INDIVIDUAL DIFFERENCES AND SENSITIVITY TO CUES OF REINFORCEMENT

BY

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ABSTRACT

Mischel (1968, 1973), challenged the trait approach to the study of individual differences by claiming that traits lacked predictive utility and that people did not display the degree of consistency pre-supposed by traits. It is suggested here that often the real difference between those who espouse the idiographic and those who espouse the nomothetic approaches is one of emphasis. It is, therefore, proposed that a theory which provides a biological basis for differential conditionability might prove to be a useful point from which to start building a theory of personality which integrates these two approaches.

Two theories, Eysenck's and Gray's, initially looked promising and so predictions drawn from these were compared in a series of three conditioning experiments. No support was found for Gray's theory, and although a degree of support was found for Eysenck's theory it was concluded that this theory lacked heuristic value. People were found to condition with remarkable efficiency, however.

Experiments four and five followed up some speculations as to the nature of anxiety, looked at consistency both behavioural and self-rated and again put predictions drawn from Gray's theory to the test, but this time abandoning Gray's assumption that differential sensitivity to cues of reinforcement is related to the introversion-extraversion dimension, and looking at differential sensitivity as a performance variable.

Evidence was found supporting Gray's speculation with respect to differential sensitivity.

No evidence of behavioural consistency was found, and no support for a general factor of consistency was found when self ratings of consistency were examined.

Cognitive variables did, however, appear to be important in determining behaviour.

It was concluded that Eysenck's theory was not robust enough to form the basis of the type of theory proposed here. Gray's theory needs some major modifications, especially in relation to the assumed relationship between active avoidance and appetitive reinforcement.

It seems that whether or not consistency will be observed is determined by many factors, not least of which is the complex relationship between situational and cognitive variables.

It is suggested that conditioning may well play an important part in determining behaviour and it might prove fruitful to follow the lead offered by Pavlov, Teplov and Nebylitsyn and move from properties of conditioning and of the nervous system to theories of personality, rather than the other way round.

It is also suggested that we should be working towards a theory which explains both why, and in what ways people were different and similar, a theory which set itself the target of describing and explaining the relationships between individual abilities, conditioning and cognitive factors, such as the use made of strategies.

CONTENTS

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CHAPTER I: THE CONSISTENCY PROBLEM

I:1 TRAITS AND CONSISTENCY:

"Penuriousness is economy carried beyond all measure. A Penurious man is one who goes to a debtor for his half obol interest before the end of the month. At dinner where expenses are shared, he counts the number of cups each drinks, and makes smaller libations to Artemis than any one..... If his wife drops a copper he moves furniture, beds, chests and hunts in the curtains..... (P)enurious men have hair cut short and do not put on their socks until mid-day; and when they take their cloak to the fuller they urge him to use plenty of earth so that it may not be spotted so soon. "(Theophrastus (372-287 B.C.) quoted by Bem & Allen, 1974).

Amusing as the above quotation might be, it serves also to emphasise the length of the pedigree of the trait theory. The famous physician Galen proposed the typology of the four humours: melancholic, choleric, sanguine, and phlegmatic. Immanuel Kant subscribed to the same classification, and more recently Wundt (1903) has observed: "The ancient differentiation into four temperaments.... arose from acute psychological observations of individual differences between people.... the four fold division can be justified if we agree to postulate two principles in the reactivity of affects: one of them refers to the strength, and the other to the speed of change of a person's feelings. "(pp.637-638; quoted by Eysenck & Rachman, 1965, p.17).

With such a long history and having been espoused by such illustrious adherents the trait theory is not to be lightly dismissed, nevertheless both this approach to the

study of personality and the personal consistency in behaviour which it is assummed to imply have recently attracted a considerable amount of criticism. Role theorists (e.g. Goffman, 1959, 1961, 1968; Sarbin & Allen, 1968; Mead, 1934) have stressed the variability of behaviour, but paid scant attention to the intra-organismic factors which might prove important in the production of behaviour.

Mischel's book "Personality and Assessment" (1968) has proved to be the herald of numerous articles both defending and attacking the trait approach to the study of personality, as well as serving to revitalize the search for new methodologies and paradigms in the field of personality study. He has challenged (1968, 1973) what he sees as being generally assummed in personality psychology: "that personality dispositions - or traits - the basic units of personality study are relatively stable, highly consistent attributes that exert widely generalized effects on behaviour." (Mischel, 1973, p.253). Having reviewed the relevant literature he proposed the thesis that situations are of critical importance in determining behaviour, and with the exception of responses dependent on intellective factors (e.g. cognitive style, problem solving etc.) correlations between responses made in different situations, by the same individual, are typically in the order of .2 to .3. Correlations too small to persuade one that people display consistent behaviour across situations.

Personality theory is not dismissed out of hand; psycho-dynamically oriented theorists, he acknowledges, have attempted to come to grips with the fact that behaviour is, to some degree, situation specific, by proposing a genotypic-phenotypic distinction. They have argued that "divers behaviour patterns serve the same enduring and generalized underlying dynamic or motivational dispositions.

"(Mischel, 1973, p.253). The approach, he admits, is "inherently logical", but it must be evaluated on the basis of the utility of its predictions. These predictions, he claims, have not been notably successful. In other words a theory is useful if it increases predictive power and psychodynamic theories have not been found to satisfy this criterion.

Psychiatric tests, he concluded, also have poor utility, and in illustration he reports a study by Lasky et al (1959) which discovered a correlation of .61 between incidence of rehospitalization and the weight of the patient's file holder, a relationship much stronger than with any reported test measured trait. Besides using this experiment to emphasize the poor utility of the trait approach, he also employs it as a salutary lesson in the dangers of assumming that a correlation reflects what it may not reflect - a causal relationship. Traits, then are "lost causes" and require, rather than provide, explanation.

Mischel's own position has not remained unassailed, and two points seem to have been most often critisized: (1) that individuals display no consistency in behaviour; (2) that situations are the primary determinants of behaviour. The focusing of criticism on these two points is unfortunate; Mischel, himself, has explicitly disavowed these as his own opinions (cf. Mischel, 1973, p.254, 262), and they add little to understanding in the area. Although in justification of the criticisms it must be said that Mischel does appear to modify his position with respect to the importance of situational influences between 1968 and 1973.

Alker (1972) attempting to champion the consistency cause quotes a study by Opton & Lazarus (1967) which, he claims, demonstrates that people are more consistent that Mischel

would allow. Whether or not this study does demonstrate consistency in behaviour is in itself questionable, but any such discussion is rendered unnecessary by the results of subsequent studies by Averill, Olbrich & Lazarus (1972). They report that two follow-up studies have failed to confirm the results of the original study.

Block (1968, 1977) has proved to be one of Mischel's severest critics. He even goes so far as to note that Mischel's review of the literature is "quite brief, 5,507 words and less than 16 pages.". "Obviously, Mischel's conclusion, whatever its degree of correctness, cannot be supported by so brief, selective and undetailed a literature presentation." (1977, p.42).

More seriously, however, as a criticism of Mischel's position, he reports findings of a study which collected data on individuals when they were in junior high school, senior high school and during their fourth decade. (These data are reported more fully in "Lives through Time" (1971)). From these longitudinal data he concludes that both data collected from well done observer rating studies and self report studies demonstrate impressive personal consistencies, and that these two types of data are found to be strongly related. The evidence for personal consistency from studies employing behavioural test responses is, however, "erratic".

Block, of course, sees data such as this as being seriously damaging for Mischel's cause, but there is infact no need for Mischel to object to any of these conclusions - they fit well enough with his general thesis. What is substantially different between the two authors is their emphasis, and the implications they draw from such data. As has already been observed, Mischel has never denied that there is some consistency in behaviour, it is the degree of consistency, which he feels is generally assummed to

to which he objects.

Similarly Mischel has continually objected that rating tests when they do not specify either the situation or the behaviour of interest function as projective test, with the result that traits have more to do with perception than behaviour, and a great deal to do with the use of labels. Mischel concedes that trait labels may in fact function as quite good descriptions of 'average' behaviour, but, as Block himself reports in relation to behavioural test data, they are poor predictors of what a person will do in a specific situation on any particular occassion. The major division between the two authors is Mischel's contention that regarding behaviour as caused by traits, and, consequently, as relatively stable, has not in fact proved to be a particularly fruitful approach. The problem in the causal trait approach is occassionaly glimpsed in Block's writing, particularly when wanting to claim that an underlying trait is real, he is forced into the position of having to contend that the observed behaviour is less important, dismissable, even, in a sense, unreal.

Mischel marvels at the extra-ordinary adaptiveness and resulting behavioural discrimination which enables the individual to deal with an ever changing environment, but the net result of this adaptibility, he believes, is the extreme difficulty experienced in demonstrating any impressive cross-situational consistency. Block's T-data reflect this discriminative facility, in Mischel's eyes, but, he suggests, that to Block "objective measures of on-going behaviour" are limited, trivial and artificial.

Alker (1972) and Bem (1972) have argued this point of specificity and cross-situational consistency by throwing quotations from Mischel at each other. Alker quotes Mischel

as saying: "Validity.... requires convergence between responses to maximally different independent stimulus measures. "(Mischel 1968, pp 13-14). On this he comments: "This statement is tantamount to asserting a direct antithesis between trait psychology and any instance of limited response generalization. "(Alker, 1972, p.8). Bem counters by quoting, "The results of construct validation research... provide evidence for the utility of categorizing behaviour in accord with the particular construct. "(Mischel, 1968, p.100).

Both authors seem to be obscuring rather than clarifying Mischel's general thesis by ignoring his theme and so placing statements outside their context. As already stated one criterion Mischel repeatedly uses in assessing the value of trait theories is their utility, as measured by their ability to predict future behaviour. With this in mind Mischel has pointed out that the commonly employed concepts of reliability and validity are not quite so distinct, in practice as opposed to theory, as is often implicitly assummed. When investigating reliability one looks at responses to maximally similar stimuli, e.g. the same test, or two halves or versions of a test presented on different occasions. When investigating validity one looks at responses to maximally different stimuli. Thus the distinction between the concepts of reliability and validity is, at least in part, a reference to the degree of generality of responding to which one wishes to refer.
It is with reference to the generality of responding, or the stimuli which trait theories have assummed to be equivalent, that Mischel concludes that people are, infact, relatively situation specific, or discriminating between stimuli. Mischel's position still allows, as a theoretical possibility, although Mischel himself obviously doubts that such a theory is possible, that a theory could exist which would predict

which stimuli, objectively distinct stimuli, would elicit identical responses from people, or at least some groups of people, Mischel's point, with reference to theories that exist, is the simple one, that trait theories have failed to do this. Rather than the quotation offered by Bem, I think Mischel's position is more succinctly expressed when he says: "It may be more meaningful to talk about the utility of both constructs and tests than their validity" (1968, p.100).

Partly in an attempt to weaken Mischel's empirical point that traits have not, in fact, facilitated prediction Block (1977) has complained that perhaps as many as 90% of the studies reported in the personality field are methodologically unsound, inadequate, "without conceptual implications, and even foolish" (p.39). The weaknesses he lists are: lack of proper operational definition; behavioural hypotheses which fail to reflect the complexisty of the concepts being studied, and the unreliability of measures employed; and the unnecessarily low power of the research designs employed.

Whether or not these points are valid, and there seems no reason to dismiss them totally, even if one feels Block is exagerating the situation somewhat, they serve to focus the attention of Block and others on the one point which Mischel easily counters - whether or not people show cross-situational consistency. Mischel's ready answer is that he has never claimed people where totally inconsistent. Similarly even if people are found to be totally consistent it does not prove that the trait approach is correct.

The real heart of Mischel's attack seems to be that to explain behaviour in terms of unobservable, infered, intro-organismic, relatively enduring variables is not a fruitful

approach. To a great extent many theorists (e.g. Alker, Block Epstein, Sarason, Watchel) who have contributed to this debate have allowed themselves to be side tracked by permitting the issues of the validity of traits and of consistency to become inextricably intertwined. They have, I feel, in the course of the debate allowed the issues of consistency and response equivalence to assume a more central and important position in the trait approach than, in other circumstances, might have been the case.

Mischel stressed the variability of behaviour in part to emphasize the important influence situational variables can have on behaviour. It seems that the trait approach has been employed to counteract this emphasis on situational importance. Indeed Bowers (1973), amongst others, has attacked Mischel for being a "situationalist", and assumming that all behaviour is dependent solely on situational variables. It is unfortunate that people are prepared to take these fixed positions; Mischel for his part has acknowledged that to say that trait theories are not useful is not equivalent to saying situations are all-determining.

The unfortunate nature of the position adopted by those trait theorist who stress consistency in response to Mischel's challenge is emphasised by Goldberg (1972), who claims that "an enormous amount of poppycock" has been expressed in the name of science, and that the "classical psychometric" position is that situations "constrain" individual differences, though the rank order of individuals on a trait should remain "relatively invariant across situations".

It is worth noting in passing that this "more reasonable" trait approach of Goldberg's is in fact nearer to what Argyle & Little (1972) designate as a dispositional

model and less an example of what Ekehammar (1974) calls "personologism", which is used "as a label for those advocating stable intraorganismic constructs, such as 'traits', 'psychic structures' or 'internal dispositions' as the main determinant of behavioural variation." Ekehammar includes both Alker (1972) and Watchel (1973) in this class of theorist.

Wallach & Leggett (1972) have also made the point that the validity of traits is a quite separate issue from the consistency one. In their case they have come down in favour of consistency, as opposed to situational specificity, but nevertheless against traits as the guiding principle. They argue that instead of looking at behaviours which it is felt might function as a sign of some hypothesized trait, we should be studying behaviours that require no justification beyond themselves as objects of study.

While on the one hand there is a deal of sound common sense in this view, one might even wish to claim that this is exactly what most experimental psychologists, in fact, do. On the other hand it appears to miss the point of the controversy. In studying specific behaviours, each from its own intrinsic interest, though we may well amass large amounts of informative and interesting data we will be no nearer to the answer of the question which is central to personality psychology - how behaviour, as a whole, is organized; why there are consistencies and variabilities. Of course there may be no overall organization of behaviour, but it is difficult to see how we will discover this with out asking relevant questions. If Wallach and Leggett do find that people are consistent how are they to explain the consistency?

In an attempt to demonstrate that people are consistent, and not simply situationally specific in the behaviour they display, Wallach and Leggett followed up some work by

Sechrest and Wallace (1964) which looked at "the stylistic aspects of drawing" - in this case the size of a drawing as defined by its height and area. Nursery school children were required to draw either a Santa Claus (experimental group) or a man (control group) on three occassions, 1st - 7th December, 15th - 21st December, 5th - 12th January. They report their results as supporting the consistency hypothesis and failing to confirm the situational specificity hypothesis. The results, however, are not unequivocal. There was a steady rise in the size of the Santa drawing over the three trials, while the size of the man showed a steady decline in size. The net result of this was, of course, that while the three trials were not found to be significantly different the trials x conditions interaction was found to be significant. This result must, I think, be explained before it can be claimed that the study demonstrates personal consistency.

In favour of the consistency hypothesis are the correlations between trials, these are typically in the .5 -.6 range. This suggests that there is infact considerable personal consistency. However, although these correlations are a substantial improvement on the typical .2 - .3 correlations, which Mischel reports, personal consistency is still contributing less than 40% of the observed variance and hardly justifies Wallach and Leggett's claim that the effect of any situational variables must, at best be quite small. Until we know with what situational similarities and differences we are dealing, what proportion of the variance was contributed by the mode of testing and responding, and to what degree these facilitate or hinder the emergence of consistent behaviour, it appears to be impossible to make any firm statement as to the magnitude of the situational effects.

This study is important, however, in that it failed to

replicate earlier findings which were regarded as supporting the situationalist position, although it is questionable whether situational specificity was, in fact, tested with all the testing sessions being so near to Christmas. Endler (1973) has also questioned the reliability of these particular behavioural responses, and in view of the fact that in two experiments different results were obtained comments: "the most reasonable interpretation at present (of this data) is that the results are inconclusive with respect to the consistency - specificity issue.... (I)nconsistent results from two different experiments do not mean consistent behaviour" (p.299).

Another approach to the consistency issue has been espoused by both Alker (1972) and Bem (1972). This is the Moderator variable strategy approach. Alker goes so far as to suggest that this strategy, as exemplified in the studies on risk taking (Kogan and Wallach, 1964, 1967), might constitute a paradigm shift. Wallach is not convinced of the revolutionary potential of this approach, and rather sees the whole thing as a "Small, conservative modification of view point," (Wallach and Leggett, 1973, p.311) providing no answer at all to the negative results with regards to the consistency issue. Mischel appears to share Wallach and Leggett's reluctance with respect to the utility of the moderation approach. He (1968) cautions that most of the interactions observed are not predicted, and explanations of them are offered post-hoc, with the consequence that great caution must be exersized in any attempt to interpret them.

Bem, however, seems altogether more optimistic and comments that "Mischel's observation that behavioural consistencies are situation specific can be translated into the assersion that individual difference are themselves a function of situational moderators."(1972, p.21)

Though he does acknowledge that the approach is rather empty until one can predict on a priori grounds which variables will serve as moderators. Nevertheless he sees this type of approach as permitting, at least, the prediction of (1) certain behaviours (2) across certain situations (3) for certain people.

Both Bem and Alker cite Wallach as being one of the main exponents of this new paradigm, but, as already noted, he displays considerably less enthusiasm than either for its utility. He comments that the data from the risk taking studies is far from clear and exemplifies his point by noting that while (a) Bem suggests consistency should be highest for those individuals scoring high on the defensiveness moderator and low on test anxiety (b) Alker believes that it should be greatest for individuals scoring high on both dimensions. Wallach, himself, claims the data supports neither contention strongly, being as strongly against as it is in favour of either position. The most telling point is, however that "subsequent attempts at replication have not been marked by success in any clear way either" (1972 p.312)

The work on expressive behaviour also cited by Bem fares no better at the hands of Wallach and Leggett. They suggest two main reasons for the effect of moderators being so unstable (1) Internal processes are sufficiently subtle to defy capture by the kinds of theorizing that have been advanced. (2) Moderator effects are statistically complex.

I:2 VARIABILITY IN CONSISTENCY

A rather more positive approach to the problem is to propose that consistency is, itself, a dimension along which people may be thought to vary and, consequently, consistency itself becomes a moderator variable. I will refer briefly

here to three studies, Bem and Allen (1974), Campus (1970, 1974), and Woodruffe (1978) which have addressed themselves specifically to this problem.

Bem and Allen (1974) argue in favour of an idiographic approach to personality, and claim that nearly all the research in personality is based on the assumption that a particular trait, or set of traits, is universally applicable. Whereas the idiographic approach acknowledges that individuals will each have their own meaningful dimensions and equivalence classes, with the result that to the degree that subject and experimenter fail to agree on equivalence classes it will be impossible to find consistency.

It is worth noting in passing that although it may be true, if undemonstrated, that people are consistent within their own idiosyncratic systems, Bem and Allen are sowing the seeds of confusion by using the term consistency in two different senses: one a (relative), correlational sense, the other an absolute or variance sense. This issue will be returned to later.

Bem and Allen compared individuals who rated themselves as high or low in variability on two traits, friendliness and conscientiousness. The data for friendliness was comprised of ratings of the subject by father, mother, peer and self. Each of these sets of ratings was reduced to a compound score made up from a global rating and a twenty-four item scale for friendliness, plus two observational ratings, one for frequency and duration of vocalization in a group discussion, the other the latency of initiating a conversation with a stooge in a waiting room. The standard deviation of scores across these measures (situations) was used as the measure of the individuals variability. As predicted a significant difference was found between high and low self

reported variable subjects in variability for friendliness across situations. Interestingly all six measures correlated more highly with E.P.I. extraversion for low than for high self rated variable subjects (mean correlations, .51 for low variable Ss, .31 for high variable Ss).

For conscientiousness they used ratings from the same four sources plus three observational measures: promptness in returning forms, amount of required course reading done, and a rating of neatness. For this dimension, conscientiousness, the global rating by the subject did not correlate with the situation item questionaire, the latter reflecting the experimenters' conception of the dimension, the former the subjects. The reported correlation was .62, (the same correlation for the friendliness dimension was .84). As already noted this in itself would lead to a lack of observed consistency. The authors, therefore, constructed a new variance score - the ratio of the variance on 23 items of the conscientiousness scale to the variance over all 84 items of the questionaire, which included traits other than conscientiousness and friendliness. Though the authors claim to have again confirmed their hypothesis, the situation is much less satisfactory with this new method of calculating variance.

Their hypothesis was: "Individuals who identify themselves as consistent on a particular trait dimension will in fact be more consistent cross-situationally than those who identify themselves as highly variable". (p.512). The consistency refered to is, of course, across situations deemed relevant to the trait by the rater, but this new measure looks at variability across situations thought revelant to the trait by the experimenters as compared with total observed questionaire variance. The conclusion should be that people who rated themselves as being low in variability on the dimension of conscientiousness are relatively less variable in relation to

situations, thought to be relevant to the trait by the experimenters, as compared to the total variance across situations thought to be both similar and dissimilar, than are people who rate themselves as being more highly variable on this dimension.

Woodruffe (1978) makes a similar point when he objects; "those who are highly variable on the other items as well as on conscientiousness will score the same as those who are extremely non-variable on both". (p.92). This measure takes into account not only variance within, but also between traits.

Woodruffe has also pointed out that even in relation to the friendliness dimension variability, or consistency, was not adequately measured, as the score of variability was, in part, made up of differences between the scores of the different raters which may, or may not, reflect actual variability in behaviour. It should more correctly, have been called variability in ratings of friendliness.

Campus (1970, 1974) was more interested in a general dimension of consistency, as opposed to Bem and Allen's trait specific consistency. She required subjects to provide TAT stories which were then rated, by the subjects themselves, for each of 17 needs. Campus, on the basis of her results,concluded that the consistent person responds in such a way as to maintain a constant self-image, while the inconsistent individual changes his view of himself in accordance with the demands of the situation, he is situationally bound. Woodruffe (1978) has pointed out that, in fact, her conclusions must be rather more limited than this as she has nowhere demonstrated a relationship between self-image and behaviour. It would have been far more useful had Campus attempted to define the individual's self image independently, rather than, as she did, using the same data first as a measure of consistency and then as a form of self description, and then to go on to relate these

two in a factor analysis. This latter aspect of her methodology leads one to question the validity of her results. Campus, herself, seems to be not entirely happy with the situation and indicates that more work, particularly with behavioural data, must be done before any degree of generality can be assummed.

Despite the fact that one must be cautious in accepting the link Campus suggests between self-image and consistency, she has, nonetheless, demonstrated that people do differ to the extent which they are consistent in presenting needs in different situations (TAT stories).

Woodruffe (1978) like Campus is most interested in global, rather than trait specific, consistency. He suggests that individual differences in consistency may, at least to some degree, be the result of having a more or less 'definite' concept of oneself. He reports a study in which each subject was rated on 14 nine point scales by a minimum of eight raters known to that subject. Apparent variability for each dimension for each subject was calculated as the sum of the absolute deviations from the mean across raters for that dimension divided by the number of raters used; the sum total of dimension-specific variability scores was used as the global variability score. An incongruity score was also calculated, this was the number of occasions on which raters attributed to the subject the opposite characteristic to that which the subject had attributed to himself.

At the global level variability correlated with non-definiteness of subjects about themselves .58 ($p < .001$) although at the dimension specific level only four out of a possible 14 correlations reached significance. Non-definiteness was found to correlate .4 ($p < .001$) with incongruity.

When the data was subjected to factor analysis, the first factor to emerge, accounting for 32.5% of the observed variance, was loaded on by variability, non-definiteness, intolerance of ambiguity (negatively) and preference for simplicity (negatively).

One is tempted to put the same objection to this study as that levelled at the Bem and Allen study; that we might be looking at the variability in rating behaviour rather than variability in the behaviour of the subject. Woodruffe has, however, considered this point and observes that the emergence of the predicted correlations between his apparent variability measure and other variables hypothesized as influencing consistency attests to the validity of the measure as a measure of behavioural variability. This point is particularly strong with respect to non-definiteness of presented personality, which occupies a central position in his arguement, and also receives support from the fact that non-definiteness is also found to correlate significantly with incongruity ratings.

If one accepts these arguements, and in as far as they reflect the standard method of validating any psychometric test, they seem reasonable, then we can regard Woodruffe's measure as being some kind of index of behavioural variability which is a step in the desired direction. The only reservation I have is that the arguement is rather one sided. If the predicted correlations do not emerge then it is not immediately clear whether this is because the measure does not in fact measure the type of variability one is interested in, or because the hypothesized relationships between variables do not, in fact, exist. We do not have an independent test of these hypotheses. This arguement becomes important in relation to the question of whether consistency is a global or trait specific dimension. Woodruffe's data are much

stronger in support of the global arguement, but support for the trait specific position may be weaker simply because it is based on a smaller data sample. If the measure of variability is rather insensitive then the correlations of dimension specific variability may well fail to reach acceptable levels. We have no way of knowing, of course, at this stage how sensitive the measure is. No correlation reported exceeds .6. This may be either because of the effect of other variables, or a lack of sensitivity of the measure of variability itself, or, of course, because no relationships do in fact exceed this magnitude.

Although Woodruffe's data do favour the global trait it would be a misrepresentation of his position, I think, to state that he would interpret it as exclusively supporting the global position and casting doubt on the trait specific hypothesis.

Despite the various criticisms which might be levelled against these three studies, taken together they do suggest both that individuals do not all see themselves as equally consistent, and it seems probable that this is a reflection of, or is reflected in, their behaviour. This strategy of using consistency itself as a moderator-variable may, in so far as people do vary in their consistency, demand some qualification of the emphasis often currently placed on inconsistency.

The fact, then, that at least some people do display consistency together with Bem and Allen's suggestion that at least on personally important dimensions, all people will display some consistency, might be added to Mischel's (1973) suggestions of why trait theories are so tenacious. He suggests that people tend to go beyond the observation of some consistency to construe a much greater degree of

consistency. Similarly people will interpret a small sample of behaviour as if it is representative, or reinterpret new behaviour to conform with pre-existing "cognitive structures or implicit personality theories". Consequently "recall based trait ratings may yield data that are systematic but unrelated to results based on direct observation of on-going behaviour as it occurs ". (1973, p.264)

Mischel also refers to a study by Jones and Nisbett (1971) who noted that subjects attributed the behaviour of others to enduring consistent dispositions while the cause of their own behaviour was attributed to specific transient factors. Such things as constant physical appearance, cognitive style, intellective ability and a restricted set of roles will all play a part in creating the impression of consistency.

Not all Mischel's hypotheses, however plausible they might be, have been confirmed when put to the test. Hayden and Mischel (1976) looked at the effect of initial impression on the interpretation of subsequent behaviour. They concluded that although initial impression may create some bias, such impressions, at least in the experimental situation, were easily changed in the light of new information. "It is the properties of the subsequent behaviours themselves that seemed by far the major determinants of the inference of underlying motives." (1976, p.122)

Some support was found for the hypothesis that subsequent behaviours congruent with initial impressions are seen as manifestations of the "real self", while incongruent behaviours were more likely to be seen as due to external transitory factors thus allowing the observer to maintain his impression of personal consistency.

Concluding this section one might wish to place two

qualifications on Mischel's earlier emphasis on variability. On the one hand people vary in the extent to which they exhibit consistency. It is now necessary that this be demonstrated at the behavioural level, and to determine whether it is a global feature of individuals, or peculiar to specific traits, or both. On the other hand people do appear to be aware of inconsistencies in behaviour, but, not unnaturally, they prefer a parsimonious explanation of behaviour which allows them to preserve an attitude of consistency.

I:3 INTERACTIONISM

Up until this point in concentrating on consistency I have implied that either the person, or certain characteristics proper to the person, or the situation is the major determinant of observed behaviour. Many theorists have rejected this position, however, and instead adopted what has been termed an interactionist position. Ekehammar (1974) has defined interactionism as "the synthesis of personologism and situationalism, which implies that neither the person per se nor the situation per se is emphasized, but the interaction of these two factors is regarded as the main source of behavioural variance." (p.1026).

Ekehammar claims that interactionism may be traced as far back as Aristotle, though somewhat more recently the position has been advocated, again according to Ekehammar, by Kantor (1924, 1926), Lewin (1938), Tolman (1935, 1951), Angyal (1941), Murray (1938, 1951), Murphy (1947), Rotter (1954), Jessor (1956, 1958), Sullivan (1953, 1964), Leary (1957). The major distinguishing feature between the classical interactionist and his modern successor, according to Ekehammar, is that while the former put forward comprehensive

personality theories without real empirical support, the latter present their propositions with empirical support, but in the absense of an elaborate theory.

Ekehammar goes on to suggest that the modern, empirically based interactionism can be divided up into four major groups of studies, connected with the work of Raush, Endler, Moos and Magnusson respectively. No attempt will be made here to examine these studies in great detail, but it will be useful to indicate the general trend of their findings.

Raush is important for, amongst other reasons, being one of the first to espouse the interactional point of view in the empirical study of personality. Perhaps more importantly, in view of Mischel's criticism of the poor predictive ability of type and trait theories, Raush's approach to the problem might well prove more useful than some of the others (e.g. Endler's partitioning of variance approach). Raush (1965) observed that there will always be a greater 'error' variance when dealing with humans than when working with lower species simply because man has a greater potential for adaptation. It maybe worth bearing this in mind when discussing the complexity of human behaviour. One side of the coin spells frustation for the theorist, the other survival for the species.

Raush's studies (1965, 1972, Raush, Dittman and Taylor 1959a, 1959b, Raush, Farbman and Llewellyn 1960) are mainly concerned with the observation of hyper-aggressive boys in residential care. They represent an improvement on those studies of consistency already cited in as far as they employ the observations of actual behaviour, even if one objects that the behaviours are 'interpreted' along the trait dimensions of aggressiveness and dominance. The children were observed in a variety of settings on two occasions 18 months apart; and at different times were compared, statistically, with two

other groups of normal children. The general trend of their findings appears to have been that as the boys matured and showed some improvement they displayed a greater differentiation between situations. The normal boys showed this differentiation to an even greater extent. Raush interprets the pattern of results obtained as follows: "The unique confluence of child and setting contributed far more to behaviour than did the summation of individual differences" (1959, p.229). In 1960 looking at the point the other way around he remarked "although individuals may differ across a variety of situations, and situations may differ across individuals, much may be lost in the artificial separation of the components" (1960, p.329).

By 1965 Raush was using the term Interaction in a somewhat different sense from before. He now looks at the extent to which a response can be predicted by knowing its antecedent act. He reports about a 30% predictability.

Woodruffe has suggested that there may well be considerably more individual consistency than is reflected in these studies. This must in some sense be true for, if in no other way, the hyper-aggressive boys must be relatively consistent across situations with respect to their hyper-aggression. Indeed Raush (1965) has commented that both situations and behavioural events are less discriminable for the very disturbed children. It is possible that the consistency of the individual is lost within the homogeneity of the group.

