zealand boys:	0.99	1.00	1.00	1.00
English girls v. New Zealand girls:	0.99	0.98	1.00	1.00
English boys v. Spanish boys:	0.97	0.98	0.99	0.99
English girls v. Spanish girls:	0.92	0.98	0.99	0.99

illegitimacy were expected to load on both extraversion and neuroticism. This model was supported by principal components analysis of the relationships among these variables at the international level. Factor scores were then computed for the industrialized Western nations (see Fig. 7.6). The United States appears to be the most extraverted nation and Japan, the most introverted.

These findings concerning the relative placement of nations with respect to personality variables are fairly well supported by international EPQ norms. Thus Americans are significantly more extraverted than English people (Eysenck and Eysenck 1971), and the Japanese more introverted and neurotic (Iwawaki et al. 1977). The latter study also showed the Japanese as significantly higher on P than the English. (P was unfortunately omitted from the Lynn and Hampson study). A study by Vaughan and Cattell (1976), using the 16PF test, revealed the Americans as more extraverted than New Zealanders, which is also consistent with the Lynn and Hampson scheme.

It must be said, however, that the placement of some of these nations does not accord with widely accepted stereotypes. Italians and Australians are widely regarded as extraverted people by comparison with, say, Swedes and Germans, but this differentiation does not appear in Fig. 7.6. Yet, since empirical comparisons, using a test such as the EPQ, do not seem to have been made with respect to these countries, it may turn out that the stereotypes are incorrect rather than Lynn and Hampson's classification.

To what extent the differences between nations reflect constitutional character traits (e.g. those arising from selective migration) and to what extent they are due to cultural learning experiences is not known, but it is reasonable to suppose that both factors are involved. The observation of Iwawaki et al. that Japanese children start off as extraverted and become progressively introverted as they grow older sug-

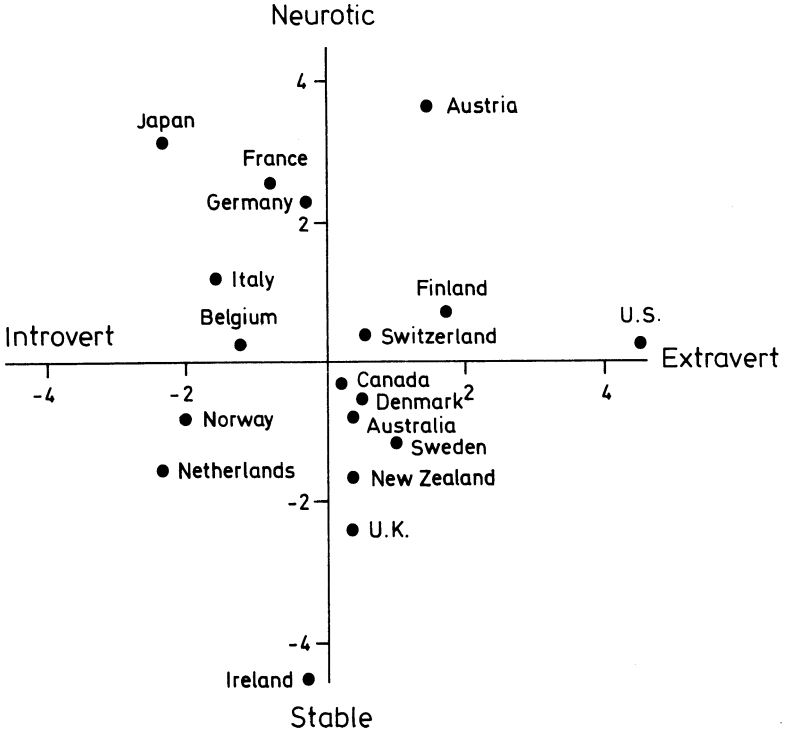

Fig. 7.6. The positions of Western nations in relation to dimensions of neuroticism and introversion–extraversion determined by analysis of demographic and epidemiological data. (After Lynn and Hampson 1975)

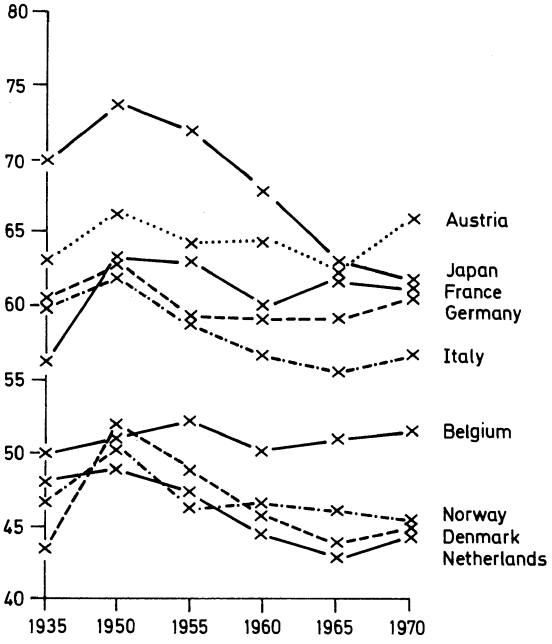

Fig. 7.7. Neuroticism levels in the nations suffering military defeat in the Second World War. (After Lynn and Hampson 1977)

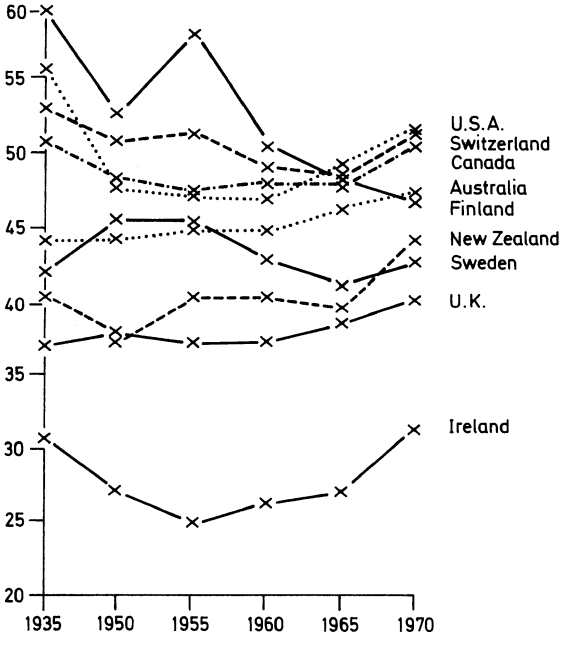

Fig. 7.8. Neuroticism levels in the nations which escaped military defeat in the Second World War. (After Lynn and Hampson 1977)

gests a cultural influence, although the same trend might also be noted in extravert countries like the United States. We also have to consider the possibility that P scores are elevated in a culture as different as that of Japan because the items in the P scale cease to be appropriate indicators of psychotism. The elevated P scores found in Nigeria (Eysenck et al. 1977) and Turkey (Irfani 1977) would seem to support such a notion, since normal English subjects endorse very few P-scale items, and any tendency towards randomness resulting from confusion in

the interpretation of items would lead to spuriously high P scores in culturally distant groups.

More recently, Lynn and Hampson (1977) have extended their work on cross-national differences in personality in a very interesting way. They analysed trends in national levels of extraversion and neuroticism over the years 1935–1970. The most interesting finding is that depicted in Figs. 7.7 and 7.8. Nations which suffered defeat in the Second World War showed an increase in N in the 15 years that followed, whereas triumphant or neutral countries tended to show a decrease. Lynn and Hampson interpret this result as reflecting the stress that is induced in a nation by defeat and occupation and argue that it lends validity to their initial analysis.

As regards extraversion, Lynn and Hampson found that E was showing a fairly steady rise in nearly all nations between 1935 and 1970. A closer analysis revealed correlations between the population's mean level of extraversion and per capita income, both within nations over time and between nations at various points in time. Apparently, then, Lynn's measure of national E is in some way related to affluence. Looking at the indices used to measure national E (e.g. cigarette consumption and divorce), this association with affluence is not surprising, and then nor is it surprising that the United States scores highest on E among Western nations.

Thus it seems that the Lynn and Hampson measure of national E is of questionable validity – it may be more of a measure of affluence. However, their measure of N is more likely to prove valid and has produced some very interesting results. Perhaps their future work might utilize indices such as crime and drug addiction to construct a measure of national P.

7.23 Conclusions

This completes the summary of the way in which personality has been shown to be connected with various forms of social behaviour. Clearly, the dimensions of E, N and P, as measured by the Eysenck Personality Questionnaire, have wide explanatory and predictive power across a variety of socially important domains. The suggestion that has sometimes been made, to the effect that there are no stable traits which enable us usefully to predict social behaviour, is thus shown to be untenable. Although learning experiences and transient environmental circumstances do have to be considered in predicting what a person will do in a particular situation, so too must their personality be taken into account, or the formula is bound to be incomplete.

The next question that is sometimes raised concerns the suitability of the three-dimensional model of personality. Some theorists are inclined to protest that three is an insufficient number of attributes with which to classify the various temperaments of human beings. The answer is that reality is nearly always more complicated than the scientific model that is devised to describe and account for it. But if a model becomes too complicated in itself, it is likely to lose its utility; scientific theories are 'convenient fictions' which provide useful simplifications of the phenomena with which they are concerned. They are seldom, if ever, true in any absolute sense. For some of the studies that were reviewed the three main dimensions of E, N and P were, in fact, broken down into components such as the impulsiveness and sociability aspects of extraversion, and some insights were thus gained, but for the most part measures of primary-order traits such as these lack clear separability when factor-analysed. This is certainly true of Cattell's 16 personality factors. Because Eysenck's three factors are fairly independent, the projections on them will allow for a great deal of individual variation, rather in the way that scores on hue, saturation and brightness will specify all the different colours we can experience.

The other complication that can be, and has been, introduced to the three-dimensional model (in case people feel that it should be more complex) is the study of interactions among the three factors. In the studies that have been reviewed above, several examples of 'quadrant analysis' were given in which it was possible to examine the interaction between E and N

in determining social behaviour of some kind. Very little in the way of interactions of P with either E or N have been reported, however, and we might ask why. This could be viewed as raising the likelihood that Gray's 45-degree rotation of the E and N factors (into the positions corresponding to anxiety and impulsiveness) is an equally good, or even superior, alternative. On the other hand, it may simply reflect the fact that there has been less research on the P dimension because it is of more recent origin, and therefore there has been less opportunity to discover interactions with other dimensions. In any case, some of the impulsiveness components in the old extraversion scale (particularly items concerned with sensation-seeking) have gone across into the new P scale in the three-dimensional system, leaving sociability as a more central of extraversion, and so some of the previously adduced interactions may have gone too (or disappeared altogether).

The kind of social behaviour exhibited by people identified according to their position on the three major dimensions is very much what would be expected on the basis of the description of these types. In that sense, much of the research that has been reviewed amounts to a construct validation of the Eysenck personality scales. Extraverts are lively, outgoing, sociable, sporty and adventurous in their general social behaviour; introverts, by contrast, are careful, controlled, quiet and withdrawn. High N scorers are likely to suffer from social anxiety as much as any other kind of anxiety, and their suffering may include sexual difficulties. Also, their attempts at self-medication sometimes bring them into contravention of drugs laws, which is then classified as a social problem. Their high anxiety may also interfere with their academic studies and work performance, particularly at exacting and stressful levels of requirement. Low N scorers are relatively well off in all these respects, although in some contexts they may appear as unmotivated, complacent and even dull (e.g. in artistic and creative circles they may actually be at a disadvantage). The high P scorer is given to sensation-seeking, risky, ambitious and frequently antisocial or bizarre forms of behaviour, with the result that he is often in trouble of one sort or another. Criminality, delinquency, drug-taking and mental disorder (of the psychotic rather than neurotic kind) are among the possible outcomes of a high P score. On the merit side, the high P scorer is more likely to be creative and to get things done, where the low P scorer might appear timid and procrastinating. The high P individual may thus excel in certain areas of art, science, business and politics. Military leaders and heroes might also have high P scores – though this idea does not appear to have been tested.

Overall, it seems advantageous to have low N and P and probably a high level of E. The latter might seem surprising, in view of the fact that the E scale was designed as independent of the two main categories of psychopathology – neuroticism and psychoticism. Nevertheless, the evidence outlined above is fairly unanimous in pointing to the conclusion that extraversion is associated with optimal adjustment, rather than introversion. Extraverts score higher on questionnaire measures of positive mental health, attempt suicide less often and are more popular with their peers. While many introverts would like to be extraverted, most extraverts are happy the way they are. Furthermore, when they do have a mental breakdown, the extraverts are more responsive to treatment and spend less time in mental hospitals. Finally, regardless of a patient's initial personality, improvement with therapy is associated with an increase in their E score as well as a decrease in N. Generally, then, it is better to be extraverted than introverted. There are, however, two reservations to this: First, extraversion seems to be a more highly valued attribute in certain cultures, e.g. the United States and Australia. In countries such as Britain and Japan, where extraversion is less highly valued, the correlation of E with mental health and success in life is apparently less striking. Second, a case could be made that while extraversion is good for the extraverted individual himself, it is not necessarily quite so good for those who have to endure his behaviour. Although usually quite happy in themselves, some of the habits of extraverts (e.g. their noisiness and irresponsibility), which are no doubt designed to maintain their level of arousal, can become quite irritating to other people (particularly the

introverts, who would prefer a solitary, quiet existence).

The same is true of the high P scorer, only more so. These individuals are sometimes capable of wreaking destruction on the lives and property of those around them, without necessarily upsetting themselves at all in the process. The ultimate in this respect is, of course, the psychopath, who is apparently totally immune to the feelings of other people. In his case, he may only regret his actions when he has been deprived of freedom by judicial process; even then, it is probably only getting caught that he regrets, not the misery and trouble he has caused to other people. The neurotic, on the other hand, usually causes more misery to himself than to other people.

Nevertheless, as we have already indicated, it would be a mistake to regard any of these three dimensions as having one unequivocably 'bad' end. Evolution has almost certainly dictated wide variation along them, because people at each end have their contribution to make to the survival of the species and society. Such an idea is supported by the absence of genetic dominance attached to either end of the P, E and N dimensions (see Chap. 4 by D. Fulker). Sometimes fear is highly adaptive (and thus high N), sometimes it is necessary to be brave and adventurous (high P). Stability in both these senses is equally important in other contexts. Some people function best when alone (introverts); others have developed their cooperative skills (extraverts). This very diversity of human society may be one of the keys to the outstanding success of our species on Earth.

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Chapter 8

A Critique of Eysenck's Theory of Personality

J.A. Gray

8.1 Introduction

The territory that psychologists explore is still largely uncharted; so to find Eysenck's model for personality in the middle of this *terra incognita* is rather like stumbling across St. Pancras Station in the heart of the African jungle. Faced with this apparition, one's first question is, not "does it work?", but "what's it for?" This, indeed, is the right question to ask. Eysenck's model bestrides the field of personality like a colossus. There have been other attempts to *describe* personality, notably Cattell's and Guilford's, and other attempts to *explain* it, above all, Pavlov's and Teplov's: but no one has tried to achieve both these aims on the same scale as Eysenck. In consequence, it is extremely difficult to see the Eysenckian edifice in perspective: there are too few other buildings with which to compare it, only the surrounding trackless jungle. It is by asking "what's it for?" that we can best provide this perspective. In answer to this question, Fig. 8.1 displays what I take to be the general structure of Eysenck's theory of extraversion–introversion (E–I) and neuroticism (N).