Despite the fact that Raush's studies may under estimate the importance of individual differences, they do serve to emphasize the importance of the interaction between person and situation, and indeed between person and person, in determining behaviour.

Two studies by Moos (1968, 1969) follow on naturally from the Raush work in as far as they look at both patients and staff in a theraputic community. The first study employed the questionaire method, but unlike most studies of this type feelings were recorded either during, or immediately after each of eleven situations. The results can be briefly summarized as follows, though it should be remembered when comparing staff and patient the two sets of data were analysed separately:-

(1) Individual differences were more important determinants of response for patients than for staff members.

(2) Individual differences were more important than settings for patients.

(3) Settings were more important than individual differences for staff members

(4) The interation was more important than either main effect, individual difference or the situation.

Although once more the importance of the interaction is clearly demonstrated nevertheless the data do suggest that both the situational and individual differences are also important. It is, however, the interaction which most impresses Moos and he comments: "settings may elicit consistent reactions from all staff; however, individual staff members also react differently to different settings" (p.58).

In a follow up study Moos looked at both questionnaire responses and actual behaviour. The two types of data are not directly comparable, for whereas the questionnaire data reflects trait type dimensions, the behaviour recorded is at a more molecular level including nodding, scratching, talking, smoking, hand and arm movement, and general movement and shifting. These data were collected on two occasions three months apart. Person differences accounted for 0-45% of the

variance, differences between situations 0-18%, and the interaction 9-38%. So once more he found, for a clinical group, that individual differences are more important than settings, with the interaction always proving to be relatively important. The results were much the same for the behavioural data, though, as might be expected, some variables were more situationally determined e.g. talking, while for others the person was the more important determinant e.g. smoking.

I feel that caution must be exercized in interpreting these results. As already noted the data for patients and staff was analysed separately and comparisons between the various proportions of variance attributed to the variables is made difficult by the fact that (a) the sample of situations was not identical for the two groups, (b) twice as many patients as staff were sampled. This second point becomes even more important when it is realized that the staff were observed only in ward setting where the roles they were expected to play and duties they were expected to perform in consequence of their position were more precisely prescribed, and may well have introduced an artificially inflated degree of homo-geneity in this group.

That this might not be the whole story is at least, suggested by the fact that those patients regarded as more 'normal' were also found to be more sensitive to situational influences, the same pattern as reported by Raush.

Moos' findings are, then, in line with those of Raush and appear to indicate that adjustment may be related to sensitivity to settings, that individual differences too are of importance, but that the interaction between the two is always relatively important.

The next group of studies to be considered are those reported by Endler and his co-workers. These studies seem

important because of their popularization of the partitioning of variance technique, and of the use of S.R. inventories. In the first paper of this series (Endler, Hunt and Rosenstein, 1962) an S.R inventory of anxiety was employed. This sampled 14 modes of response across 11 situations. On the basis of this study they concluded that situations were substantially more important than individual differences in determining behaviour. By 1966 Endler and Hunt acknowledged that the simple comparison of the means square from the analysis of variance was not an appropriate method of looking at this problem, and consequently reanalysed the earlier data together with some new data. This new analysis indicated that in all cases sampled the proportion of variance attributed to person effects was greater than that attributed to situation effects. The results range from 5.75 to 10.42% for person effects, 5.25 to 7.29% for situation effects, while the interaction always contributed around 10% of the variance. Modes of response too, as might be expected, consistently contributed a substantial proportion of the observed variance.

In a 1969 report (Endler and Hunt 1969a) data are reported which indicate the extent to which previous results can be generalized. They increased the number of samples, 22 male samples, 21 female samples, the number of situations and the number of modes of responses. Even with this increased range of sampling the variance contributed by situational differences never exceeded 15% (13.7%) for males and 20% (19.9%) for females, nor that from individual differences 10%, the two way interaction (person x situation) usually contributed approximately 10% of the variance.

In a further study (Endler and Hunt, 1969b) Endler and Hunt compared the results of an S.R inventory for Hostility with those of the anxiousness S.R inventory. Here it was

found that of the total observed variance a substantially larger proportion was contributed by individual differences for the hostility, than for the anxious, dimension. The relevant figures are, for men: anxiousness 4.44%, hostility 19.08%, for women: anxiousness 4.56%, hostility 14.82%, with the person by situation interaction again consistently around the 10% level. Most of the increase in the importance of the person seems to be accounted for in terms of a decrease in the importance of modes of response. This really raises the question of whether or not modes of response should be included in the analysis of this kind of data. Cartwright (1975) has pointed out that "subjects do not respond to the modes of response; they respond through the modes" (p.410). The point here is that if one is interested in the trait termed anxiety the way it is expressed is substantially irrelevant and these modes of expression, when included in the analysis, will contribute variance which serves to obscure the person and situation effects. It can also be argued that it is inappropriate to employ modes of response which are functionally equivalent from the individual's point of view. These points turn to a large extent on what we mean when we talk of a person being consistent, and this will be returned to later.

Woodruffe has observed that the modes of response employed may not, in fact, be comparable which, if true, might indicate as Endler and Hunt (1968, p.314) claim, that we can not generalize, with reference to consistency across traits. Though it may in fact be true that different dimensions do operate differently it can hardly be claimed that this has been demonstrated in the present experiment. What Endler and Hunt have shown is that the modes of responses they thought to be relevant to the anxiousness dimension showed,

relative to other sources of variance, more differences between themselves than did those modes of response thought to be relevant to the dimension of hostility. They certainly seem to claim too much, on the basis of the reported data, when they say "individual differences in the intensity of a trait of hostility are genuinely more prominent than individual differences in the intensity of a trait of anxiousness". (p.314).

In 1973 responding to criticisms by Alker (1972) an attempt was made to increase the range of individual differences by including data from neurotics and psychotics, as well as 'normals', in the analysis. When the data from the three groups were analysed together person differences were found to contribute 9.66%, situations 16.11% and the person by situation interaction 18.14% of the observed variance. However, for the three groups seperately individual differences contributed 0.93 to 4.2% for the 'normal' groups; 12.13% for the neurotic sample and 18.78% for the psychotic sample, of the total variance. The proportion of variance contributed by situational differences was reduced for both clinical groups, and the person by situation interaction was also found to be reduced for the psychotic group. Results again reminiscent of Raush's and Moos'.

Following Endler's line of reasoning both the person and the situation are important to some extent, but it is the interaction between the two which seems to be of special importance. Despite the effort expended by Endler and his co-workers in dividing up variance he does not seem to be able to offer any pronouncement more substantial than Mischel's observation that the relative importance of each source of variance will depend on the situations, behaviours and individuals sampled, and to take up any more fixed a position would be to create a pseudo-controversy.

Several other studies have employed this S.R inventory and partitioning of variance approach (e.g. Sandell, 1968, Larr, Suziedelis and Kinnane, 1969, Bishop and Witt, 1970; Endler and Okada, 1975, Golin and Nartz, 1976, Zuckerman and Mellstrom 1977, Dworkin and Kihlstrom 1978) but they are of interest mainly with respect to particular traits and behaviours and are less relevant to the consistency - interaction issue.

The final set of studies to be discussed in this section are those by Magnusson and his co-workers, particularly Ekehammar. There are two major differences between these studies and those already discussed, the first is the stress placed on the psychological, as opposed to physical, environment; and the second is that they follow in the correlational tradition of personality research. In 1968 Magnusson, Gerzon and Nyman reported the results of an experiment in which the subjects had been observed and rated on two occasions. Ratings were made along a number of trait-like dimensions, and amount of (length of) talking time was recorded. Two variables were manipulated.

(1) Whether or not the subject was part of the same group on the two occasions

(2) Whether or not he was required to perform the same task on both occasions. This yields four conditions. Results were correlated across the two occasions. Ekehammar (1974) summarizes the findings as follows: "The analysis showed that the relationship was random when both group composition and task were varied simultaneously, whereas the co-efficients were moderate to high when the two situations were identical and differed either in group composition or task" (p.1038). This summary is not strictly true, at least for the "length of talking time" measure, were a correlation of .418 is reported for the maximum dissimilarity condition. This is the smallest of the four correlations, but still larger than most of those Mischel (1968) reports. It is also worth

noting in passing that the condition of maximal similarity does not yield the highest correlation, this is associated with a constant task, but a changed group (.636 - .895). Too much should not be made of these points, however, partly because apart from the "length of talking" measure, the condition of maximal dissimilarity does usually yield a correlation approximating zero, as Ekehammar claims; so the moderate correlation associated with this one measure is not systematically replicated. With regard to the maximally similar condition, this may well be maximally similar in objective terms, but Magnusson has stressed that it is subjective similarity that is of critical importance.

On the basis of these results Magnusson concluded that behaviour showed a poor generality and consequently the trait approach must be abandoned and he opted for an inter-actionist approach as the most useful alternative.

A later study by Magnusson and Ekehammar (1975) seems in many ways, to be the high water mark of the empirical research in relation to interactionism. Whereas most research has been primarily concerned with demonstrating that no one source of variance is the sole determinant of behaviour, there has been some reluctance to take up the challenge of explaining how the variables interact. This study does not answer the question, but it does appear to be making a step in that direction by taking up Hunt's (1965) challenge, when he said "...from either understanding variations in behaviour or making clinical predictions, we should be looking towards instruments that will classify people in terms of the kinds of responses they make in various situations".

Magnusson and Ekehammar attempted to come to terms with this problem by reducing the number of situations sampled to 3 types, or equivalence classes, and responses to

two equivalence classes. This was achieved by employing factor analysis. They classified the subjects into "homogeneous groups on the basis of their anxiety profiles for different types of reactions across different categories of situations". For this they used the method of latent profile analysis. Both the situations and the responses employed were thought to be relevant to the trait of anxiety.

Three profiles were extracted for both males and females. The definitions of the two sets are substantially similar and so I will quote only those for females, as an example of the type of profiles described: Profile I is characterized by (a) a low anxiety level (b) high trans-situational consistency (c) high response consistency; Profile II by (a) a high level of anxiety, (b) comparatively high trans-situational consistency and (c) a low response consistency; Profile III by (a) a moderate anxiety level (b) low trans-situational consistency and (c) low response consistency.

The problem with this study, interesting as these profiles are, is that no attempt has been made to demonstrate that the inclusion of the situational and response consistency elements results in a predictive ability superior to that of the traditional trait theory which employs only the first of the three elements in these profiles. Nor, indeed, that this method of describing people and predicting from the descriptions is superior to asking people specific questions about specific responses in specific situations. In other words even if we are unclear about the manner in which elements do interact, does a method which explicitly acknowledges the interaction of elements permit a greater generalization or predictiveness than do more traditional approaches?

Another disappointing aspect of the study is that

although the authors talk at length about how the individual functioning and the uniqueness of the individual is important we are still presented with averaged group data.

The problem really is whether the mean group equivalence classes for situations and responses are those of particular individuals. Magnusson and Ekerhammar attempt to come to terms with this problem in a later paper (1978) when they try to evaluate, empirically, what they see as a central tenet of the interactionist position that "behaviour should be more similar across situations which are perceived as similar i.e. situation response data would then be congruent to situation perception data for single individuals". They continue, "the view presented above is a crucial point in the interactionist interpretation of behaviour" (p.42)

They then report data which supports this position that individuals do behave more similarly across situations perceived by them as being similar. However, these findings cannot be generalized too enthusiastically at this stage because only self report data, no actually observed data, were used. Magnusson and Ekehammar acknowledge that it is necessary for similar demonstrations to be made employing other types of data.

While the work of all these groups was probably necessary for a useful appreciation of the complex determinants of behaviour, that of Magnusson and Ekehammar is somewhat more impressive than the rest. They have attempted to synthesize the interactionist approach and use it, albeit in a limited way in describing people. They have also done more than pay lip service to the contention that each individual will interpret situations differently and, consequently, behave differently.

Out of the substantial literature on this topic two

other papers need to be mentioned, even if only briefly. The first, by Argyle and Little (1972), points to four rather than three, approaches to personality. These are the trait, and situationalist, the interactionist and what they term the dispositional approaches. According to the dispositional approach the behaviour of the individual does vary across situations, but at the same time individuals maintain a constant rank order across situations. This is distinct from the interactionist approach in that according to this latter approach not only do people differ across situations, but the pattern of responding is idiosyncratic. Strictly speaking only this last approach is interactional in the statistical sense of the term, but most authors when talking of interactions are referring to the conjoint action of both the person and the situation and in this sense the dispositional model is also an interactional model.

In the empirical section of their paper they report results consistent with others already discussed. At least in as far as percentage of variance is an indication of importance as a determinant of behaviour neither the person nor the situation are the sole determinants of behaviour and neither the trait or situational models, as described by Argyle and Little, are supported. However, in as far as these variables do account for some portion of the observed variance, following the same line of reasoning, one can dismiss neither the person nor the situation as individually unimportant, as opposed to their importance in the interaction, as a determinant of behaviour. Argyle and Little plumb for the interactional model, dispite the fact that their data do not offer unequivocal support for this position.

The final paper is one by Bowers (1973). Although he

makes several salient points about the shortcomings of the situationalist position this paper is of interest here primarily because it reviews studies where the proportion of variance accounted for by the person, the situation and the interaction between these two is reported. over the 19 studies reviewed the mean proportion of variance accounted for by individual difference is 12.71%, by the situation 10.17%. The interaction contributes more of the variance than either of these main effects in 14 of 18 comparisons. In 8 of the 18 comparisons the interaction accounts for a greater proportion of the variance than the sum of the two main effects.

So even with this larger sample, at least as far as the partitioning approach is concerned, although the interaction appears to be of primary importance neither the person nor the situation can be dismissed as unimportant.

I:4 LIMITATIONS OF INTERACTIONISM

Ekehammar (1974) concluded his review of interactionism by proclaiming: "Thus, if interactionism is not the Zeitgeist of to-day's personality psychology, it will probably be that of to-morrow's" (p.1045). This proclamation seems to be based on faith rather than evidence, on the belief that recent empirical studies have developed tools which permit the proper testing of the interactionist hypothesis. Do the analysis of variance studies really test the classical interactionist positions?

The two most common terms, consistency and interaction, are employed so loosely that people are using them to mean quite different things. It has already been remarked that Lay (1977) has accused Bem and Allen (1974) of switching between meaning of the term consistency, in mid-article.

To quote him at length: "consistency is (sometimes) viewed in terms of inter-subjective differences in behaviour between situations, the lack of differences implying consistency. This view is implicit in all studies employing a variance component analysis. In contrast, cross-situational consistency may be defined in terms of the relationships of behaviour across situations (over subjects), high correlations between situations implying consistency regardless of absolute differences. This latter view is entailed in any correlational analysis of behavioural consistency. People may then exhibit differences in their level of behaviour across situations but nevertheless still be 'consistent' in the sense of maintaining relative ranks across those situations". (p.143).

The first of the two meanings of consistency, referred to by Lay, is tied to an absolute view point. Not only absolute consistency, but also includes the assumption of absolute, as opposed to relative, measurement, otherwise the comparisons made can have no meaning. But, particularly where questionnaire and self rating data is concerned, it is doubtful if any of the data employed in looking at consistency ever reaches this absolute status. This is a practical point with relevance to the type of data proper to the variance model. It is not supposed to be an observation in favour of, or against, either meaning of the term. Although, of course, in as far as inappropriate data has been used it does weaken the claim that a particular 'type' of consistency has been, or has not been, demonstrated.

Magnusson and Endler (1977) have indicated that the issue is even more confused by pointing out a third distinct meaning of the term. If the first two meanings of the term may be called absolute and relative consistency respectively, then these authors suggest that coherence is a suitable

Synonym of the third meaning. Coherence refers to an observed lawfulness of behaviour, which permits a degree of prediction, but the regularity is found in the pattern of responding rather than in a set of stereotyped responses.

Magnusson and Endler pointed out that when discussing this problem, what might be termed the context of consistency is of considerable importance, that is whether one is referring to consistency across similar or dissimilar situations. It sometimes seems as though some anti-trait theorists assume that once a person has been defined as belonging to a particular type, or located at some point on a dimension then it is legitimate to compare his behaviour across any two situations assumming the trait theorist would predict exactly the same response in both. Of course, no trait theorist has ever claimed this. Most trait theorists would claim that there are a limited number of situations relevant to any one dimension or trait. Even then behaviour would be expected to be more similar the more similar the situations were. Similarity still needs to be defined, of course, and definitions either in terms of perception or in terms of relevant traits, without care, could easily become circular. One might, of course, regard the fact that no trait is useful for prediction in every situation, and possibly that no trait is the most relevant way of describing every individual, as yet another weakness of the trait approach to personality.

One might decide to follow, for example, Bem and Allen and limit the discussion of consistency to traits relevant to the individual; or alternatively talk only of consistency across 'similar' situations. These solutions would not, I think, solve the problem of whether or not people are consistent because they miss the heart of the problem. The clue to the real problem of consistency lies in the fact that the term itself is used in a variety of ways. The problem is not simply a linguistic one, although the literature no doubt

has been greatly multiplied by the fact that people were misunderstanding each other. The real disagreement seems to be one of emphasis. Different groups of theorists want to stress different aspects, or view points, of human behaviour; its adaptiveness, its lawfulness, its orderliness, its idiosyncratic but regularly patterned aspects. It is like a mass exercise in Kelly's constructive alternativism. Most of the view points bring with them their preferred types of causes but these are usually inferred from the behaviours they purport to explain, and the most interesting statements are usually about observables and relationships of observables. Even if one is interested in the causes, these seldom are as mutually exclusive as might appear to be the case when reading some authors. It is perfectly possible that a particular type of nervous system might be labelled, at the behavioural level, with a trait name, but might also facilitate or hinder learning, which in turn might lead to the subject describing various sorts of experiences.

In other words to a substantial degree the controversy lies in assuming things - view points - to be mutually exclusive when they are, in fact, not. They are all, if you wish, special views of the truth. We might also follow Ryle and point out that often we assume that all 'causes' are of the same type, when in fact they are not. We are making category mistakes.

One might regard the three meanings attributed to consistency as three levels of generalization, or of specificness. There is no necessity to have a one to one correspondence, a regularity of correspondence, between levels. It may be impossible to translate from one level to another without distortion, and loss of peculiar and useful insights which characterize a particular approach. For example, while it

seems evidently true that what a person perceives as being relevant in a situation will determine his behaviour, nevertheless it seems patently wrong to suggest that we can have no idea of what he will do unless we know exactly what he thinks. Our lives are mostly made up of predictions of what others will do. Predictions made on the basis of our impressions of others and what we feel is relevant in the situation. These predictions may, admittedly lack precision, are at times wrong, but are normally good enough for us to avoid disaster, and to interact with others quite successfully.

The interactionist position is often seen as the way forward because it attempts to weld different types of elements together to give improved prediction, but does it, in fact solve, the consistency problem? Ekehammar places great hope in the new 'tool' developed by the interactionist, the partitioning of variance technique. As reported above the person by situation interaction is consistently found to contribute a substantial proportion of the observed variance. Therefore, it is argued, the interaction is of substantial consequence. Golding (1975) maintains that conclusions drawn from the partitioning of variance studies "a propos the theoretical issue of consistency are logically specious." (p.279).

He presents a matrix of hypothetical data in which perfect rank order is maintained across situations by people, and across people by situations. Despite this consistency, rank consistency, utilizing the normal method of variance partitioning, both person and situation are attributed with only a trifling proportion of the variance. When Golding employs his 'generalizability co-efficient', however, this rank consistency is clearly reflected. He observes at one point, "In order to index consistency across situations in the rank order sense, differences across situations - their

tendency to elicit differing magnitudes of behaviour in question (situation main effects) are irrelevant." (p.281). Of course, this is true if one is interested exclusively in rank order consistency, but it should be noted that people were, in the example, inconsistent, in the absolute sense. They behaved differently in different situations.

It seems that Ekehammar's faith is some what misplaced; the most appropriate method of calculation, depends on the form of consistency, or regularity, in which one is interested. The tool is contributing its own problems. The whole exercise of partitioning variance seems to be misguided. It was useful once to partition variance, by what ever method, to demonstrate that there was no single source of variance in behaviour. Perhaps a repetition of the exercise was justified to indicate that divers samples will give comparable results. A third study might be justified if it demonstrates that the results may be different for different traits. Continual repetitions of the technique do not, however, add to our understanding of the "interaction". Continual repetition that it is important does not provide theoretical insight.

Indeed, it is not clear that the 'new tool' is an appropriate way of looking at the type of interaction proposed by Ekehammar's classical interactionists. As with consistency, loose, imprecise use of terminology has resulted in confusion. Olweus (1977) goes even further in his condemnation of the variance technique: "The variance component technique as used in this context appears to have brought about more confusion than clarity." (p.224).

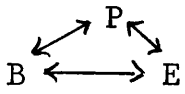
Simply dwelling on the short comings of this technique brings us no nearer to the solution of the problem. A more appropriate next step would be to follow authors like Bandura (1977) and Olweus (1977) and attempt to make explicit

what is meant when it is claimed that behaviour is the product of an interaction. Bandura has outlined three possible interactionist approaches:-

(1) A unidirectional notion of interaction, where the person and the situation are treated as independent entities which combine to produce behaviour. This is often signified by the formulation: B = f(P,E) (B = behaviour; P = person; E = environment).

(2) A bidirectional approach, which acknowledges that the person and environment influence each other, but retains a unidirectional view of the course of behaviour. B =f ($P \rightleftarrows E$)

(3) The third approach he calls reciprocal determinism, here "behaviour, other personal factors, and environment all operate as interlocking determinants of each other" (pp.9-10).



Olweus (1977) has proposed essentially the same group of meanings for the term interaction (1) "unidirectional interaction, namely how two or more independent variables (person and situation variables) are combined or connected in their relationship to a dependent variable, the individual's behaviour or reaction". (P.225).

(2) An interdependency of person and situation, the types of inseparability referred to by Bowers (1973). "In sum , the situation is a function of the person in the sense that the observer's cognitive schemes filter and organize the environment in a fashion that makes it impossible ever to completely separate the environment from the person observing it." (Bowers, 1973, p.328).

(3) The third form of interaction differentiated by Olweus is Bandura's reciprocal determinism "Here the individual and

the environment are seen as mutually influencing one another and the emphasis is often on processes over time: Environmental events effect the person's responses, which in turn affect the environment, which again influences the person's responses, etc." (Olweus, 1977, p.225).

The time element though important, is less important for Bandura who feels that though logically seperable it is misleading, and in fact impossible, to separate out e.g. the person and his behaviour, or the environment and the person. In this sense models '2' and '3' are clearly related. (4) The final meaning of the term offered by Olweus is the meaning of interaction normally associated with analysis of variance - i.e. a statistical interaction. "As in the first mentioned meaning it is here a matter of unidirectional interaction of quite a special character". (p.225)

In reviewing interactionism Ekehammar (1974) observed "The empirical evidence supported almost without exception the interactionist view, which means that the relative magnitude of the Person x situation interaction variance was usually greater than the relative magnitude of person or situation variance." (p.1034).

This seems to indicate quite clearly that Ekehammar has restricted himself to just one meaning of the term interaction and consequently pre-empted what appears to be the most important question: How are personal, situational and behavioural variations related?

Even if the question is reformulated as "How do individual differences and situations combine in evoking behaviour?" (Olweus, 1977, p.226), thus slanting the question towards a unidirectional meaning of interaction (meanings '1' and '4'), the question may be answered in ways which do not include a statistical interaction.

Olweus provides two examples. The first exemplifies the situation where the statistical interaction is a partial determinant of behaviour:

$$B = a + b_1 X_1 + b_2 X_2 + b_3 X_1 \cdot X_2$$

(a,b = constants, x_1 = values or a person variables, X_2 = values or a situation variable. In the second the statistical interaction is quite unimportant:

$$B = a + b_1 X_1 + b_2 X_2$$

So the anova approach is properly applicable only to the investigation of one meaning of the term interaction and as such is no good as a tool for investigating which is the correct, or even most useful, meaning of the term. It has already been pointed out that some meanings of the term do not really allow a meaningful distinction between person and situation and so to talk of how they might combine would be a nonsense. Raush (1977) made this point quite forcefully when he remarked that the division between person and situation, or 'me' and 'not-me' as he calls it, is "somewhat elastic and not wholly fixed" (p.290). He gives the example of spectacles and beards which though definable as objects apart from 'me' nonetheless have such an intimate relationship with 'me' that I cannot feel 'myself' without them. This, clearly has overtones of James' concept of the 'Material Self'. Marriage, in terms of the person - situation distinction, is an even more vexed question. Raush's own empirical work certainly does not belong to the unidirectional,let alone statistical, concept of interactionism. He introduces the concept of time, chains of events and reciprocal relations between, in particular, individuals.

It seems to be a truism that both the person and the situation bear an important relationship to behaviour. As for interactions Mischel (1973) has objected that they are

difficult to interpret, often post hoc, and not replicable. Nisbett (1977) has objected that they lack any real precision, and in consequence are notoriously difficult to disprove. They are resorted to only, or at least mainly, he claims, when the main effects have failed, whereas one should not look at interactions until one is sure of the main effects. Whether or not these criticisms are justified it seems they are levelled exclusively at the statistical form of interactionism. This is the form which, certainly here, has born the brunt of the criticism, but I feel it would be short sighted to abandon this technique at this stage, though it certainly must not be persued to the exclusion of others.

None of the types of theory referred to here may in the end prove to be true. Indeed it seemsunlikely that any one theory will prove to be universally useful. Until one of them emerges as having a clear advantage, then, it seems most sensible to pursue them all, or attempt syntheses of them. There is always a danger of attempting to predict beyond the range of convenience of a theory, or to attempt facile translations of theories thus distorting them or denuding them of their complexity. To complain that a particular theory does not predict at a more or less general level is usually inappropriate. More relevant criticisms seem to be that it is not explicit as to its level of generality/specificity; and appropriate predictions drawn from it are inaccurate or inconsistent, or that it is surpassed in its range of predictions by another theory.

Mischel challenged the trait position because he found that people were less consistent than he felt they ought to be if trait theories were correct. The issues of consistency, traits, and person-situation relationships have since then been looked at some times as related and at other

times as independent issues. It has already been pointed out that there are different levels of personal consistency and different ways in which the person - situation relationship can be viewed. The weakness of the trait position is that the traits must be inferred from the things they are purported to cause. However there are fewer problems if we read the statement, "He is talking because he is an extrovert", as a dispositional statement, rather than a mechanical cause - effect one. That is, if the statement is of the sort: Extroverts are the type of people who do social things and it can therefore be expected of him that he will talk; rather than of the sort: there is something internal to this person which is labelled extroversion and it is this which causes him to speak.

Traits may also be seen to be useful at another level, or within another system of explanations. People may employ traits as categories which determine their responses. Thus if having categorized a person predisposes us to behave towards that person in a particular way, then the trait may, in some respects, be viewed as being as 'real' as the intra-organismic variables which are hypothesized to govern adaptiveness.

Some theorists have suggested recently that the next step in the study of personality might be an attempt to categorize situations. This, as an exercise on its own, does not seem to me to be a promising direction. It may or may not be a sensible thing for personality psychologists to talk about people to the exclusion of situation, to do the converse certainly does not seem sensible. A categorization of situations can only be usefully performed as an integral part of a theory of personality which employs individual variables, situational variables and their relationship.

Rather than attempt to be exhaustive this review has attempted to indicate that there is considerable dissatisfaction with the traditional trait theory approach to personality psychology; that situational as well as individual variables and their relationships are being seen as more important than they were previously. However neither simple notions of consistency and interactions nor one single research tool appears to be any more universally applicable than are the trait theories they are attempting to succeed. It was suggested that the trait approach and the variability, or adaptability approaches may not, in fact, be antithetical. It will be suggested later that it may be possible to marry the idiographic and the nomothatic approaches to personality in a single theory.

CHAPTER II: SOCIAL LEARNING THEORY

Mischel (1968, 1973) observed that there was nothing inherently illogical about type and trait theories, their problem was a pragmatic one - they did not work. However almost twenty years before Mischel's pronouncements Ryle (1949), in his influential book "The concept of Mind", had pointed out that the problems were more fundamental, the status of dispositional concepts was often misunderstood with the result that invalid conclusions were frequently drawn. "There is, however, a special point in drawing attention to the fact that many of the cardinal concepts in terms of which we describe specifically human behaviour are dispositional concepts, since the vogue of the para-mechanical legend has led many people to ignore the ways in which these concepts actually behave and to construe them as items in the descriptions of occult causes and effects. Sentences embodying these dispositional words have been interpreted as being categorical reports of particular but unwitnessable matters of fact instead of being testable, open hypothetical and what I shall call 'semi-hypothetical' statements" (p.113). So dispositional words do not refer to causes but to one or more episodic events.

Enjoyable as it might be to high light the short-comings of other theories, and to point out that they do not perform the job they purport to do, the exercise may well prove to be fruitless if some alternative principles for the organization or explanation of behaviour are not offered. The alternative set of principles offered by both Mischel (1973) and Bandura (1977) is embodied in what is known as

social learning theory. As these authors explicitly relate social learning theory to the issues of consistency and interactionism I shall restrict this outline of the theory to the form in which it is presented by these two authors.

As an alternative to internal motivators and dispositions social learning theory stresses the importance of learning in determining behaviour. There has also been an attempt made to include in the theory findings from diverse areas of psychology, particularly cognitive psychology, with the result that its formulations have moved away from the more mechanical ones associated with animal studies and place greater stress on the cognitive aspects of learning set, the generation and abstraction of rules, the role of language and in particular observational and vicarious learning.

As Mischel has remarked "the term 'behaviour' has been expanded to include virtually anything that an organism does, overtly or covertly, in relation to complex social and interpersonal stimuli". (1973, p.268).

Mischel summarizes the aims of social learning theory as follows: "The proposed cognitive social learning approach to personality shifts the units of study from global traits inferred from behavioural signs to the individual's cognitive activities and behaviour patterns, studied in relation to the specific conditions that evoke, maintain, and modify them and which they, in turn, change." (1973, p.265).

The two authors present essentially similar points of view the major difference being that while Mischel largely limits himself to person variables relevant to learning Bandura devotes more time to describing the general determinants of behaviour. So while Bandura outlines four processes, attentional, retentional, motor reproduction and motivational, which determine what is learned, Mischel

stresses the importance of various competencies, both cognitive and behavioural, which may be subsummed under the various processes.

Mischel suggests that, because of the importance of the organization and transformation of information cognitively, a person's intellective abilities will be critical determinants of his behaviour. He goes so far as to suggest that the first factor to emerge from many analyses of personality data, and often labelled adjustment or ego-strength, may well be an intellective factor. With those of greater intellectual ability displaying more appropriate responses, (or at least reporting that they do.). Consequently he believes that the best predictors of social and interpersonal adjustment are these intellective factors.

Of course, what a person will learn to do is determined not only by his cognitive but also by physical abilities. Factors such as whether one has available behaviours or skills which maybe combined to form a more complex behaviour, and the availability and accuracy of feed back may all facilitate, or retard, the development of a skill.

What will be learned will also depend to a considerable extent on the encoding strategies employed. When Mischel discusses encoding strategies he appears to mean something like personal constructs as proposed by Kelly (1955). As Bem and Allen have pointed out these constructs may or may not overlap with those of the observer, and to the extent that they do not the individual will appear, from the observer's point of view, inconsistent, or to have failed to learn a particular response. This is not to say, of course, that even within one's own system one will behave with perfect consistency. To the extent that the perception and encoding of new information will be dictated by the existing schemata (organization) so

as to allow integration and facilitate prediction we can expect substantial consistency - though more widely for behaviours with greater functional utility than those more closely related to specific stimuli. To the extent that the existing schemata prove to be inadequate we can expect less consistency. Looking at the problem from this view point we would also predict that the individual focusing on certain aspects of his behaviour - those he regards as having functional significance - may well construe himself as consistent while others e.g. the psychologist, focusing on aspects that are conceived as being of theoretical significance, might well "objectively" regard him as lacking cross situational consistency.

Mischel's various points could be subsummed under Bandura's retentional process which includes both symbolic coding and cognitive organization. He also includes both symbolic andmotor rehearsal of particular behaviours. With respect to attention Bandura remarks that such factors as the sensory capacity of the observer and his arousal level will play their part. As for the variations in encoding strategies here Bandura points out that perceptual set will play its part too in determining just what will be observed, and so be available for encoding. Again with reference to consistency this leads to the prediction that if a different set is being employed on different occasions, then even objectively identical situations may well elicit different responses.

Bandura also introduces past reinforcement at this point, not simply as a mechanical device for strengthening learning, but rather, he suggests that previous reinforcement may lead one to attend to, or focus on, certain aspects, because it is these which have proved, in the past ,to be

indicators of the availability, or probability, of reinforcement. The importance is not so much that a particular element of the situation is a reliable indicator of the likely outcome, but that the individual believes it to be so, and consequently attends closely to it.

Of course, what is learned is not dependent solely on the observer, such characteristics of the stimuli as its distinctiveness, complexity, commonness, and functional value will all have their consequences.

The last process in the learning chain proposed by Bandura is the motivational stage. This is the stage at which reinforcement is often called upon to bear a heavy load of explanation. Reinforcement has an important role to play in social learning theory too, but Bandura has rejected any automatic S-R view of learning. Instead he favours the view that the importance of reinforcement is not so much in learning as in the regulation of behaviour. Rewards are important in learning in as far as they create anticipation. When it is believed that a particular behaviour will lead to reward, or the avoidance of punishment, then it is more likely that attention will be focused on that behaviour, that it will be coded, stored and rehearsed. Referring to experiments by Bandura, Grusec and Menlove (1966) and Rosenthal and Zimmerman (1977) Bandura concludes that once attention is drawn to modelled activities incentives fail to increase the amount of learning. He goes on to observe that if operant conditioning is viewed in this light it is merely a special case of observational learning, where what is observed is the efficacy of one's own behaviour.

The stress placed on observational learning might lead one to conclude that if social learning theory were correct then originality could not exist and people must turn out to

be mere carbon copies of each other. In fact, people do behave in original ways; how does the theory deal with this state of affairs? Bandura points out that the individual is not exposed to a single model but to many, besides the people he actually knows there are others he may observe from time to time. He may acquire new patterns of behaviour via the written or spoken word, either from descriptions of the behaviour of others, real or imagined, or from explicit instructions. There is also television which provides a potentially rich source of models. Even the range of models is not the whole story, the individual is more than a patchwork of modelled responses. New responses, and patterns of responses, may be synthesized from acquired ones as these acquired ones prove to be of greater or lesser functional value. In the environment peculiar to the individual original syntheses will be facilitated by differantial rewards and punishments of specific aspects of the new responses.

In addition to these features Bandura, as already noted, rejects the view that responses are merely added together in a mechanical way, peripherally. Instead he stresses the importance of central mechanisms. This gives rise to what he calls abstract modelling. That is people go beyond the response which is modelled and attempt to extract the salient features, and to formulate rules which will themselves be used later as the guiding principles of behaviour, new, possibly original, behaviour. It is in this way, Bandura suggests, that language may be learned.

Other people serve not only as models of behaviour, but often as cues to elicit a particular behaviour. They may also serve as inhibitors or disinhibitors, stimulus enchancers or emotional arousers.

In line with the view that people are not merely pushed

and pulled by internal motivators and external stimuli Bandura argues that we do not merely respond but we also interpret. This point is intimately related to the ideas of abstract modelling, rule extraction, and anticipation. It is not the simple pairing, he maintains, of stimulus or response with a particular outcome, which is important, but the degree of predictability, or magnitude of the correlation derived which is important. He argues that if this were not the case then partial reinforcement could not work. For these schedules to be effective they must be based on the belief that a certain proportion of responses will be rewarded, rather than the response being controlled by its immediate consequences. Certainly, at least with humans, with whom we can communicate in a way we cannot with other species, it is far more economical to tell them that specific conditions will obtain, than to allow them to discover this for themselves. Verbal instructions are effective with humans. Similarly if condition X is seen to follow response Y, the individual may not make the 'appropriate' response if he believes that these contingencies no longer apply.

One of the variables which may persuade one that the observed contingencies apply generally is the degree to which one can identify with the model. Bandura reports a study by Crooks (1967) in which cries of distress were heard each time the model monkey touched certain objects. In a free play situation the observer monkey avoided these objects. Effects such as these are hightened if the observer has experienced pain along with the model, and of course lessened if he has never experienced pain (Church (1959)). This may be why phobics respond with modelled behaviour more readily when it is performed by an incompetent model, with whom they can identify, rather than when a facile efficient

performance is witnessed. In the same vein, Stotland (1969) reports that people respond more emotinally to the sight of a person undergoing pain if they imagine how they, themselves, would feel in the situation rather than how the other person feels.

The underlying theme Bandura is presenting seems to be that observation on its own does not of necessity give rise to learning, predictability is a critical determinant. This may, in part at least, explain why animate, as opposed to inanimate, objects are more likely to become phobic objects - they are less predictable.

It would be foolish to go to the extreme and say awareness was the whole answer and all behaviour was under voluntary control. This seems patently not to be the case. Phobics, for example, are often painfully aware that their behaviours are irrational, but, nonetheless, cannot gain voluntary control over them. Bandura has suggested that certain situations may arouse fearful or anxiety producing thoughts which in themselves are not under voluntary control. Self arousing events may have two components one of which is modifiable voluntarily, the other requiring disconfirming experience.

I am not too convinced by Bandura's sally in this direction. At times he appears merely to relate events which are already adequately described, this is not always a futile exercise, alternative construing sometimes provides insight, but I am not sure it does here. The elements which satisfy me least are his rather too free use of mentalistic terms such as voluntary, awareness and rational. Earlier aspects of the theory also use mentalistic concepts, but these are more clearly tied down, concepts such as anticipation and abstraction clearly indicate, within the reference of

the theory, the ability to generalize and predict and organize episodic events to a greater extent than is possible with what might be called a naive peripheral behaviourist view point. The theory could undoubtedly be improved if it was acknowledged that all explanations do not necessarily observe the same laws of relationship, but this does relatively little damage to the theory up to this point. The theory only seems to become really vague, and not particularly useful, when the concepts of volition and awareness are introduced. They do not appear to be adequately thought out or defined. What are the mechanics and relations of the two? Does volition affect or effect behaviour? If it does what does it mean to say some phobias are partly under voluntary control? Is the behaviour changed or is it not? By phobia does he mean more than behaviour e.g. what the person says, his physiological reactions, and his behaviour, with only one part under voluntary control? This is really not satisfactory. It is well known that these three aspects are not well correlated. What do we conclude if somebody reports fear but does not behave with avoidance? That he is a self-report phobic, but not a behavioural one? If he says he is not afraid but behaves as though he is - that he has voluntary control over his reports but not over other behaviour? or that there are two 'volitions'? or, as we do not know the relationship between volition and awareness, that he is aware of what he wants to say but not of what he wants to do, or vice versa.

Ryle has argued that volition was introduced as a close relative of responsibility, but if it is looked at closely it fades away into an infinite regression. One is responsible for an act only if one wills it, but does one have any responsibility for willing? Is it voluntary?

If not surely the act can not be culpable or praise worthy. Therefore voluntary control of volition must be postulated and voluntary control of these and so on. Similarly exactly how volition and awareness influence behaviour is not made explicit.

These points are made less as an attack on any philosophical position Bandura may hold but simply to point out that these terms, as he uses them do not appear to increase our predictive powers beyond the point allowed by existing descriptions and explanations. Nor are they defined with such precision that they provide explanations of greater parsimony.

To return to outlining the theory, both Mischel and Bandura stress the importance of expectancies. Mischel talks of stimulus and behavioural expectancies, while Bandura adds, what he calls, "efficacy expectations", the belief in ones ability to perform the activities. That is even if a person believes a certain outcome will follow reliably a specific behaviour he is less likely to attempt the activity if he believes he cannot complete it successfully. Various factors will influence this belief: success at similar tasks in the past, the availability of sufficient information to instruct oneself through the task, vicarious experiences, either watching or being told about the performance of others, persuasion and exhortation either by oneself or others, and one's emotional state. Yet other factors will influence whether or not one attempts the activity.

Expectancies and contingency rules govern which behaviour will be performed, and this will be the one which leads to the subjectively most valued outcome available. If the situation contains some specific information it will be this which determines behaviour. Whereas if there is no

particular information of specific salience,behaviours of general functional utility will be employed. "A persons expectations mediate the degree to which his behaviour shows cross-situational consistency." (Mischel, 1973, p.272) The individual will, of course, believe that particular stimuli will, at times, predict, more or less reliably, specific outcomes. These relationships may be idiosyncratic or held in common by a culture. Mischel has observed that in reality the relationship may be no better than a correlation of .2 or .3, but this might well be good enough to reliably elicit a response if a partial reinforcement schedule prevails. Of course the subjective value of the outcome will be of critical importance, but even here perfect, absolute, consistency is not predicted. Factors such as the rewards or punishments others receive will alter the value of the experienced out come, the self evaluation employed, social and physical conditions obtaining will all have their effects.

Extrinsic rewards are obviously of importance, as are intrinsic ones e.g. stepping out of the rain, but both of these seem to have a larger part to play in the regulation of behaviour, rather than the learning of it. After all a skill may not be interesting, intrinsically rewarding, or of use in obtaining external rewards until it can be performed competently. External incentives can be used to maintain performance until proficiency is established, but this is usually not the case. Nor do men appear to be totally at the mercy of external reinforcement, not only are courses of action pursued in the absence of external reinforcement, but at times in spite of the existing reinforcement contingencies. It is for this reason that both Mischel and Bandura lay great stress on the importance of self-evaluation and self reinforcement.

Bandura, having reviewed a number of relevant studies, concluded that conditional self-reward is, or at least can be, learned from models and goes on to comment: "Once achievement standards are adopted through example or percept, self regard becomes conditional upon valued attainment." (p.143). The overall result of this state of affairs is that positive self-regard, and avoidance of negative self-regard, become the primary incentives themselves, with external rewards merely helping to maintain these standards, and providing differential support for the more and less adaptive ones. On the importance of this internal regulatory system Mischel comments: "A comprehensive approach to person variables must take account of the individual's self regulatory systems. These systems include: the rules that specify goals or performance standards in particular situations: the consequences of achieving these criteria; self instruction and cognitive stimulus transformations to achieve the self control necessary for good attainment; and organizing rules (plans) for sequencing and termination of complex behavioural patterns in the absence of external supports and, indeed, in the face of external hinderances." (1973, p.275).

The highest standards are not always upheld, and the methods used to avoid the activation of negative self-evaluative processes have long been outlined by social psychologists. Such processes as: attempting moral justification, or employing euphemistic labels, are used; or alternatively minimizing, ignoring or misconstruing the consequences of one's behaviour; or a third form of strategy is to dehumanize the victim, or attribute to him the blame for one's behaviour. "It is self-exonerative processes rather than character flaws that account for most inhumanities", declares Bandura (p.158).

The story appears to have come almost full circle, starting with the main emphasis on individual differences and intra-organismic variables, moving on to an appreciation of the importance of situational variables, and then on again to the importance of individual differences and variables which are inferred from their consequences. So that towards the end of his book Bandura, sounding rather like Allport, says, "environment is only a potentiality until it is actualized by appropriate behaviour." (p.195)

People construe situations, have expectations of outcomes, perform behaviour which in turn either demands a new construction of the situation or gives rise to new expectations. Which comes first the person or the situation? It is really a hen and egg type problem and Bandura believes it is better to appreciate the intimate relationship, or reciprocal determinism as he calls it, than pretend that one element is of paramount importance. It is for this reason that he rejects the partitioning of variance type approach to the problem and the more simple statements about interaction. "Because personal and environmental sources of influence function as inter-dependents rather than separate determinants, research aimed at estimating what percentage of behavioural variation is due to persons and which to situations does not throw much light on the interactive aspects of regulatory processes. Nor is evidence that much of the variation is usually due to joint effects of personal characteristics and situational conditions especially instructive. Rather, to elucidate the process of reciprocal interaction between personal and enviromental influences, one must analyze how each is conditional on that of the other." (p.197).

It is for this reason that Bandura supports the research approach adopted by Raush. We can all call to mind people

who by their own behaviour force others to confirm their hypotheses, as Raush's hyper aggressive boys do. Or imagine two rats in a Skinner box one pushing a lever thus avoiding punishment, the other not, and experiencing the punishing environment which remains only a potential for the first rat. So separation of the person the environment and behaviour is always some what arbitrary and artificial

Despite the fact that social learning theory stresses the importance of the individual and intra-organismic, unobservable variables it is a very different type of theory to trait theories. Some might argue that it merely makes explicit what is often implicit in trait theories, or that the problem with trait theories is more to do with the manner in which they are employed than what they attempt to say. I have no doubt there is some truth in these objections. Nevertheless social learning theory is attractive because it starts from the observation that people are different and, within their own behaviour, variable. Despite the dissatisfaction already expressed with the way the intra-organismic variables are employed, and the additional uneasiness that they are at times employed simply to replace rather laborious but adequate descriptions, they seem more attractive than those employed by trait theorists (a) because they are integrated in a wide, more generally applicable theory (b) because it seems, at this stage, to be a more straight forward exercise to link them both to physically observable structures, and to observable lawful relationships.

This second point seems to me to be important because it suggests that if, for example, dimensions of the nervous system can be described, which form the basis for individual differences in abilities to learn - the kind of work Pavlov, Teplov, Nebylitsyn and others of that school

have been reporting - then at a gross level we will be able to describe people on trait-like dimensions, but also preserve the necessity of requiring detailed information before specific predictions can be made.

One other remark needs to be made before I close this section. Neither Bandura nor Mischel attempt to come to terms with the thorny problem of what constitutes an explanation. At one point Bandura observes that conditioning does not provide an explanation of behaviour merely a description of it. I think if he had allowed more space for the development of the idea he may have been forced to conclude that many of the statements which trait theories make now are perfectly acceptable. Often we attribute to them a status which is not properly theirs and this results in confusion, but in so far as they serve as useful descriptions and categorizations of behaviour they are both useful and desirable. It is not always advantageous to have descriptions in miniscule detail employing large numbers of variable.

In summary then in as far as social learning theory, despite its limitations, attempts to draw together the findings of different areas of psychology and in as far as it explicitly acknowledges the intimate relationships between people, situations and behaviour it is considered an appropriate starting place from which to attempt to understand either individual differences or behaviour.

CHAPTER III: EYSENCK'S THEORY OF PERSONALITY

3:1 INTRODUCTION

It has been claimed in the previous chapters: (a) that people tend not to behave with absolute consistency; (b) that the term 'consistency' has been used in a rather loose manner, leading to some confusion, and that it seems unlikely that trait theorists would maintain that people are absolutely consistent; (c) that statements to the effect that both the individual and the situation are important determinants of behaviour, or that these two interact with each other, verge on being tautological. Without greater elaboration they are useless in helping to predict behaviour or understand individual differences; (d) that the application of learning principles, with the explicit acknowledgement that people differ in their abilities, would help us in appreciating the organization of behaviour.

It might appear at this stage that this emphasis on the uniqueness of the individual and preference for an idiographic approach to personality study is complemented by an implicit assumption that there can be no possibility of nomothetic generalization. It is not necessary, however, that the idiographic and nomothetic approaches should be viewed as mutually exclusive.

It seems probable that within a single culture, and possibly to a lesser degree within a single species, there will be a considerable degree of similarity in both reinforcement history and generalization, there is certainly a considerable pressure to conform. This, one would expect to produce an overwhelming similarity between individuals, but, it has

been argued, people are, to a substantial extent, quite different from one another. One reason suggested for these differences was that individuals have differential learning ability. However this bald statement, if elaborated, could supply the basis for broad generalizations about individuals, or groups of individuals. If we elaborate the statement that people differ in their ability to learn or condition and instead construe this ability in dimensional terms, the dimension running from 'good' learners, or conditioners, to 'poor' learners, or conditioners, then we ought to be able to make some fairly broad statements about people, at least with reference to behaviours which are assummed to be learned. At this stage we would be making dispositional statements and be in some danger of becoming embroiled in circularity. One way of breaking out of this circularity would be to suggest a neurological basis for our conditionability dimension, thus providing a priori grounds for the prediction of differential conditionability. This, in turn, would have the effect of leading us to claim that to be identical in behaviour two people would need to have not only identical learning histories but also identical nervous systems, or at least a functional equivalence in the interaction of learning and nervous system. The addition of a nervous system dimension might also provide us with a richer set of hypotheses; the nervous system differences underlying differential conditionability may also give rise to other more or less predictable differences in observed behaviour.

This is only one of a potentially infinite number of possible solutions of the generality-specificity problem. However, viewed in this light theories such as Eysenck's biological model of personality do provide a possible link between the nomothetic and idiographic approaches, and obviate

the necessity of proposing two types of theory, one attempting to predict and explain the fine detail of behaviour and stressing the differences between people, the other supplying dispositional and patterned descriptions of behaviour and emphasizing the similarities between individuals.

3:2 EYSENCK'S THEORY AND CONSISTENCY

A convenient way to start the discussion of Eysenck's theory is by 'locating' it in the consistency-interactionist issue. In his book "The Structure of Human Personality" (1960) Eysenck offers the following definition of personality: "Personality is the more or less stable and enduring organization of a person's character, temperament, intellect and physique, which determine his unique adjustment to the environment". (p.3). The fact that he feels it necessary to include the term 'organization' in his definition, in itself, indicates he would not be keen on espousing any specificity view of personality. Perhaps more importantly he conceives of the organization, rather than behaviour, as being "stable and enduring", which might be read as implying that what consistency there is to be found must be sought in generalized patterns of behaviour, what was earlier termed coherence, and rank order consistency, but not at the level of absolute consistency.

Eysenck is proposing a nomothetic theory and places the emphasis on the relative consistency and coherence of behaviour, but it would be a misrepresentation of his position to suggest that he rejects, in consequence, any possibility of specific responding. After an extended discussion on Hartshorne and May's (1928) study on the moral behaviour of children he observes: "There is truth in the contentions of the adherents of the theory of

specificity, as well as those of the adherents of the theory of generality; the problem ceases to be a theoretical one, and becomes instead quantitative and empirical". (1960, p.8-9). (The previous chapters illustrate the danger of dismissing the problem quite so easily).

Nevertheless it is statements at the typological level on which Eysenck consentrates, believing these to be of particular importance. Indeed he observes "Classification is an absolutely fundamental part of the scientific study of personality; a satisfactory typology is as necessary in psychology as was Mendeleyeff's Table of Elements in Physics". (Eysenck and Rackman, 1965, p.15). He views his three dimensional model of personality as going someway towards this ideal.

However he does provide an explicit description of the relationship between the specific responses people make and the more generalized typological level which he proposes.

"We are dealing with four levels of behaviour organization. At the lowest level, we have the specific responses.... These are acts, such as responses to experimental tests or to experiences of everyday life, which are observed once, and may not be characteristic of the individual. At the second level, we have what are called habitual responses.... These are specific responses which tend to occur under similar circumstances, i.e. if the test is repeated, a similar response is given, or if the life situation recurs, the individual reacts in a similar fashion. This is the lowest level of organization; roughly speaking, the amount of organization present here can be measured in terms of reliability coefficients, i.e. in terms of the probability that on repetition of a situation behaviour will be consistent.

"At the third level we have organizations of habitual acts into traits... These traits - irritability, persistence, rigidity, etc. - are theoretical constructs, based on observed inter-correlations of a number of different habitual responses; in the language of the factor analyst, they may be conceived of as group factors.

"At the fourth level, we have organization of traits into a general type; in our example, the introvert. This organization is based on observed correlations, this time on correlations between the various traits which between them make up the concept of the type under discussion. Thus in our example, persistence, rigidity, subjectivity, irritability, and various other traits would form a constellation of traits inter-correlating among themselves, thus giving rise to the higher order construct, the type". (Eysenck, 1947, quoted Eysenck, 1960, p.13-14).

Thus the theory is made up largely of, what have been termed, dispositional statements, with each higher order abstraction being a more generalized summary of groups of specific responses which have been found to correlate. When Eysenck says, "persons who behave in a social manner in one situation tend to behave in a social manner in other situations." (1967, p.37). He cannot be arguing for an absolute consistency, but rather that people who display those specific and habitual responses subsummed under the trait title of sociability in one situation are likely to display those or similar behaviours in other situations. Further, as we are dealing with a correlational system, those individuals who emit relatively large numbers of social responses in one situation, in other situations will also emit large numbers of social responses, relative to other people, although their absolute level of "behavioural" sociability may

not be constant from situation to situation. I say "behavioural" sociability because Eysenck might well claim that the level of sociability is, in fact, relatively constant at least in as far as it is a property of the nervous system. That is, the genotypical sociability, or extraversion, is constant, though its phenotypical manifestations may display some inconsistency. As already observed the genotypical-phenotypical distinction saves Eysenck from the circularity of the: "he is sociable because he is an extravert, and he is an extravert because he is sociable", type. It is at the phenotypical level that situational influences are hypothesized as having their major effect. "Primary traits, such as sociability, impulsivity, ascendency, optimism, and so on, which combine to make up our phenotypical concept of extraversion, arise through the confluence of a person's genotype, i.e. his excitation/inhibition balance, with a great variety of environmental influences." (1967, p220-221). He offers the following diagramatic sketch of the relationship between the genotypic and phenotypic levels.(cf.Fig.3:1)

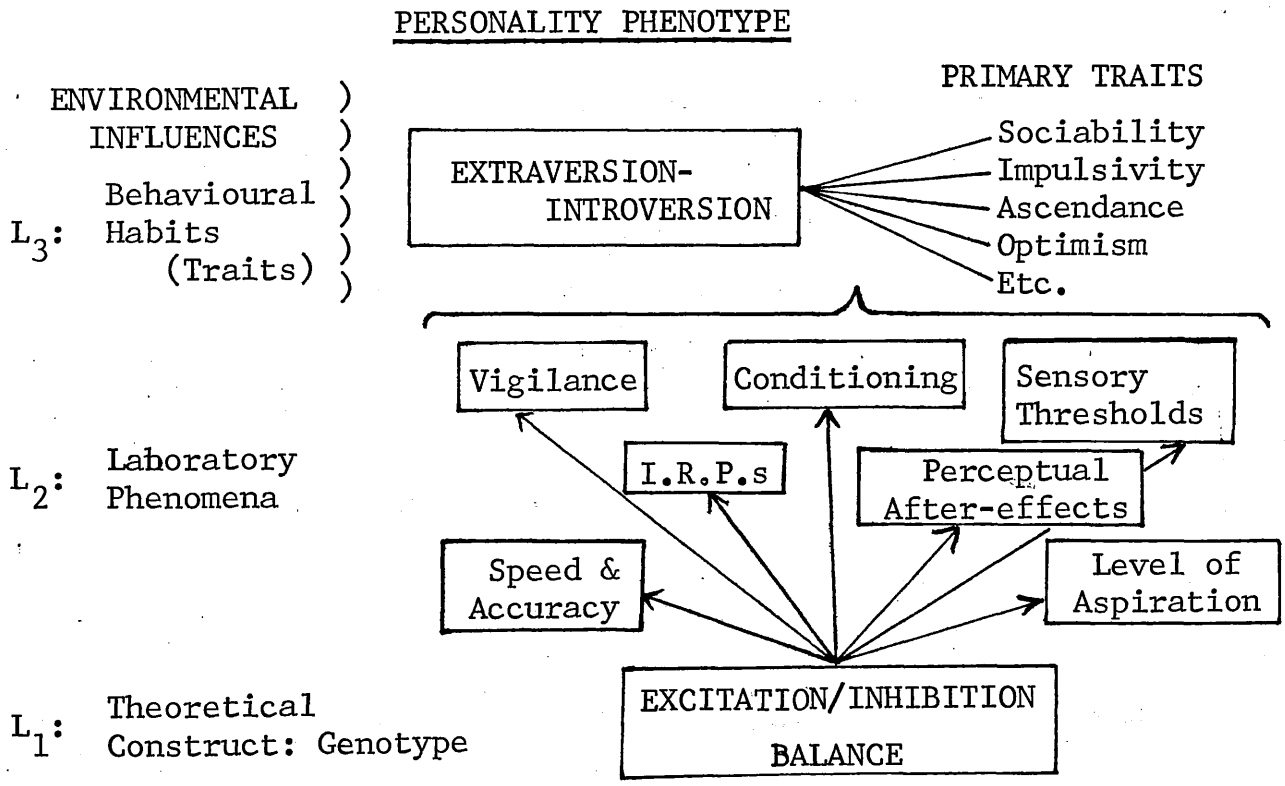


FIG 3:1 Relation of Personality phenotype to genotype and environment.
(From Eysenck, 1967, p.220).

3:3 THE DIMENSIONS OF PERSONALITY

As for the actual structure of Eysenck's model, he proposes that there are three major dimensions of personality: extraversion - intraversion, neuroticism and psychoticism. Working on the correlational principles outlined above he arrives at these three factors largely on the basis of factor-analysis, but he argues that they have a validity beyond the factor analytic technique. In fact he claims to have found similar dimensions in macaco mulatto monkeys, again using a factor technique. (Chamove, Eysenck and Harlow 1972). He comments: "It might at first seem surprising that similar factors emerge from two different species, but there are good reasons for expecting such agreement. The first reason is linked with the simple fact that monkeys and men (and rats also) have similar anatomico-physical structures to subserve emotional/fearful behaviour, i.e. an autonomic system and a visceral brain, and arousal behaviour, i.e. cortex linked with an ascending reticular formation; one would expect individual differences in behaviour to be linked with difference in the functioning of both these systems, and these behavioural differences would be expected to be more similar to those observed in humans, the closer the species under measurement was to Homo sapiens". (Chamove et al, 1972, p.502).

So the reality of the dimensions may be viewed as being at the level of the organization and functioning of the nervous system.

Of the three dimensions psychoticism is dealt with in least detail, it is also of least importance in the present context. Eysenck and Eysenck (1976) have reviewed the relevant literature. They conceive of the dimension as being orthogonal to both the extraversion and

neuroticism dimensions, "predisposing persons to pychosis in varying degrees, and inherited as a polygenetic character; this predisposition would extend into the psychopathic and criminal, and anti-social fields, but not into that of dysthymic neuroses" (1976, p.22).

3:4 EXTRAVERSION

Of the other two dimensions extraversion-introversion has attracted more attention. At the behavioural level he has offered the following descriptions of individuals at the extremes of this dimension: "The typical extravert is sociable, likes parties, has many friends, needs to have people to talk to, and does not like to read or study by himself. He craves excitement, takes chances, often sticks his neck out, acts on the spur of the moment, and is generally an impulsive individual. He is fond of practical jokes, always has a ready answer, and generally likes change; he is carefree, easy going, optimistic, and likes to 'laugh and be merry'. He prefers to keep moving and do things, tends to be aggressive and lose his temper quickly; altogether his feelings are not kept under tight control, and he is not always a reliable person.

"The typical introvert is a quiet, retiring sort of person, introspective, fond of books rather than people, he is reserved, distant except to intimate friends. He tends to plan ahead, 'looks before he leaps', and distrusts impulses of the moment. He does not like excitement, takes matters of everyday life with proper seriousness, and likes a well ordered mode of life. He keeps his feelings under close control, seldom behaves in an aggressive manner, and does not lose his temper easily. He is reliable, somewhat pessimistic, and places great value on ethical

standards." (1963 (a), p.52).

Moving beyond simple behavioural description Eysenck sees the antecedents of this theory in the work of Pavlov and Hull, with the result that the biological basis of personality dimensions, the constructs of excitation and inhibition, and conditioning all play central roles in the theory. He procedes by relating these to the phenotypical descriptions of personality types in two postulates. The "Postulate of Individual Differences" states: "Human beings differ with respect to the speed with which excitation and inhibition are produced, the strength of the excitation and inhibition processes, and the speed with which inhibition is dissipated. These differences are properties of the physical structures involved in making stimulus response connections".

The "Typological Postulate" states: "Individuals in whom excitatory potential is generated slowly and in whom excitatory potentials so generated are relatively weak, are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterical-psychopathic disorders in cases of neurotic break-down; individuals in whom excitatory potential is generated quickly and in whom excitatory potentials so generated are strong, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in cases of neurotic breakdown. Similarly, individuals in whom reactive inhibition is developed quickly, and in whom strong reactive inhibitions are generated, and in whom reactive inhibition is dissipated slowly are thereby predisposed to develop extraverted patterns of behaviour and to develop hysterical-psychopathic disorders in cases of neurotic breakdown; conversly, individuals in whom reactive inhibition is

developed slowly, in whom weak reactive inhibitions are generated, and in whom reactive inhibition is dissipated quickly, are thereby predisposed to develop introverted patterns of behaviour and to develop dysthymic disorders in case of neurotic breakdown." (1957, p.114).

It is worth pointing out that Eysenck has made a number of unsupported assumptions in these postulates: That the speed of development of both excitatory and inhibitory potentials is positively correlated with the strength of those processed, i.e. fast development correlates with strong potentials; that the speed of development of inhibitory potentials is negatively correlated with speed of dissipation; that there is a negative correlation between both the speed of development and strength of excitatory and inhibitory potentials.

These contentions are of importance in view of recent suggestions (e.g. Eysenck, 1966, 1967; Gray, 1965, 1967) that the extraversion-introversion dimensions might be similar to certain nervous system dimensional properties proposed by Pavlov and his followers. Eysenck (1967) has speculated that introverts might correspond to Pavlov's weak nervous system types, while extraverts correspond to the strong nervous system types. Gray(1967) has suggested that the extraversion-introversion dimension might be related to strength of the excitatory processes, or possibly to equilibruim of dynamism. Such identifications have not always been readily accepted. Nebylitsyn (1966) White, Mangan, Morrish and Siddle (1968), and Strelau (1970) have all questioned whether or not Eysenck's concept of inhibition is identical to that of Pavlov and Teplov. Strelau comments: "Summarizing the foregoing evidence leads us to the conclusion that EYSENCK's concept of

inhibition, inconsistent in itself, differs basically from Pavlov's and his followers' views on the question. It would be therefore irrelevant to compare Eysenck's two types with Pavlov's weak and strong types from the point of view of convergence of views." (1970, p.22). Prior to this comment he discusses a number of difficulties in identifying the extraversion-introversion dimension with either the dimensions of strength of the nervous system or with dynamism of the nervous system.