As shown, the aim of the theory is to explain why it is that some people (but not others) manifest certain kinds of behaviour ('socio-psychiatric' in Fig. 8.1) in the real world.

One important kind of behaviour in this respect is called by Eysenck 'dysthymic', that is, a cluster of psychiatric symptoms which appear to have in common the emotion of anxiety (phobias, obsessional-compusive rituals, anxiety state) and that part of the emotion of depression ('neurotic depression') which is hardest to differentiate from anxiety, if it is possible to do so at all (Frith et al. 1979). This kind of behaviour is the prerogative of individuals simultaneously high on N and low on E ('neurotic introverts'), and it has been a stable component of the problem to which Eysenck's successive models have been addressed.

The other important kind of behaviour to which Eysenck's theory of E–I and N has been addressed has been a more shifting target. In the first full-scale model (Eysenck 1957) it was seen principally as hysteria, i.e. the classic (but rarely encountered) paralytic or anaesthetic symptoms of conversion hysteria. But it was then found that individuals manifesting these symptoms were not, as Eysenck's (1957) model supposed them to be, neurotic extraverts but rather neurotic ambiverts (Sigal et al. 1958). Eysenck's emphasis subsequently shifted (e.g. Eysenck 1964a) to criminal or 'psychopathic' behaviour (though this was already included in the 1957 theory), i.e. anti-social acts ranging in seriousness from careless driving to brutal murder. Such 'psychopathic' individuals were also supposed to be neurotic extraverts. Subsequent research, however, has yielded a more complex picture: (1) they are frequently (Passingham 1972) but not always (e.g. Hare and Cox 1978) high on N; (2) usually not (Passingham 1972) but sometimes (e.g. Shapland and Rushton 1975) high on E; and (3) sometimes, even when not high on E measured alone, jointly high on N and E (Burgess 1972). This is reasonably satisfactory for Eysenck's theory, since point (3) can plug the hole opened up by point (2). But major stresses are posed for the theory by the further discovery (Eysenck and Eysenck 1976) that such individuals are also usually high scorers on the third of Eysenck's orthogonal dimensions of personality, psychoticism (Eysenck and Eysenck 1976), or P. We shall consider these stresses later.

The overriding questions to which Eysenck's theory is directed, then, are these:

(Q. 1) What makes some individuals dysthymic (where this is short-hand for 'susceptible to the range of dysthymic symptoms')?

(Q. 2a) What makes some individuals hysteric (i.e. susceptible to the symptoms of conversion hysteria)?

(Q. 2b) What makes some individuals psychopathic (i.e. prone to the commission of a great variety of anti-social acts)?

There is a further Eysenckian question which is not directly within the scope of this chapter, but which the empirical relations between psychopathy and psychoticism will occasionally make relevant:

(Q. 3) What makes some individuals psychotic (i.e. susceptible to psychotic symptoms as found in schizophrenia or endogenous depression)?

But if these are the questions which the theory is intended to answer, we have already seen, in the shift from Q. 2a to Q. 2b, that they are not totally independent of the answers given. This is not in itself a criticism: it is in the nature of scientific advance that the development of theory and data alters the form of the questions we ask. But it does place a further obstacle in the way of clear vision of the Eysenckian edifice. As the Chapter proceeds we shall see further examples of this kind of (probably inevitable) interaction between the different levels of theory construction which Fig. 8.1 presents as separate.

Among these various levels the most important links in the chain are: (a) the link between the socio-psychiatric level and the personality level (i.e. between levels 1 and 2 in Fig. 8.1); and (b) the link between the personality level and the explanatory mechanisms (taken from learning theory and physiological psychology) used to account for personality (i.e. between level 2 and levels 3–6 combined). For (*a*), personality is an explanatory concept, for (*b*) it is an explicandum. At first sight, the mode of explanation used is radically different in the two cases. Indeed, it is not clear that one would wish to talk about (*a*) as explanatory at all, since it seems a simple matter of description: all one has to do is to locate dysthymics, hysterics, psychopaths and psychotics in the personality space defined by N, E–I and P (using questionnaires, laboratory tests or whatever other means). However, matters are not so simple, as the following discussion will show.

8.2 Personality Description

The principal tool used to establish Eysenck's personality dimensions has been that of factor analysis (e.g. Eysenck and Eysenck 1969, 1976). Although it is an immensely valuble and powerful tool, it is essential to understand the limitations which hedge the conclusions one may draw from it. Factor analysis can establish the *minimum* number of independent dimensions (and, inferentially, the minimum number of independent causal mechanisms) needed to account for the data. This is not trivial. For example, the demonstration that tests differentiating groups of neurotics and psychotics from a group of normals require at least two independent dimensions for their description is successfully used by Eysenck (1960) to show the inadequacy of the Freudian theory of neurosis and psychosis. But factor analysis cannot determine whether the minimum number of independent dimensions is a better description of underlying causal mechanisms than is a larger (sometimes much larger) number of separate but correlated factors.

This is the well-known problem of choosing between a description in terms of a large number of primary, oblique factors, as practised for example by Cattell, and a small number of higher-order orthogonal factors or dimensions, as preferred by Eysenck. It is known that the two types of description are mathematically interchangeable, as confirmed empirically by Eysenck and Eysenck (1969); and it is also clear that there are no mathematical grounds for choosing between them. This has the important implication that the choice of factor-analytic level at which to work (primary or higher-order) is not just a matter of description, it is part of theory-building. If, as Eysenck does, one then goes on to seek causal explanations of the fac-

A. Eysenck's 1957 Theory

Level	Phenomenon	
Socio-Psychiatric	1. Type of disorder	Introverts: dysthymia Extraverts: hysteria
		↑
Personality	2. Socialization	in Introverts: good in Extraverts: poor
		↑
Learning/Emotion	3. Conditioning of fear	in Introverts: good in Extraverts: poor
		↑
	4. Conditionability	in Introverts: good in Extraverts: poor
		↑
Physiology	5. Excitation-inhibition balance	Introverts: excitable Extraverts: inhibitable

B. Eysenck's 1967 Theory

Level	Phenomenon	
Socio-Psychiatric	1. Type of disorder	Introverts: dysthymia Extraverts: psychopathy
		↑
Personality	2. Socialization	in Introverts: good in Extraverts: poor
		↑
Learning/Emotion	3. Conditioning of fear	in Introverts: good in Extraverts: poor
		↑
	4. Conditionability	in Introverts: good in Extraverts: poor
		↑
Physiology	5. Arousability	in Introverts: high in Extraverts: low
		↑
	6. Neural substrate	ARAS: in Introverts: high activity; in Extraverts: low activity

Fig. 8.1. The structure of Eysenck's 1957(A) and 1967(B) and Gray's 1970a(C) theories of extraversion–introversion (E–I). These three theories all apply to individuals high on the neuroticism (N) factor. Panels D and E show Gray's theory after rotation of the Eysenckian dimensions of E–I and N by 45° to produce two new dimensions of anxiety (D) and impulsivity (E), as shown in Fig. 8.2. Two alternative theories of impulsivity are distinguished as 3a and 3b in panel E. ARAS: ascending reticular activating system. The feedback loop mentioned at 6 in panel C is shown in Fig. 8.4; part of the system mentioned at 4 in panel D is shown in Fig. 8.3

C. Gray's 1970a Theory of Extraversion

Level	Phenomenon	
Socio-Psychiatric	1. Type of disorder	Introverts: dysthymia Extraverts: psychopathy
Personality	2. Socialization	in Introverts: good in Extraverts: poor
Learning/Emotion	3. Conditioning of fear	in Introverts: good in Extraverts: poor
Learning/Emotion	4. Susceptibility to threat	in Introverts: high in Extraverts: low
Physiology	5. Arousability	in Introverts: high in Extraverts: low
Physiology	6. Neural substrate	Feedback loop comprising ARAS, frontal cortex, septal area and hippocampus — in Introverts: high activity; in Extraverts: low activity

D. Gray's Theory of Anxiety after Axis Rotation

Level	Phenomenon	
Socio-Psychiatric	1. Type of disorder	Dysthymia
Personality	2. Type of individual	High trait Anxiety
Learning/Emotion	3. Susceptibility to threat of punishment	High
Physiology	4. Neural substrate	Septo-hippocampal system, its monoamine afferents, and frontal cortex

E. Gray's Theory of Impulsivity after Axis Rotation

Level	Phenomenon	
Socio-Psychiatric	1. Type of disorder	Psychopathy
Personality	2. Type of individual	High impulsivity
Learning/Emotion	3a. Susceptibility to promise of reward	High
	3b. Susceptibility to aggressive behaviour	

tors so chosen (link b, above), it is a commitment to the view that the important causal influences work along *these* axes rather than others.

This, of course, is a commitment that one has at some time to make. One cannot search for explanation simultaneously for an indefinite range of phenomena. But it is important to recognize that, in choosing one particular factor-analytic description of the data as the explicandum for link *b* in the complete explanatory chain, one is playing a hunch, no matter how well that hunch is backed up by arguments (e.g. Eysenck this volume) concerning the simplicity and replicability of the description used or the pragmatics of tackling the biggest structures in the data first. Nor is it clear that this particular choice has to be made at all; for it is not necessarily the case that causal influences will be at work at one level of the factor-analytic description but not at others. Indeed, there is already good evidence to the contrary. The clearest case comes, not from the study of personality, but from the ability domain, where, in a biometrical genetic twin study, Martin and Eaves (1977) have demonstrated the simultaneous and independent operation of additive genetic influences in the determination of the general factor of intelligence (*g*) and several of Thurstone's Primary Mental Abilities. There is also evidence, albeit more indirect, that the same sort of thing can occur in the personality domain. Thus Eysenck and Eysenck (1969), using Corcoran's 'lemon-juice test' (the salivary response to lemon juice), have shown in an elegant experiment that this relates more strongly to the higher-order factor of E–I than to either of its principal subfactors, impulsivity (Imp) or sociability (Soc). But two other, equally biological, tests have shown better correlations with Imp than with E–I: eye-blink conditioning (Eysenck and Levey 1972) and the response to caffeine (Revelle et al. 1980). It is probable, therefore, that in personality as in abilities, biological causation works at *both* the dimensional *and* lower-order factorial levels, of which there may be, of course, many more than one (Eysenck and Eysenck 1969).

This is not the only obstacle in the way of a rational choice of the type of factorial description with which to work. There is also the problem of rotation. Despite Cattell's (e.g. 1965) strenuous arguments in favour of the rules of simple structure, there appear to be no non-arbitrary mathematical criteria for deciding where to rotate one's factors, at whatever level (higher- or lower-order) one chooses to work. Once again, therefore, the decision where to place factors or dimensions in the space that they define is a *theoretical* one: it is to play a hunch that it is here, not there, that the causal influences will be found.

Worse still, the problems of level and rotation are themselves interconnected. Suppose one finds, as did Eysenck and Levey (1972) and Revelle et al. (1980) in the studies cited above, that an experimental measure relates better to Imp than to E–I. This could imply, as I took it to do above, that the line of causal influence is at the subfactor level occupied by Imp. But, in the two-dimensional space defined by E–I and N, questionnaire items which are highly loaded on Imp are positively correlated with N as well as with E. Therefore, if one stayed at the dimensional level, but rotated Eysenck's two factors by about 45° (as proposed on independent grounds by Gray 1970a), one of the resulting dimensions would correspond so closely to the Imp sub-factor that it would be difficult, if not impossible, to decide whether this subfactor or the newly rotated 'impulsivity dimension' (Imp-D) is the best correlate of eye-blink conditioning or the response to caffeine.

There is still a third way (besides the choice of level of factoring and rotation) in which the factor-analytic description of personality is itself a matter of theory construction. Suppose we wish to ask the question, do men and women differ in overall intelligence? On the face of it, this is a simple empirical question to which there should be an easy answer: all we need do is measure *g* in a sufficiently large random sample of the two sexes. If we do the experiment, we shall find that the sexes do not differ in *g* (provided we use a white population of the kind that the intelligence tests were standardized on). But this result will tell us very little about underlying differences between the sexes; for it is a straightforward product of the

theoretical biases of the psychologists who constructed the intelligence tests. They believed that men and women are of equal intelligence. They therefore constructed their tests so as to reveal this fact. This was not difficult to do, since the *pattern* of intelligence does differ between the sexes (Buffery and Gray 1972): women score relatively higher than men on tests of verbal ability, men relatively higher than women on tests of visuo-spatial ability. Thus, by judicious selection of the right mix of items or tests of the two kinds one can obtain any result one wants when it comes to comparisons of *g* between the sexes.

Something very similar has been at work in the construction of questionnaires to measure E–I and N. As we have seen, E–I is composed of two principal subfactors, Imp and Soc. Imp correlates positively with N, Soc negatively. One can therefore vary the correlation of E–I with N by altering the mix of Imp and Soc items in the questionnaires that measure E–I (see Eysenck's discussion in Chap. 1 of this volume). Over the years, the changes that have taken place in these questionnaires have led to the gradual disappearance of the small (around 0.2–0.3) but consistent negative correlations that used to turn up between E and N in the Eysenck Personality Inventory (EPI), until these two dimensions are now very close indeed to orthogonality in the Eysenck Personality Questionnaire (EPQ).

Now there is nothing reprehensible in this process of change. In the absence of empirical or theoretical grounds to the contrary, it makes sense to build your measuring instruments so that they give results that are as simple as possible and in accord with theory. As Eysenck makes clear in Chap. 1 of this volume, these are familiar scientific principles, principles that underlie, for example, the development of the thermometer. Thus, if we have no grounds for believing that the sexes differ in overall intelligence, the simplest decision is to construct intelligence tests so that they do not differentiate between the sexes; and if we have a two-dimensional space in which to work, it is simplest to use two axes, such as E–I and N, that are at right angles to each other. But there is a corollary: if we have built sexual equality into our intelligence tests, we cannot use them to ask (empirically) whether the sexes differ; and if we have built orthogonality between E–I and N into our personality tests, we cannot use them to ask (empirically) whether the causal influences operative in this domain are independent of each other.

This kind of incestuous relationship between theory construction and questionnaire development continues to be influential in Eysenck's work. One point at which this will be important to our argument concerns the subfactor of impulsivity. Recent studies of items specially designed to measure this trait have revealed correlations with P, as well as with E and N, and the correlations with N have been variably positive and negative (Eysenck and Eysenck 1978; Zuckerman et al. 1978). In reaction to these empirical findings, together with other evidence that criminals and psychopaths score highly on P (Eysenck and Eysenck 1976), Eysenck (personal comment 1979) has begun to redefine the concept of impulsivity and, using factor-analytic methods, to relocate the scales used to measure this trait in the three-dimensional space whose axes are E, N and P. This new 'impulsivity' loads on E and P rather than on E and N. From one point of view, this makes good sense. By altering Imp in this manner one obtains a simpler description of the way in which psychopaths differ from the normal population (namely, they are high on Imp), and we have seen that this is one of the major questions to which Eysenck's work is directed. But the interrelations between the different parts of the Eysenckian edifice are too complex for such a move to be made without consequential changes elsewhere. For example, Imp as it was measured before these recent changes predicted the diurnal rhythm in response to caffeine described by Revelle et al. (1980), but after these changes it no longer does so (Revelle, personal comment 1979). Thus if Imp is moved from the E/N to the E/P octant of the three-dimensional space defined by E–I, N and P, we shall need a new term to designate the factor that runs from stable introvert to neurotic extravert and which predicts the diurnal rhythm in reactivity to caffeine (Revelle et al. 1980). We shall also need to find out whether eye-blink condi-

tioning (Eysenck and Levey 1972) is better predicted by new- or old-style Imp.