Nor are the Russians ready to accept Eysenck's assumption of the negative relationship between excitatory and inhibitory processes. Nebylitsyn (1966) insists that, "Each of the nervous system properties should be separately measured for the inhibitory and excitatory processes; when using a quantitative approach, this involves the determination of two 'absolute' values for the given property." (1966, p.21). Thus allowing for the properties of strength of the excitatory and inhibitory processes to be positively or negatively correlated, or for them to be independent of one another.

Strelau (1970), assumming independence of strength of the nervous processes according to excitation and inhibition, compared scores on the extraversion-introversion dimension with scores on various nervous system properties, these were measured by means of a questionnaire which is in itself unusual. He reports the following correlations:

TABLE 3:1 Correlations between the Properties of the Nervous System and Eysenck's Basic Personality Dimension.

	Extraversion		Neuroticism	
	A*	B*	A*	B*
Strength of excitatory process	0.449	0.476	-0.478	-0.557
Strength of inhibitory process	-0.007	0.028	-0.450	-0.526
Mobility of nervous process	0.667	0.652	-0.300	-0.215

(From Strelau, 1970, p.23)

(*'A' and 'B' refer to correlations obtained in two separate experiments. 'A' was based on a sample of 78 Ss, 'B' on a sample of 159 Ss. E and N were measured by a translated version of the M.P.I.)

These results suggest that neither extraversion nor neuroticism can be claimed to be identical with any of the nervous system properties measured, but perhaps show greater correspondence with Marton's (1972) suggestion: "it seems reasonable to suppose that such a complex personality dimension as extraversion-introversion may be collectively determined by several basic properties of the nervous system, forming a complex constitutional disposition" (1972, p.223).

3:5 THE BIOLOGICAL BASIS OF EXTRAVERSION

It might be as well at this stage to indicate what Eysenck wishes to imply by the terms excitation and inhibition. In what he calls the "weak" version of the concepts excitation is regarded as the facilitation of cortical events underlying perception, learning and responding, whereas inhibition is the depression of these processes. Thus both excitation and inhibition are conceived of as central states. In the 'Strong' version of these constructs he attempts to relate excitation and inhibition to the activities of particular neurological systems and structures.

Behavioural differences between extraverts and introverts, Eysenck proposes, are related to differential thresholds of particular loci, associated with facilitation or inhibition, in the ascending reticular activating system (ARAS), and the effect this has on the over all functioning of a cortico-reticular-loop. Eysenck suggests that while the ascending classical pathways carry detailed sensory and executive information to the cortical projection areas, collateral

fibres from these pathways are elaborated in the reticular formation (R.F.). Ascending fibres from the R.F. provide a non-specific afferent bombardment of the cortex, and it is this state of "functional t onus" which Eysenck refers to as 'arousal'. Impulses of both the classical pathways and the cortical projections into the R.F. travel more rapidly than those of the collaterals, this difference in transmission latency being sufficient to allow the cortex to exercise an inhibitory influence over the R.F. "Thus", remarks Eysenck, "the cortex is able to modify its own arousal in response to specific information reaching it through the ascending afferent pathways; in this way a much more refined adjustment to the exigencies of the situation becomes possible and learning and conditioning factors can affect the state of the cortex in ways which would be difficult to account for if such reciprocal action could not be postulated." (1967, p.239).

Differences, then, between introverts and extraverts are assummed to be a reflection of the overall functioning of the cortico-reticular loop rather than simply of differential threshold for elaborating 'arousal' or synchrony (inhibitions) of particular loci in the R.F. The theory, then, is one of a functional balance between excitatory and inhibitory processes, rather than the absolute magnitude of either.

Besides this cortico-reticular loop Eysenck suggests there is also evidence for a "centrifugal regulation of afferent influx". The implication of this appears to be that the afferent impulses, themselves, may be potentiated or inhibited as appropriate. He quotes Samuels (1959) as saying: (a) "At the level of the sensory receptors, the spinal cord, and in the specific sensory paths prior to the point at which they give off collaterals to the reticular formation, both the arousal and the cue effects of stimuli may be

controlled". (b) "In the reticular formation itself, the arousal effects of stimuli may be enhanced or inhibited". (c) "In the cortex, the cue value of the stimuli may be affected." (quoted Eysenck; 1967, p.240). He continues by quoting Hernandez-Peon (1961) as saying "the reticular mechanisms of sensory filtering are formed by feedback loops, with an ascending segment from second-order sensory neurons to the reticular formation and a descending limb in the opposite direction. Such an arrangement prevents over-activation of sensory neurons and, therefore, an excessive bombardment of the brain by afferent impulses. Their exclusion takes place at the entrance gates of the central nervous system. The first sensory synapse functions as a valve where sensory filtering occurs." (quoted Eysenck, 1967, p.240-241). The implication for the extraversion-introversion dimension is, presumably, that individuals differ in their ability or tendency to enhance or inhibit incoming stimuli., Presumably these circuits are in some sense more sensitive in the introvert, so that he enhances low strength signals to a greater extent, or/and begins to inhibit them at an objectively lower signal intensity.

The R.F. is also, some (e.g. Jung and Hassler (1959)) would argue primarily, a motor co-ordinating centre, and consequently, given the importance already placed on the differences in the R.Fs. of introverts and extraverts, it would be hypothesized that there ought to be detectable differences in the gross motor movements of introverts and extraverts.

The net result of all this is, of course, that introverts, as compared to extraverts, are assummed to have lower sensory threshold, to show a decrease in efficiency at lower levels of arousal, to condition more efficiently given

certain appropriate conditions, while extraverts will become easily bored, be inefficient on detailed tasks, take more rest pauses etc. Eysenck has reviewed a large number of studies which compare the performance of introverts and extraverts on a wide range of tasks, such as vigilance tasks and memory, studies comparing them with respect to involuntary rest pauses, sensory and pain thresholds, various perceptual phenomena, perceptual defence, flicker fusion, motor movement, achievement and aspiration levels, the effects of various drugs, autonomic reactivity, conditioning etc. As these reviews (e.g. Eysenck 1953, 1957, 1967) are readily available as well as several collections of relevant papers (e.g. Eysenck 1960, 1971, 1976). There seems to be little point in duplicating the information especially as only those concerned with conditioning are of central relevance here, and these will be dealt with below. The results of these studies might be summed up by saying that there appears to be some support for Eysenck's model, but they have so far failed to provide unequivocal support for the various hypotheses derived from the theory (c.f. Storms and Sigal. 1958).

3:6 TRAITS OF EXTRAVERSION

There has been some debate as to whether the extraversion-introversion dimension is, in fact, unitary by nature (e.g. Carrigan, 1960, Eysenck and Eysenck, 1963(b), 1967(b), 1978, Sparrow and Ross, 1964, Howarth and Brown, 1972, Howarth, 1976, Buss and Plomin 1975, Plomin, 1976). Two primary factors, sociability and impulsivity, are usually found to constitute the higher-order factor of extraversion. Of course, given Eysenck's approach, out-lined earlier, which assumes that lower order traits should be coalesced to form the more important typological factors, which form the basis

for classification in personality psychology, this is not a particularly surprising finding. The problem revolves very much around what one means to imply by the use of the term unitary. Eysenck and Eysenck (1963(b)) have remarked: "If by unitary is meant simply 'composed of non-independent constituent units' then our results suggest that E is indeed a unitary factor. If what is meant is 'composed of units related in such a way that their correlations form a matrix of rank one', then clearly E is not a unitary factor." (p.52) Eysenck argues that impulsivity and sociability are non-independent, correlating about 0.5, and so are more usefully construed in terms of the higher order factor extraversion.

At exactly which stage one stops looking for higher order factors, seems to be based to a large extent on theoretical bias, one's emphasis, and which kind of factors one feels are most useful. Eysenck (1967), for example, has argued that primary factors, traits, tend to demonstrate a substantial instability and in factor analytic studies difficult to replicate. The second of these arguements seems not to be true for impulsivity and sociability, at least, Cattell (1973) has also taken issue with Eysenck over the replicability of primary traits. While Krug (1978) has taken issue with him in respect of the reliability of primary traits. Although, harking back to Eysenck's model once more, it does seem to be true, by definition, that descriptions will become less 'reliable', i.e. reflect less coherence or rank order consistency, as one moves from the typological level to the specific responding level. As Eysenck himself was quoted as saying earlier "the problem ceases to be a theoretical one, and becomes instead quantitative and empirical". The 'nearer' the trait level is to the type level the more reliable we can expect it to be,

the 'nearer' to the habitual response level the less stable. With the additional problem that all primary traits will not be equally stable superimposed on the reliability of the measurement technique.

Plomin (1976) has suggested that both impulsivity and sociability can be further sub-divided; Eysenck and Eysenck (1978) added venturesomeness as another primary factor worthy of consideration; Eysenck and Eysenck (1969), report 11 and 14 interpretable factors, employing different types of solutions, Howarth and Brown (1972) report 15. The problem seems to be, at least in part, that what you get out of factor analysis depends on what you put in, and as Eysenck and Eysenck (1978), have observed: "whatever factors may be, they are surely concepts, and concepts are useful in bringing order into the chaos of unselected observation, but they do not have any sort of physical existence.... Thus concepts can be useful or useless; they cannot be true or untrue." (pp. 1252 - 1253). Their validity, in this sense, rests largely in their ability to permit one to make accurate prediction.

It seems to be pointless to consider different factor solutions or indulge in factor naming exercises apart from the theories which spawned these factors. The utility of Eysenck's theory rests largely on the hypothesized differences between individuals at the neurological level. It is these differences which allow predictions to be made in relation to behaviour. The theory, at least at its present stage, does not permit a meaningful distinction, at the biological level, between impulsivity and sociability and so there seems to be little point in arguing which is more 'real', the primary or secondary factors, if the primary factors have no relevance at the fount of hypotheses. Although it should be noted in

passing that it has been speculated that impulsivity may give a truer reflection of biological differences than sociability (Eysenck & Levey, 1972, p.217). Of course, the primary factors still retain their utility at the descriptive level.

3:7 NEUROTICISM

To move on then to the final dimension of the Eysenckian system, Neuroticism, or emotionality, this is again considered to be independent of both extraversion-introversion and psychoticism. Neuroticism has been largely over-shadowed by extraversion in Eysenckian research. There are probably many reasons for this, but two seem likely to have been of some importance. Neuroticism as described by Eysenck is a close relative of anxiety, and there already exists a substantial body of research work on this dimension, coming largely from Spence, Taylor, and the so called Iowa group. This group viewed anxiety as being a reflection of Hullian drive (D), and so of being of primary importance in conditioning, while Eysenck has argued that it is the extraversion-introversion dimension which is of central importance in conditioning, and so it was this, the extraversion-introversion,dimension which became the focus of research activity.

The other reason for extraversion-introversion becoming a focus of research activity is that this is regarded as a status dimension, extraverts and introverts are always different,while neuroticism is a process dimension with the result that differences between neurotics and stables will only be evident in the face of emotion provoking stimuli.

Eysenck (1967) quotes Jones' (1960) description of neuroticism: "Neuroticism or vunerability to neurosis

implies low tolerance for stress, whether it be physical as in painful situations, or psychological as in conflict or 'frustration'. In learning theory terms an individual scoring high on a factor of neuroticism would be characterised by a high level of drive in avoidance situations. High appetitive drives are not necessary to the theory and it may be that the high drive of neurotics is aroused only in situations of threat or ego-involvement". (p.41).

It is often assummed that neurotics, as opposed to stables, will display greater autonomic reactivity. Eysenck (1967) reviewed a large number of studies comparing the reactivity of neurotics and stables, such indices as EEG, electro-dermal activity, muscle tension, catecholamine excretion, pupilary responses, heart rate, cardiovascular activity have all been used, and although some studies do report that neurotics are more labile the more general finding is that the neurotic groups fail to show appropriate recovery after the stressful simuli is removed, and often maintain elevated values of the index under consideration. Part of the problem in attempting to investigate lability in this way may be, of course, the almost insurmountable methodological one that the laboratory testing situation in itself is sufficiently anxiety provoking to create differences between the groups, differences which are reflected in a complex manner in the peripheral indices.

Another problem in looking at responsiveness in this way is that the peripheral indices do not correlate well among themselves, and so to test adequately the responsivity hypothesis one would need to record several indices simultaneously. As Lacey (1950) has commented, subjects "respond with a hierarchy of activation, being relatively over reactive in some physiological measures, under reactive in others,

while exhibiting average reactivity in still other measures. These patterns of responses seem to be idiosyncratic; each subject's pattern is different. For a single stressor, patterns of responses have been shown to be reproducible, both upon immediate retest and over a period of nine months. Moreover the pattern of responses obtained with one stimulus condition tends to be reproduced in other quite different stimulus conditions." (quoted Eysenck, 1967, pp.68-69).

3:8 THE BIOLOGICAL BASIS OF NEUROTICISM

Whereas the behavioural differences along the extraversion-introversion dimension were assummed to reflect differences in the central nervous system, particularly a cortico-reticular loop, behavioural differences along the neuroticism dimension are assumed to be reflections of differences in the limbic system and the autonomic nervous system.

Eysenck has followed Papez (1937) and MacLean (1958, 1960) in assumming that the limbic system: the hippocampus, fornix, cingular gyrus, and especially the hypothalamus, in particular the manillary bodies of this structure, as being responsible for emotional expression, organization and experience. The hypothalamus is seen as being of central importance executing a variety of executive functions. The limbic system, or as Eysenck terms it the 'visceral brain", exercises control over the autonomic nervous system, but is "perhaps more effective with sympathetic than with parasympathetic stimulation." (Eysenck, 1967, p.236). "The sympathetic system is, in general, more diffuse in its effects than the parasympathetic, certain parasympathetic reflexes may occur without the involvement of other parts of the parasympathetic system, whereas the sympathetic system tends

to discharge as a whole ". (Morgan, 1965, quoted Eysenck, 1967, p.236). Eysenck regards the extent of this diffuse discharge as reflecting the level of activation-greater activation leading to a more diffuse discharge.

It should not be imagined, however, that the two systems, the cortico-reticular loop and the visceral-brain autonomic system, are entirely independent of one another. Both the cortex and the RF exercise some measure of control over the limbic system, while collaterals from the visceral brain pass into the RF with the result that in states of high emotional arousal the cortex will be 'aroused' or 'excited' in much the same way it would when the organism is subjected to intense stimulation. This, of course, leads to the interesting possibility that although the two dimensions, extraversion-introversion and neuroticism, are normally independent, at times of increased activation, i.e. visceral brain activity, the neurotic will behave in a manner typical of the introvert as a result of the increased cortical excitation. Thus we would predict that for extreme values of neuroticism there will be found a correlation between neuroticism and introversion. Eysenck (1959) has reported findings in line with this hypothesis. If anxiety is a conditionable state we might also expect to find a correlation between these two dimensions for extreme values of introversion, assumming that conditionability increases with introversion.

Apart from the breakdown of the independence of two of his dimensions Eysenck suggests that in the situation of the breakdown of the distinction of the cortico-reticular system and in the cortico-hypothalamic system we have an example of the Yerkes-Dodgson law in effect. In normal, non-emotional, situations introverts are assumed to reach optimal levels of cortical arousal earlier than extroverts with the result that

introverts are likely to reach optimum efficiency at lower objective levels of stimulus intensity and similarly evidence a decline in efficiency earlier. Eysenck quotes Gellhorn and Loofbourrow (1963) as saying that in those situations where the hypothalamus "starts firing nearly maximally under the combined influence of discharge from the reticular formation and the sense organs, the latter impinging on the reticular formation and hypothalamus not only directly but also via the neo-cortex and limbic cortex. Under these circumstances the differentiation in activation pattern and function which exists between various cortical areas under strictly physiological conditions is lessened. The resulting 'functional' decortication is not the result of a 'cortical conflict' ... but is due to an excessive excitation of the hypothalamic system which is incompatible with the differentiated action of the cortex necessary for attention and the higher nervous processes" (quoted Eysenck, 1967, pp.237-8).

3:9 CONCLUSION

In summary, then, Eysenck presents us with a model which breaks away from the circularity of the dispositional statement by suggesting a biological under-pinning for the proposed personality types. These neurological differences between individuals suggest both that there will be certain gross differences in the behaviour of individuals, and also a predictable difference in conditionability, which may still be regarded as an important determinant of detailed behaviour. These biological differences also predispose the individual to respond differentially to emotional stimuli, and this predisposition too is thought likely to influence both efficiency and, in certain circumstances, conditionability. The theory also places some importance on situational variables as

individuals are hypothesized as having different sensory threshold and being differentially sensitive to emotion provoking stimuli, suggesting that there will be more or less predictable relationships between the person and the situation.

This chapter began with the claim that the nomothetic and idiographic approaches to the study of personality were not, necessarily, mutually exclusive. In the previous chapter the stress was placed on the importance of learning and the consequent individuality it produced. The present chapter has emphasized the similarity between people located at a common point on a dimension. The link between these two is that these personality dimensions, in as far as they are observable, are assummed to be reflections of neurological variations, and these neurological differences between individuals are hypothesized as having either facilitating or debilitating effect on conditioning. Thus those individuals who possess the "facilitatory" nervous system will be expected to display "learned behaviours" to a greater degree than those who do not possess such a nervous system. Thus what a person will do in any particular situation will depend on what he has learned to do, but what he has learned to do will depend, to a considerable extent, on his nervous system. Generalized statements can, according to Eysenck's model, be made about the individuals ability to learn, statements about what he has learned are idiographic.

The correctness, then, of Eysenck's hypothesis as to the relationship between his introversion-extraversion dimension and conditionability is central in determining the utility of Eysenck's theory in linking these two approaches. The present chapter has outlined Eysenck's model from which the prediction, that introverts will show superior conditionability to extraverts, given appropriate conditions is drawn. The next chapter

will review the emiprical data relevant to this hypothesis.

CHAPTER IV: PERSONALITY AND CONDITIONING

4.1: INTROVERSION-EXTRAVERSION AND CONDITIONING

Eysenck is not alone in relating a personality dimension to differential conditionability. Whereas, as pointed out in the previous chapter, Eysenck has related the dimension of introversion-extraversion to conditionability, Spence (Spence and Taylor, 1951; Spence and Spence, 1964) has related conditionability to the dimension of anxiety. There already exist several reviews of the relationship between personality dimensions and conditionability, most notably one each by the two main protagonists, Spence (1964) and Eysenck (1965), and perhaps the most comprehensive review of all by Lovibond (1964) which covers not only empirical work, but also a number of theoretical and methodological problems. Rather than attempt to duplicate these reviews I will draw extensively from them adding some more recent data.

Eysenck began his review by stating "Eysenck (1957) has put forward the hypothesis that conditioning would correlate with introversion (I). This hypothesis is based on two major theoretical assumptions: (a) Extraversion (E) is a phenotypic set of behaviour patterns which is related to genotypic differences in the relative ease of arousal of cortical excitation and inhibition, extraverts showing greater inhibition, introverts greater excitation.... (b) Cortical inhibition depresses conditioning and facilitates extinction; this assumption follows directly from Pavlov's theoretical concepts and experimental demonstrations. It would also be expected that cortical excitation would facilitate conditioning provided that the optimum degree of excitation

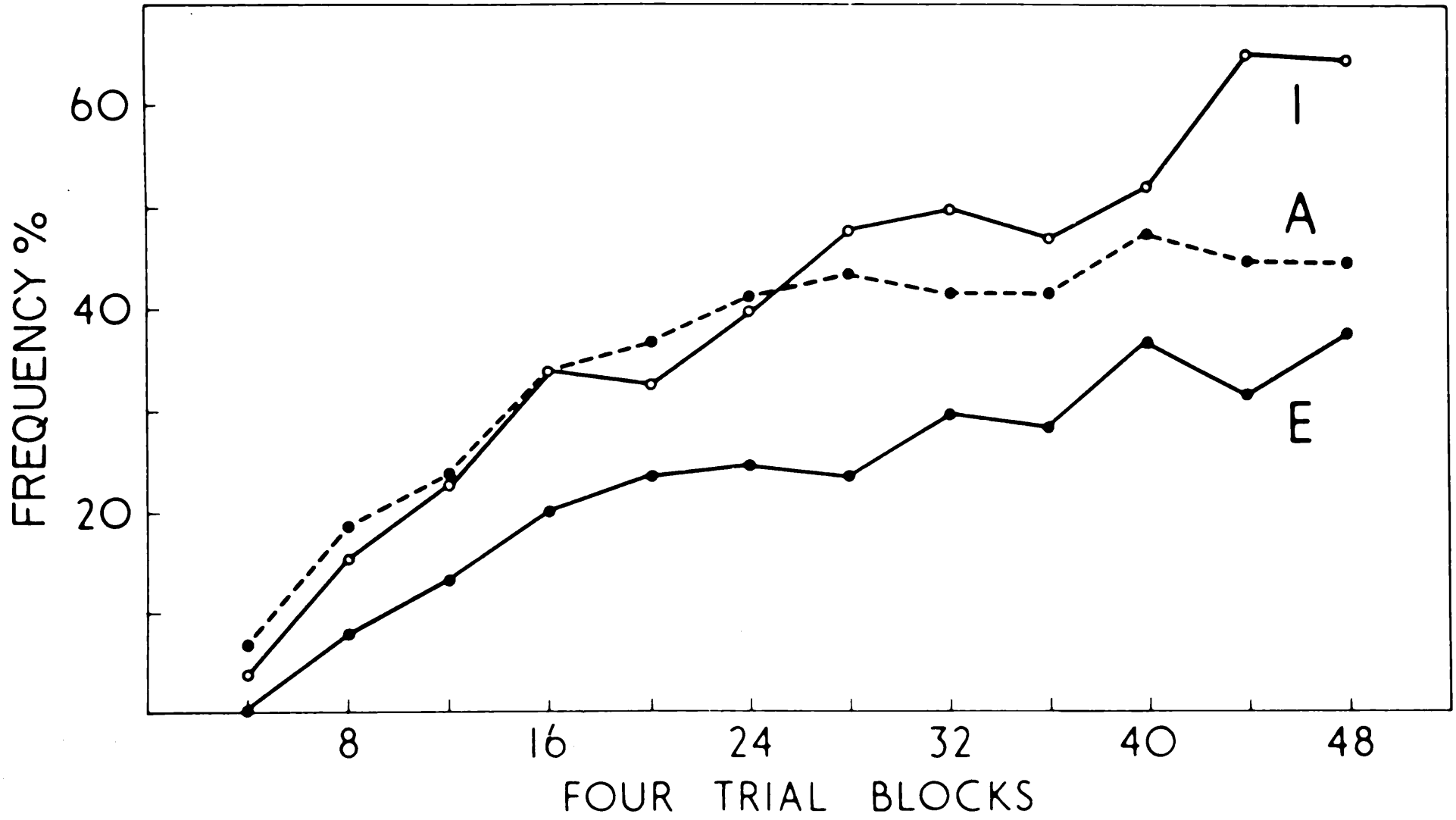
has not yet been reached." (1965, p.258).

It is worth considering these points a little more carefully before dashing off to see whether or not introverts are usually found to condition more efficiently. Cortical excitation is held to facilitate conditioning, but this will only be up to some optimal level, as will be recalled from the previous chapter, increasing excitation above this level leads to a disruption in learning and performance. The result of this is that in under-arousing conditions introverts will be expected to condition more efficiently, because their higher level of excitation ensures that they will be nearer the optimum level of arousal. However, this same state of affairs also ensures that they will surpass this optimum level earlier than the extravert, with the result that the extravert, in over-arousing conditions, will be nearer to the optimum level and so it is he who is expected to condition more efficiently.

Turning to the other process which Eysenck sees as important, inhibition, here it is the extravert who is expected to have the lower threshold. Inhibition is assummed to be detrimental to the formation of CRs with the result that to the extent that the prevailing conditions favour the development of inhibition a greater difference will be evident between the extravert and the introvert, with the extravert showing the greater depression of performance. Thus if the greater conditionability of the introvert is to be witnessed there must be a careful choice of parameters.

Eysenck has long pointed to four parameters which are crucial: (1) partial reinforcement, (2) weak UCS, (3) small CS-UCS interval and (4) differential learning. '1', '3' and '4' all favour the build up of inhibition while '2', obviously, favours the introvert with his higher cortical excitation; as the UCS increases in intensity it becomes more

FIG. 4:1(a) Rate of eyelid conditioning for introverts, ambivents and extraverts under weak UCS conditions (Taken from Eysenck and Levey, 1972; p.211)



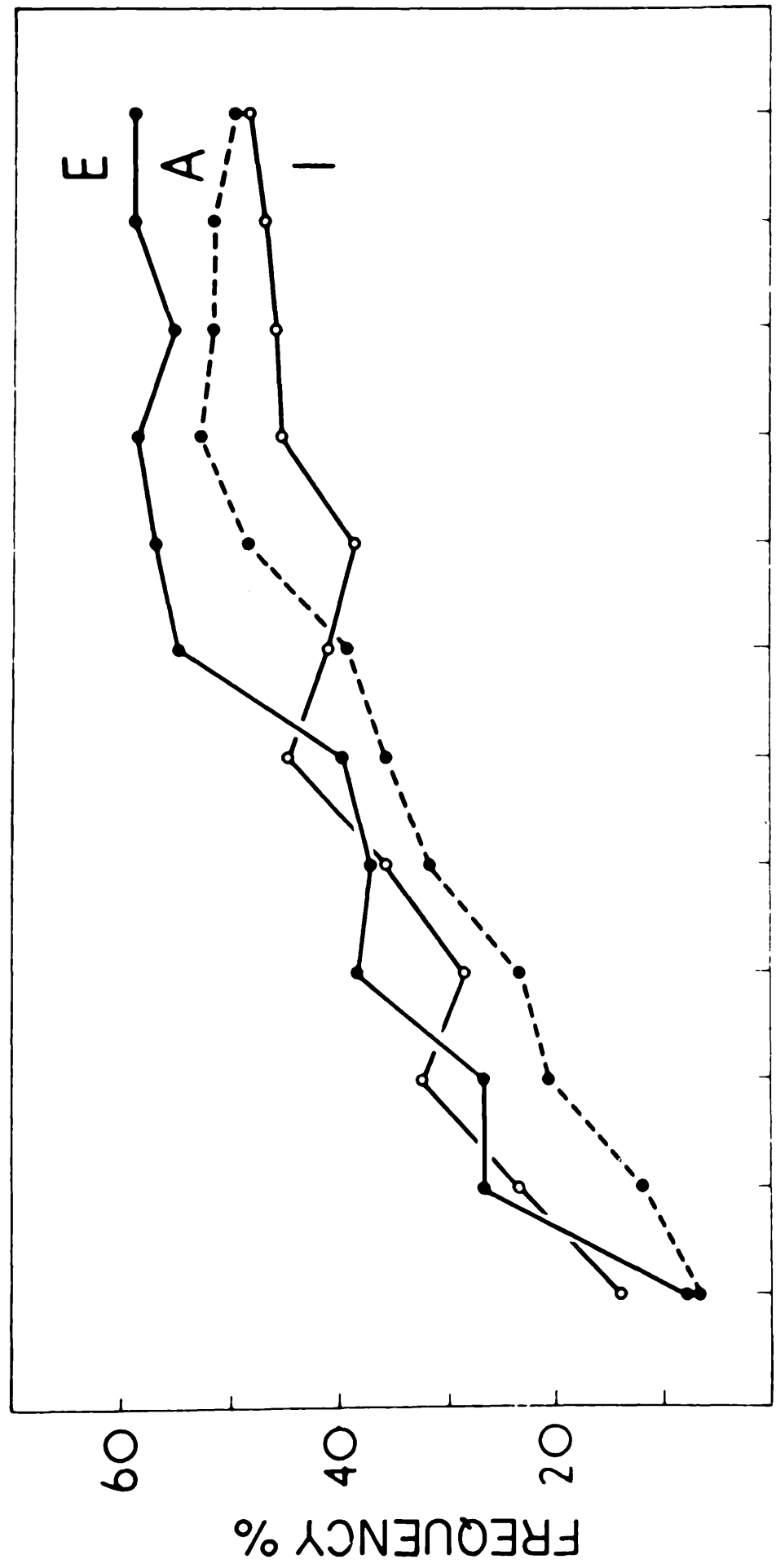


FIG: 4:1(b) Rate of eyelid conditioning for extraverts, ambiverts and introverts under strong UCS conditions (Taken from Eysenck and Levey, 1972; p.211)

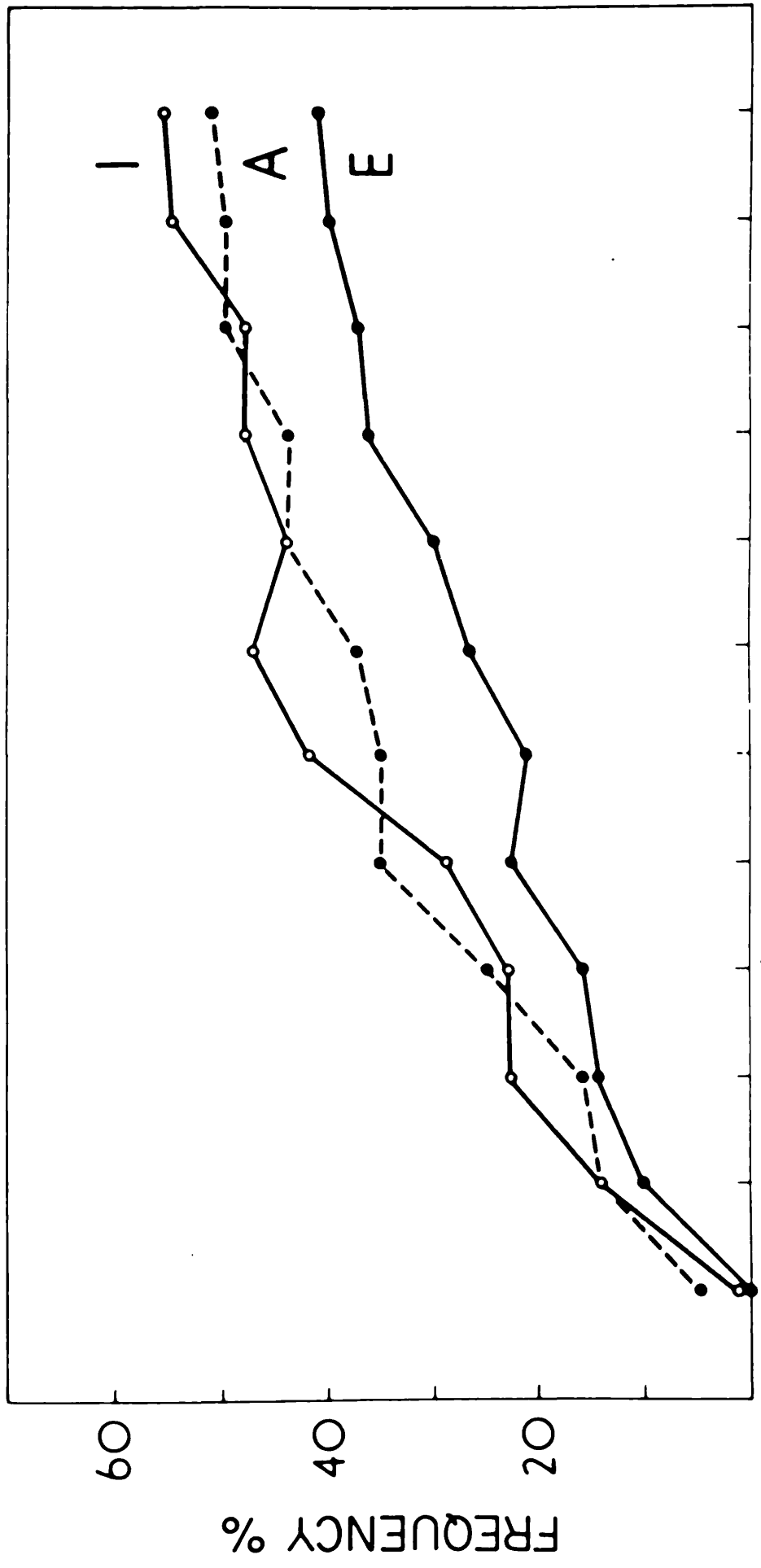


FIG: 4:2(a) Rate of eyelid conditioning for introverts, ambiverts and extraverts under short CS-UCS interval conditions. (Taken from Eysenck and Levey, 1972; p.212).

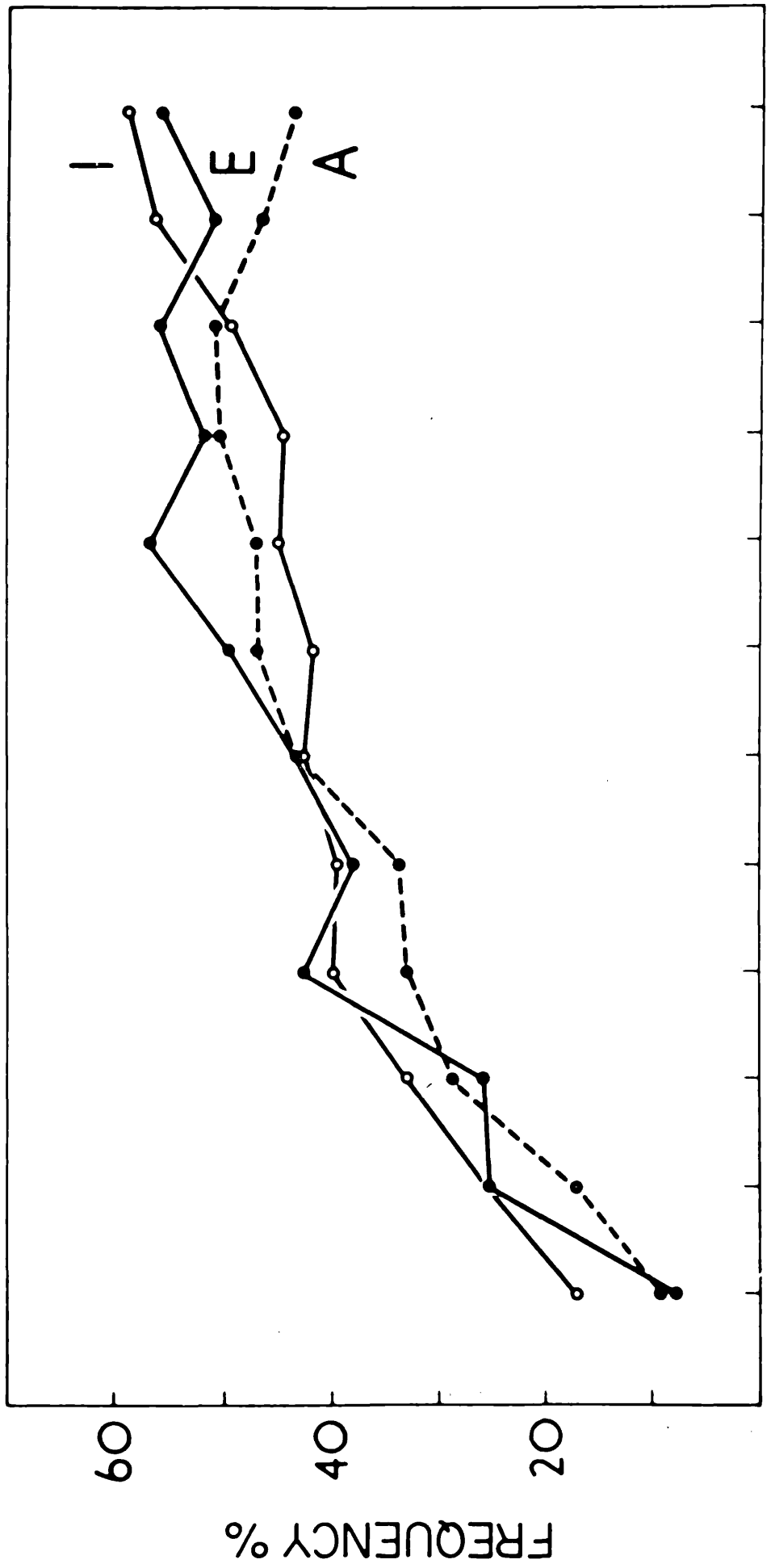


FIG: 4:2(b) Rate of eyelid conditioning for extraverts, ambiverts and introverts under long CS-UCS interval conditions. (Taken from Eysenck and Levey, 1972; p.212).

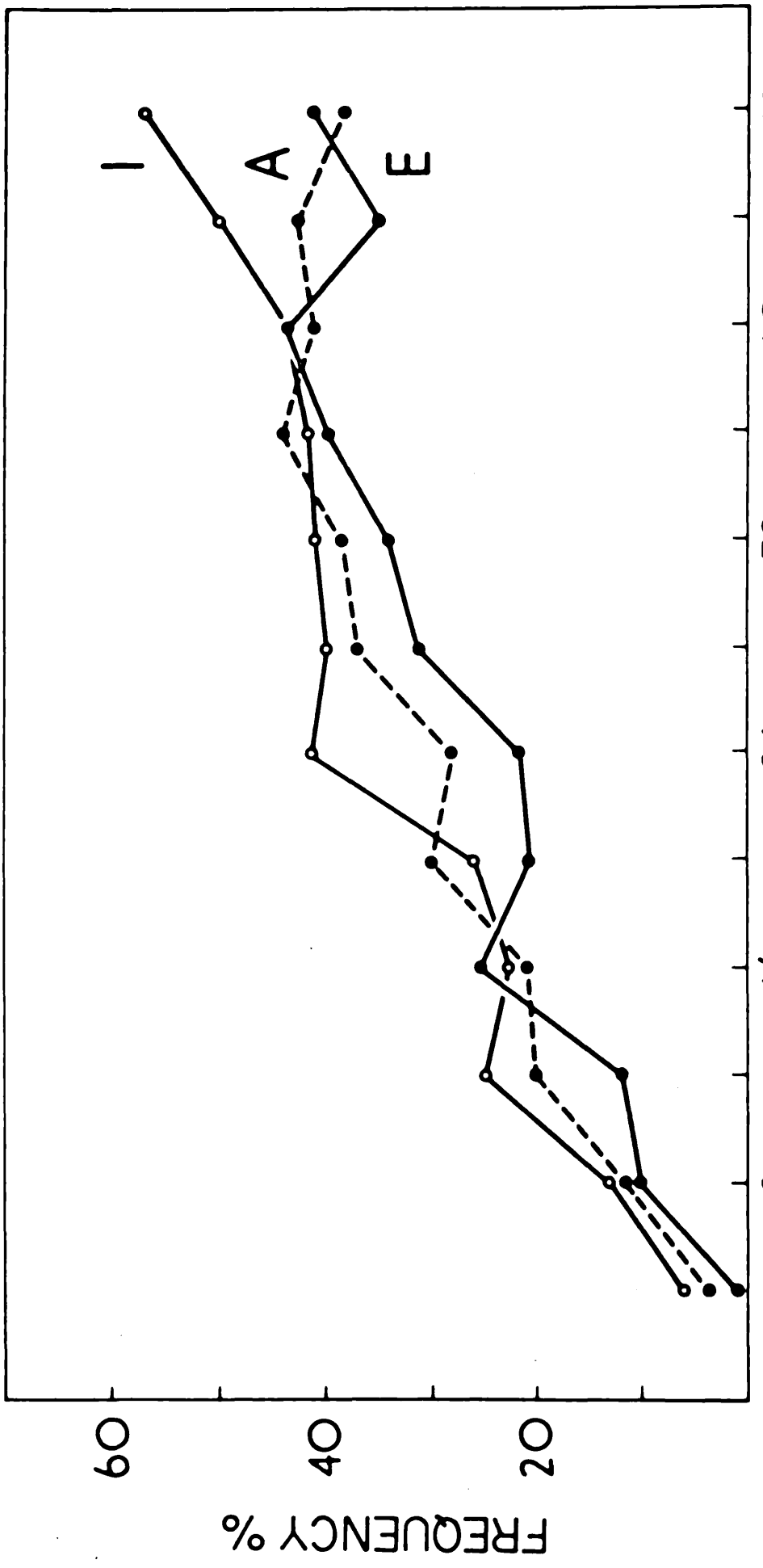


FIG: 4:3(a) Rate of eyelid conditioning for introverts, ambiverts and extraverts under partial reinforcement conditions. (Taken from Eysenck and Levey, 1972; p.213).

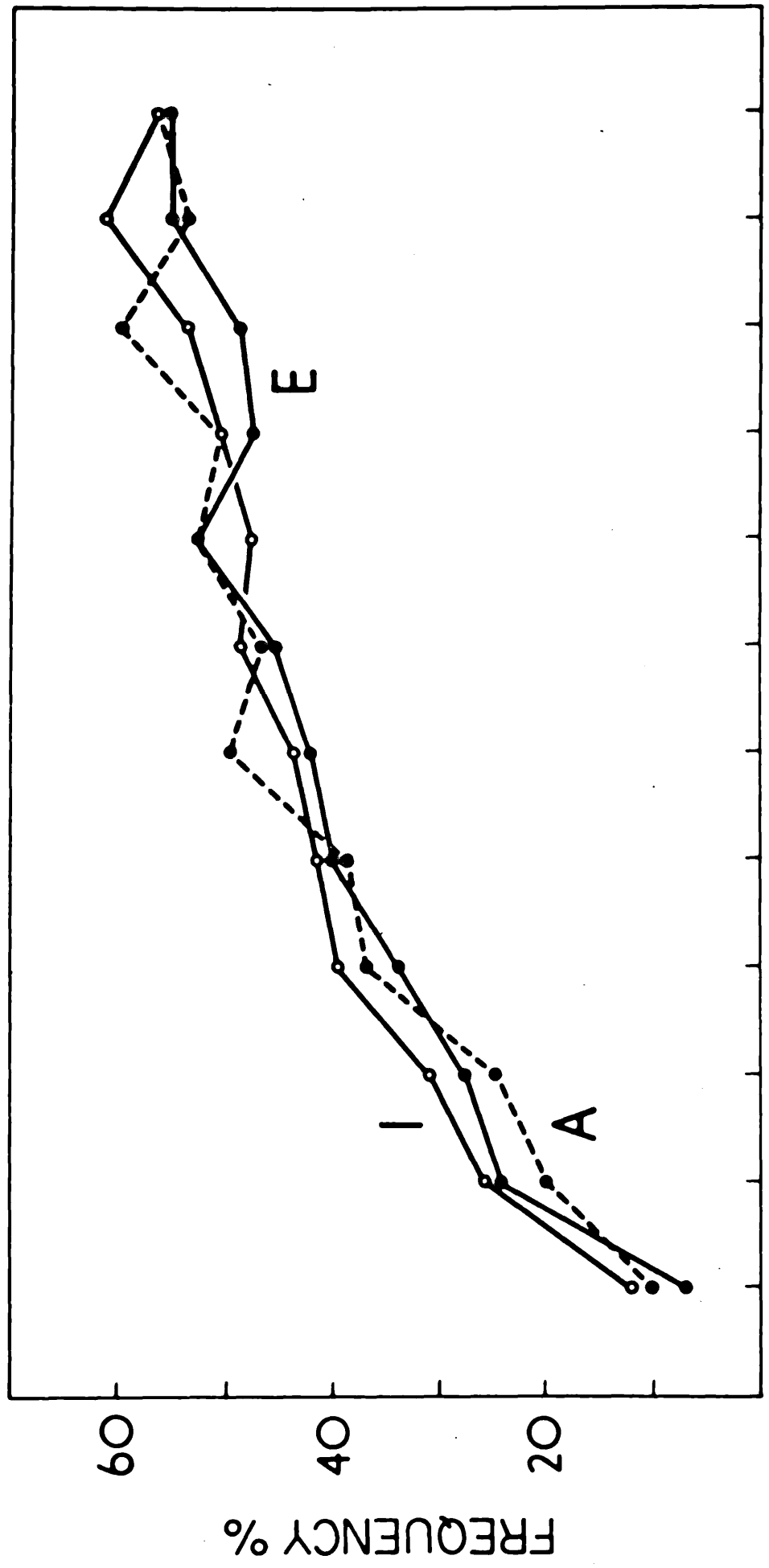


FIG: 4:3(b) Rate of eyelid conditioning for extraverts, ambiverts and introverts under 100% reinforcement conditions. (Taken from Eysenck and Levey, 1972; p.213).

likely that the situation will become an over arousing one, at least for the introvert, and hence precipitate a decline in performance.

Eysenck and Levey (1972) have reported a study putting these assumptions to the test. 144 subjects were employed in an eye-blink conditioning situation. They were classified as introvert, ambivert and extravert on the basis of the Maudsley Personality Inventory (MPI; Eysenck, 1959). 100% reinforcement was compared with 67%; 400 msecs CS-UCS interval with 800 msecs; and 6 lb/in^2 UCS strength with 3 lb/in^2. Figs 4:1 - 4:3 demonstrate the effect of changing these experimental parameters. As can be seen from the comparison of figs 4:1(a) and 4:1(b) an increase in the UCS strength has the effect of creating a situation in which it is the extravert who emerges as conditioning more efficiently. Similarly, increasing the CS-UCS interval, or employing 100% reinforcement as opposed to 67% reduces any difference in conditionability between the groups.

These authors report that extraverts condition well under favourable conditions, i.e. strong UCS, 100% reinforcement and a long CS-UCS interval, but evidence hardly any conditioning at all under unfavourable conditions; whereas the introverts ultimately showed almost the same degree of conditioning whether or not they experienced favourable conditions, though the shapes of the learning curves under these different sets of conditions were somewhat different. Though it is difficult to know how far this latter finding can be generalized as the conditions used in this experiment cannot be regarded as extreme.

In considering studies which purport to test hypotheses drawn from Eysenck's theory, then, it will be necessary not only to attend to the reported results, but also to examine the design of the study. Eysenck divides his review into

TABLE 4:1: EYEBLINK CONDITIONING: PARTIAL REINFORCEMENT

AUTHOR	Ss	CORRELATION With E	CRITERION
Franks 1956	60 normals & neurotics	-.48	R
Franks (1957)	55 students	-.46	E
Brebner (1957)	8Es vs Is students	(-.61)a	E
Symons (1958)	8Es vs Is students	(-.48)a	E
Shagass & Kerenyi (1958)	30 neurotics	-.38	R & S
Franks & Leigh (1959)	80 neurotics	-.26	E
Franks (1963)	21 alcoholics	-.10	E
Franks (1963)	28 normal volunteers	-.01	E
Field & Brengelmann (1961)	33 criminals	-.17	E
Das (1957)	63 students	-.08	E
Willett (1960)	80 Youths	-.08	E
Sweetbaum (1963)	56 patients	-	Rb

a Coefficients are not comparable with the rest of the table as calculations are based on extreme groups only.

b clinical diagnosis

R = Guilford's R scale; S = Guilford's S scale; E = MPI E Scale (Taken from Eysenck 1965 p.260).

TABLE 4:2: EYEBLINK CONDITIONING: 100% REINFORCEMENT

AUTHOR	Ss	CORRELATION With E	CRITERION
Barendregt & Ree (1961)	41	-.29	Heron's E
Spence & Spence (1964)	160 students	-.08	E
Farber, Spence & Brechtoldt (1957)	103 students	.007	R
Faber, Spence & Brechtoldt (1957)	103 students	.37	C
Al-Issa (1961, 1964)	90 apprentices	-.31	E

(E - MPI E Scale; R - Guilford's R scale; C - Guilford's C scale)

three major sections: (1) studies of eye blink conditioning using a partial reinforcement schedule; (2) studies of eye blink conditioning using a 100% reinforcement schedule, and (3) studies of electrodermal response conditioning.

Table 4:1 summarizes the studies in group 1 indicating the number and type of subjects used, the criterion for defining extraversion and the correlation between extraversion-introversion and conditioning. It will be noticed that of the first eight studies only the final two by Franks (1963) failed to yield a significant negative relationship between introversion-extraversion and conditioning. The final four studies all failed to disclose such a relationship, however, and Eysenck has offered a variety of objections to them.

The Field and Brengelmann (1961) study, Eysenck points out, employed prisoners as subjects. He questions whether the experimental conditions were adequate, particularly with regard to extraneous noise. Perhaps more pertinently, he also questions the use of the MPI-E scale. There is some doubt as to whether this is an adequate instrument for defining extraversion in the prison setting. The questionnaire contains a number of questions related to sociability. "These questions are clearly inappropriate in prison," Eysenck says, "and may invalidate the scale as a measure of E." (1965, p.262).

The problem in Das' (1957) study, too, is the relevance of the MPI to the sample population. This time the subjects are drawn from various cultural and racial groups, and Eysenck comments that "it does not seem likely that the MPI scales can be used indiscriminantly for such divergent groups" (1965, p.261). Similarly in the Willett (1960) study he suggests that the subjects were too young for the MPI scores to be regarded as reliable.

Sweetbaum's (1963) study is placed in a subgroup on its own because although a 100% reinforcement schedule was not employed, given the importance of inhibition in distinguishing the performance of the two groups, it is questionable whether a 90% schedule, as used here, allows sufficient opportunity for the growth of inhibition, and the consequent superiority of the introvert in forming CRs, to manifest itself. Eysenck also suggests that the conditions of this experiment were productive of anxiety which condition also militated against the appearance of a significant negative correlation between conditioning and introversion-extraversion.

If Eysenck's qualifications on these studies are accepted then the bulk of the data do support the hypothesized relationship. Eleven of the twelve studies yielded the predicted negative correlations, though only six of the twelve did so to any significant degree. Eysenck concludes: "Taking all the studies in the partial reinforcement group together, we find a relationship between conditionability and E with a p value of less than 1 in 1,000,000, even including all the studies on whose adequacy we have thrown doubt." (1965, p.265).

Turning to the second group of studies, those using continuous reinforcement (CRF), as would be expected from the theory and from the Eysenck and Levey (1972) study this relationship is no longer evident, (c.f. Table 4:2). CRF, of course, favours the conditioning of the extravert, but the actual magnitude of the relationship observed in any particular study will depend on a number of variables besides the reinforcement schedule, including the UCS strength, the CS-UCS interval and whether or not discrimination learning was employed.

More recent studies appear to be following the same pattern of results as those reported by Eysenck. Besides

the Levey (1972) study, Jones (1975), and Piers and Kirchner (1969) have selected parameters appropriate to the testing of hypotheses drawn from Eysenck's theory, and report results supporting Eysenck's predictions.

McPherson (1965), however, strikes a discordant note. Like two of the studies quoted by Eysenck in support of his position (Brebner (1957), Symons (1958)), McPherson's data were collected from an Aberdeen Ph.D.. Eysenck (1965), remarked of Brebner's study that: "There was a significant interaction with sex, male introverts conditioning relatively quicker than female introverts and male extraverts less quickly than female extraverts." (1965, p.260). McPherson has pointed out, however, that the data were less favourable to Eysenck's position than this statement appears to indicate. There was, in fact, no significant difference between the extraverted and introverted female groups. Furthermore, while all extraverts displayed some degree of conditioning, 3 of 11 introverts failed to do so.

McPherson reports that in his own study extraverts produced more CRs in both the acquisition and extinction phases than did the introverts, though not significantly so. He continues by stating that when his own data are combined with those of Symons no significant difference in condition-ability is found between the extraverted and introverted subjects. "Thus the Aberdeen work appears to show little evidence that introverts are more conditionable". (1965, p.484). he concludes.

While bearing in mind this note of caution it nevertheless appears to be the case both that there is some substance to Eysenck's theory, and that he is correct when he warns of the dangers of neglecting the paramaters of relevance.

When attention is shifted to the third group of studies,

those employing electrodermal conditioning, the position appears to be less favourable to Eysenck's cause (cf. Table 4:3).

TABLE 4:3 ELECTRODERMAL CONDITIONING

AUTHOR	Ss	RESULTS	CRITERION
Franks (1956)	60 normals & neurotics	-.25	R
Lykken (1957)	59 Sociopaths, neurotics & normals	Sig.	MMPI
Vogel (1960)	18 alcoholics	Sig.	E
Vogel (1961)	40 alcoholics	Sig.	E
Vogel (1961)	40 normals	Sig.	E
Becker (1960)	62 students	N.S.	E
Martin (1960)	23 students	N.S.	E
Halberstam (1961)	56 dysthymics, hysterics & norms	Sig.	Diagnosis
Becker & Matteson (1961)	40 students	N.S.	R MMPI
Davidson et al (1964)	73 students	N.S.	E
RECENT STUDIES			
Davidson et al (1964)	40 neurotics	N.S.	E
Morgenson & Martin (1969)	115 normals	N.S.	E
Wilson (1968)	15 High School Students	.45	E
Slubicka (1972)		Sig.	E
Mangan (1974)	25 normals	N.S.	E
Lovibond (1963)	100 normals	N.S.	E

R - Guilfords R scale; E - MPI E scale

Eysenck (1965) reports that Lykken (1957) found a negative relationship between introversion-extraversion and conditioning. This study, while it did use discriminant conditioning which according to Eysenck (1962) should favour the build up of inhibition, also employed 100% reinforcement and a UCS (an electric shock) described as "decidedly unpleasant, producing in most cases a pronounced startle effect." Given this mixture, some parameters favouring the superior conditioning of the introverted subjects, others favouring the extraverts, and given the absence of data indicating either which of these parameters is the most salient, or how they interact to produce the conditioning effect in this situation, this study cannot really be quoted in favour of, nor as evidence against, Eysenck's theory. Of the studies quoted by Eysenck four failed to yield significant negative correlations. Of the Becker (1960), Becker and Matteson (1961) and Davidson et al (1964) studies Eysenck believes that the UCS was of a "very emotion-producing strength" and "it comes as no surprise, therefore, that E did not correlate with conditioning in these experiments." (Eysenck, 1965, p.265). In Martin's (1960) experiment, too, the UCS appears to have been of a suspect strength (a 110 db tone).

To this point Eysenck's objections to various studies appear to have some justification. However, I fail to see how he can claim that Lykken's (1957) study supports his position, for the reasons given above, nor how he can list it with those studies whose UCSs are classified as "relatively weak" (Eysenck, 1965, p.265) when the UCS in this case has been described as "decidedly unpleasant." (p.263).

Another cautionary note appears to be in order here; though most of Eysenck's objections appear justified one must avoid according them a status that is not properly theirs.

These objections are suggestions, or hypotheses, which, by and large, attempt to throw some light on why predictions apparently drawn from Eysenck's theory failed to be confirmed. The real reason for these 'failures' may be quite different from those suggested. It is possible that all the parameters relevant to the testing of this theory have not been specified, the results may possibly be due to chance, the theory itself may be incomplete, or completely wrong. Often data are simply not available to evaluate an objection in a particular case.

For one of the studies to which Eysenck did object the data are, however, available. Becker and Matteson (1961) anticipated Eysenck's objections; after attempting to estimate the effect of the UCS intensity they comment: "If shock level were positively correlated with the criterion conditioning score used to test Eysenck's hypothesis, then this difference in shock level could have acted to suppress a relationship between CSR conditioning and R (Guilford's R was used as a measure of extraversion). However, the correlation between shock level and criterion conditioning score was found to be -.11. The correlation between shock level and amplitude conditioning score was -.29. The negative relationship, though insignificant, may be related to the fact that shock level was increased if the GSR to shock decreased appreciably. The above findings support the conclusion that the method of determining shock level did not produce systematic biasing effects which would necessitate modification of the main findings." (1961, p.429). The main findings were that while conditioning was not related to the introversion-extraversion dimension it was related to anxiety.

Here then is one instance in which it appears that Eysenck's 'explanation' is incorrect. Even this study cannot be regarded as providing no support for Eysenck's theory,

however. The theory predicts that extraverts will require a stimulus of greater intensity than will introverts to produce a response of equivalent magnitude; this is what was found in this study, though the difference was not significant.

Of the studies in table 4:3 reported by Eysenck both Martin (1960) and Davidson et al (1964) used CRF and both failed to find a significant relationship between introversion-extraversion and conditioning. Of the more recent studies only one, Slubicka (1972), has yielded results in the direction predicted by Eysenck, while studies by Davidson et al (1966, 1968), Morgenson and Martin (1969), Wilson (1968), Mangan (1974) and Lovibond (1963) have all failed to confirm Eysenck's predictions. Various objections could be leveled against most of these experiments as tests of Eysenck's theory. For example only in Wilson's (1968) study was a partial reinforcement schedule employed. In this study, however, a positive correlation (.45, $p < .05$) was recorded between extraversion and conditioning, although only 15 subjects took part in the experiment.

On the basis of the above experimental data it certainly cannot be concluded that Eysenck's position, relating personality to conditioning, is well supported in electrodermal conditioning. Lovibond (1964) has commented: "Possible reasons for the failure to demonstrate a stronger relationship, other than the invalidity of the theory, include unreliable conditioning measures, and inadequacy of the E scale as a measure of extraversion. By and large it is clear that the reliability of both eye blink and GSR conditioning measures is reasonably satisfactory". (1964, p.120).

Eysenck has expressed his opinion of this view in no uncertain terms in replying to a similar comment by Franks (1963). "He (Franks) suggests 'that it is perhaps the

technique used to measure extraversion which is most suspect (p.306). This is not a tenable view, in our opinion; questionnaire measures of E are reliable and valid (Eysenck and Eysenck, 1964) and in the absence of direct evidence invalidating the questionnaire used, speculation of this kind is not helpful." (Eysenck, 1965, p.262).

In conclusion then it would seem that Eysenck's hypotheses have fared better in eye blink conditioning studies than in studies of electrodermal conditioning. It is not easy to decide why this might be, whether because of a lack of generality or because of some peculiarity of electrodermal conditioning. However rather than enter into speculation it can be pointed out that the majority of studies quoted, unfortunately including some of those Eysenck quotes as supporting his own hypotheses, have not appropriately controlled those parameters which are of theoretical importance in influencing the observed relationship between the introversion-extraversion dimension and conditioning. It would seem rash to offer any firm conclusions at this stage, there appears to be data both in support of, and against, the theory. It would seem that the case must remain open for the time being.

4:2 ANXIETY AND CONDITIONING

While Eysenck has argued that it is the introversion-extraversion dimension which is primarily responsible for differential conditionability, Spence has argued that it is anxiety in which we should be interested. Spence has used Hullian learning theory as the foundation of his approach. Taylor (1956) indicated those aspects of the theory which are relevant to the proposition that conditionability is positively related to anxiety when she said: "According to Hull, all habits (H) activated in a given situation,

TABLE: 4:4 IOWA STUDIES OF CONDITIONING PERFORMANCE OF PRESELECTED HA AND LA SCALE Ss

Iowa Experiments	No. trials	Ready Signal	UCS (psi)	No. Ss	Percent CR		Diff. (H-L)	p
					HA	LA		
1) TAYLOR (1963)	80	Yes	1.6	60	59.6	27.9	21.7	.001
2) SPENCE-TAYLOR (1951)	100	Yes	.6	50	48.2	33.8	14.4	.05[a]
	100	Yes	2.0	50	55.0	41.7	13.3	.05
3) SPENCE-FARBER (1953)	60	Yes	1.0	64	48.8	34.1	14.7	.05
4) SPENCE-BEECROFT (1954)	50	Yes	1.0	45	56.5	36.3	20.2	.02
5) SPENCE-WEYANT (1960)	100	No	.25	36	41.8	28.6	13.2	.1[b]
	100	No	2.0	36	65.4	53.2	12.2	.1
6) SPENCE (UNPUBLISHED)	80	Yes	.25	60	36.5	21.6	14.9	.02[c]
	80	Yes	1.5	60	48.0	38.8	9.2	.02

a The F Value based on all groups provided a p value of .01

b The F value based on all groups provided a p value of .05

c The F value based on all groups provided a p value of .01

TABLE 4:5 NON-IOWA STUDIES OF CONDITIONING PERFORMANCE OF HA AND LA SCALE Ss

Non-Iowa Experiments	No. trials	Ready Signal	UCS (psi)	No. Ss	Percent CR		Diff. (H-L)	p
					HA	LA		
1) HILGARD et al (1951)	60	Yes	1.6	20	32.5	32.4	2.8	-
2) PROKASY-TRUAX (1959)	20	No	3.0	20	36.0	48.0	-12.0	-
3) BARON-CONNER (1960)	80	No	1.6	36	44.9	27.8	17.1	.01
4) KING et al (1961)	80	Yes	1.5	32	37.4	51.6	-14.2	-
5) KING et al (1961)	80	No	1.5	32	66.8	64.0	2.8	-
6) KING et al (1961)	80	Yes	1.14	40	49.0	51.3	-2.3	-

TABLE 4:6 STUDIES OF PERFORMANCE OF HA AND LA SCALE Ss TO POSITIVE CS IN DIFFERENTIAL CONDITIONING

Study	Ready Signal	No. Ss	Percent CR		Diff. (H-L)	p
			HA	LA		
1) SPENCE-FARBER (1953)	Yes	37	50.0	39.7	10.3	.05
2) SPENCE-FARBER (1954)	Yes	52	41.1	31.7	9.4	.09
3) PROKASY-WHALEY (1962)	Yes	60	54.5	39.5	15.0	.05
4) PROKASY-WHALEY (1962)	No	70	60.2	56.7	3.5	-

TABLE 4:7 IOWA STUDIES INVOLVING UNPUBLISHED DATA FROM POSTSELECTED Ss

Study	No. trials	Ready Signal	UCS (psi)	No. Ss	Percent CR		Diff. (H-L)	p
					HA	LA		
1) RUNQUIST & ROSS (1959)	41-80	Yes	1.0	27	70.0	44.3	25.7	.05
2) RUNQUIST & SPENCE (1959)	41-80	Yes	1.0	34	57.0	38.2	18.8	.05
3) INDIVIDUAL DIFFERENCES: (UNPUBLISHED)	1-80	Yes	1.0	52	41.8	45.5	-3.7	-
4) EXTINCTION: (UNPUBLISHED)	1-30	No	2.0	39	64.3	53.6	10.7	.05
5a) CONDITIONING PROBLEM: MALE (UNPUBLISHED)	31-60	No	.6	67	61.8	47.6	14.2	.05
5b) CONDITIONING PROBLEM: FEMALE (UNPUBLISHED)	31-60	No	.6	31	71.0	59.0	12.0	-

combine multiplicatively with the total effective drive state (D) operating at the moment to form excitatory potential or E ($E = f(H \times D)$). Total effective drive in the Hullian system is determined by the summation of all extant need states, primary and secondary, irrespective of their relevancy to the type of reinforcement employed. Since response strength is determined in part by E, the implication of varying drive level in any situation in which a single habit is evoked is clear; the higher the drive, the greater the value of E and hence of response strength. Thus in simple, non-competitional experimental arrangements involving only a single habit tendency, the performance level of high drive Ss should be greater than for low drive groups." (Quoted by Lovibond, 1964, p.121). Anxiety is considered by Spence to exhibit the properties of a drive and, given Hullian theory, high anxiety individuals are expected to show superior conditioning "in simple, non-competitional" situations.