So far, then, we have identified three ways in which the description of personality is the construction of a theory about the organization of personality and about the relation of the questions Eysenck is asking (Questions 1, 2a, 2b and 3) to that organization of personality: the choice of factor-analytic level, of factor-analytic rotation and of items to be analysed. Not only do these choices determine the framework within which the Eysenckian questions are asked, they alter the nature of these questions. We have already seen one example of this. Having decided that the interesting theoretical objects were individuals at the extremes of the E–I dimension, and having discovered that hysterics were ambiverts, Eysenck stopped asking questions about conversion hysteria (Q. 2a) and started asking questions about psychopathy (Q. 2b). But, of course, he could have instead decided that the interesting theoretical objects were individuals high on N and not extreme on E–I and continued to investigate conversion hysteria. More recently, faced with a comparable dilemma, he has taken the opposite decision: having found that psychopaths are not the simple neurotic extraverts he had thought, he has continued to investigate them, while realigning the impulsivity scale so as to describe them more directly.

Before we pass on to consider, in the next section, Eysenck's attempt to construct biological explanations of the personality framework he has constructed, notice that the success or failure of such a biological explanation cannot in any straightforward manner resolve the issues identified in the present section. As we shall see, the method used to evaluate these explanations is, in general terms, to derive and test predictions as to the behaviour of introverts and extraverts (or high and low impulsives, etc.) in a laboratory situation. Suppose the prediction is verified. Provided the questionnaires on which the personality description is based do not contain questions asking about the laboratory behaviour investigated (e.g. amount of salivation to lemon juice, ease of eye-blink conditioning), this provides evidence that the factor-analytic description has indeed captured something that goes beyond itself. Furthermore, to the extent that the predictions made and successfully tested are clearly derived from a well-articulated theory and, in the absence of that theory, have an otherwise low a priori probability of being correct, an experiment of this kind provides evidence about the kind of underlying causal influences captured by the factor-analytic description. But, on its own, a demonstration of this kind cannot show which of several competing factor-analytic descriptions is to be preferred. This is because the correlations between factors at different levels (e.g. E–I and Imp) or between different rotations of a factor at the same level (e.g. E–I and Imp-D) or between factors made up of slightly different items (e.g. new- and old-style Imp) are usually sufficiently high for one to obtain the same basic pattern of results whichever description one uses. It is only by comparing the relative success of different descriptions in predicting the expermental results, as done for example by Eysenck and Eysenck (1969) in their work on the lemon test or Revelle et al. (1980) in their studies of reactivity to caffeine, that clarification of these issues can be gained. And to conduct such experiments on any large scale (covering both many laboratory tests and many alternative modes of description) is a formidable undertaking which has not yet been approximated. This point will need to be borne in mind in the next section, where we concentrate on the degree to which Eysenck's explanations of the underlying nature of E–I have been a success. Even if our conclusion is that the degree of success is very great, this would still leave open the issue whether a still better account could be provided by an alternative descriptive framework, perhaps one that (as in the parallel case of intelligence) included causal influences working at more than one factor-analytic level.

8.3 Biological Explanation

In the previous section we saw how in Eysenck's model personality serves as a theoretical and descriptive framework within which to locate

the groups of individuals (dysthymics, hysterics or psychopaths, psychotics) in whom he is interested. In the present section we shall consider his attempts to explain this personality framework itself in terms of a theory based on the study of learning and physiological psychology. Note that we are now two steps removed from the overriding Eysenckian questions. Shifts in the relationship between personality and socio-psychiatric phenoma (of the kind considered in the previous section) are likely to affect the adequacy of the biological explanations considered in the present section, and vice versa. However, in order to keep the present discussion within tolerable bounds, we shall take account of interactions of this kind only when it is essential for the arguments pursued.

Eysenck has offered two explanations of E–I, one in 1957 (in *The Dynamics of Anxiety and Hysteria*) and one in 1967 (in *The Biological Basis of Individual Differences*). The two accounts have much in common, so much so that crucial differences between them have often been obscured. At this point the temptation is to enter into a long consideration of the concepts used in each theory, asking questions like, is the 'excitation-inhibition balance' (1957) the same sort of thing as 'arousal level' (1967)? But the acid test of whether two theories are the same or different is whether or not they make the same predictions. There are clear examples in Eysenck's work of cases in which the two theories did make different predictions, and these are the most instructive examples for us to consider. I shall argue from these that the two theories are in important respects clearly different; and that, while there were good empirical reasons for Eysenck to shift to the 1967 model (which is developed further in the present volume), there are also data which the 1957 theory could, but the 1967 theory cannot, account for. I shall then go on to show that there are other data that neither theory can account for.

Let us first consider the ways in which the 1957 and 1967 theories are the same. As set out in Fig. 8.1, both theories suppose that (given high scores on N) the crucial difference which determines whether an individual is likely to become dysthymic or hysterico-psychopathic is the ease with which he forms conditioned reflexes during the process of social development. One who conditions easily becomes an introvert (and so susceptible to dysthymia), one who conditions with difficulty becomes an extravert (and so susceptible to hysteria or psychopathy). This connection is mediated by conscience formation, which is seen as the establishment of a set of Pavlovian conditioned reflexes. An over-developed conscience (in the introvert) predisposes to dysthymia, an under-developed one (in the extravert) to anti-social behaviour.

The differences between the 1957 and 1967 theories occur at the top and the bottom of the chains shown in Fig. 8.1. As we have already seen, at the top, where the 1957 theory stresses hysterical symptoms, the 1967 theory stresses the anti-social behaviour of the psychopath. The change at the bottom of the chain is that in 1957 differences in conditionability were said to reflect differences in the excitation-inhibition balance, of unknown physiological origin, while in 1967 they were said to reflect differences in arousal level and to originate in the ascending reticular activating system (ARAS).

The switch from talk about the excitation-inhibition balance to talk of arousal level often appears to be, and sometimes is, no more than a change of vocabulary. For, like arousal level, the excitation-inhibition balance is, in spite of its name, a uni-dimensional construct: individuals who generate 'excitatory potentials' with ease *eo ipso* generate 'inhibitory potentials' with difficulty and vice versa. Thus there is a natural equation between individuals with 'high arousal level' and 'high excitation/low inhibition', and between individuals with 'low arousal level' and 'low excitation/high inhibition'. Each member of the former pair is said to be more highly conditionable and each member of the latter pair less conditionable (in general: but we shall come across exceptions later). But in practice there are important differences in the connotations of the two concepts, and these have led to different predictions in the two cases. Roughly speaking, the 1957 arguments stressed the inhibition aspect of the excitation-inhibition balance, whereas notions of inhibition do not naturally enter into the concept of arousal level

at all, or, if they do, it is only at high values of stimulus intensity (Gray 1964a, b). Moreover, the particular concept of inhibition used by Eysenck in 1957 – a blend of Pavlovian internal inhibition, Hull's reactive (I_R) and conditioned ($_sI_r$) inhibition and Koehler's stimulus satiation – leads to predictions which can in no way be derived from the concept of arousal level (Gray 1967).

It is worth pointing out *en passant* that the changes which took place at the top and bottom of the Eysenckian chain between 1957 and 1967 were probably connected. The excess of inhibition seen by Eysenck to produce extraverted behaviour gives a very natural and direct explanation of the symptoms of conversion hysteria (i.e. paralyses and loss of sensation). It is much easier to abandon inhibition in favour of arousal level when the behaviour that needs explanation is psychopathy rather than hysteria.

The volume of experimental research reviewed by Eysenck in the *Dynamics of Anxiety and Hysteria* and in subsequent papers bearing upon the 1957 theory is far too large for any summary of it to be possible here. Instead, I shall take a number of specific examples of experimental phenomena to show the very diverse consequences the data have had for the theory in different areas. Sometimes the theory stood up well to testing; sometimes it did not, and the new arousal theory was able to resolve the problem; sometimes both theories, the 1957 model as well as the 1967 one, can handle the data; and sometimes neither theory fares very well.

Consider first a case of uncomplicated success for the theory: Spielman's (1963) demonstration, replicated by Eysenck (1964b), that extraverts show more pauses in a tapping task than do introverts. The prediction that this should be so is derived straightforwardly from the combination of two hypotheses: (1) with repeated taps, I_R should build up as a state inimical to the further production of taps and so give rise to pauses; (2) extraverts should build up more I_R than introverts. Note that Spielman's result cannot be derived in any very obvious manner from arousal theory. Note also that, with the virtual demise of Hull's theory of reactive inhibition in its home territory of learning (Gleitman et al. 1954), Spielman's result stands as a rare monument to its one-time usefulness.

Consider next a case in which results which were at one time a major source of support for the inhibition theory are now seen as support for the arousal theory. If one treats perception as a response, then one can apply to it Hull's theory of inhibition in much the same way that it is applied to the motor response of tapping. From this, one can predict that extraverts (who build up more I_R) will show a greater decline over time in a vigilance test. Findings of this kind were reported by Bakan et al. (1963) and Claridge (1960). Claridge's (1960) experiment also showed that adding a second signal for detection improved the performance of extraverts, an effect attributed to 'disinhibition' along lines familiar since Pavlov's description of this phenomenon; the second signal impaired the performance of introverts, this effect being attributed in a more *ad hoc* manner to 'distraction'. So far, so good. But other studies produced less satisfactory results. Bakan (1959) concurred with Bakan et al. (1963) and Claridge (1960) in finding introverts to be better than extraverts at a vigilance task. But this did not arise because of a steeper decline in performance over time in the extraverted subjects: on the contrary, the introverts showed a steeper decline, their superiority being due to an *initially* high level of performance. Further complications were added by Colquhoun and Corcoran's (1964) observation in a letter-cancelling task that introverts performed better than extraverts in isolation and in the morning, but extraverts performed better in groups or in the afternoon. The effect of group testing might have been handled by treating it as a 'disinhibitor' for extraverts and a 'distractor' for introverts, along the lines pioneered by Claridge (1960). But there was no obvious way to handle time of day within inhibition theory.

In these dire straits a knight in shining armour was the merest necessity. He arrived in the guise of arousal theory, his banner bearing a strange device: the inverted U-shaped curve. It would be tedious to recount all the miracles which that curve enabled him to perform (Corcoran 1972). Disinhibitors and distractors both became arousal increments, improving the per-

formance of extraverts by taking them closer to the peak of optimum performance and impairing that of introverts by taking them beyond it. Suitable locations of the two groups on the curve at the start of an experiment, combined with suitable assumptions about the direction of change in arousal level over the session, were able to accommodate both the cases in which extravert performance declined more swiftly with time in the session and those in which introvert performance did so. Even the time-of-day effect proved tractable: there is evidence from physiological measurements that arousal level rises steadily over the day (e.g. Blake 1971); thus, by the afternoon, it will bring introverts over their peak and extraverts closer to it. In short, we have an excellent example of scientific progress: the new theory handled the data that the old theory accounted for, but also the anomalies. This, indeed, was the case over much of the data: there were very good reasons why arousal theory superseded inhibition theory during the late 1960s.

My third example concerns a case in which inhibition and arousal theory were used, a decade apart, to make precisely opposite predictions. In his 1957 book (p. 196) Eysenck deduces from inhibition theory that extraverts should have higher critical flicker fusion (CFF) thresholds, and he regards the evidence (such as it was) as supporting this deduction. The deduction sounds plausible enough: in order to perceive the next flash, the perceptual effect of the present flash must be curtailed; the curtailment is a task for inhibition; therefore the better the inhibition (as in extraverts), the higher should be the CFF. But in his 1967 book (p. 104) Eysenck deduces that *introverts* should have higher CFF. Now, the argument runs, introverts are more aroused than extraverts, so they should behave as though they were in receipt of higher sensory bombardment; CFF goes up with higher stimulus intensity (Gray 1964b); so introverts should have a higher CFF than extraverts. There is rather more substantial evidence supporting this prediction than the reverse one; so, once again, arousal theory proved to be more effective than inhibition theory. But it is more important to note that, given that the two theories were used to make *opposite* predictions to each other, they cannot merely be rival formulations of the same essential principles. Furthermore, it is clear that the arguments used to derive the two predictions are quite different in kind. Thus, if some data can be predicted using inhibition (but not arousal) theory (e.g. Spielman's tapping result), while other data can be predicted using arousal (but not inhibition) theory (e.g. performance in vigilance tasks), one cannot argue that these are merely two facets of the same theory. If both theories are needed, it follows that extraverts and introverts differ in more than one fundamental way.

Just such a portmanteau attempt to put the two theories together was made in the case of my fourth example, that of eye-blink conditioning. It will already be clear, from our discussion of the overall structure of Eysenck's theory, that conditioning is of central importance to this theory. There has been a correspondingly substantial effort, both theoretical and experimental, to bring order into this field, starting with Franks' (1957) demonstration that introverts form conditioned eye-blink responses better than extraverts. Over the years this conclusion has had to be hedged with limitations. The most complete investigation of these limitations was performed by Levey (Eysenck and Levey 1972). For our present purposes the interesting thing about this study is the derivation of the hypotheses it was intended to test. These were that introverts would condition better than extraverts under three conditions: (1) under partial rather than continuous reinforcement; (2) under low- rather than high-intensity reinforcement (strength of air-puff); and (3) under a short rather than long CS-UCS interval. The first of these predictions was derived from inhibition theory: the unreinforced trials of the partial reinforcement schedule were seen as building up (presumably Pavlovian internal) inhibition to a greater extent in extraverts than introverts, so that the performance of the former would be depressed relative to the latter. The other two predictions were derived from arousal theory, by way of a parallel between weakness of the nervous system (interpreted by Gray 1964a, b, as high level of arousal) and introversion (Eysenck 1967; Gray 1967). High arousal

was seen as boosting the effective intensity of the UCS (and so favouring conditioning with a low-intensity air-puff) and rapidity of responding to the CS (so favouring conditioning with a short CS-UCS interval). Note that, once more, the kinds of argument used to make the two kinds of prediction are quite different: we are dealing with two theories joined like Siamese twins, not one with a Janus face. The results of Levey's experiment were in support of predictions (2) and (3), but not prediction (1). Thus, as it turned out, arousal theory needed no help from inhibition theory: it was able to predict all the major results on its own.

My final example concerns the reminiscence effect. This is a case in which, after a substantial experimental investment (Eysenck and Frith 1976), it is established that extraverts and introverts differ reliably in their performance, much is known about the ways in which their performance differs and the factors that affect these differences, but neither inhibition nor arousal theory (nor any other to my knowledge) can satisfactorily account for the data. Yet the initial work on reminiscence seemed to establish it as the greatest success of inhibition theory. This treats the reminiscence effect (i.e. the improvement in skilled motor performance which is seen after a rest pause, usually measured as time-on-target on a pursuit rotor) as being due to the dissipation of I_R. Since extraverts should build up more I_R than introverts, they should – and do – display a correspondingly greater reminiscence effect. But, sadly for inhibition theory, this is not because of a greater pre-rest depression of performance in the extravert, as the theory predicts, but rather because of better post-rest performance (Star 1957). In an effort to apply arousal theory to the phenomenon and so resolve this anomaly, Eysenck and Frith (1976, p. 287) drew an analogy with the situation in paired-associate learning. Here it had been shown (Howarth and Eysenck 1968) that extraverts are superior to introverts on measures of short-term retention (periods of minutes), but introverts are superior on measures of long-term retention (24 h), a pattern of results that can be explained in terms of the effects of arousal level on the consolidation of memory. The intervals over which the extravert shows higher reminiscence effects are comparable to those at which he shows superior retention of paired associates. All would be well, therefore, and arousal theory triumphant once more, if it could be shown that at a retention interval of 24 h the extravert superiority on the reminiscence effect is replaced by an introvert superiority. The relevant experiments, however, have failed to support this prediction (Eysenck and Frith 1976, p. 287). So we are left with a well-established laboratory phenomenon, the greater reminiscence effect of the extravert, which defies explanation in terms of either inhibition or arousal theory.