Tables 4:4 - 4:7 are taken from Spence's review of the available relevant conditioning studies. It can be seen from table 4:4 that all the Iowa studies report a significant difference in conditioning between the high anxiety (HA) and low anxiety (LA) groups. Added to this table should be a study by Spence and Spence (1964), using 160 subjects, 100 males and 60 females. They report a correlation of .242 ($p < .05$) between conditioning and neuroticism, (the MPI - N scale was used) and a correlation of .224 ($p < .05$) between conditioning and anxiety, as measured by the Manifest Anxiety scale (MAS). These significant correlations held only for the male sample, however, the respective correlations for the female sample were .157 (N.S.) and .067 (N.S.). So in this study Spence's predictions received only partial confirmation.

One is curious to know why those studies performed in

Spence's own laboratory, table 4:4, yield consistently significant results whereas those experiments performed elsewhere are much less encouraging to Spence's propositions, (c.f. Table 4:5). Spence suggests this may be to do with the experimental conditions employed, in his own laboratory the stress is placed on using naive subjects in anxiety provoking situations. He says: " a deliberate attempt was made in the Iowa studies to provide conditions in the laboratory that might elicit some degree of emotionality". (1964, p.135). He describes the experimental procedure as follows: "On coming into the experiment, S at first saw an impressive array of electronic recording equipment and was then led to an adjoining room in which was located an isolated, screened cubicle. The latter contained a dental chair (sic) in which S was seated in a reclining position, while a head band was placed on his head and a plastic piece was fastened to his upper eyelid. After completing the instructions, the illumination in the cubicle was reduced to a low level of semi-darkness and S was informed that if need arose, he could get in touch with E by means of a microphone placed on a stand within his reach. The door to the cubicle and the door to the adjoining room in which E worked were then closed and S was left in isolation." (1964, p.135).

I think there would be little argument that these conditions are correctly described as anxiety provoking. That these experimental procedures are of central importance receives some support from a study by Kimble (Ominsk and Kimble, 1966). Kimble was co-author with King, in the three King et al studies listed in table 4:5. These studies not only failed to support the anxiety-drive hypothesis but in two of the three studies actually report results in the opposite direction, LA subjects producing more CRs than HA subjects,

though these results were not statistically significant. According to Kimble these studies were conducted in an environment not calculated to be anxiety provoking. The Ominsk and Kimble (1966) study, however, made use of a room which is described as being like a refrigerator or bank-vault in which the subject was left alone. This study, contrary to the earlier ones, does report results in line with the Spence-Hull hypothesis.

The importance of employing the appropriate parameters are further emphasized when we consider studies that, instead of manipulating anxiety by selecting HA and LA subjects,

choose individuals in more or less stressful situations. Several studies have employed so called 'real life' stress: Beam (1955) used students awaiting their doctoral examination in an electrodermal conditioning study, Sweetbaum (1963) used patients awaiting a surgical operation, and Willett (1963) compared youths taking part in a stressful selection procedure with those who had already been selected, all report that those subjects experiencing the more stressful situation displayed the greater degree of conditioning.

Encouraging as these results are, the story is not one of unmitigated success as Hobson (1968, 1969) and Beck (1963) report superior conditioning from HA groups while minimizing generalized anxiety. Franks (1956, 1957) does not report whether his experimental procedures may or may not be considered anxiety provoking, but he did not find any relationship between anxiety and conditioning.

Becker and Matteson (1961) and Piers and Kirchner (1969) have both reported studies supporting Spence's hypothesis. Though the latter study is not at all straight forward. Both N (from the EPI) and MAS were found to correlate significantly with conditioning, .341 ($p < .01$) and .305 ($p < .01$)

respectively. When MAS was corrected for extraversion the correlation fell to .24 ($p < .05$), and as might be expected the correlation disappeared (.06), when the effect of N was partialled out. Extraversion (E) was also found to correlate with N (.39), and with conditioning -.223 ($p < .05$). Unlike the standard Spence experiment a 50% reinforcement schedule was employed and no effort was made to arouse anxiety.

As in the case of Eysenck's hypothesis it appears that not only is it important to adhere to the relevant parameters, but different forms of conditioning yield different results. Studies by Bitterman and Holtzman (1952) Gilbert and Davenport (1960) Lacey, Smith and Green (1955), Becker (1960), Wilson (1968), Morgenson and Martin (1963) Lovibond (1963) and Mangan (1974) all employed an electrodermal conditioning paradigm, and all failed to discover any relationship between conditionability and anxiety.

It should be noted, however, that the last two studies are somewhat different from the rest; Lovibond (1963) used both an aversive UCS (electric shock) and a non-aversive UCS (slides of nude females), while Mangan (1974) used only the non-aversive UCS (slides of nude females).

Bindra, Paterson and Strzelecki (1955) also employed a non-aversive stimulus, a lollipop, in the study of salivary conditioning. They failed to find any significant relationship between anxiety and conditioning and concluded that a specific drive, a defensive reaction, was being utilized in eyeblink conditioning and that it would be only under circumstances where such a reaction was activated, that a relationship between conditioning and anxiety would be apparent. This, of course, is the position held by Eysenck, he maintains that the Hullian "irrelevant" drive is not a primary determinant of conditioning, and that anxiety will be related to conditioning

only to the extent that it is a measure of a drive relevant to the conditioning situation.

Lovibond (1964), has, however, cast doubt on the value of the Bindra et al study, commenting that it is open to serious methodological criticism (c.f. 1964, p.124). He reports that this experiment has been repeated in Adelaide, but this time with a control group. The curves for both groups, he reports, were identical in form to those obtained by Bindra et al (c.f. 1964, p.151), but there was no significant difference between the experimental and control groups.

It seems then that like Eysenck's, Spences's hypothesis fares better with eyeblink than with electrodermal conditioning. It appears to have no success at all when faced with appetitive reinforcement, but perhaps this is not too unexpected in view of the stress Spence places on the importance of anxiety producing situations. However, this finding does appear to place sever limitations on the usefulness of the theory.

4:3 EYSENCK AND/OR SPENCE

It might seem that there is an implicit assumption that if one of these theories is correct then the other must, of necessity, be wrong. Neither theorist, however, claims that the two theories are mutually exclusive. Spence, for example, has said: "It should be noted, however, that our interpretation of the MA scale data in terms of the facilitating effects of drive on conditioning performance is not necessarily incompatible with Eysenck's hypothesis concerning the greater susceptibility of extraverts to I_R. Our view becomes contradictory to Eysenck's only when he adds the assumption that neuroticism is not a factor affecting

performance." (Spence & Spence, 1964, p.148).

As remarked above the main difference between the two theorists is whether or not Hullian "irrelevant" drive, i.e. "one that is not reduced by performing the behaviour being studied"(op. cit, p.148) facilitates conditioning. Spence argues that it does, while Eysenck states: "Individuals higher on N are innately predisposed to react strongly and lastingly to certain classes of stimuli with an innervation of their sympathetic nervous systems". He continues, "Task-relevant drive may be created through changes in the experimental conditions, that is, through manipulation of attitudes (Spence, 1964) through the threat of surgery (Sweetbaum, 1963) or possibly in other ways (Willett, 1964). Under these conditions sympathetic innervation will be greater and will last longer in Ss high on N, and consequently correlations will be observed between N and conditioning." (Eysenck, 1965, p.266).

Eysenck, then, predicts that N will be related to conditionability only where certain task relevant drives, usually stress, are aroused. This prediction would appear to be supported by the Bindra et al (1955), Lovibond (1963) and Mangan (1974) studies. Nor does there appear to be any clear evidence in support of the contention that "irrelevant" drive is a critical determining factor in differential condition-ability.

Pursuing this line of arguement even further Lovibond (1964) has briefly reviewed some of the electro-physiological work, and after quoting such authors as Malmo (1958), Wolpe (1958) and Anokhin (1961) he suggests that "individuals in a state of strong anxiety, whether chronic or acute, will manifest a decreased capacity for forming positive reward conditional reflexes." (1964, p.128). Quoting his own (1963) study he

observes that low scorers on the Eysenck N scale performed at a higher level, though not significantly so, than high scorers in conditions of positive reinforcement. He then proceeds in an attempt to marry the two theories in the following formulation:

"A reconciliation between the views of Eysenck and Spence and the accumulated evidence may be effected by the acceptance of the following propositions:

1. The level of cortical excitation, which determines the speed of formation of the conditional linkages, is dependent on
   (a) the degree of subcortical arousal
   (b) the general excitability of the cortex, i.e. the capacity of the cortex for developing excitation from a given level of subcortical arousal.
2. Individuals differ with respect to three dimensions of nervous activity which are at least partially independent:
   (a) general excitability of the cortex,
   (b) defensive arousability, and
   (c) appetitive arousability.
3. The personality dimension which is related to general excitability of the cortex, and hence to a general factor of conditionability, is introversion-extraversion, and the relationship is such that introverts will manifest higher levels of general cortical excitability, and conditionability than will extraverts.
4. The personality variable of anxiety is related to level of defensive arousal, and a group factor of aversive conditionability.
5. The relationship between different arousal systems is a mutually inhibitory one so that, e.g., a high level of defensive arousal at a particular time will, reduce

the capacity for appetitive arousal, and hence appetitive conditioning performance.

6. The relationship of reciprocal inhibition between different types of arousal will be to some degree asymmetrical. Because of the generally greater strength of defensive arousal, the inhibitory effects of this type of arousal are likely to be more readily observed than inhibitory effects of nondefensive arousal". (Lovibond, 1964, pp. 126-127).

Lovibond, then, suggests that although there may, indeed, be a general factor of conditionability, based perhaps on Eysenck's introversion-extraversion dimension, it will also be necessary to consider the type of reinforcement being employed and the differential sensitivity of the neurotic to these. One might be lead to predict that in those situations where both appetitive and aversive reinforcers are being employed the high N group should be particularly poor in the formation of positive conditional links; the low N group, however, having a lower potential for defensive arousal and, therefore, presumably less differentiation between systems which are mutually inhibitory, ought to form both appetitive and aversive conditional links with difficulty. Thus we might expect the high N group to be highly efficient in forming negative conditional CRs with little likelihood of positive conditional CRs being elicited, as we draw nearer the stable (low N) end of the dimension, there ought, in this kind of conflict situation, to be little likelihood of any conditioning due to the mutually inhibitory effects of the two systems.

A recent study by Kantorowitz (1978) has also added to the speculation that the type of reinforcement used might be an important determinant of which groups are ultimately found to condition. Kantorowitz (1978) used eight male

subjects in a study "involving conditioning and deconditioning of sexual arousal". Using the EPI to classify subjects he reports a correlation of .88 ($p < 0.01$, 2 tailed) between E and tumescence conditioning, and of -.76 ($p < 0.05$, 2 tailed) between E and detumescence conditioning. N was found to correlate -.51 and .25, (both N.S.), to the two forms of conditioning respectively. These latter correlations are less important as the range of N scores was rather restricted. With regard to the E dimension the results may be accounted for on the basis of Eysenck's theory, if we regard the tumescence conditioning as a situation of high arousal, and detumescence as a situation of lowered arousal. Kantorowitz, however, has suggested that we might regard the pre-orgasmic tumescence as an appetitive situation, while detumescence is an instance of satiation and, in consequence, non-rewarding. Thus, the results of this experiment might be interpreted as indicating that the extravert is better at conditioning to appetitive rewards while the introvert is better at conditioning to aversive reinforcement. Kantorowitz, then, like Lovibond casts an element of doubt on Eysenck's suggested relationship, but these two authors themselves propose different solutions.

The importance of distinguishing between different types of reinforcers will be dealt with at some length in the next chapter when Gray's theory of personality is discussed. Before we turn to that theory, however, I think it ought to be acknowledged that not everyone is happy to accept, not only that particular personality dimensions are peculiarly related to conditionability, but even that it is appropriate to talk of humans conditioning at all. Brewer (1974), for example, has entitled an article "There is no convincing evidence for operant or classical conditioning in adult humans." This seems to be a typical expression of the

growing, and fashionable, discontent with behaviourism or anything which smacks of behaviourism. As Mackintosh has commented a "common attitude today is that conditioning is a dull and mechanical form of learning, whose rather trival laws.... are fully understood and do not merit further study" (1978, p.43). Concordant with some of the views expressed earlier and to be proposed later (e.g. the importance of expectation and the experiment of Hernstein and Hindline (1969)) Mackintosh suggests that we might profitably view conditioning as a process of detecting relations between events. No apology needs to be made, I think, if this sounds a bit "cognitive", as Mischel has indicated, the meaning of the term "behaviour" has been steadily expanding over recent years.

The main point of attack of Brewer's arguement is that people are often aware of what they are doing, or of the reinforcement contingencies in a conditioning experiment, and, therefore whatever is going on is not properly called conditioning. Brewer is contributing to the learning with/without awareness issue and one might challenge him on several points, but it seems sufficient here to acknowledge that awareness obviously does appear to influence the process of learning and considerably more could usefully be learned about it. This in itself illustrates that the laws of conditioning are not in themselves, as yet, fully worked out, or more properly, there is as yet no theory which adequately describes them or their workings. The scope of all things must, however, be limited, and so this problem (reluctantly) must be put aside. It will not form the focus of the experimental work, although sidelong glances will, occasionally, be cast at it. As noted in the first chapter, although what goes on within the organism may be a critical determinant of behaviour, we still need to observe something before we

know what might have happened.

4:3 CONCLUSION

In summary then, the argument worked through to this point is that people may not display an absolute consistency in their behaviour and to understand and predict, in detail, the behaviour of the individual it might well be necessary to study that person's learning history. However, it was felt that if a general factor of conditionability, particularly one with the physiological basis made explicit, could be identified we might well be able to build up a theory which not only married the idiographic and nomothetic approaches, but might also tell us when it was proper to make each type of statement and what type of statements might be made e.g. nomothetic statements of universal validity, or limited to a particular culture. It was suggested that Eysenck's theory of personality might "fit the bill". In the previous chapter the theory was outlined, while in this present chapter the empirical evidence of the relationship between the introversion-extraversion dimension and conditionability has been examined. The empirical evidence relevant to the Spence-Hull hypothesis was also examined, partly because anxiety, according to Eysenck, is related to both the introversion-extraversion and the neuroticism dimensions, and partly because it is the other major theory which relates conditionability to a personality variable.

Studies of classical eyeblink conditioning seem to yield some support for both Eysenck's and Spence's position. At the same time these studies emphasize the importance of adherence to theoretically relevant parameters. Studies of electrodermal conditioning provided no clear support for either theory, though many of these studies are of questionable validity as tests of these theories. It appears then that

no general factor of conditionability has been unequivocally demonstrated. While a further complication appears to have arisen. It seems that there may be at least two subfactors of conditionability, one related to appetitive reinforcement, the other to aversive reinforcement. It is the implications of this suggestion which will be dealt with in the next chapter.

CHAPTER V: DIFFERENTIAL SENSITIVITY TO CUES OF REINFORCEMENT

5:1 INTRODUCTION

Gray (1975a) as a conclusion to his book said "one purpose of this book is to serve as a scaffolding for an eventual theory of the physiological basis of personality. The relation between such a theory and a theory of learning may be expressed in this way. Learning theory is an attempt to describe the general structure of the conceptual nervous system which is common to all the members of a given species (or even a group of species). A theory of personality is an attempt to account for differences in behaviour between individual members of the species in terms of systematic variation in the properties of the subsystems or components which go to make up this general conceptual nervous system. If one can go one step further and align these subsystems with the neural and/or endocrine structure and function in the real neuro-endocrine system, one has constructed a theory of the physiological basis of personality" (1975a, p.348). There is sufficient in the above quotation to justify some reference to Gray's theory here, but the theory has two other claims to our attention: it is put forward as a modification of Eysenck's theory of personality; and expectation, so important in the social learning theory, out lined earlier, plays a central role in this approach too.

The theory has grown out of a substantial body of work on animal learning, particularly work on learned emotions and emotional responses which Gray has reviewed extensively (e.g. 1971, 1972, 1975a, b, 1978). Gray has opted for a two process theory, coming "down firmly for the view that classical and

instrumental conditioning involve fundamentally different processes" (1975, p.81). He parts company with Hull as to the importance of drive reduction as the central principle of reinforcement: "The most one can say at present is that the drive-reduction hypothesis is almost certainly wrong; the drive-induction hypothesis is a tautology, and that the drive-increment hypothesis is only a gleam on the horizon". (1975, p.199).

A theory which does appear to have been influential in the development of Gray's theory is Amsel's (1958, 1962, 1967) theory of frustration. Frustration, or the non-delivery of an expected reward, is assummed to set up an unconditioned internal state with all the properties of an aversive drive. It differs from fear only in the operations necessary for eliciting it - punishment for fear, non-delivery of an expected reward for frustration. This leads to a second, symetrical, hypothesis that non-delivery of an expected punishment will be rewarding.

The importance of expectation to the theory is already evident, neither of these situations, non-delivery of a reward or of a punishment, could in themselves be rewarding or aversive apart from the expectation which is hypothesized as their accompaniment.

Because of the importance of expectation to the theory Gray sets some store by an experiment of Hernstein and Hineline (1969). Rats were placed in a situation in which they were offered the choice between two frequencies of shock. If they pressed the available lever shocks were delivered at unpredictable intervals with a mean frequency of M times per minute. If they did anything else, shocks, at equally unpredictable intervals, with a mean frequency of N time per minute (where $N > M$) were delivered. A lever press obtained

for the rat a period of time at shock frequency M, this period lasting until the next shock was programmed, and then the higher frequency was reverted to, unless the lever was pressed again. It was found that the rats' responding was proportional to the degree of reduction in shock frequency produced by such a response. Gray concludes from this that "the animal can:

(a) Measure the average frequency of shocks it receives when not responding

(b) Store this frequency

(c) Measure the same quantity during periods of time after it responds

(d) Compare this with the stored frequency for shocks when not responding; and

(e) Choose that behaviour which is associated with the lower shock frequency." (1975, p.333).

Thus the ongoing process "enables the organism to compute the probability of the UCS following the CS at a number of different intervals of time following CS onset, and to compare this with the probability of the UCS occuring without the CS preceding it by the interval in question. If the probability of the UCS occurring at the specified interval after CS onset is greater than it would be without the CS having occurred at that time, the CS acquires a positive conditioned significance. If the two probabilities are alike, the significance of the 'CS' remains neutral. If the probability of the UCS occurring at the specified interval after CS onset is actually lower than if the CS had not occurred at that time, then the stimulus becomes a negative or inhibitory CS". (1975, p.335). Thus rats can form extremely complex expectation.

An alternative strategy in demonstrating the importance of expectation is to demonstrate that certain procedures will

affect expectation, and in consequence related behaviour, resulting in differences in behaviour between experimental and control groups. Before this can be done it must be established whether or not all forms of expectation will be influenced by the same procedures, or if there are different forms of expectations mediated by different systems and consequently affected by different procedures. Gray (1972a) has suggested, as noted above, that there is evidence to suppose signals of punishment and of non-reward are functionally identical, as are signals of reward and non-punishment. Thus we have two classes of 'signals' those eliciting approach behaviour, in which he includes active avoidance, and those promoting behavioural inhibition or passive avoidance.

Although this schema undoubtedly makes for parsimony one feels some reluctance in accepting that there is a fundamental distinction between active and passive avoidance, and even less happy when this assumption is employed as part of the foundations of a theory. Gray (1975) has commented on the problem, himself, pointing out that in an active avoidance situation all responses except one are punished, while in a passive avoidance situation only one response is punished. If we start to expand the class of non-punished responses in active avoidance situations at what point does it become passive avoidance? Conversely if the class of punished responses is expanded in the passive avoidance situation, at what point does it become active avoidance?

Gray's defence is simply that conditions found to adversely affect passive avoidance leave active avoidance unaffected.

For the sake of clarity I will deal with the two hypotheses separately, firstly with data relating to passive avoidance, and the relationship of punishment and non-reward,

then with active avoidance and the relationship of reward and non-punishment, followed by an outline of Gray's model and his proposed modification of Eysenck's theory.

5:2 PASSIVE AVOIDANCE, PUNISHMENT AND FRUSTRATIVE NON-REWARD

Gray (1975) reports three experiments, two employing rats (Adelman and Maatsch, 1956; Daly, 1969) and one employing pigeons (Terrance, 1971). These experiments, he maintains, demonstrate that a CS of non-delivery of reward (Rew-CS), whether it is a discriminative stimulus (S^{Δ}) or acquires this signal value during the extinction phase of a conditioning experiment, is aversive and animals will work to escape from it or avoid it. In the Adelman and Maatsch experiment animals actually learned the task, jumping onto a ledge, more quickly to escape from the $\overline{\text{Rew}}$-CS than to gain a food reward.

Though these experiments appear to demonstrate the aversive quality of the $\overline{\text{Rew}}$-CS, they cannot be claimed to demonstrate the functional identity of $\overline{\text{Rew}}$-CS and Pun-CS. Data from drug experiments do, however, go some way to establishing this identity. Miller (1964) reported that using small doses of sodium amytal (15-20 mg/kg interperitonally in rats), doses too small to have an anaesthetic effect, the behavioural effects of both punishment and passive avoidance was adversely influenced. Gray (1975) has commented that the selectivity of the drug is "truely remarkable". "It causes the behaviour of animals exposed to $\overline{\text{Rew}}$-CS to resemble the behaviour of animals not so exposed, but it does not alter the behaviour of animals exposed only to Rew and Rew-CS. This statement is true for simple extinction; for the partial reinforcement acquisition effect; for the effects of partial reinforcement on acquisition even when these take the reverse direction to the usual one (PRF animals running slower than CRFones);

for the partial reinforcement extinction effect; for the Crespi depression effect and the depression effect in the Skinner box; and for suppression of the response to the negative stimulus during discrimination learning; and this list is by no means exhaustive". (1975a, p.287).

Gray has followed Amsel in assumming that the partial reinforcement effect is, to some degree, due to frustration after the non-delivery of an expected reward. This frustration eventually becoming the discriminative stimulus (S^D) for approach. Gray, therefore, reasons that if amylobarbitone selectively influences the $\overline{\text{Rew}}$-CS then it ought to abolish the partial reinforcement effect, but have no effect on groups trained on a continuous reinforcement schedule. There have now been a number of studies (e.g. Ison and Pennes (1969), Gray,(1969), Gray and Dudderidge (1971), Capaldi and Sparling, (1971)) reported using amylobarbitone during partial reinforcement training. The results of these studies indicate that the partial reinforcement extinction effect is at least considerably attenuated by the drug.

Gray (1975b) has suggested that the effects of the drug are probably confined to the responses to Pun-CS and $\overline{\text{Rew}}$-CS, rather than to punishment and reward themselves. The probable site of action for the drug, he suggests, is the septo-hippocampal system. This system, according to Gray, functions as a behavioural inhibition system, a 'stop' system, and as such is involved not only with passive avoidance, but also with the orienting reflex; inhibiting behaviour in order to facilitate the intake and evaluation of information.

Gray (Gray, Quintao, Araujo - Silva, (1972)) has attempted to replicate some of the drug findings with lesions of this system in an attempt to verify that this is the hypothesized 'stop' system, and site of action for the drugs.

FIG: 5:1 Effects of medial septal lesions on the partial reinforcement extinction effect in the alley at one trial per day.
CRF, Continuous reinforcement.
PRF, Partial reinforcement.
(Taken from Gray, 1978; p.427)

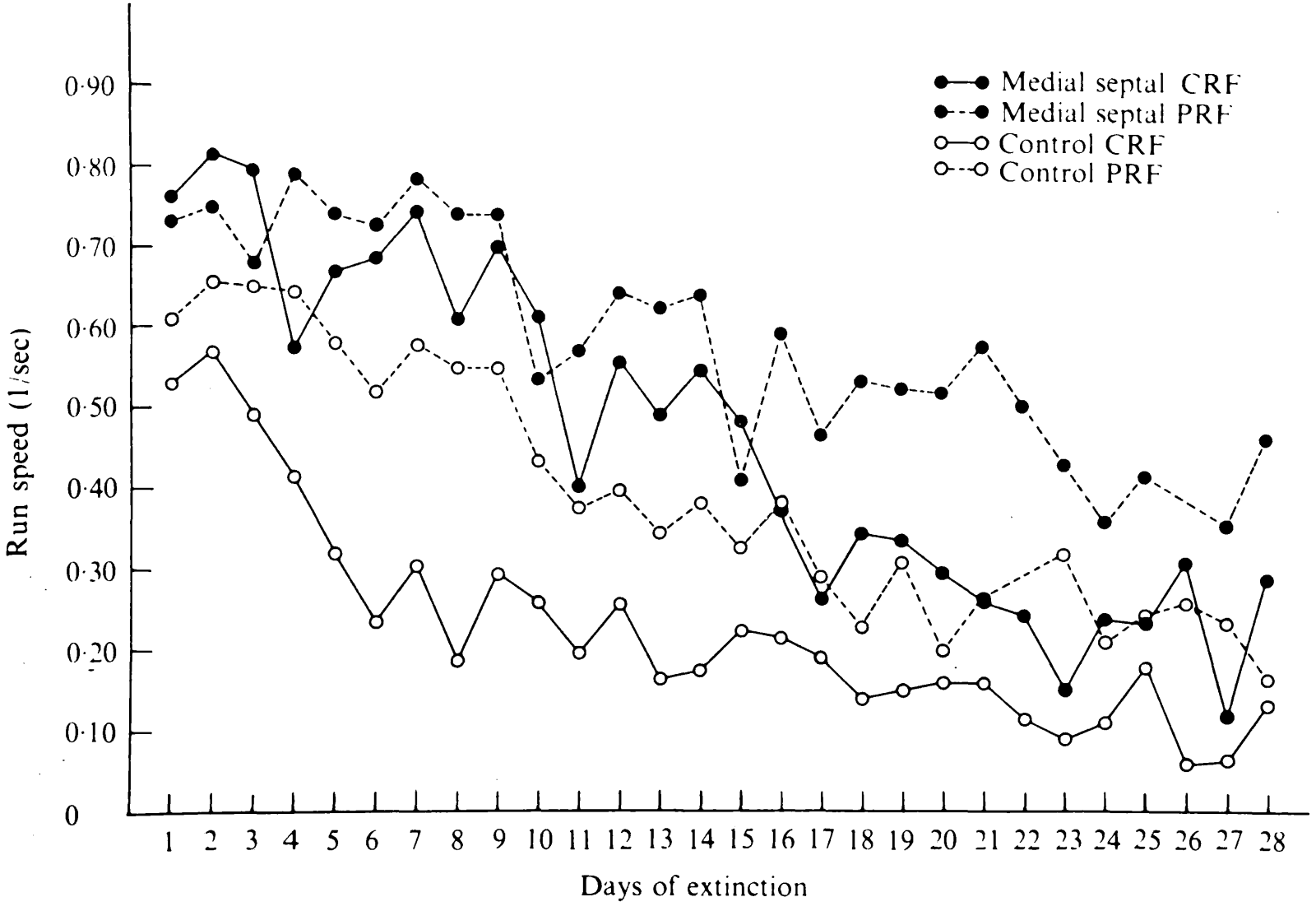
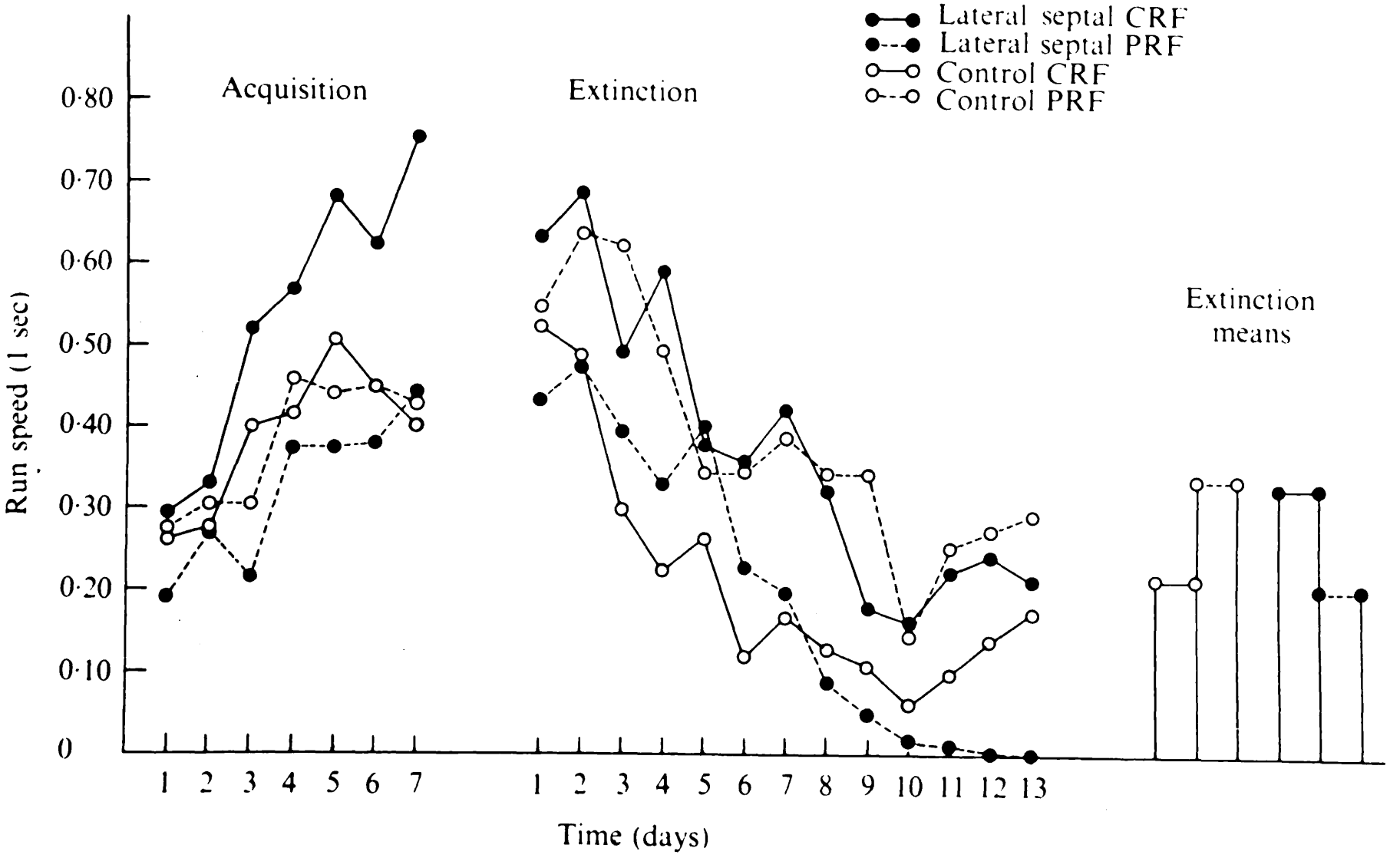


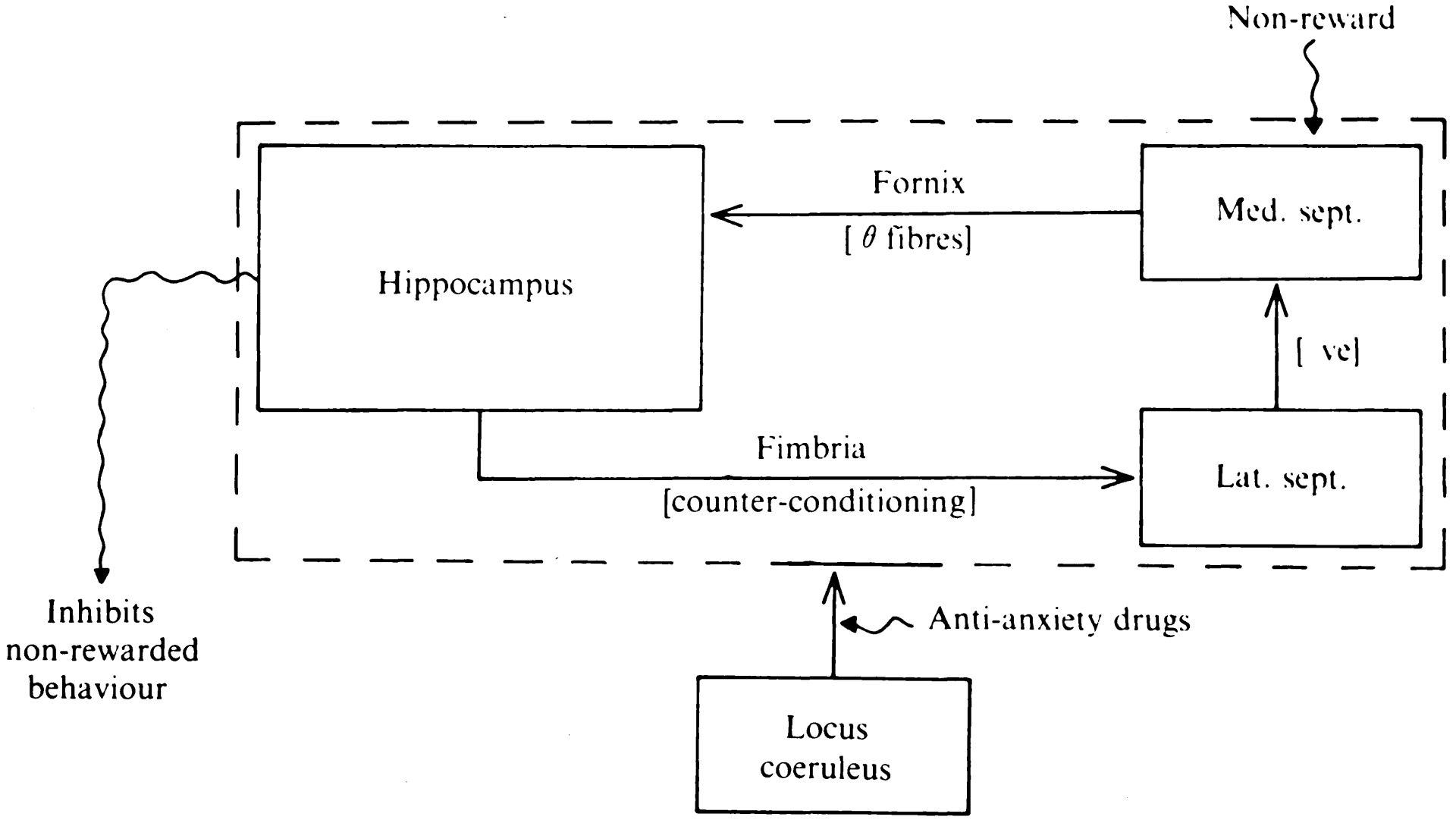
FIG: 5:2 Effects of lateral septal lesions on the partial reinforcement extinction effect in the alley at one trial per day.
CRF, Continuous reinforcement.
PRF, Partial reinforcement.
(Taken from Gray, 1978; p.427)



He reports that septal lesions increase resistance to extinction after continuous reinforcement (CRF) but decrease resistance after partial reinforcement (PRF). Henke (1974) has reported similar results. Gray (1978) has reported a study by Feldon, Rawlins and Gray in which lesions were limited to either the medial or lateral areas of the septum. "Our results," he reports," show clearly that at a 24 hour inter trial interval, the increased resistance to extinction seen after CRF training in the septal animals is due to medial septal damage, but the decreased resistance to extinction after PRF training (and the consequent abolition of the PREE (partial reinforcement extinction effect))is due to lateral septal damage." (1978, p.426). However, as is often the case, the data are somewhat more complex than this simple statement would seem to suggest. As can be seen from fig. 5:1 there is increased resistance to extinction in both groups of lesioned animals as compared to the appropriate control group. While it can be seen, from fig. 5:2 that, as Gray reports, the lesion of the lateral septal area produces a decrease in resistance to extinction in the PRF group, as compared to the appropriate control group, but it also produces an increase in resistance in the CRF group. Nor is it immediately evident from the model offered by Gray why the lateral CRF group should show the greatest acquisition. The result becomes still more difficult when it is noticed that there appears to be no real difference, on trial 7, the final acquisition trial, between the two control groups; Gray does not report whether or not this difference is significant.

Nevertheless he proposes the following hypothesis. "We have tried to relate this double dissociation between the

FIG: 5:3 A model for the role of the septo-hippocampal system in counter-conditioning (Taken from Gray, 1978; p.428)



behavioural effects of medial and lateral septal lesions to the organization of the SHS (septo-hippocampal system) by way of the hypothesis shown in Fig. 5:3. According to this hypothesis the medial septal area is the recipient of information, conveyed via an unknown route by secondary frustrative stimuli, concerning the imminence of non-reward. This information is conveyed to the hippocampus by way of the theta - producing fibres which travel in the dorsal fornix (Myher, 1975, Rawling unpublished observation). The hippocampus has the job of inhibiting the non-rewarding behaviour (by an unknown route) while determining the best behavioural strategy in the changed circumstances. (This period of behavioural inhibition and uncertainty is subjectively experienced as anxiety.) Under conditions in which the best strategy is in fact to continue with the original behaviour (as on a PRF schedule), the hippocampus sends a message (perhaps via the fimbria) to the lateral septal area which in turn via septal interneurones (De France, 1976), inhibits or otherwise alters the medial input to the hippocampus. The operation of this hippocampo septal pathway underlies the phenomenon of counter-conditioning". (1978, pp.426-428).

The association of frustration and anxiety assummed in this model is evident from the above quotation, but it is not clear whether orienting also produces anxiety; and what of selective attention? It is well known that the hippocampus plays an important part in this process. Perhaps the answer here is that it would be an error to assume that the hippocampus acts in a single undifferentiated manner.

It might be well to put some of Gray's speculations into perspective by noting that they are not uniquely his, and he has not always been the first to propose them. Milner (1971) for example summarizing his review of the effects

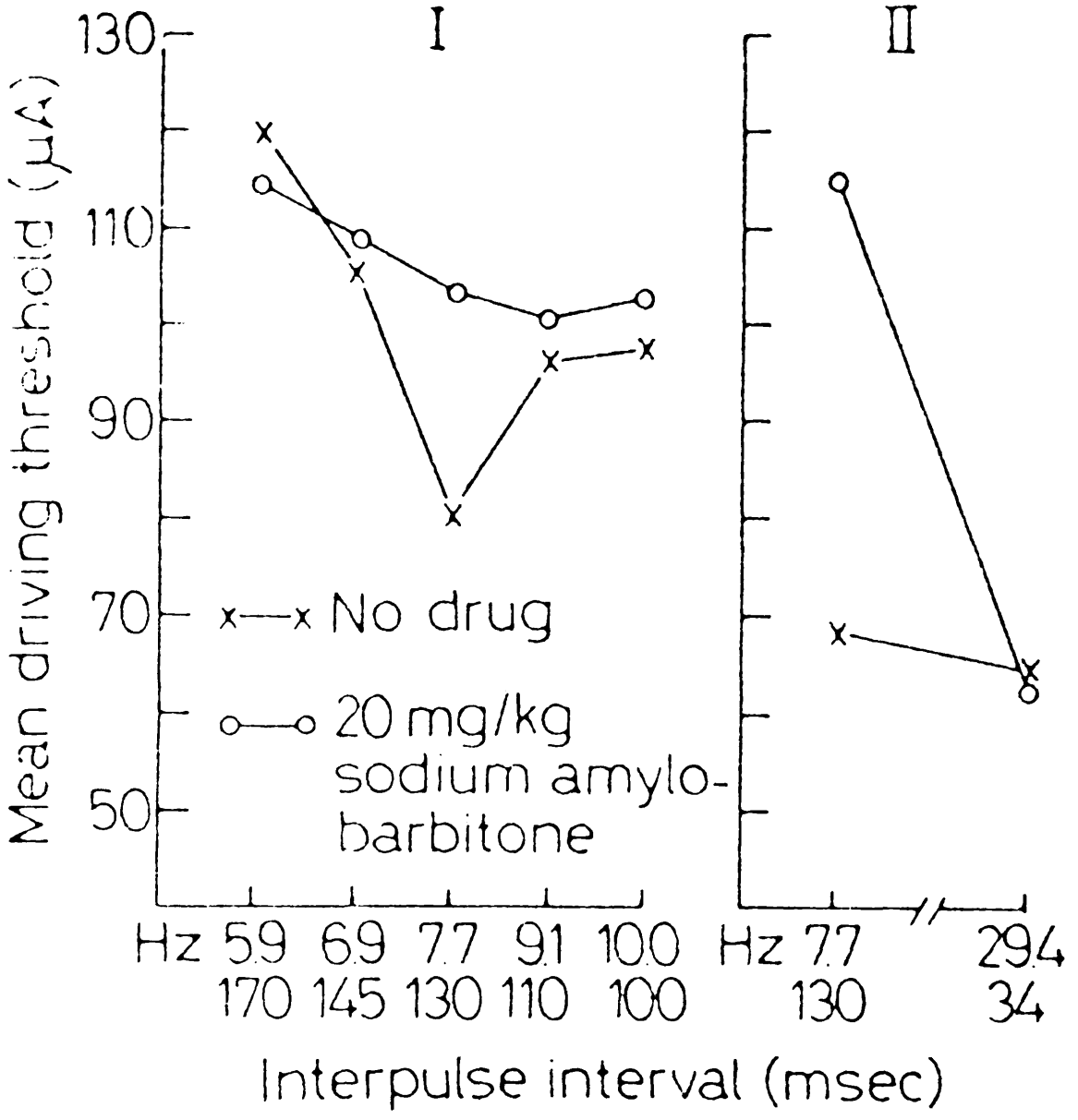


FIG: 5:4 Effects of 20mg/kg amylobarbitone sodium on threshold for septal driving of hippocampal theta rhythm of driving frequency in two groups (I and II) of five rats.
(Taken from Gray, 1972; p.111).

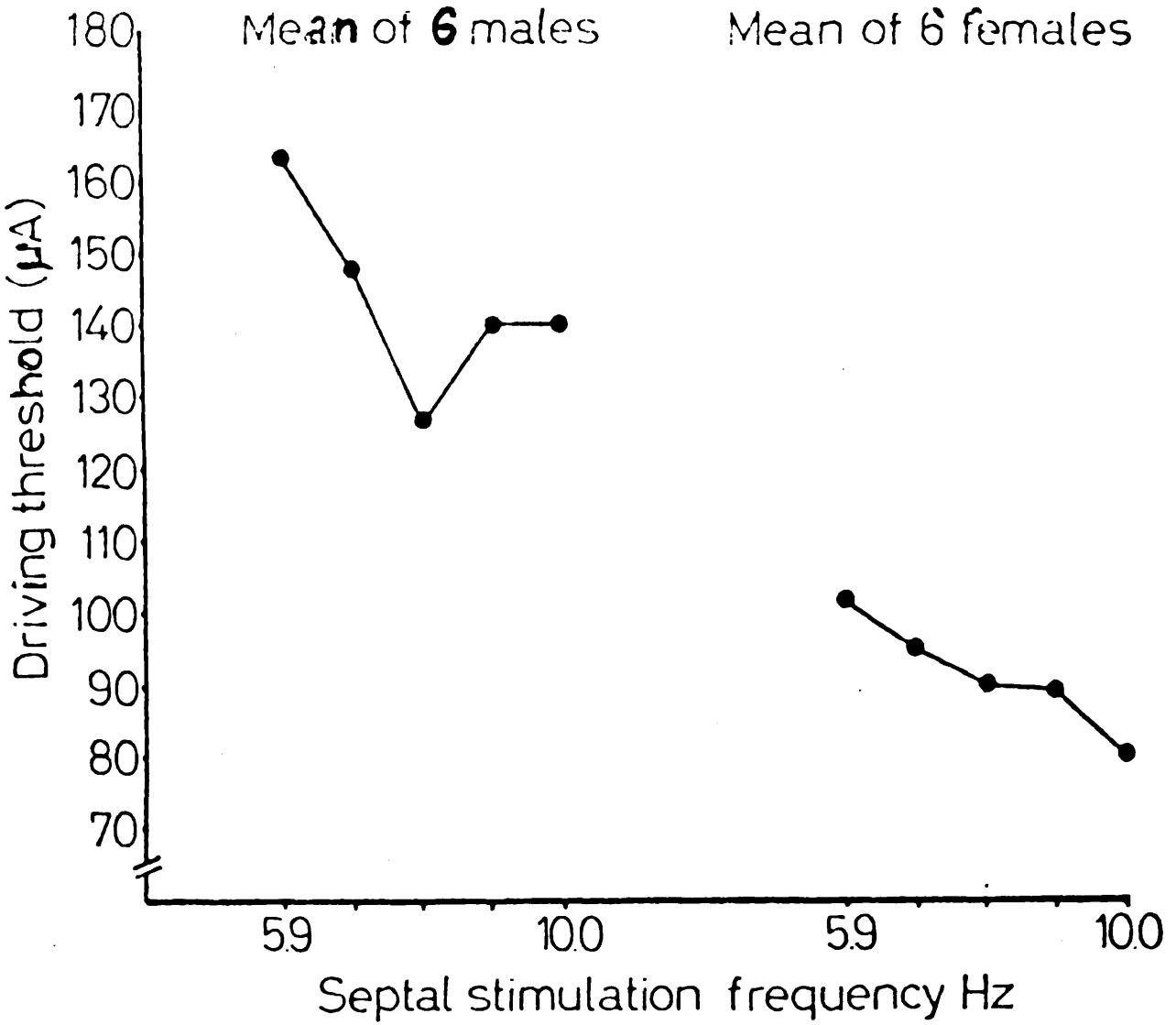
of various lesions concludes that (1) the septum and hippocampus are important for response inhibition; (2) normally activated by cues of non-reward and novelty; (3) that it is necessary to distinguish between active and passive avoidance. (C.F. Milner, 1971, p.376).

A third research technique Gray has employed is the blocking and stimulating of the electrical activity of the hippocampus. Following Stumpf (1965) he claims that the medial septal nucleus functions as a pace maker cell for the hippocampal theta rhythm. Bearing in mind the fact that: (1) amylobarbitone does not eliminate the theta rhythm (2) theta is found to accompany behaviours not affected by amylobarbitone, Gray suggested a 'frequency specific' hypothesis, after observing the theta activity of free-moving rats (Gray and Ball; 1970). He suggested that three distinct frequency bands exist: a low amplitude irregular theta band of 6-7.5Hz accompanying receipt of reward and the performance of fixed action patterns; a regular intermediate amplitude band of 7.5-8.5 Hz accompanying exploration and frustrative non-reward; and a regular, high amplitude band 8.5-10Hz accompanying approach behaviour and active avoidance. Since the drug affects only behaviour associated with the intermediate band, it should only be this band, 7.5-8.5 Hz, which is affected by amylobarbitone.

The next step (Gray and Ball; 1970) was to investigate the effect of the drug on theta driving. They employ free-running rats with bipolar-electrodes implanted in the medial septal area and the dorsal hippocampus. "Driving was judged as synchrony between the stimulating pulses, 0.5 milliseconds in duration, applied to the septal area and the theta waves recorded from the hippocampus" (1972, p.111).

As can be seen from fig. 5:4 in the no-drug conditions

FIG: 5:5 Mean thresholds for septal driving of hippocampal theta rhythm as a function of driving frequency in male and female rats. (Taken from Gray, 1972; p.114)



there is a substantially lower driving threshold at 7.7Hz. and this is selectively elevated by the drug, as the hypothesis would predict. Other drugs, including alcohol, are found to have the same selective effect, (Gray, 1975(b)).

The situation is thrown into some confusion, however when it is realized that these data come only from male rats, and that female rats have relatively flat "theta driving curves", and , at least in novel situations, are less fearful than males. Gray comments: "It seem probable, therefore, that there is a fundamental difference between the sexes in the response of the hippocampus to stimulation of the septal area. In the light of the sex differences in fearfulness (Gray 1971), it is perhaps no coincidence that this difference takes the form that the female theta driving curve in the undrugged state (fig. 5:5) is closely similar to the curve obtained from the male rat which has received amylobarbitone or alcohol: Electrophysiologically as well as behaviourally, a female rat resembles a tranquilized male one". (1972(a), p.116). One might assume from this that the threshold at the critical 7.7Hz was actually higher in female rats than in male rats, but, as the fig (5:5) shows, the contrary is true. In fact there is no over lap of the two curves, the females showing lower threshold values for all frequencies. While this apparent conflict may be solved by reference to relative values, or activity, in different systems rather than absolute values; one feels somewhat reluctant to generalize these findings across species knowing the limitations of generalizations across sexes within one species. Gray, however, summarizes the results of a number of studies which provide support for his frequency specific hypothesis as follows: "these experiments offer evidence that: (1) theta driving during extinction speeds up extinction; (2) theta driving on a

random 50% of rewarded trials creates a "pseudo partial reinforcement extinction effect", that is, an increase in subsequent resistance to extinction; (3) theta blocking by electrical stimulation of the septal area on the non-rewarded trials of a partial reinforcement schedule reduces resistance to extinction relative to normal controls trained on partial reinforcement; and (4) lesions to the medial septal area which disrupts the theta rhyth markedly attenuates the partial reinforcement extinction effect. All these results are readily understood on the hypothesis that amobarbital affects behaviour by antagonizing the normal theta response to frustrative non-reward" (1972(b), p.189).

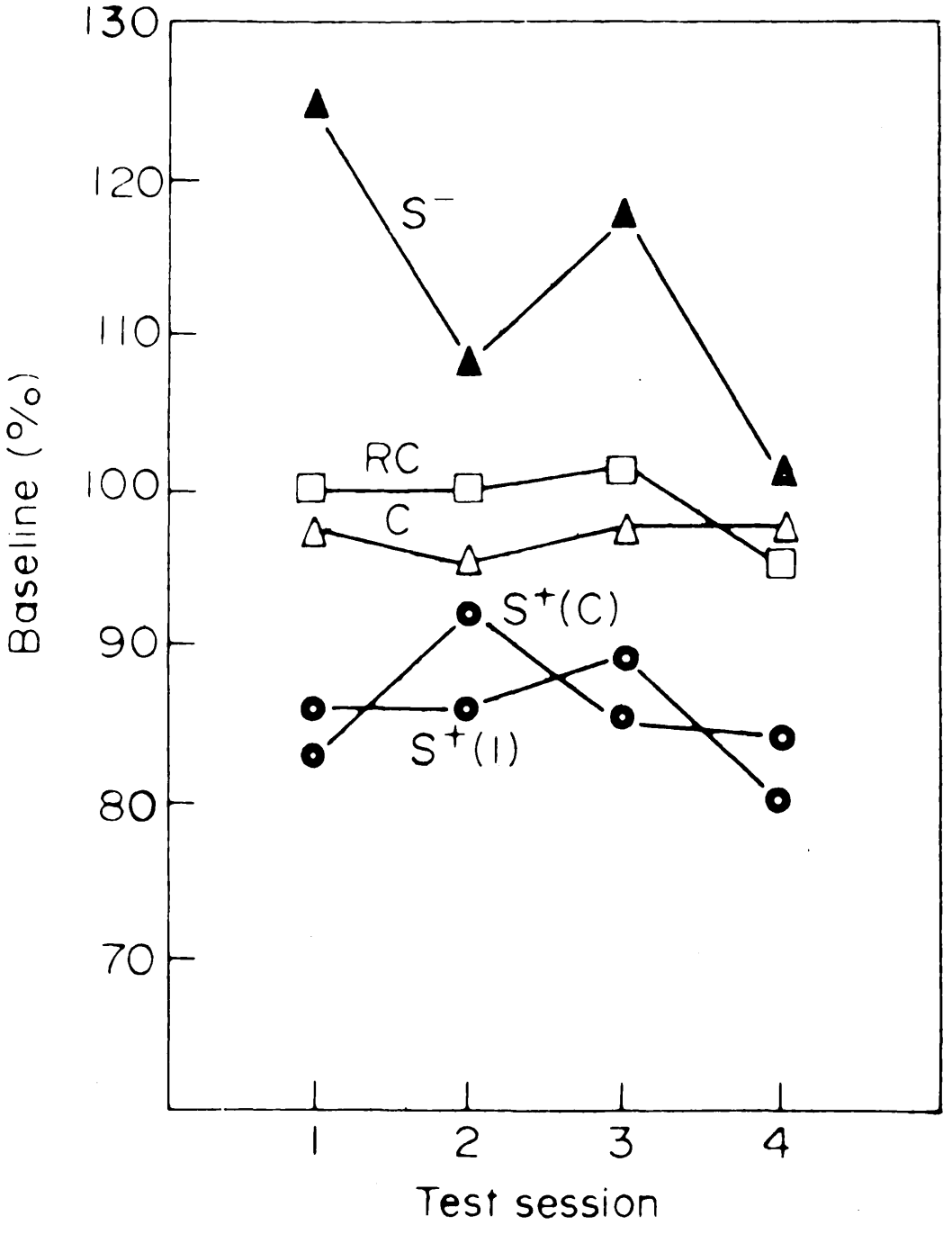
It appears then that the septo-hippocampal system may well be involved in passive avoidance, but Gray has suggested that impairment of passive avoidance and retardation of extinction also follow lesions of the frontal cortex, particularly orbital frontal lesions. In consequence he concludes that the frontal cortex is likely to be the site of the cortical representation of the hippocampal system.

This then is one element of Grays model, of a frontal-septa-hippocampal system which mediates behavioural inhibition, and passive avoidance, and is activated by cues of punishment and frustrative non-reward.

5:3 ACTIVE AVOIDANCE, REWARD AND RELIEVING NON-PUNISHMENT

In contrast to the previous hypothesis, in support of which Gray marshals a considerable amount of evidence, Gray's second hypothesis that a $\overline{\text{Pun}}$-CS is functionally equivalent to a Rew-CS, and both are mediated by the same system that produces active avoidance is far less well supported. As Gray himself comments, this hypothesis is "as yet virtually innocent of contact, positive or negative, with the harsh

FIG: 5:6 Effects of stimuli paired with Reward and Non-reward on Sidman Avoidance Responding. Performance of a previously learned avoidance response over 40 trials by groups of subjects for which the test tone had signalled no food, S-; had been unrelated to food, RC; had not been previously presented, C; or had signalled the occurrence of food in either classical conditioning, S+(C), or on instrumental S+(I) situation.
(Taken from Gray, 1975; p.342)


130
120
110
100
90
80
70
Baseline (%)
S⁻
RC
C
S⁺(C)
S⁺(I)
1
2
3
4
Test session

world of experimental fact." (1975a, p.341).

He does report a study by Grossen, Kastansek and Bolles (1969) in which a tone was used as a CS of reward, for some animals, or non-reward for others. "Now, if there is a functional equivalence between Pun-CS and Rew-CS, we might expect the Rew-CS to produce the same kind of change in the rate of avoidance responding (in a Sidman avoidance situation) as does the Pun-CS, that is, to decrease it." (1975a, p.341).

Indeed, as can be seen from fig 5:6 (the S+(c) group is the relevant one) the Rew-CS group is found to respond at a lower rate relative to the two control groups (Labelled 'C' and 'RC'). It is also evident from the figure that the Rew-CS increases the rate of responding in relation to the control groups. The S+(I) group is included in an attempt to demonstrate that the suppression effect is not due to some motor artifact. It is unfortunate, however, that no Pun-CS was included in the same experiment, this would have made for a more convincing demonstration.

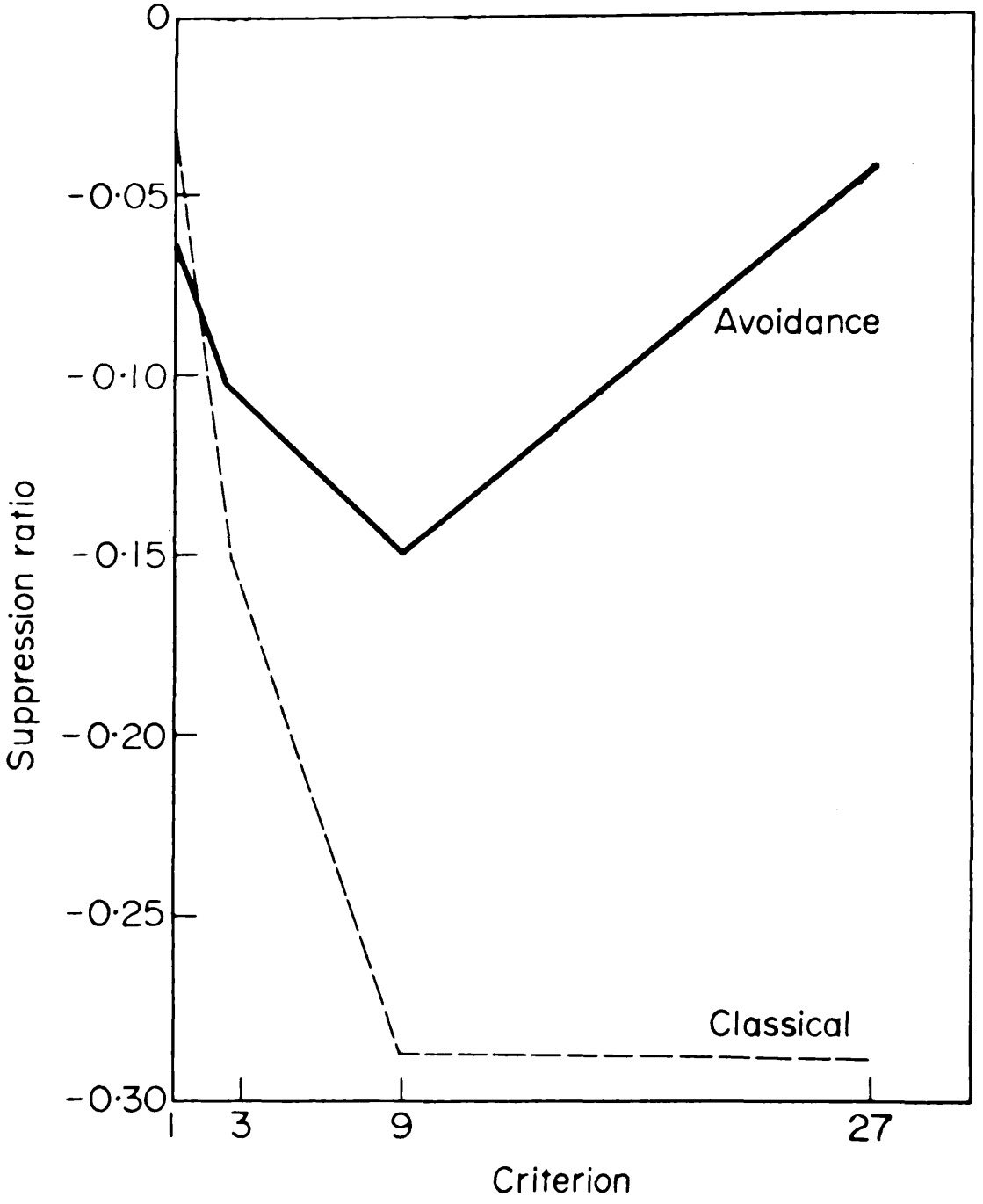
If the Pun-CS can be construed as a "warning-signal" with aversive qualities to be avoided, then, suggests Gray, the Pun-CS may be regarded as a 'safety signal' with appetitive qualities. Thus he believes that in a Sidman avoidance the animal is actually approaching the appetitive 'safety signals' rather than escaping from, terminating or avoiding aversive stimuli.

In view of the paucity of the evidence in the area, the whole hypothesis is too problematical for one to be totally confident of Gray's analysis. For example, he reports a study by Kamin et al (1963) in which one group of animals was exposed to a CS and unavoidable shock, 3, 9 or 27 times; a second group to a CS and avoidable shock, in a shuttle box, again 3, 9 or 27 times. The test phase of the experiment was

the presentation of the CS while the animal was bar-pressing for a reward; the more aversive the CS the greater should be the suppression of the bar pressing, this is known as a conditioned emotional response (CER). "The conditioned emotional response (suppression of bar-pressing) increased in magnitude during the early stages of training in the shuttle box, but as performance (in the 'avoidance' group) reached successively higher criteria the suppression ratio fell again. This finding is, of course unexpected from the point of view of the unmodified Mowrer two process theory, according to which as fear of the warning signals goes down, so ought performance of the avoidance response. According to the present view, however, this result makes good sense: fear of the warning signal extinguishes as it is no longer followed by punishment (because the animal is successfully avoiding it), but the avoidance response is now maintained by the positive reinforcement of the safety signals (those produced by shuttling to the currently safe side of the apparatus)" (1975, p.325-6).