These data, and others like them, give grounds for several conclusions: first, inhibition theory does not work as a general account of the underlying basis of individual differences in E–I; second, arousal theory does a very much better job; but third, even when inhibition theory gave way to arousal theory, there were already phenomena (e.g. Spielman's findings with the tapping test, introvert–extravert differences in the reminiscence effect) which the new theory could not adequately explain.

The replacement of the inhibition theory by the arousal theory of E–I was a gradual process, commencing about 1960. By this time the major lines of the *general* theory of arousal (that is, independent of its application to individual differences) had been set out in the work of writers such as Freeman (1948) and Duffy (1962), and the connection with Moruzzi and Magoun's (1949) discovery of the ARAS had been made clear. Conversely, Hull's general theory of reactive inhibition was in serious trouble (Gleitman et al. 1954). The earliest suggestions that the new theory of arousal could be applied to E–I came from Claridge (1960) and Corcoran (1962). The process was completed in Eysenck's 1967 book, *The Biological Basis of Personality*. It is clear from this book that a major influence on the evolution of Eysenck's thinking had been the appearance for the first time in English of a comprehensive treatment of Pavlov's theory of personality (Teplov 1964), of Teplov's subsequent development of this theory (Gray 1964a) and of my own attempt to translate that part of this development which concerns 'strength

of the nervous system' (SNS) into the language of arousal (Gray 1964b).

A comparison between the development of Pavlovian theories in the Soviet Union and the development of Eysenck's theories reveals some instructive parallels. As Teplov (1964) and Nebylitsyn (1972) make clear, Pavlov's initial theory of SNS was a theory of individual differences in ease of conditioning. This part of Pavlov's views (available in English in the standard translations of his work on conditioned reflexes since 1927) was influential in Eysenck's decision to allot a key role to the concept of conditionability in his 1957 theory, as he makes clear in *The Dynamics of Anxiety and Hysteria*. Subsequently, however, the data, combined with certain logical tangles in Pavlov's original theory, produced a major shift in emphasis, first in Pavlov's own work and then in that of his Soviet successors, towards a theory of SNS which no longer accords conditionability any particular importance (Gray 1964a, b; Nebylitsyn 1972). The central concept in this new theory of SNS, that of 'working capacity', instead bears strong resemblances to the concept of arousal level (Gray 1964b), while individual differences in conditionability (assuming them to exist, for Soviet data on this score are no more convincing than Western data) now belong to a second dimension, independent of SNS, termed by Nebylitsyn (1972) 'dynamism'. In his 1967 book, Eysenck explicity drew a parallel between the arousal theory of E–I and the Teplov-Nebylitsyn theory of SNS and proposed the hypothesis that introverts are equivalent to individuals at the weak pole of the latter dimension. (We shall return to this hypothesis later in the chapter.) He did not, however, abandon the notion that differences in conditionability are central to the E–I dimension. With this exception, the parallel between the two strata of Pavlovian theory and the two Eysenckian strata is almost complete. In part, of course, this parallel reflects the direct influence of Pavlov's thinking on Eysenck's. But more importantly, it also reflects the experimental findings that were made in East and West. Although these were quite different in kind, the pressure they exerted was the same: towards some kind of arousal theory of the two dimensions of personality, E–I and SNS.

Since the early 1960s, the arousal theory of E–I has accumulated many successes, and there is now an impressive body of data which it can accommodate more or less well (Eysenck 1967; this volume, *passim*). But, at the same time, certain alarming cracks have opened up in the Eysenckian edifice. In keeping with the general assignment of this chapter, I shall not dwell on the successes of arousal theory (though, as we shall see, some of the problems that have arisen themselves testify to its power); rather, I shall probe the cracks.

The first of these concerns the time-of-day effects that we have already encountered in Colquhoun's and Corcoran's (1964) experiment on letter cancellation. At first it appeared, as indicated above, that this could be handled by three postulates: the inverted U, the greater arousal of introverts relative to extraverts and the postulate that arousal rises over the day. But in 1967 Blake measured body temperature over the day in groups of naval ratings (forced by their service to keep the same hours of waking). He found that introverts had a higher temperature in the morning (in agreement with the postulate that they are more aroused than extraverts), but by late afternoon and evening the higher body temperature belonged to the extravert. Now this in itself would not be too much of a problem. I have never seen much value in the faith that physiological measures get closer to 'arousal level' than do behavioural measures that fit properly into a coherent theory (Gray 1964b). So it is perfectly acceptable to take the view that body temperatures is all very interesting, but not a direct measure of arousal level as this construct is used within Eysenckian theory. But recently matters have taken a more sinister turn for this theory. Revelle et al. (1980) have put together the suggestion from Blake's data that introverts and extraverts have different diurnal rhythms in arousal level with Eysenck's *behavioural* theory of arousal and have made the important demonstration that, for the purposes of this theory, introverts and extraverts swop places between early morning and late evening. Since these are important experiments and have only recently appeared in the literature (though similar findings had been reported earlier by Blake 1971 and Fol-

kard et al. 1976), I shall go into them in some detail.

The subjects in the experiments completed an academic-type test similar to the familiar American Graduate Record Examination (GRE). They performed this test under three conditions: baseline (no time pressure, no drug), time pressure, and time pressure plus caffeine. The three conditions can reasonably be taken, within the general rules of arousal theory, as representing ascending levels of arousing stimulation in the order given. Assuming the usual inverted U relating arousal level and performance, and assuming that introverts are chronically more highly aroused than extraverts, standard Eysenckian arousal theory predicts that introverts should out-perform 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. This is exactly what Revelle et al. (1980) found when testing was done in the morning; though with the important qualification (to which we shall return) that the key predictor of performance was not the superfactor, E–I, but the subfactor, Imp. Thus, if one stops here, one would hail a further success for arousal theory. But Revelle et al. (1980) also carried out the identical experiment in the evening, with exactly opposite results: extraverts performed better than introverts in the baseline condition, extraverts were hampered and introverts helped as one went from baseline progressively through the other two conditions; and again the critical factor was Imp. 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. For this theory is an attempt to account for stable features of the personality. One is not a dysthymic in the morning and a psychopath at night. One does not even (I imagine, though I do not know that the experiment has been done) fill in a questionnaire as an introvert in the morning and an extravert at night. Yet Revelle et al.'s (1980) data clearly indicate that introverts (more accurately, individuals low on Imp) have a higher arousal level than extraverts (individuals high on Imp) in the morning, but a lower arousal level in the evening. Such a relative shift in diurnal rhythms might be expected to produce a modulation in mood in introverts and extraverts (perhaps explaining, for example, the different diurnal rhythms in depth of depression sometimes seen in reactive and endogenous depression: Mayer-Gross et al. 1969): but how can it give rise to stable personality traits?

The second crack is separate from, but related to, the first: together they compose, so to speak, a compound fracture. As we have seen, Eysenck's theory requires that introverts condition better than extraverts, so that they may form stronger consciences. The key evidence for this proposition has been the superiority of introverts at eye-blink conditioning, which we have already discussed. Leaving aside the problem that, given the almost total absence of evidence for a general factor of conditionability, it is not clear that one can generalize from eye-blink conditioning to whatever conditioned reflexes may make up the conscience, the data gathered by Eysenck and Levey (1972), *precisely to the degree that they support the arousal theory of E–I*, show that conditionability simply cannot fill the role allotted to it in Eysenck's theory. As we have seen, Levey's experiments showed that introverts (more accurately, individuals low on Imp) develop conditioned eye-blink responses better than extraverts (individuals high on Imp) only under conditions that favour individuals who are relatively more highly aroused (low air-puff intensity, short CS-UCS interval). While this is gratifying for the arousal theory of E–I, it carries with it the corollary that, if introverts are to form better consciences than extraverts, parents must on average choose circumstances that are relatively low in arousal potential to carry out the conditioning procedures that give rise to the conscience. This proposition is, to say the least, not intuitively obvious.

But worse is to come. Since it was Imp, not E–I, which best differentiated the good and bad conditioners in the Eysenck and Levey (1972) study, there is a clear possibility that the pattern of behaviour observed by Revelle et al. (1980)

in their study of performance on the GRE could also hold in the case of eye-blink conditioning. Levey's data were presumably gathered during normal day-time hours. What would his findings have been had he tested his subjects at 7–8 p.m., the evening time in the Revelle study? Since the same arousal theory that predicts the effects of UCS intensity and CS-UCS interval on eye-blink conditioning also predicts the effects of time pressure and caffeine on GRE performance, this theory must predict the same patterns of individual differences for both kinds of test. We should therefore predict that individuals *low* on Imp would do *worse* than individuals high on Imp if tested for eye-blink conditioning under *low*-arousing conditions in the *evening*. Clearly, this is an experiment which ought to be done. But, whatever its outcome, the arousal theory of E-I would remain discomfitted. If the prediction is not confirmed, this would reveal an inconsistency in general arousal theory; for the same individuals that would be high on arousal (relative to another group) on one test would be low on arousal on a second. If the prediction is confirmed, general arousal theory would be strengthened, but Eysenck's problem would be greater than before. For now, in order for the introvert to end up with a better conscience than the extravert, parents would need, on average, to use low-arousing conditioning techniques in the morning and high-arousing conditioning techniques in the evening. One may have succeeded in suspending disbelief until now; but this proposition must defeat credulity.

The third crack in the Eysenckian facade is one that we have already encountered, in the section on personality description. It is caused by an apparent slippage of biological significance away from E–I in favour of Imp. When careful study has been made of the relation of both the super-factor and the subfactor to an experimental measure of biological significance (something that has, to be sure, not been done very often), it has tended to be Imp that is the most strongly related factor, with E–I playing this role rarely and the other subfactor of E–I, Soc, playing it so far not at all.

Thus, as we have seen, Imp is the clearest correlate of eye-blink conditioning (Eysenck and Levey 1972) and of the diurnal rhythms in body temperature (Patkai 1970; M.W. Eysenck and Folkard 1980) and in reactivity to time pressure and caffeine (Revelle et al. 1980). At the real-life level, Burgess (1972) has found Imp (or, at any rate, an amalgam of N and E that must closely approximate to Imp) to be the best correlate of criminality; similar findings have been reported by Eysenck and Eysenck (1976, p. 125). E–I (as distinct from either of its subfactors) has been shown, so far, to be the best correlate only of salivation in response to lemon juice (Eysenck and Eysenck 1969); this is a measure whose relation to personality is, interestingly, *not* subject to diurnal shifts (Corcoran 1972). As I tried to make clear in an early section of this chapter, there is not necessarily any need to choose between E–I and Imp to locate *the* line of causal influence: each of these factors could represent one such line. One intriguing possibility, given the very little evidence we have, is that E–I reflects stable individual differences in arousal level (i.e. ones that do not vary over the day), while Imp reflects the modulation of this by differing diurnal rhythms in arousal. Certainly, that is a line of research woth pursuing further. But it is not one that could shore up the failing Eysenckian structure pictured in Fig. 1. For that structure allots a central role to conditionability; and conditionability lines up with Imp (Eysenck and Levey 1972). If we leave aside the connection between Imp and diurnal rhythms and the problems to which this gives rise, we might be tempted to solve the problem by shifting from E–I to Imp (whether treated as a subfactor or as a rotation of E–I at the dimensional level) as the main explanatory focus of Eysenck's theory. But closer consideration shows that such a shift would rob this theory of one of its major, and most attractive, features.

Eysenck's theory of E–I, in 1967 as in 1957, offers a simultaneous account of the behavioural characteristics believed to lie at the poles of this dimension, provided that N is high in both cases: good conscience formation, given high N, leads to dysthymia, and poor conscience formation, given high N, leads to hysterical or psychopathic behaviour. Thus the promise is held out that, by unravelling E–I, we shall

gain an understanding of both these diverse kinds of condition – a promise rich in parsimony and scientific power. But a shift from E–I to Imp cancels that promise. For Imp is itself positively related to N, with the consequence that, if its high pole is neatly and usefully located among the psychopaths, its low pole is unhelpfully buried among the stable introverts of this world, people who quietly write books, tend their gardens and by and large do not come near the psychiatric clinic. Nor are we much helped if we relocate new-style Imp closer to E–I, but bending off into the third dimension of P (Eysenck and Eysenck 1978). For this factor no longer yields the very interesting biological correlate with diurnal rhythms yielded by old-style Imp (Revelle, personal comment 1979); nor do we know that it will continue to yield the correlation with eye-blink conditioning reported by Eysenck and Levey (1972) for old-style Imp.

8.4 An Alternative Theory

I conclude from the arguments in the previous section that Eysenck's theory cannot perform the task that he set it. But a theory is only ever killed by a better theory. Do we have one? If we recall that a new theory must not only account for the anomalies that face an old one, but also continue its successes, the answer to this question is 'almost certainly not'. But I shall try to outline a few ground-clearing operations which may smooth the way for an eventual new theory and to indicate a few ideas that may be useful for such a theory.

As indicated in the previous section, it was one of the great merits of Eysenck's theory that it attempted to account for dysthymia and hysteria/psychopathy within a single unified framework. But this attempt appears to have failed. As a first step towards the construction of a new theory, therefore, it may help to abandon this ambition. This step was, in fact, part of the modification to Eysenck's theory that I proposed in 1970, though I did not properly draw out this implication until later (Gray 1972, 1973), and the point has still not been fully taken by commentators on my alternative model (e.g. Powell 1979). The basic postulates of this model (Fig. 8.2) are as follows:

(1) The lines of causal influence are rotated by 45° from Eysenck's dimensions. The two new resulting dimensions are anxiety (Anx-D), running from Eysenck's stable extravert (low anxiety) quadrant to his neurotic introvert (high anxiety) quadrant; and imoulsivity (Imp-D), running from the stable introvert (low impulsiv-

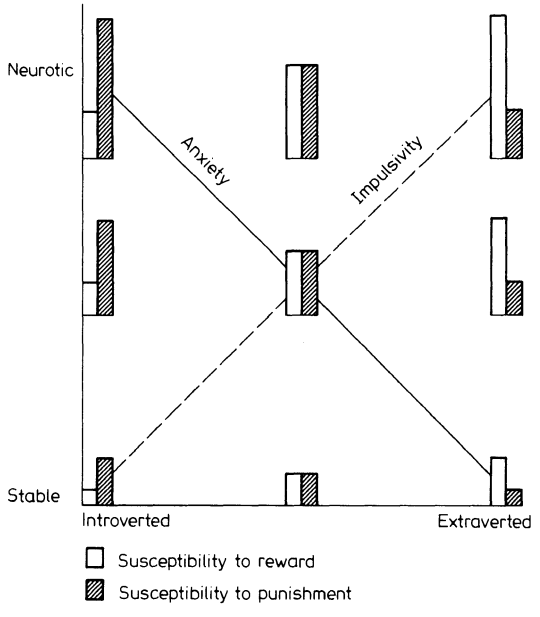

Fig. 8.2. Proposed relationships of (a) susceptibility to signals of reward and susceptibility to signals of punishment to (b) the dimensions of introversion–extraversion and neuroticism. The dimensions of anxiety and impulsivity (diagonals) represent the steepest rates of increase in susceptibility to signals of punishment and reward respectively. (Gray 1970a)

ity) to the neurotic extravert (high impulsivity) quadrant.

(2) Increasing levels of Anx-D reflect increasing levels of sensitivity to signals of punishment, signals of non-reward and novelty. There is an underlying physiological system (the 'behavioural inhibition system'. BIS: Gray 1977), activity in which controls the level of Anx-D, and which consists of an interacting set of structures comprising the septo-hippocampal system (SHS), its monoaminergic afferents from the brain stem and its neocortical projection in the frontal lobe (Gray 1978; Gray et al. 1978).

(3) Increasing levels of Imp-D reflect increasing levels of sensitivity to signals of reward and signals of non-punishment. There is an underlying physiological system, independent of that which underlies Anx-D, activity in which controls the level of Imp-D. Little progress has been made in describing the structures that go to make up this system.

(4) E–I and N are secondary consequences of the interactions 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 non-reward and novelty than they are to signals of reward and non-punishment) 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.

It will be clear from this account that Gray's (1970a) model allows the explanation of dysthymia to proceed in isolation from that of impulsive behaviour (we shall leave aside for the moment the question, "what, in real-life terms, is the syndrome seen in highly impulsive individuals?") and vice versa. In 1979 this feature of the model has the bonus that it automatically solves the problem posed for Eysenck's theory by the discovery that Imp, rather than E–I, is the line of clearest causal influence. Indeed, the model gains in plausibility by this discovery. The arguments used in its support in my 1970 paper turned largely on data concerning dysthymia and matters related to dysthymia (anti-anxiety drug action, the effects of frontal lobe lesions in Man, the effects of damage to the SHS in experimental animals, etc.). The location of the Imp-D factor in the model was determined largely by considerations of symmetry: if Anx-D was to move through 45°, it was convenient to have Imp-D do the same. The spin-off from such a rotation was that it provided an intuitively plausible account of anti-social behaviour: an individual high on Imp-D is thought to be highly sensitive to the rewards available for anti-social conduct and not strongly sensitive to the potential punishments. A prediction that follows from the rotation is that the Imp-D factor should be a good place to look for lines of causal influence; and it is encouraging to find evidence that this prediction can be verified. But, as we shall see later, it is much harder to argue that the particular biological correlates of Imp that have been found are of a kind that Gray's (1970a) model can predict. So the rotation may be correct, but the postulated psychophysiological mechanisms that prompted this rotation wrong.

Arguments in favour of treating anxiety directly, rather than as an amalgam of neuroticism and introversion, were presented in the original statement of the model (Gray 1970a), and there is no need to restate them here. The most important of these arguments turned on the fact that certain physiological treatments have been shown to reduce dysthymic symptoms. If we regard dysthymia as a mixture of high N and low E, we have to suppose that these treatments have two effects, reducing the one and increasing the other. It is (it was argued) more parsimonious to attribute to these treatments a single effect, that of reducing anxiety directly. A further feature of the approach adopted by Gray (1970a) was that it treated reactive depression as essentially the same as clinical anxiety, this equivalence being seen as parallel to the equivalence established in experiments on animal learning (Gray 1975) and drug action (Gray 1977) between conditioned frustration (involved in depression) and fear (a synonym for anxiety). Since 1970 there have been several developments which strengthen the case then made in favour of the model in respect of its treatment of anxiety.

First, as summarised by Powell (1979), it has become clearer that damage to the frontal lobes (one of the physiological treatments singled out by Gray, 1970a, as reducing anxiety) does indeed have this effect, and also that it reduces depressive symptoms (Smith et al. 1977). Powell (1979) questions the evidence that this treatment reduces introversion (though not the evidence that it reduces N) and believes that this is a stumbling block for Gray's (1970a) model. But this is to misunderstand that model. A reduction in anxiety symptoms without reduction in introversion is a challenge to *Eysenck's* theory, since this treats dysthymia as a consequence of low E together with high N; it *supports* the view that it is more parsimonious and more effective to regard anxiety as the primary state affected by frontal lesions. Other recent neuropsychological evidence (Kelly et al. 1973; Mitchell-Heggs et al. 1976) reviewed by Powell (1979) similarly lends support to Gray's rather than Eysenck's model. These investigators found that cingulectomy reduced anxiety and depression, again without change in E–I. The cingulate cortex receives important inputs from those areas of the thalamus to which the SHS projects, and in turn it sends important inputs to the hippocampal formation (Swanson 1978). Furthermore, a lesion in this part of the brain is likely to disrupt monoaminergic afferents to the hippocampus which travel as part of the cingulum bundle (Azmitia and Segal 1978). Thus, given the general anatomical architecture attributed by Gray (1978; Gray et al. 1978) to the BIS, it is to be expected that damage to the cingulate cortex will have the clinical effects described. The fact that these effects are produced by both frontal and cingular lesions with only small increases in the E score (Powell 1979) does not invalidate Gray's (1970a) model. At best, it suggests that the correct rotation of Eysenck's dimensions would be to leave anxiety closer to N than to E–I, a suggestion that can also be derived from the rather higher loading of the Manifest Anxiety Scale on N than E–I (Gray 1970a) and from the higher loading of Imp on E–I than on N (Eysenck and Eysenck 1977). The value of 45° used for this rotation in my 1970 paper was intended only to be schematic, and there were already indications that a smaller rotation would be more appropriate (see Gray 1970a, footnote to p. 263).

Second, the evidence that reactive or neurotic depression is essentially the same disorder as an anxiety state has been strengthened. Brown and Harris (1978) have presented evidence that depression is caused by loss of various kinds, in agreement with the thesis that this condition is a reaction to stimuli associated with frustrative non-reward (Gray 1970a). Since the behavioural effects of such stimuli are antagonized in experimental animals by anti-*anxiety* drugs (Gray 1977; Feldon et al. 1979), this is consistent with the postulated equivalence of anxiety and depression. Direct evidence for this equivalence has been provided by a study at Northwick Park Hospital (Frith et al. 1978) in which, rather than being diagnosed 'depressed' or 'anxious', a series of patients were randomly allocated to anti-anxiety or anti-depressant medication, and the effects of both kinds of drugs were scrutinized symptom by symptom. It was found that both kinds of drug had very similar overall profiles of action; differences were minor (e.g. anti-depresants preferentially reduced feelings of hostility, while anti-anxiety drugs had a greater effect on autonomic symptoms); and both kinds of drug alleviated *both* anxiety *and* depression. Note that we have already seen (in the previous paragraph) that lesions to the frontal lobes or to the cingulate cortex similarly alleviate *both* anxiety *and* depression (Powell 1979). The biggest stumbling block in the way of acceptance of the equivalence of anxiety and neurotic depression, given the above findings, is the wide-spread view that anti-depresants work by increasing the effective functioning of noradrenergic mechanisms in the brain (Van Praag 1977), coupled with the less widely held view that anti-anxiety drugs work by *reducing* noradrenergic function (Lidbrink et al. 1973; Gray 1977). However, recent neuropharmacological evidence has demonstrated the complexity of the feedback controls over the release and post-synaptic effects of noradrenaline in the brain (e.g. Langer 1977). It now appears possible that the popular view of anti-depressant action is exactly wrong, and that with prolonged medication (necessary for the clinical effect of these drugs) compensatory mechanisms

in central noradrenergic pathways are such as to give rise to a net *reduction* in the post-synaptic effect that they produce.

Third, as detailed in several recent papers (Gray 1977, 1978; Gray et al. 1978; Lidbrink et al. 1973; Stein et al. 1973; Tye et al. 1977; Guidotti et al. 1978), there has been a considerable advance in our knowledge of the brain systems that mediate anxiety in experimental animals; but this is not the place to go into these issues.

There are, then, considerable advantages in this kind of frontal attack on anxiety and impulsivity, besides the fact that it offers a convenient solution to the problem posed for Eysenck's E–I theory by the discovery that more causal influences appear to lie along the Imp axis than along the E–I axis. Let us turn now to a second problem which we identified as being faced by Eysenck's theory: the problem of conditionability. Can Gray's (1970) model also offer a solution to that difficulty?

It was, in fact, to the problem of conditionability that Gray's (1970a) model was chiefly directed. As shown in Fig. 8.1, it substituted for Eysenck's postulate that introverts condition in general better than extraverts (and so form the conditioned fear reflexes that make up the conscience better), the postulate that introverts are more susceptible to fear (and so again form conditioned fear reflexes better). In this way, it is possible to preserve the overall structure of Eysenckian theory while recognizing the evidence that introverts condition better than extraverts only under special circumstances, as detailed above. A link between this new postulate and Eysenck's arousal theory of E–I was maintained by supposing that higher arousal effectively amplifies the strength of aversive events, giving rise to the greater susceptibility to fear of the introvert. This position is tenable, however, only if (a) conditioning of fear continues to lie along the E–I axis, and (b) the arousal differences between introverts and extraverts are stable, not diurnally varying. Thus the evidence, cited above, that both these propositions may be false is as much a problem for Gray's (1970a) modification of Eysenck's theory as it is for this theory in its unmodified form.

There is, however, an alternative mode of argument available for Gray's (1970a) model which remained largely implicit in the 1970 paper, but has recently been spelled out somewhat more clearly (Gray 1979b). Remember that what we are trying to explain is the occurrence in predisposed individuals (dysthymics) of symptoms characterized by anxiety: anxiety states; phobias, whether delimited or of the wide-ranging kind found in agoraphobics or social phobics (Marks 1969); obsessional-compulsive rituals, which may be regarded as active avoidance behaviour (Gray 1971) and whose association with anxiety can be demonstrated by prohibiting the patient from performing the ritual; and neurotic depression, on whose similarity with anxiety I have commented already. In terms of a theory I have based on the effects of anti-anxiety drugs on the behaviour of experimental animals (Gray 1977, 1978), anxiety may be defined as a state that is produced by stimuli associated with punishment, by stimuli associated with frustrative non-reward or by excessive novelty. Thus to say, as Gray's (1970a) personality model says, that dysthymics are individuals who are excessively sensitive to these three kinds of stimuli is already to offer an explanation for their psychiatric symptoms: there is no special need to call on the process of childhood socialization, which Eysenck uses to account for their tendency to display such symptoms. Indeed, to pair strange bed-fellows, Eysenck's theory is at one with Freud's in its emphasis on the special importance of childhood experience, but differs in this respect from my own.

In place of this emphasis on childhood experience, one may call upon two other routes. First, if one is excessively sensitive to threat, disappointment or novelty, this must continue to influence one's behaviour directly as an adult. Thus, if an unpleasant event occurs in the life of an adult dysthymic, he will be as likely to form a conditioned phobic reaction to stimuli associated with that event as he would have been to react in this way as a child. The individual cited by Eysenck (1977) as developing a phobia of a particular pattern of wallpaper after being thrashed in sight of it by an irate husband is perhaps a case in point. But this much is common to Gray's (1970a) model and Ey-

senck's general theory of E–I, Eysenck saying that the dysthymic conditions better where I say he is more sensitive to stimuli associated with punishment. More interesting is the possibility of *innate* fear reactions. This possibility is of particular importance, given the strength of recent attacks on the conditioning theory of phobias (Eysenck 1979). To bring it out clearly will require a digression to describe these attacks.

According to the 'standard' conditioning theory of phobias, usually attributed in the first instance to Watson and Rayner (1920), there are a few stimuli which are innately capable of eliciting fear reactions (Watson listed loud noise, pain and sudden loss of support), and the strange panoply of adult phobias then arises through Pavlovian conditioning between these (as UCSs) and a random assortment of conditioned stimuli that happen to have the right temporo-spatial association with them. This theory can easily be wedded to either Eysenck's theory or Gray's modification of it (Fig. 8.1) by pointing out that dysthymics will develop such Pavlovian CRs particularly strongly, whether because they condition well or have strong reactions to conditioned aversive stimuli. But there are a number of formidable objections to it (Eysenck 1979; Gray 1979b). The most important of these are as follows. (1) The stimuli that elicit phobias are not a random sample of stimuli; some (e.g. closed spaces) are greatly over-represented, others (e.g. cars) that are associated with objective dangers are under-represented. (2) The times of onset of phobias are not a random sample of ages; there is a predominance of onsets in early adult life (Marks 1969). (3) Phobic stimuli unaccompanied by their UCS ought to undergo extinction; of course, they do not, or they would not constitute a psychiatric problem. In an effort to deal with problems 1 and 3, Eysenck (1979) has made use of Seligman's (1971) concept of 'preparedness' and allied it to a new theory of phobias of his own. According to this theory, some stimuli are 'prepared' (by evolutionary development) to enter into association with aversive UCSs; furthermore, once such an association has been formed, the phobic power of the prepared CS can be further increased by presentation *without* reinforcement from the UCS (a process termed 'incubation'). One can still preserve the link between this new theory of the formation of phobias and personality theory by supposing that neurotic introverts form such prepared conditioned reflexes more strongly than do other individuals (for the same reasons as before: Fig. 1).

Now, while I accept the difficulties for the standard conditioning theory of phobias briefly outlined above, I do not find Eysenck's solution to them satisfactory (Gray 1979b). First, objection 2 above is not answered by this solution. Second, the evidence for the process termed by Eysenck 'incubation of fear' is poor (Bersh 1980). Third, the concept of 'preparedness for conditioning' is a confused and unnecessary amalgam of two clearer notions: that of innate reactions to releasing stimuli and that of conditioning proper. I have discussed the evidence for innate fears elsewhere (Gray 1971). One of the interesting features of such fears is that they are often subject to maturation: that is, they appear *without learning* at a given stage in ontogeny. This concept, which is well supported in the animal literature (Gray 1971, Chapter 2), seems eminently suitable to cope with the evidence that certain phobias appear in human beings during erarly adult life, especially those concerned with social interaction (social phobias, agoraphobias: Marks 1969). Among the stimuli that are innate releasers for fear in animals are those that occur during well-defined forms of social interaction (Gray 1971); it seems reasonable to suppose that something similar occurs in the less well-defined social interactions of our own species and that the relevant stimuli have a particularly strong effect at that time in development when social interaction begins to play an important role in life, namely, late adolescence. Thus the reasons which led Seligman (1971) and Eysenck (1979) to talk of preparedness for conditioning can more simply be seen as reasons to talk in terms of the already familiar concept of innate fears. If we do this, we meet objection 2, above, to the standard conditioning theory. Furthermore, the concept of conditioning can then be reserved for its proper task, as defined by Pavlov right at the outset of his research into conditioned reflexes

(Gray 1979a), namely, to account for those cases in which there is *no* biologically prepared connection between the stimulus and the response it elicits.

I conclude from this digression that an important way in which dysthymics are different from other individuals is that they are particularly likely to display certain kinds of innate fears, of which the most important, clinically speaking, are those that arise during the course of social interaction in late adolescence and early adulthood. On this view, conditioning is not involved at all; so that the difficulties posed for Eysenck's theory and a conditioning version of my 1970 modification of this theory are completely bypassed. All we need say is that the same BIS that mediates responses to stimuli that have been associated (by conditioning) with punishment or non-reward also mediates response to stimuli that innately elicit fear. There is one piece of evidence from the animal laboratory that supports this move. Destruction of the septal area, a key structure in the postulated BIS (Gray 1970b, 1978), causes rats to reduce their 'personal space': that it to say, after such a lesion, the rat actively seeks out companions, stays closer to them and is less daunted by aggressive behaviour on their part (Jonason and Enloe 1971; Poplawsky and Johnson 1973). This is an area, however, that clearly merits much more research.

If we go far along this path, we shall cut the explanation of dysthymia loose from its one-time mooring in the territory of conditioning; just as we have already cut the factor of anxiety loose from its moorings to E–I and N. This move seems to me the right one to make. It is true that there are in the literature clear-cut reports implicating conditioning in the genesis of neurotic fears, of which the wallpaper case mentioned above (Eysenck 1977) is a particularly good example. But these cases can be treated as secondary to the primary influence of high sensitivity to threat: if one is highly sensitive to stimuli associated with punishment it must follow that one will in general condition easily with aversive UCSs. And much else about dysthymic behaviour is difficult to see naturally as the result of conditioning. Neither a social phobia nor a neurotic depression is easily treated as a conditioned reflex. Some descriptions of agoraphobia look as though they include an innate reaction, producing an initial panic attack, which then serves as the basis for an ever-spreading circle of conditioned phobic responses. The approach outlined here would enable one to regard both these features of agoraphobic behaviour as stemming from the same cause, namely, an over-reactive BIS. No doubt many such mixtures of innate responses and learning must exist, in differing proportions. If both parts of the mixture are essentially the same in mechanism, it will be of comparatively little importance to determine the proportions in which they occur in any particular instance.

Anyone familiar with the successes of behaviour therapy in the treatment of phobias, and the kinds of theoretical account that have usually been given for these successes (e.g. Eysenck and Rachman 1965), could by now be forgiven a certain sense of bewilderment. For these successes have been gained by treating phobias as though they *were* conditioned reactions and then subjecting them to extinction. If they are *not* conditioned reactions, what has the success of behaviour therapy been due to?

Recent evidence, however, suggests that the efficacy of behaviour therapy has rather little to do with the precise theories on which the therapeutic methods were based. For details of the therapeutic procedure that these theories would suggest are of critical importance – e.g. the ordering of the hierarchy of presentation of phobic items and the presence or absence of relaxation after presentation of an item – turn out to play an insignificant role, if any: all that matters seems to be the total amount of time for which the patient is exposed to the phobic stimulus – the greater the exposure, the greater the therapeutic effect (Teasdale 1977). Now this poses something of a dilemma: if one gets better by being exposed to the phobic stimulus, why does one get ill by being exposed to it in the first place? And why does one not get better in the natural course of exposure in the real world without ever needing the attention of a therapist? One possible answer to the first of these questions is that, at the first exposure, there was a UCS. But it is often very difficult to identify such a UCS, which is as

often postulated for theory's sake as observed or recalled. In any case, if we once start talking of innate fear stimuli or prepared stimuli which subsequently undergo incubation, the role played by the UCS is at best vestigial. The second question is even harder to answer and has been part of the impetus for Eysenck's (1979) therory of incubation; a theory with which I have already expressed my dissatisfaction (Gray 1979b).

An answer to both questions which is consistent with the general line of argument advocated here was proposed by Watts (1971) in a pioneering treatment of behaviour therapy as habituation (see also Lader and Wing 1966). If much of the behaviour 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 habituation of the kind described by Sokolov (1960; Horn and Hinde 1971). And if habituation underlies behaviour therapy, the key variable would be expected to be – as it is – total exposure time; although Watts (1971) was also able to identify other likely variables and to show that these too affect treatment outcome as predicted. Thus an important bonus of the present approach is that it may give rise to a rapprochement between theory and practice in the vital field of therapy. Furthermore, there is an encouraging convergence between the ideas that have been here pursued at the psychological level and developments that have recently taken place at the physiological level in studies of the BIS. As already mentioned, the heart of the BIS appears to be the septo-hippocampal system. In experiments on the involvement of the SHS in reactions to conditioned frustrative stimuli we have obtained evidence in support of the model presented in Fig. 8.3 (Gray et al. 1978). Quite independently, Vinogradova and Brazhnik (1978) have shown that the same system is responsible for habituation of orienting responses (OR: Sokolov 1960) to novel stimuli.

Thus we may conceive the dynamics of the elicitation and elimination of anxiety reactions at their simplest (i.e. in the case where no conditioning is involved) as follows. Upon first exposure to an innate stimulus for fear, there is an activation of the BIS which will be greater, the more dysthymic the individual. With repeated exposure to the stimulus, habituation will normally occur; but if the initial anxiety reaction is too strong, the individual may avoid future

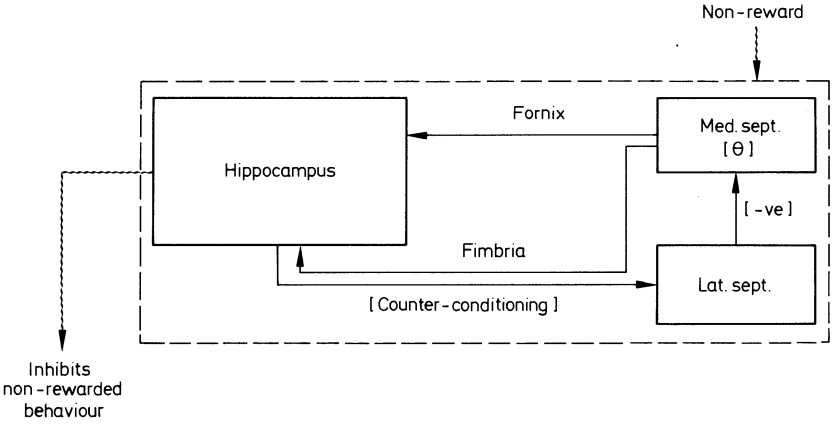

Fig. 8.3. A model for the role of the septo-hippocampal system in counter-conditioning (Gray et al. 1978). According to the model, signals of non-reward are received by the medial septal area and transmitted to the hippocampus via the fornix and fimbria, along with impulses causing the appearance of a theta rhythm in the hippocampus. The hippocampus has the task of inhibiting the non-rewarded behaviour and determining the best behavioural strategy in the changed circumstances. If the correct strategy is to persist with the original behaviour (as when it is occasionally rewarded as well as occasionally non-rewarded), the hippocampus cancels the message received from the medial septal area by way of its connections to the lateral septal area ('counter-conditioning'). By substitution of 'novelty' for 'non-reward' and 'habituation' for 'counter-conditioning', Vinogradova and Brazhnik's (1978) model for habituation of orienting responses may be obtained (Gray et al. 1978)

exposure to the stimulus, so that a phobia develops. If self-exposure is allowed to take place sufficiently often, recovery (habituation) will occur without therapeutic intervention. If the case comes to behaviour therapy, habituation takes place in the clinic. If the original innate reaction spreads to other stimuli by conditioning, this should (on the present hypothesis) complicate matters a little, but change nothing essential in these processes.

It seems possible, then, to construct a reasonably coherent theory of dysthymia and its therapy along these lines, with conditioning reduced to a much more peripheral role than it plaxs in Eysenck's theory of E–I. But the acid test between the two theories must come from experiments specifically designed to pit them against one another at points where they make different predictions. This has not systematically been done. Work in my own laboratory has been almost exclusively concerned with testing the model of anxiety at the animal level (Gray 1978; Gray et al. 1978), although in experiments with children we have shown that, as the model requires, neurotic introverts are particularly sensitive to frustrative non-reward, while neurotic extraverts show evidence of heightened sensitivity to reward in an operant conditioning task (Nicholson and Gray 1972; Gray and Nicholson 1974).

The latter demonstration goes to the heart of the difference between the two theories: for, whereas both Eysenck and Gray concur in expecting stronger conditioning in introverts when aversive UCSs are used, Eysenck's theory continues to predict this where appetitive UCSs are concerned, but Gray's theory now predicts *extravert* superiority in conditioning (see Fig. 2). Nicholson and Gray's (1972) experiments offer some evidence in support of Gray's prediction on this point, but it is only indirect (stimulus generalization of responding for reward was wider in extraverts). But there have been three other experiments which offer more direct evidence of the same kind. Gupta and Nagpal (1978) have reported that introverts learn better when punished for wrong responses on Taffel's (1955) verbal conditioning task, but extraverts learn better when rewarded (with social approval) for correct responses. Seunath (1975) has similarly shown that, on a pursuit rotor task, introverts learn better when punishment is used and extraverts, when reward (money) is used. The third study (Kantorowitz 1978) demonstrated superior sexual conditioning (tumescence in response to a nude slide associated with orgasm) in introverts than extraverts. This study also demonstrated superior introvert conditioning of detumescence to a nude slide associated with the period immediately after orgasm. It is perhaps possible to interpret this as a species of conditioned frustration, given that, in the animal laboratory, there is good evidence that satiation (of food or water) has many properties in common with non-reward (Morgan 1974). Thus, to the extent that evidence is available, it is all consistent with the view (Gray 1970a) that introverts condition better with aversive stimuli, but extraverts better with appetitive stimuli. But this is such a central point of difference between the theories of Eysenck and Gray that it clearly calls for much more empirical investigation.

We have so far dealt with two of the anomalies posed by the data for Eysenck's theory, those arising from the apparent slippage of the major line of causal influence from E–I to Imp and from the failure of the conditionability postulate. The third anomaly concerned the nature of the biological basis of Imp and in particular the fact that it is apparently concerned with mood (the diurnal rhythm in arousal level) rather than temperament. Does the present approach help to deal with this anomaly? The short answer is, no. It is just as difficult for Gray's model as for Eysenck's to transmute mood into temperament; and I can think of no alternative theory of E–I for which this difficulty would be any less acute. On the other hand, the fact that the present approach divorces the accounts of impulsivity and dysthymia, where Eysenck unites them, at least limits the scope of the problem posed by the relation between Imp and diurnal rhythms. We still need to explain how the permanent characteristics of individuals high on Imp arise; but this need not affect the conclusions we have already drawn concerning anxiety.

It is very much harder to get any kind of grasp on the problem of impulsivity than on

the equivalent problem of anxiety, because much of the basic descriptive work remains to be done. In the case of anxiety, we have a well-delimited set of clinical symptoms whose personality correlates have been established for many years. Consider how difficult it would be to pursue any kind of detailed argument if what we were trying to explain was, not the personality characteristics of dysthymics, but those of psychiatric patients, with all their assorted syndromes, from schizophrenia to psychogenic ulcers. Yet this is the kind of situation with which we are faced at the opposite pole of the E–I dimension. Eysenck's (1964a) theory of criminality was meant in the first instance to apply to most if not all criminals, together with a wide range of other kinds of anti-social individuals. It could hardly have been otherwise, since only very recently has there been any kind of attempt to produce a personality taxonomy that would distinguish between individuals who commit different kinds of anti-social acts (e.g. Eysenck and Eysenck 1976, Chapter 8; Shapland 1978). And the problem is compounded by recent evidence that both criminals (Eysenck and Eysenck 1976) and individuals who score high on questionnaires measuring impulsive and sensation-seeking behaviour (Eysenck and Eysenck 1978) are high P scorers. For this carries the implication that the data relating individual differences in the diurnal rhythm in arousal level (Revelle et al. 1980) and in eye-blink conditioning (Eysenck and Levey 1972) to Imp (in the sense of high N, high E) do not necessarily apply to individuals who show impulsive behaviour of interest in the real world (criminals, psychopaths, drug-users, etc.). Thus it may be that we have not one but two half-completed puzzles: a biologically interesting personality trait (old-style Imp) searching for the real-life behaviour that it produces; and some interesting real-life behaviour searching for the biologically based personality trait (new-style Imp?) that produces it.

In the absence of clearly posed questions it is impossible to offer useful answers. Thus, until the taxonomy of anti-social behaviour and its personality correlates has been cleared up, there is no point in trying to develop an extensive theory of the kind now possible in the case of dysthymia. Thus I shall content myself with a brief recapitulation and reconsideration of a previously proposed hypothesis (Gray 1970a, 1973).

This hypothesis has already emerged in the preceding discussion. It treats the dimension of impulsivity (i.e. the axis from Eysenck's stable introvert to his neurotic extravert quadrant) as one of increasing sensitivity to signals of reward or of the active avoidance of punishment (Fig. 8.2). On this hypothesis, impulsive behaviour occurs because of the attractions of the rewards which it is likely to procure, a proposition which has a certain face validity. This hypothesis could be seen as most naturally applicable in Eysenck's neurotic extravert quadrant, since it is here that the absolute sensitivity to reward is thought to be highest. Alternatively, it could be applied to the extravert in general, since *ex hypothesi* the balance between the attractive effects of reward and the deterrent effects of punishment favours reward in extraverts no matter what the N score. A third, equally logical, possibility is to treat the *stable* extravert as being likely to engage in impulsive behaviour, since it is in this kind of individual that the deterrant effects of punishment are smallest; though this line of argument need have nothing to do with the *factor* of impulsivity at all, since it can be derived straight-forwardly from Gray's (1970a) model of anxiety, of which it is simply the negative pole (Fowles 1980).

That all these lines of argument can be used is not necessarily a disadvantage to Gray's (1970a) model, since there is evidence both from the study of psychopaths (Hare and Schalling 1978) and from questionnaire studies of self-reported impulsive behaviour (Eysenck and Eysenck 1978) that two kinds of extraverted individual, both prone to impulsive behaviour, can be differentiated, one with high N scores, the other with low N. In the field of psychopathy, these have been termed primary (low N) and secondary (high N) psychopaths; and the primary psychopath in particular has been shown, as Gray's (1970a) model predicts, to be particularly poor at forming passive avoidance responses when exposed to punishment (Hare 1978). In the questionnaire studies, the differentiation has been between individuals who merely

act on the spur of the moment (high N) and those who are prepared to take real risks (low N), e.g. by parachute-jumping, scuba-diving or drug-taking (Zuckerman 1979; Eysenck and Eysenck 1978). This pattern of findings, too, is in accord with the view that threat will be least effective in the stable extravert. Further evidence which gives general support to this position may be found in a recent review by Fowles (1980). He has examined the literature on the conditioning of autonomic responses and concludes (1) that the conditioning of the galvanic skin response (GSR) is particularly closely related to Gray's (1978) BIS (note the concordance between this conclusion and our earlier discussion of orienting responses, of which the GSR is a prime example: Sokolov 1960); and (2) that extraverts, and psychopaths in particular, are usually poor at GSR conditioning (see also Hare 1978).

No necessary complication is brought into this picture by the fact that, in both the real-life studies of psychopaths (Eysenck and Eysenck 1976) and the questionnaire studies of impulsive behaviour (Eysenck and Eysenck 1978), the individuals I have treated in the previous paragraph as being high on E are also high on P. For one could simply rotate the plane in which the dimensions of anxiety and impulsivity (Gray 1970a) lie to an appropriate degree into the Eysenckian third dimension of P, thus introducing a positive correlation between E and P. This rotation would leave anxious individuals at the low pole of P (in accordance with observation: Eysenck and Eysenck 1976, p. 118) and so changes nothing in our previous treatment of anxiety. Such a rotation would treat the Eysenckian dimensions of E–I, N and P as frames of reference, analogous to lines of latitude or longitude, while locating the lines of causal influence and real-life phenomena (analogous to mountain chains or rivers) elsewhere.

If one were to rotate the anxiety and impulsivity dimensions into the P dimension, along the lines just suggested, one could perhaps finesse the difficulty posed by the relation of Imp to individual differences in the diurnal rhythm in arousal level, with its attendant problem of deriving the stable personality characteristics of the psychopath from what is apparently a mood factor. For present indications are that the changes in the location of Imp in the three-dimensional space defined by E, N and P, recently suggested by Eysenck and Eysenck (1977 1978) in order to bring out more clearly its relation to P, have weakened the capacity of this factor to predict the diurnal rhythm in arousal (Revelle, personal communication, 1979).

Finessing these problems in this context will not, of course, make them disappear entirely. We shall still need an explanation of the relation between high E/high N scores to the pattern of low morning arousal and high evening arousal described by Blake (1971), Patkai (1970), Folkard et al. (1976) and Revelle et al. (1980), and also some understanding of the real-life syndromes to which this pattern gives rise.

There is, however, no obvious reason why these syndromes should include anti-social behaviour. If the assocation between psychopathy and the diurnal rhythms in arousal level throws any light on the relation between personality traits and the real world, this light illuminates, not the nature of the psychopath's conduct, but the organization of his working day. In fact, we are suddenly in possession of a plausible answer to an interesting question which, so far as I know, no one has thought to ask before: why do we work in the day and play at night? On the face of it, there is nothing to stop us organizing parties, gambling sessions, pub-crawls or sexual orgies at 8 a.m.; then, sated with the pleasures of the day, we could start work in the afternoon. I stand upon correction from the anthropologists, but I suspect that human beings universally prefer the opposite arrangement: work first, parties later. Might the explanation be that this arrangement is well-suited to the peak arousal levels of both the low-impulsive individual (who likes to work) and his opposite number (who prefers parties)?

This line of thought offers some illumination, then; but the substantive problems remain. Does a person's particular pattern of diurnal change in arousal level have anything to do with his predisposition towards or against anti-social conduct? If there is any relation between sensitivity to reward and anti-social conduct, how, in turn, does sensitivity to reward relate to the pattern of low morning/high evening

arousal? And, perhaps most importantly of all, what is the relation between individual differences in diurnal rhythms in arousal and stable differences in arousal level? This last question can be posed more acutely: *are* there stable individual differences in arousal level? Certainly, the literature summarised by Eysenck (1967) and in the present volume gives overwhelming support to the proposition that such differences exist and that they are correlated with E–I. But, in the great majority of cases, time of day was not a variable in the relevant experiments and is probably not even mentioned in the description of the methods used. Given that experiments are normally performed as part of the working day, it is reasonable to suppose that nearly all published reports refer to times at which the diurnal rhythm in arousal would favour the introvert or be neutral as between the extremes of the E–I dimension. This would give rise to the general pattern of the literature: for any given experimental task, about half the studies report the result that would be predicted from a hypothesis of higher introvert arousal, while the other half report no effect of E–I. The only experimental measure reported to show higher introvert arousal at all times of day, so far as I know, is salivation in response to lemon juice (Corcoran 1972): but one swallow, even of lemon juice, does not make a summer.

This amounts to a formidable uncertainty in the data; and an equally formidable research programme will be required to resolve it. It will be necessary to take systematically all the tasks which have given data that fit with the arousal hypothesis of E–I and reinvestigate them in both the early morning and the late evening. Preferably, this should be done with subjects whose sleeping and waking times are controlled, as in Blake's (1967) original report. At the same time, sufficient personality tests will need to be administered, and their subsequent analysis will need to be sufficiently sophisticated from the factor-analytic point of view, for it to be possible to determine which factor within the overall Eysenckian three-space is best correlated with individual differences in the diurnal rhythm in arousal and which (if any) with stable individual differences in average arousal level.

Until some such research programme has been conducted, it will be difficult to comment with any precision on the remaining question that needs to be asked about the alternative to Eysenck's theory that has been sketched in the foregoing pages: 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? On the assumption that a re-examination of these data, taking into account diurnal rhythms, reaffirms the importance of stable individual differences in arousal level, I think it clear that the answer to this question must be 'no'.

To take but one example, the evidence is good that (during working hours) sensory thresholds are lower in introverts than extraverts (Eysenck 1967). This is easily explained by the assumption that introverts have higher arousal levels. Any account in terms of sensitivity to threat of punishment or non-reward would, in contrast, be tortuous, assuming it to be viable at all. It is just possible that an account in terms of orienting responses to relatively novel stimuli (which Gray's 1970a model would predict to be higher in introverts than extraverts) could make some headway, given that sensory thresholds are lower during the occurrence of ORs (Sokolov 1960); but in the absence of specific data on the role these responses might play in determining differences in sensory threshold as a function of E–I, this is purely speculative. If experiments were to show (a) that introverts show stronger ORs to threshold sensory stimuli than extraverts and (b) that this produces their lower sensory thresholds, this would raise the possibility that Gray's (1970a) model could account for the sensory threshold data as well as Eysenck's. But, even supposing the experiments turned out that way, there are many other findings for which this particular approach would not pay off, so that it would be necessary to dethrone arousal theory piecemeal at each point. It is difficult to see that such an *ad hoc* approach would have any merit.

Note, however, that, as I showed in the first part of this Chapter, arousal theory has itself been unable to account for a number of findings that have been left over from the 1957 stratum

of Eysenck's work: for example, Spielman's (1963) demonstation that extraverts display more pauses on a tapping test; the greater reminiscence effect on the pursuit rotor in extraverts (e.g. Star 1957); or the evidence that, under rather special circumstances (Broadbent 1961), extraverts show a larger kinaesthetic after-effect than introverts (Eysenck 1955). It would appear, then, that no single theory can at present account for all the available data.

Faced with such a situation, it is natural to try an amalgam of several theories. This, for example, is essentially the strategy adopted by Brebner and Cooper (1974). After a careful review of the literature (disclosing some of the same shifts in the emphasis of Eysenck's theory over the years that have been noted in the present chapter), and taking into account some new findings of their own in a reaction time task, these workers propose that one should distinguish between central mechanisms involved in stimulus (S) analysis and in response (R) organization. Both mechanisms can be in a state of inhibition or excitation, generating four variables to describe the state of the organism: S-excitation and S-inhibition, R-excitation and R-inhibition. Roughly speaking, S-excitation corresponds to arousal level and R-inhibition to reactive inhibition; accordingly, introverts are thought to be normally higher on S-excitation and extraverts, on R-inhibition. In addition, however, Brebner and Cooper (1974, 1978; Brebner and Flavel 1978) take their data to indicate that extraverts are higher on R-excitation and S-inhibition. It would take us beyond the confines of this chapter to go into this approach more fully, and the reader is referred to the original papers. There are, however, dangers in this approach: a consortium of hypotheses tends to render any one of them untestable.

It is possible that the problem, as I have posed it, has no solution because none is necessary. We have learned from the work of Martin and Eaves (1977) in the abilities domain that there may be biological reality both at the super-factor and at the subfactor level (see the discussion earlier in this chapter). Thus the theory of anxiety I have advocated may be able to coexist peacefully with a theory of E–I in terms of stable individual differences in arousal level. High sensitivity to signals of punishment, non-reward and novelty would then be one of the subfactors contributing to general arousal level; indeed, it would be surprising if such a subfactor did *not* contribute to general arousal level. In the last analysis, then, the hypothesis that anxiety consists in heightened sensitivity to these kinds of signals might be subsumed into a general theory of individual differences in arousal level. But, before this can be achieved, much further research into the joint taxonomy of tasks and traits, along the lines indicated above, will be needed. Theories such as Brebner and Cooper's (1974) would need testing in the same way: an untestable consortium of hypotheses would become a powerful theory if it could be shown that each hypothesis corresponds to a separate subfactor of E–I.

8.5 Coda 1: Strength of the Nervous System

It is to be hoped that, if anyone undertakes this daunting task of taxonomy, he will include in his battery of tests some that are good measures of SNS (Nebylitsyn 1972). For this Pavlovian dimension of personality has at least as good a claim as any other to represent stable individual differences in level of arousal (Gray 1964a, b). But note that all of the caveats voiced above concerning the status of E–I in this respect are equally valid for SNS. The Russians, to whom we owe virtually all our knowledge of this dimension of personality, have an introvert working day like the rest of the world. Their descriptions of method have nothing to say about the time at which their experiments are run; but it is safe to assume that they are not normally run late in the evening. Thus it is possible that, if they were to examine the influence of time of day on their experimental results, this would be just as profound as it has been in Western research.

If we were to assume, nonetheless, that both SNS and E–I represent stable individual differences in average level of arousal, and that they

do this at the same point in the factor-analytic hierarchy, it follows that they must be the same factor. This hypothesis has been proposed before, introverts being identified with individuals with a weak nervous system (Eysenck 1967; Gray 1967). The evidence that I reviewed in 1967 was largely, but not completely, in its favour. Since that time, there has been sadly little addition to the relevant empirical material, and the balance of probabilities has remained much the same. Data on reaction time as a function of stimulus intensity (a measure on which individuals with a weak nervous system show a relatively shallow increase in reaction time as stimulus intensity decreases) have not supported the hypothesis either in Western (Mangan 1972) or in Soviet (Zhorov and Yermolayeva-Tomina 1972) research. On the other hand, Shigehisa and co-workers (Shigehisa and Symons 1973; Shigehisa et al. 1973) obtained results comparing introverts and extraverts which bear a striking resemblance to those reported by Yermolayeva-Tomina (1964) comparing the weak and strong nervous system; the relevant experiments investigated the effects on visual and auditory thresholds of simultaneous heteromodal stimulation. Also on the positive side of the account is Frigon's (1976) report that introverts resemble individuals with a weak nervous system (Nebylitsyn 1972) in showing greater extinction with reinforcement of a conditioned alpha-blocking response in the EEG. It is clear that this problem calls for very much more research.

8.6 Coda 2: Psychoticism

Although the third Eysenckian dimension, P, does not strictly fall within the scope of this book, we have strayed so close to it in the foregoing discussion of impulsive behaviour that some final words are needed to show how this dimension might fit within the general lines of the approach that has been adopted. Two hypotheses have been advanced which are relevant to this issue.

The first (Gray 1973) is that P reflects an increasing tendency towards aggressive behaviour in response to unconditioned punishment or frustrative non-reward. As in the case of anxiety, a corresponding brain system (the 'fight-flight system') was proposed, consisting of structures in the amygdala and medial hypothalamus. Both parts of this hypothesis were speculative at the time it was put forward, and they remain no less so today. If one wishes to make use of it in connection with the foregoing discussion of impulsivity, one might propose that both primary (low N, high E) and secondary (high N, low E) psychopathy would gain an increasingly aggressive colouring as the P score rises. But this can be regarded as no more than a suggestion for future research.

The second hypothesis has better empirical support. On the basis of certain very curious dissociations that have been observed between autonomic measures and more 'central' measures of arousal in schizophrenics, high P scorers and normal individuals given the psychotomimetic drug, LSD-25, Claridge (1967, 1981) has suggested that psychotic states arise from a breakdown in the homeostatic relations between a 'tonic arousal system' and an 'arousal modulating system'; the latter is given the tasks of maintaining tonic arousal at levels appropriate to the subject's environmental situation and of regulating the subject's response to sensory input. This is not the place to go into these proposals in any more detail (see Claridge 1981). I shall comment here on only one feature of the hypothesis.

As pointed out by Claridge (1981), there is an interesting resemblance between his model and my own notion (Gray 1972) of a feedback loop between the ascending reticular activating system and the septo-hippocampal system (Fig. 8.4). Claridge's (1981) suggestions is that the SHS corresponds to his arousal modulating system (it is of course already common ground that the tonic arousal system corresponds to the ARAS). If this view were correct, the system depicted in Fig. 8.4 would contribute to arousal level (and so to E–I) in virtue of the net level of activity achieved when the feedback loop is operating correctly; and it would contribute to psychotic behaviour to the extent that the breakdown in the feedback loop allowed unusual states of arousal to arise. In support of his

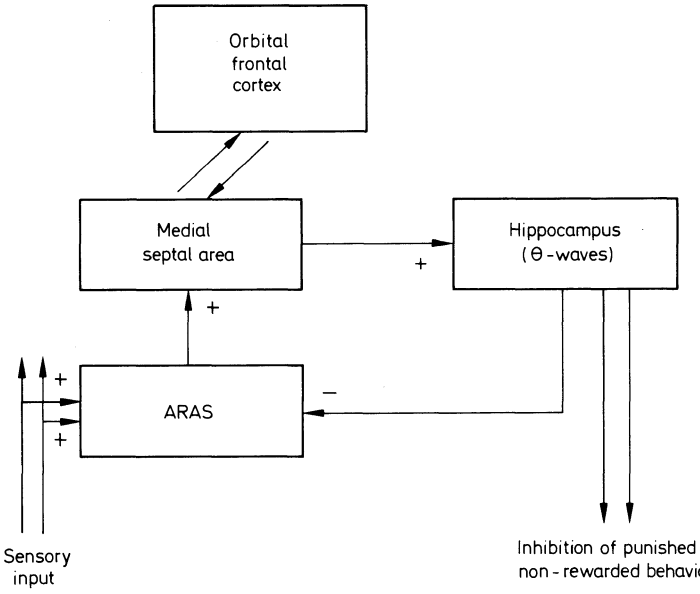

Fig. 8.4. Negative-feedback loop whose activity is presumed to underlie (a) sensitivity to signals of punishment and non-reward and thus also (b) individual differences in 'anxiety' (Fig. 8.2). (Gray 1972)

proposal, Claridge (1981) points out that a number of other lines of evidence have suggested the possibility of hippocampal dysfunction in schizophrenia (Venables 1973). This hypothesis has, however, one major disadvantage. By attributing both E–I and P to the same brain structures (albeit functioning in different modes), one runs counter to the data that support the independence of these dimensions and the independence of the psychiatric states to which they correspond, i.e. neurotic anxiety and psychosis (Eysenck and Eysenck 1976). Nonetheless, Claridge's proposal is an intriguing one which deserves further experimental investigation.

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Epilogue

H.J. Eysenck

The contents of this volume will have demonstrated, more than any argument could do, that the dimensional and causal analysis of personality has produced a paradigm which shows all the classical signs of a scientific theory – including the presence of many anomalies and the suggestion of improvements in the theory by certain changes in some of its defining features. The chapter by Gray is particularly valuable in pointing up both the anomalies and the existence of alternative hypotheses. He does not mention one slightly embarrassing aspect of the paradigm, which can best be characterized as its almost excessive over-inclusiveness; there are many experimental findings which agree vaguely with what one might have anticipated, but which cannot in any rigorous sense be deduced from the theory. One or two examples may clarify this point.

Kiff et al. (1980), working at a special unit for the study of respiratory virus infection, studied 52 volunteers who were housed in special accommodation, isolated from external sources of such infection. Subjects were quarantined for 3 days and then inoculated with two rhinoviruses in succession; dependent variables were symptoms and virus shedding, a rigourously objective measure of degree of infection. Of a large number of psychological measures and tests used to predict infection, only extraversion showed a strong degree of relation to the criteria used, with introverts subject to heavier colds than extraverts. It may be possible to explain this finding after the event by suggesting that the mechanism of *augmentation* discussed previously on these pages may have played a part, but such an explanation would be largely verbal; the link between personality theory and observed effect is almost wholly missing.

Again, consider a recent experiment (Franzen 1979), in which the author predicted (and found) that introversion, as measured by various questionnaires, was very significantly correlated with sodium content in blood serum. A large sample was used, and the experiment was replicated, with positive results, on a second large sample; the fact must be accepted as correct. Explanation of the observed correlation, on the other hand, is difficult. There is a line of argument which relates the role of sodium ions to the propagation of the nervous current through neurons, and this is the line taken by Franzen; however, the connection is somewhat nebulous and defies rigorous theoretical statement.

As a third example, consider the correlation between field dependence and extraversion (e.g. Loo and Townsend 1977). It seems quite reasonable that people who are extraverted, i.e. relating to the external environment, should be more field dependent, seeing that the 'field' in question is external, but this again is a purely verbal solution to the problem; no rigorous deduction can be made from any of the theories mentioned in this book.

As a last example, taken on purpose to illustrate the extraordinarily wide range of phenomena covered by the concept of 'extraversion', we may take a study by Harley and Sargent (1980) on ESP performance in the Ganzfeld. They found that extraverts did significantly better than introverts in two separate experiments. This result is in agreement with Eysenck's (1967b) prediction, and in line with much other work summarized by Palmer (1977); nevertheless, it cannot be said that the relation follows a very rigorous line of argument. (Altogether, there have been 12 studies producing significant correlations between E and ESP; all of these

have been in the positive direction.) The best that one can say in each of these widely different cases is that if extraverts were to score differently from introverts, then this is the direction one would expect the difference to take. There are literally hundreds of such facts in search of a theory (Morris 1980); an *embarras de richesses* which demonstrates the weakness of any proposed theory as clearly as do the anomalies and failures listed by Gray.

While thus admitting the chrysaline state of theory construction, I would not necessarily accept some of the arguments put forward by Gray as leading to an abandonment of the arousal model of introversion. The difficulties raised for the model by time-of-day changes in arousal are potential rather than actual; I have already referred in the first chapter to M.W. Eysenck and Folkard's criticisms of the Revelle study, on which Gray largely relies. These are so far-reaching as to make its citation in criticism of the arousal hypothesis premature. Much work will certainly have to be done to clarify the issue, but to date this is no more than a small cloud upon the horizon; it cannot bear the burden placed upon it by Gray.

Gray's own positive contribution is, I think, a major one, but it can probably be accommodated within the arousal model of introversion. If introversion is regarded as a personality factor mediating increasing levels of sensitivity to signals of *punishment* and extraversion as a personality factor mediating increasing levels of sensitivity to signals of *reward,* this leaves neuroticism as a factor closely related to sensitivity to all sorts of signals; I doubt whether the relation proposed by Gray would really alter things very much or would explain a larger number of phenomena. We would have to explain why arousal was related in this manner to punishment and reward, but this could be done along the lines of my prediction regarding the extraverts' greater toleration of pain – indeed, this prediction embraces Gray's findings (Eysenck 1967a). Sensitivity to rewards is more difficult to explain, but the same is true of Gray's theory in this respect.

Gray's version of the theory encounters some experimental difficulties which are not faced by the arousal theory. For example, the Barr and McConaghy (1972) study mentioned in Chap. 5 finds a positive correlation between aversive and appetitive conditioning; this fits in well with the arousal theory but goes counter to Gray's. We may perhaps agree that more decisive crucial experiments are needed to decide between the two versions of the theory; at the moment the original version can probably account for a larger number of findings, but there are some areas in which Gray's formulation appears to fit better.

What has been said of Gray's version of the theory may also be said of that proposed by Brebner and his colleagues. Their contrasting of stimulus analysis and response organization, with introverts high on stimulus analysis (arousal) and extraverts higher on response inhibition (reactive inhibition), constitutes an interesting attempt to combine the arousal model with the inhibition model that preceded it. This is an ingenious proposal that does account for many of the facts; but it suffers from the difficulty of explaining why these two mechanisms are so closely related and what it is that ties them together.

It is not the purpose of this epilogue to argue the advantages and disadvantages of these various theories and hypotheses; it will be clear from a careful reading of the various chapters in this book, and particularly that contributed by Gray, that all theories have areas in which they are successful, others in which they encounter difficulties. This is a predictable consequence of the fact that we are here dealing with a scientific paradigm; such paradigms can be recognized by the fact that while many if not most facts can be aligned with a particular theory, there are many anomalies, and hypotheses are put forward in abundance to take care of these anomalies. The fact that all those concerned in the debate argue within a certain systematic framework of fact and theory, a nomological network constituted of agreed methods of experimentation and argument, signifies that this paradigm satisfies the criteria put forward by Kuhn.

I am more concerned here to consider the consequences of the existence of the paradigm for psychological research in the field of personality. If it be agreed that we are here dealing

with a paradigm, then we should be able to enjoy all the advantages of 'normal science', i.e. the concentrated problem-solving attack on the remaining anomalies, which, according to Kuhn, characterizes normal science and constitutes perhaps its greatest advantage. Accordingly, we may perhaps expect in the future, even more than in the past, a determined effort to subject the remaining problems and difficulties, so well outlined by Gray, to an experimental attack which would concentrate on the differential predictions made by the various theoretical statements. It is this sort of attack that scientists are particularly good at, and we may hope that many if not most of the outstanding problems will find a ready answer.

In the course of such research the original theory is likely to be savaged beyond recognition; that is the usual fate of theories in science. Theories are stepping stones on the way to better experiments and the establishment of general laws; they are necessary but expendable. Modern psychologists tend to disparage theories; I believe they are wrong in doing so. Darwin, as always wise in his counsel, gave this advice to budding scientists: "Let theory guide your observations, but till your reputation is well established be sparing in publishing theory. It makes persons doubt your observations." And as his self-appointed bulldog. T.H. Huxley, pointed out: "Those who refuse to go beyond fact seldom get as far as fact." Facts are all-important in science, but they can never be completely separated from a theory, which gives them meaning and status. Hence the importance of theory in creating a paradigm; without theory no paradigm.

Psychologists have grown very sceptical of theories, particularly wide-ranging theories such as the Hullian, no doubt on the maxim: Once bitten, twice shy. But such scepticism can be overdone and can become harmful for the future development of science. To quote Darwin again: "I am not very sceptical – a frame of mind which I believe to be injurious to the progress of science. A good deal of scepticism in a scientific man is advisable to avoid much loss of time, but I have met with not a few men who, I feel sure, have often been deterred from experiment or observation which would have proved directly or indirectly serviceable." Critical, yes; sceptical, no. Criticism is the life-blood of science, but it must be constructive. Scepticism is negative and destructive; it does not advance the work of science.

Is the hope that in future the work of 'normal science' will quickly and securely advance our knowledge of personality optimistic or realistic? Psychologists are much less subject to the discipline of science than are physicists or astronomers; they only rarely gear their research to the problems thrown up by the progress of their science, but prefer to jump on passing band waggons, which are as mysteriously abandoned after a while – usually just when a paradigm is on the point of being created. Why is there no research on level of aspiration, which once claimed a rich harvest? What happened to cognitive dissonance, just when a higher synthesis seemed on the horizon? Why has work on the Ash-type phenomenon mysteriously ceased? As a paradigm advances, the going begins to get rough; anomalies accumulate, and serious thought has to be given to changes in theory. It seems to be at this point that students (and their advisers) get discouraged; just when a major research effort, guided by novel hypotheses, seems to bring great rewards within reach, everyone seems to lose interest, and the budding paradigm is cut off in the prime of life. It cannot be denied that when a paradigm reaches an advanced stage, as happened to that discussed in this book, considerable theoretical difficulties arise which require deep thought, a considerable knowledge of the literature and originality of approach. The constraints of established facts make so many hypotheses untenable that only deep, long-continued deliberation will suggest alternative solutions. Such thought is anathema to many Ph.D. students, whose main ambition is the acquisition of a marketable degree; hence the preference for easy, new topics which require nothing more than a superficial knowledge of psychometric or experimental technique, but which make no real contribution to science. Hopefully the future will see an improvement in this situation and a revived interest in theory, in the establishment of paradigms and in the design of crucial experiments, capable of deciding between alternative hypotheses.

Another difficulty in future research in this field is the inevitable growth in the size and complexity of experiments. Consider simply the requirements imposed by the existence of three major dimensions of personality, each of which should be represented in the design at three levels – high, medium and low – in order to guard against curvilinear regressions. This gives us 27 groups; with sex taken into account, we have 54 groups, and even so we are disregarding possibly important differences in intelligence! If each group contains a minimum of five subjects (and this is a very modest requirement, even in an analysis of variance design), we require 270 subjects, carefully selected from a much larger number in order to satisfy the requirements of the design for special combinations of P, E and N scores!

But this of course is not all. Time of day requires replication of the experiment, half the sample being tested in the morning, the other half in the evening; this might be difficult to accommodate within even such a large sample and may require duplication. Most experiments would, in addition, require several parameter values to be explored; even setting the number at only three, this would treble the sample size. It will be clear that the easy-going, small-scale type of experiment, carried out by Ph.D. students in a relatively short period of time, using a dozen subjects or so, is not likely to give us the kind of information required; something on a much larger scale is needed. This is of course what has happened in physics and the other hard sciences; it is unlikely that psychology can escape the same fate. Unfortunately, the whole organization of research in psychology is geared to an earlier, less concentrated approach; it will be difficult to shift over into a higher gear. It is possible to cut the ideal design down to size in various ways, but much thought will have to be given to the best ways of doing so without losing vital information. Keeping values of theoretically less important personality variables constant at some intermediate value would reduce the burden drastically, but would also make impossible the investigation of interaction effects between personality traits, which is one of the most important, but least investigated, aspects of personality study.

It may be asked, quite seriously, whether such great effort in time and energy is really worth while. Perhaps we are dealing here with evanescent phenomena, characteristic of certain limited populations, for a limited period of time; other cultural groups, or other temporal periods, might throw up quite different dimensions of personality? This seems distinctly unlikely. In the first place, as I have tried to show in the first chapter, these major dimensions of personality have a history going back over 2000 years and probably even further; we can clearly recognize modern personality 'types' in ancient writings. Secondly, the relation between personality and genetically determined anatomy and physiology (and even biochemistry) suggests that we are dealing here with something very fundamental, anchored in our biological nature. And thirdly, there is evidence that identical dimensions of personality can be discovered in many countries and nations differing profoundly from the English-speaking groups from whom most of the experimental evidence is derived.

Eysenck and Eysenck (1981) have summarized the results of 14 studies, employing almost 15000 subjects, in which adults and children from many different countries (India, Japan, Nigeria, Brazil, etc.) were administered the EPQ (Eysenck and Eysenck 1976); intercorrelations were calculated between items for males and females separately, and factor analyses performed, followed by Promax rotations. Table 1 below shows the indices of factor comparison for each group, separately for P, E, N and L, comparing each foreign sample with the original English standardization group; also given are some values for an English quota sample group. It will be seen that with very few exceptions the indices of factor comparison are above the value of 0.95, and indeed most are above 0.98. This shows fairly conclusively that we are not dealing with some shadowy wisps easily blown away by the wind, but with substantial, ever-recurring patterns of human behaviour grounded on firm biological foundations.

The almost universal similarity of mental disorders, whether neurotic or psychotic, indicates not only that these are biologically determined, but also suggests that relationships established

Table 1. Indices of factor comparisons for born-cultural comparisons

	P	E	N	L
1. English standardization males v. English quota-sample males:	0.99	1.00	1.00	1.00
English standardization females v. English quota-sample females:	0.99	1.00	1.00	1.00
2. English males v. Yugoslav males:	0.97	0.97	1.00	0.98
English females v. Yugoslav females:	0.97	0.99	1.00	0.99
3. English males v. French males:	0.98	1.00	1.00	1.00
English females v. French females:	0.97	1.00	1.00	0.98
4. English males v. Indian males:	0.97	0.99	0.99	0.96
English females v. Indian females:	0.95	0.99	0.99	0.98
5. English males v. Greek males:	0.94	0.99	0.98	0.98
English females v. Greek females:	0.89	1.00	0.96	1.00
6. English males v. Nigerian males:	0.98	0.99	0.99	0.98
English females v. Nigerian females:	0.66	0.91	0.92	0.93
7. English males v. Portuguese males:	1.00	0.99	1.00	1.00
English females v. Portuguese females:	0.99	0.98	1.00	0.99
8. English males v. Australian males:	0.93	1.00	0.99	0.99
English females v. Australian females:	1.00	1.00	0.99	0.99
9. English males v. Iranian males:	0.98	0.99	0.99	1.00
English females v. Iranian females:	0.94	1.00	0.98	0.99
10. English males v. Brazilian males:	1.00	0.99	1.00	1.00
English females v. Brazilian females:	0.99	0.98	1.00	0.99
11. English males v. Japanese males:	0.95	0.99	0.98	0.98
English females v. Japanese females:	0.99	0.99	1.00	0.99
12. English schoolboys v. Japanese boys:	0.96	0.99	0.99	0.89
English schoolgirls v. Japanese girls:	0.97	1.00	0.94	0.98
13. English boys v. New Zealand boys:	0.99	1.00	1.00	1.00
English girls v. New Zealand girls:	0.99	0.98	1.00	1.00
14. English boys v. Spanish boys:	0.97	0.98	0.99	0.99
English girls v. Spanish girls:	0.92	0.98	0.99	0.99

in European countries and in the United States may have wider validity (Al-Issa 1981). Gray has mentioned some of the problems raised by the hysterical and psychopathic disorders, in connection with their position in a dimensional system of personality description; recent articles by Eysenck (1980a, d) discuss the evidence and suggest theories of the genesis of these disorders that are perhaps a little more defensible than the earlier version criticized by Gray. There is little doubt that personality is an important variable in deciding who is to succumb to mental disorder and what kind of disorder he is likely to develop. Equally, there seems to be little doubt that personality is relevant to response to treatment (DiLoreto 1972). In my own theory, all successful therapeutic methods (behaviour therapy, psychotherapy, psychoanalysis, logotherapy and even the events which produce spontaneous remission) are mediated by the extinction process (Eysenck 1980b); this links up directly with the theory that neurotic disorders are essentially produced by Pavlovian conditioning (Eysenck 1980c). The implications for personality theory are clear, as explained briefly in Chapt. 5; extinction has interesting correlations with personality and with acquisition. Gray prefers a rather different account, using the concepts of sensitization and habituation, rather than conditioning and extinction. Here again the differences are probably less clear-cut than they appear at first, and crucial experiments will be difficult to design. Here, as elsewhere, we are in the middle of what may be the most interesting phase in the development of a far-reaching theory; there are indications that the theory is along the right lines, but there are alternative versions which cry out for crucial experiments to be performed.

In summary, we may say that it is over 2000 years since man first speculated on the major dimensions of personality and recorded obser-

vations that raised hopes of eventually being able to give a scientific account of individual differences in character, temperament and behaviour. It is only recently that proper experimental studies have subjected the resulting theories to critical examination. Let us hope that the much-increased tempo of research into this topic, which has been characteristic of work in this field, will continue and that better theories, leading to general laws, will emerge from this concentration of scientific effort on one of the most important, and most interesting, paradigms of our young science.

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