It is the shuttling, then, which produces the safety signals i.e. we have a situation in which the cues to approach appear to be indistinguishable from the approach response itself. The situation becomes even more complex when it is recognized that in a series of experiments Taub and Berman (1968) deprived rhesus monkeys of virtually all sensory feed back, exteroceptive, proprioceptive and interoceptive. Never-the-less whether the deafferentation was carried out before or after training the animal was able to learn and maintain the appropriate avoidance response. Thus learning took place when any possibility of secondary reinforcement, positive or negative, seemed to have been eliminated. Gray himself is forced to comment: "the only items which can

FIG: 5:7 Fear elicited by Warning Signals at different levels of avoidance training. Conditioned suppression (0 = no suppression) produced by a warning signal paired with unavoidable shock 1, 3, 9 or 27 times in the "classical" group, or to which the animals have made 1, 3, 9 or 27 consecutive avoidance responses in the avoidance group. (Taken from Gray, 1975; p.326).


0
-0·05
-0·10
-0·15
-0·20
-0·25
-0·30
Suppression ratio
Avoidance
Classical
1
3
9
27
Criterion

develop secondary aversive and secondary rewarding properties are the motor commands themselves. But to push the language of classical conditioning and secondary reinforcement this far is obviously to take two-process theory beyond the point up to which it remains useful." (1975a, p.338). Thus we arrive at a position, if we do accept this explanation, where the cue to approach is identical to the command to approach. Given that the animal can learn with no information other than the presence or absence of the UCS and the issuing of the motor-command it seems evident why Gray could not simply say that the Pun-CS in the shuttle box over trials becomes a Pun-CS. Gray's problem here turns to a considerable extent on what constitutes the necessary and sufficient conditions for the formation of secondary reinforcers, and also what information will be used, even preferred, when it is available. Even this strategy, however, will not really allow Gray to suppose that the most obvious CS in the environment, the warning signal, becomes a safety signal. If this CS did gradually become a safety signal, and assumming that the reasoning applied to the data of this and the previous experiment is sound, we would expect an increase in the rate of bar-pressing for a reward on presentation of this CS as it acquired positively reinforcing properties. As can be seen from fig. 5:7 even after 27 trials there is still a depression in the rate of responding.

It is worth noting in passing that on one hand these experiments seem to suggest that a secondary reinforcement might not be necessary for the learning of active avoidance, and on the other the Pun-CS, if any exist, in the Kamin et al experiment appears to be of a fundamentally different kind from the Rew-CS employed in the Grossen et al experiment. There seems little use in labouring this point any longer

especially as Gray himself is obviously unhappy with having to assume that the motor-command is the $\overline{\text{Pun}}$-CS.

There is, however, another aspect of Gray's analysis which is equally unsatisfactory. Gray claims that as the Pun-CS is no longer followed by punishment, because of the successful avoidance response, it extinguishes and in consequence loses its aversive quality, as the Kamin experiment is assumed to demonstrate. While the avoidance response itself is not assum to extinguish because it is an approach response to an appetitive stimulus. What according to Gray is the animal expecting as a result of this response? Nothing; and as this expectation is never disappointed the response never extinguishes. Surely this analysis cannot be correct, for $\overline{\text{Pun}}$ to have any meaning it cannot simply mean 'nothing', but must mean the absence of some aversive event that is expected. What status can this expectation have, in Gray's schema if it is not itself aversive? If the animal does not respond to the $\overline{\text{Pun}}$-CS with the aversive state of 'expectation of $\overline{\text{Pun}}$' it is difficult to see how $\overline{\text{Pun}}$ and $\overline{\text{Pun}}$-CS can have any meaning. Yet this is exactly what Gray seems to be claiming - that although the animal no longer expects a punishment it is relieved when it does not arrive.

It is evident that one of the basic assumptions in Grays analysis is that if an aversive UCS is not presented in some relationship to the CS the expectation of the UCS extinguishes. This is not necessarily the case, however. Gray (1975a), himself, quotes a study by Soltysik (1960) in which after 120 presentations of the CS without the UCS, the CR could still be elicited on the 121 trial on presentation of the CS, the important difference between Soltysik's situation and a traditional extinction situation is that the CS was presented in conjunction with a conditioned inhibitor (CI)

for the 120 trials.

It is quite possible, of course, that Gray's analysis of the situation or something along the lines he suggest, is correct, but it does seem that there are a number of weaknesses, and in view of the shortage of data his hypothesis ought to be treated with the greatest of caution.

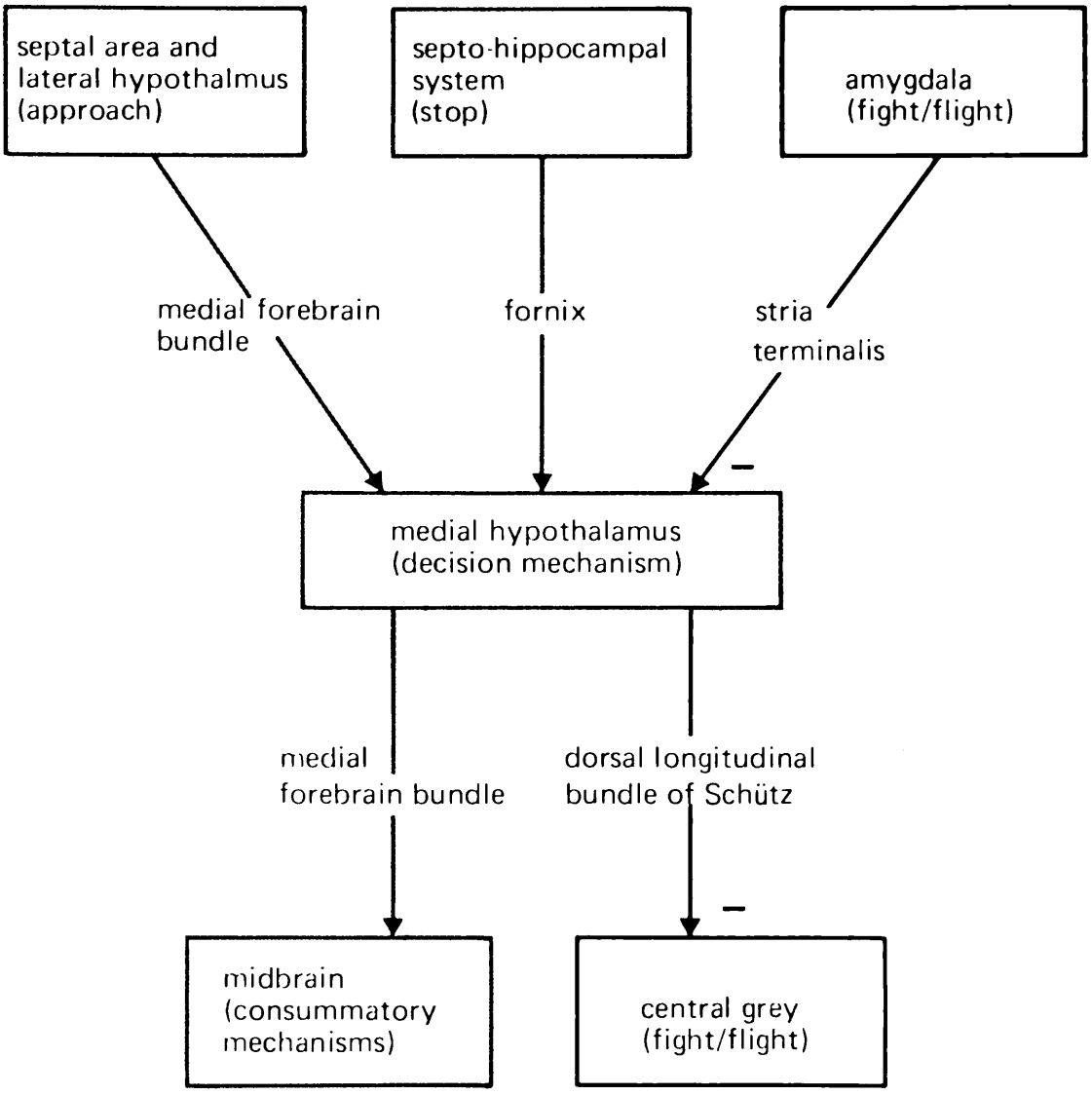
Data in support of a physiological system mediating Rew-CS and Pun-CS are similarly weak. It has already been pointed out that although lesions of the medial septal area and the administration of certain drugs impairs passive avoidance no adverse effect is found in active avoidance, in fact in conflict situations such as two-way avoidance learning in the shuttle box, improved learning and performance is often reported. Another piece of evidence used by Gray is that stimulation of Old's (1965) reward system has on one hand been found to impair the CER (Brody and Conrad; 1960) while on the other it actually improved active avoidance.

Again supporting the hypothesis that Pun-CS and Rew-CS are mediated by the same system he reports that the administration of chloropromizine and cortical depression (c.f. Olds and Olds (1965)), both disrupt self stimulation and learned escape behaviour. The disruption of spreading cortical depression was accompanied by reduced firing of the neurones of Olds reward system.

This then is the second element of the Grayian hypothesis, an approach system, responsive to cues of reward and relieving non-punishment, mediated by Olds reward system, the septal area, the lateral hypothalamus and the medial forebrain bundle. It must also be noted that the evidence for this system is much weaker than that for the septo-hippocampal stop system.

For the sake of completeness it ought to be noted that Gray proposes a third system. This system,

FIG: 5:8 Inter relationships between three emotional systems. The medial hypothalamus appears to act as a nodal point in the resolution of the influences proceeding down stream from the limbic structures. These promote approach behaviour (from the septal area via the medial forebrain bundle) or fight/flight behaviour (from the amygdala via the stria terminalis). The medial hypothalamus may inhibit both these outcomes via its control of the final common pathways in the midbrain, and this inhibition is intensified by influences proceeding from the septo-hippocampal stop system via the fornix and mammillary bodies. (Taken from Gray, 1971).


septal area and lateral hypothalmus (approach)
septo-hippocampal system (stop)
amygdala (fight/flight)
medial forebrain bundle
fornix
stria terminalis
medial hypothalamus (decision mechanism)
medial forebrain bundle
dorsal longitudinal bundle of Schütz
midbrain (consummatory mechanisms)
central grey (fight/flight)

postulated largely on the basis of Deutsch and Deutsch (1966) and the work of de Molina and Hunsperger (1962), he labels the fight/flight system. It is activated by unconditioned aversive stimuli and mediated by the amygdala and the stria terminalis. This system is of less importance, however, in the present context.

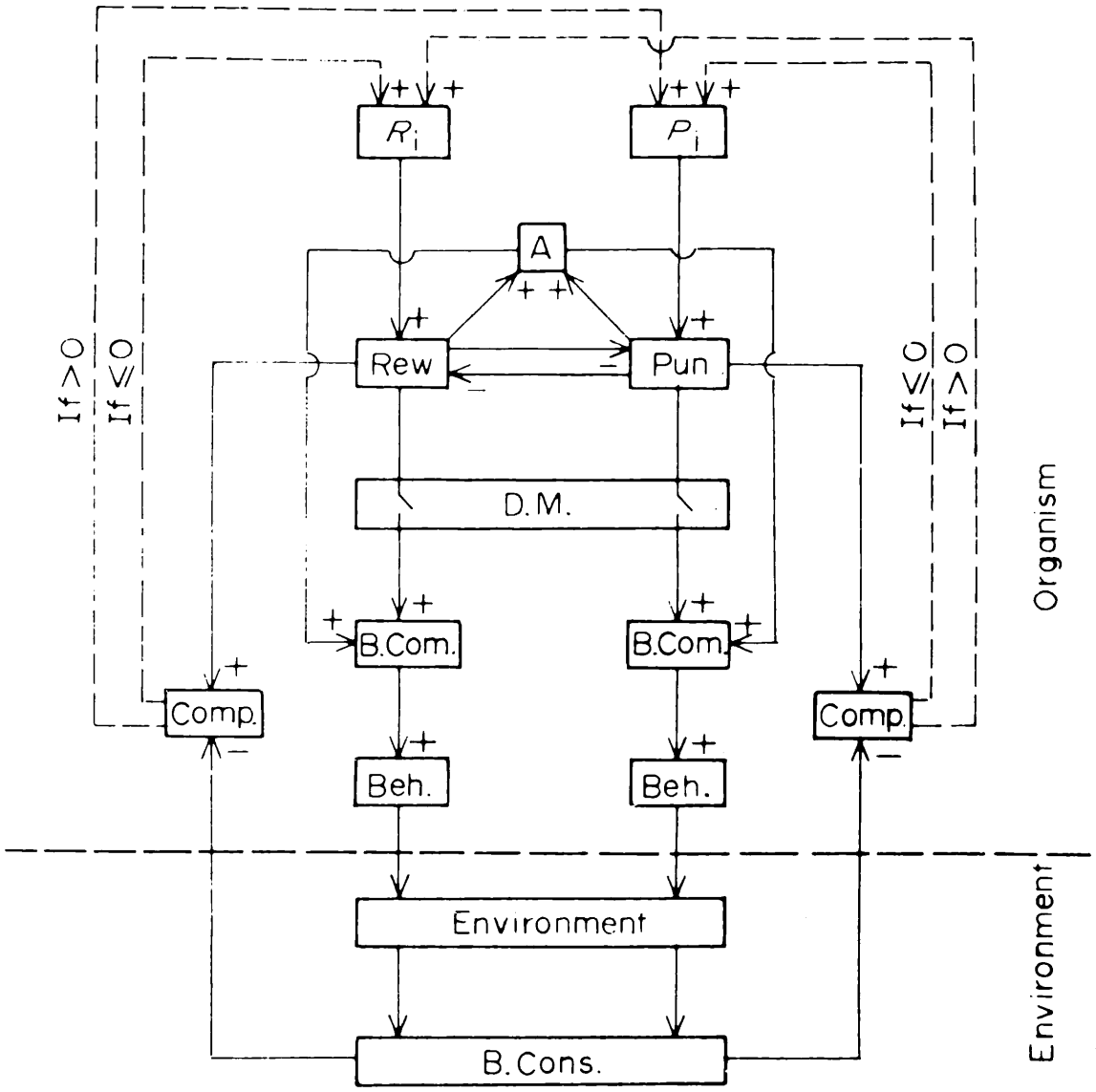
5:4 GRAY'S MODEL

Figure 5:8 provides a sketch diagram of the three physiological systems, and the manner in which they are suggested to interact. One feature of the diagram which is immediately evident is that while there are three systems feeding into the medial hypothalamus there are only two outputs from it. This is because the septo-hippocampal system exercises only an inhibitory relationship over the other two systems.

Gray (1972a, 1975a) suggests that the medial hypothalamic nucleus exercises inhibitory control over the pathway for fight/flight behaviour. It is the amygdala which, upon receipt of appropriate input inhibits the medial hypothalamus, producing, in consequence, a disinhibition of the fight/flight system. The medial hypothalamus functions as a decision mechanism in the sense that input from the septo-hippocampal system is thought to potentiate the inhibition of the fight/flight system with the result that the neurological, and consequently, behavioural outcome on any particular occasions is the result of the balance of activity proceeding from these two structures, the amygdalo-hypothalamic-midbrain (fight/flight) system and the septo-hippocampal (stop) system.

Gray proposes a symetrical organization with respect to approach behaviour. It is the lateral hypothalamus which is this time assummed to be inhibited by the medial

FIG: 5:9 Block diagram of the arousal-decision model. R_i and P_i: inputs to the reward and punishment mechanisms, Rew. and Pun. D.M.: the decision mechanism. A: the arousal mechanism. B.Com.: behavioural command to "approach" (on the reward side) or to "passively avoid" or "stop" (on the punishment side). Beh.: the observed motor behaviour. B.Cons.: the consequences (rewarding or punishing) of the behaviour that occurs. Comp.: comparator mechanisms which compare the actual consequences of behaviour with the expected consequences and make the appropriate reward and punishment inputs. Dashed lines indicate inputs on trial n+1 as a result of classical conditioning of exteroceptive, introceptive or proprioceptive CSS to the consequences of behaviour on trial n. (Taken from Gray, 1975; p.361)


R_i
P_i
A
Rew
Pun
D.M.
B.Com.
B.Com.
Comp.
Comp.
Beh.
Beh.
Environment
B.Cons.
If > O
If ≤ O
If ≤ O
If > O
Organism
Environment

hypothalamus, perhaps the ventro medial nucleus, this inhibition being enhanced by the septo-hippocampal system, and disinhibition resulting from appropriate input via the medial forebrain bundle from the septum.

Figure 5:9 proposes a model which attempts to incorporate some of the implications of the previous figure and also the various hypotheses set out earlier. This figure is taken from Gray and Smith (1969) and consistent with the hypothesis set out above inputs into the reward side of the model, the septal-lateral hypothalamic-medial forebrain system, will produce approach behaviour. The behavioural effects of this will feed into a comparator, presumably at the level of the amygdala, which if the reward is equal to, or exceeds that expected will produce an input into the reward system, but if the reward is less than that expected will produce an input into the punishment side of the model and activating the 'stop' system. Thus yielding a functional identity between cues of punishment and cues of non-reward.

Conversely an input into the punishment mechanism will produce 'stop' commands and passive avoidance. Again the actual punishment is compared, (again probably at the level of the amygdala), with the expected punishment. If the expected punishment proved to be greater than the actual punishment then there is an input into the reward side of the mechanism. Thus providing for a functional identity between cues of reward and cues of non-punishment.

The Reward and Punishment mechanisms are considered (Gray and Smith, 1969), to be mutually inhibitory partly on the basis of data from studies of self stimulation (Olds and Olds (1965)), and partly as a way of explaining trial by trial changes in the system. This inhibitory activity together with the decision mechanism, presumably the midline of the

hypothalamus, may be viewed as determining the probability of a particular response. While the arousal mechanism (marked box 'A' in the diagram), which presumably involves the ARAS, which has an output to (energizing?), the behavioural command mechanisms, and inputs from both the reward and punishment mechanisms, is included in an attempt to account for the phenomenon of 'drive summation'. As such it may be viewed as determining the intensity of behaviour. The result of all this is that observed behaviour may be regarded as a function of the probability of behaviour, reflecting the antagonistic activity of the two sides of the system, and the intensity of behaviour, reflecting the level of activity in the system as a whole. Gray and Smith (1969) have proposed a mathematical model to describe these relations.

Appealing as this model may be on first inspection, there do appear to be a number of difficulties. Let us take the distinction between active and passive avoidance as a starting place. From what has gone before, and from the model itself, it is evident that this distinction is central in Gray's model building. However, it is equally obvious that while active avoidance is defined in terms of some activity passive avoidance is defined very much in terms of what it is not. If we attempt to define these two types of avoidance in common terms it might be said that in active avoidance the probability of some response (R) will increase, while in passive avoidance the probability of some R will decrease, which is equivalent to saying that the probability of one or more other Rs will increase, here we would consider 'standing still' or 'freezing' an R. Thus the distinction between the two types of learning appears to be less to do with what the animal does and more to do with what the experimenter observes.

Similarly when we turn to Gray's model we do not find that the two types of avoidance as easily distinguishable as Gray seems to suggest. For the organism to expect a punishment there must be a prior input into the punishment mechanism, with the result that some R is inhibited. If the result of this is that the animal avoids punishment then there should be an input into the reward side of the model with the result that some other R is activated. This seems to be identical to Gray's description of active avoidance with the exception of the initial stage of inhibition. Is it this inhibitory stage then which distinguishes the two forms of avoidance? This hardly seems likely as the animal is making some R, which must therefore be 'preferred', which must be terminated or inhibited in order to perform the active avoidance R. In terms of the model then active and passive avoidance appear to be indistinguishable.

There are, of course, two distinct questions tangled up here: (1) Is there in reality a distinction between the two types of avoidance, e.g. are they mediated by different mechanisms; (2) Does Gray's model adequately reflect this distinction. Obviously if '1' is not true the '2' is not possible.

Another aspect of the model which is not entirely satisfactory, is the effect of the activation of the reward and punishment mechanisms, and the relation of these to the comparators. From the observations of Gray reported earlier, and from the fact that the mechanisms were out lined partly on the basis of the observation of self stimulation activity it would seem that the activity of these mechanisms is synonymous with reward and punishment. Indeed he says: "Now it takes no great leap of imagination to suppose that these results indicate the existence in the brain of two

fundamental motivational systems, a 'reward' mechanism and a 'punishment' mechanism. That is to say, the common denominator of the heterogeneous class of events which an animal finds rewarding (e.g. food, water, copulation) is that they cause an increase in the activity of the reward mechanism of the brain, while the common denominator of such diverse punishments as electric shock, loud noise, sudden loss of support, and so on, is that they cause neurones to fire in the brain punishment mechanism". (Gray, 1972a, p.182).

However when talking about the relationship between these mechanisms and the comparators it is not immediately evident that this activity is itself rewarding. "The reward mechanism now sends a signal to the 'comparator for reward' informing it of the kind and amount of reward which can be shortly expected to occur (on the basis of signals from the environment which the reward mechanism is itself receiving). That is to say, it sends to the comparator for reward a copy of the stored memory that, whenever these conditioned stimuli (the secondary rewarding stimuli) have occurred in the past, they have been followed at such-and-such an interval of time by this unconditioned stimulus". (Gray, 1972a, p.187).

If we assume that the activity of these mechanisms is itself rewarding or punishing there appears to be a further problem. If we take the punishment side of the model for example, a stimulus entering the system via P. must pass through the punishment mechanism before any other behaviour is initiated or inhibited. In consequence even if the effects of behaviour are themselves rewarding, e.g. the termination of a shock, it is difficult to see how the model can ever account extinction as the punish mechanism will always be activated, what is more it will be activated before the rewarding mechanism is activated with the result that although the punishment

mechanism may attenuate the activity of the reward mechanism, the converse cannot occur initially.

One solution might be to postulate that Pi itself, as well as the activity of behavioural command mechanism, becomes a cue for reward. It will be remembered that this solution was also suggested earlier, and that Gray appeared to reject it. There it took the form of suggesting that the warning signals gradually, over a series of successful avoidance responses, became safety signals too. However for this suggestion to work it seems some modification to the model is needed, perhaps (1) a direct link to the reward side of the model and (2) some form of communication with comparators, other than via the punishment mechanism. An alternative approach might be to divide the punishment mechanism in two, one part a store which (a) received input from the punishment mechanism and (b) was able to inhibit this mechanism (c) also receiving an input from Pi, and supply the output to the comparator. These suggestions might permit a distinction between conditioned inhibition and extinction, with the result that the lack of responsiveness of the rat to the discriminative stimulus, the warning signal, in the shuttle box might be due to conditioned inhibition rather than extinction.

Similar problems arise in respect of the reward side of the model. For example, according to the model the non-presentation of an expected reward causes an input into the punishment mechanism with the eventual result that the approach response is inhibited, but what effect does this have? Does it reduce the frustrative effect of non-Reward, the animal no longer being frustrated because it does not attempt to get the reward? Is this termination of frustration rewarding, we must assume so on Grays schema, thus producing feed back into the Reward mechanism? What is the result of this renewed

feed back? Renewed approach, for the reward mechanism has no control over behavioural inhibition?

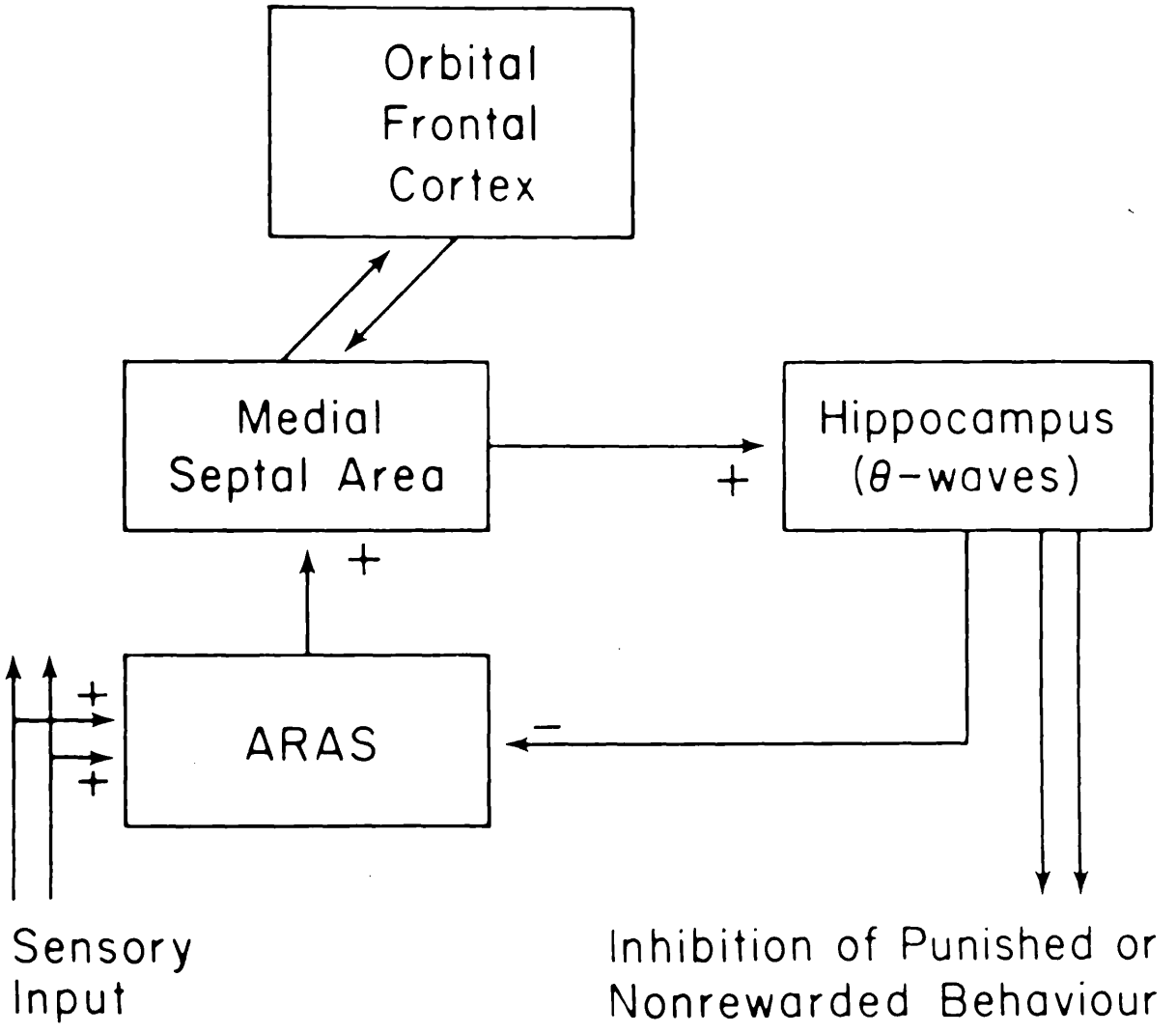
Another problem is the use of the term inhibition. Gray (1975a) has dealt at length with the concept of inhibition, and yet in as far as the punishment mechanism is the stop system we must assume it mediates the orienting response, a form of external inhibition, but it also mediates behaviour-inhibition in response to a Pun-CS, which is an internal or conditioned inhibition. So it seems from Gray's model that both types of inhibition are mediated by the same mechanism. But Gray, himself, concludes: "The first question - the reality of the difference between external inhibitory response decrements and internal inhibitory response decrements - is most easily dealt with: the conditions which give rise to each of these and the time course followed by each, on which the Pavlovian classification is based, seems so diametrically opposed to each other that it is difficult to see how a single mechanism could be responsible for both." (Gray 1975a, p.90).

Some, if not all, of these problems might be solved if the manner of functioning of the various mechanisms was made explicit, or if the model were placed in some more obvious relationship to the general functioning of the organism. The force of these observations is obviously that the model, as it stands at the moment, is not entirely satisfactory, but, perhaps more importantly, they are intended to emphasize the point that it is not always evident, and seldom unequivocal what predictions should be drawn from this model.

5:5 SOME PROPOSED MODIFICATIONS OF EYSENCK'S THEORY

Gray has proposed extensive modifications in the conceptualization of two of Eysenck's personality dimensions,

FIG: 5:10 Negative feedback loop whose activity is presumed to under-lie the dimension of introversion-extroversion. (Taken from Gray, 1975; p.193).



extraversion and neuroticism. The two dimensions will be dealt with in this order.

As has already been observed Eysenck suggests that the central difference between the extravert and the introvert is one of excitation/inhibition balance; introverts showing a relative predominance of excitation, this in turn being assummed to underlie their superior conditionability. This greater excitation is assummed to reflect greater activity in the cortico-reticular loop. Gray (1970, 1972b), has suggested that this system is not entirely distinct from the septo-hippocampal stop system, and goes on to suggest that the pacemaker cells of the medial septal nucleus produce the theta rhythm in the hippocampus as a result of neural input from the midbrain reticular formation (RF). He also proposes that there is a hippocampal-RF link of an inhibitory nature (c.f. Adey, Segundo and Livingston (1957), Livingston (1959)) causing inhibition of upstream conductance "from the midbrain RF to the thalamic parts of the ARAS". The whole of this forming a feed back loop with the result that increased activity in the ARAS will produce : (1) inhibition of the ARA itself; (2) activate the septo-hippocampal stop system and consequently produce 'act' inhibition (c.f. Fig. 5:10). In addition the hippocampal and septal areas are assummed to exert an inhibitory influence over sensory input, with the result that increased activation of the ARAS will also inhibit sensory input. This behavioural inhibition and inhibition of sensory input, both centrally and perhaps more peripherally, it will be recalled, were all suggested to be characteristic of the introvert, according to Eysenck's theory. Gray, therefore, suggests that it is the level of activity in the septo-hippocampal-reticular system which under-lies the dimension of introversion-extraversion.

The inter-relationship between these two systems is further emphasized by Gray when he observes that barbiturates, (sodium amobarbitone is a barbiturate), and alcohol are known to have an extraverting effect (Eysenck, 1967); and secondly lesions of the frontal cortex also have an extraverting effect. Of course, amylobarbitone is the main drug employed by Gray to reduce activity in the septo-hippocampal system, while, as pointed out earlier the frontal cortex, particularly the orbital section, is seen as the site of the cortical projections of the septo-hippocampal system, and lesions of the frontal cortex are found to have effects similar to lesions of the septal area. Thus, Gray reasons, if the same techniques are found to produce increased extraversion in humans and decreased activity in the septo-hippocampal system in other animals, the introvert must be characterized by a higher level of activity in the septo-hippocampal system. On the basis of Gray's observations it would also be predicted that the introvert ought to condition passive avoidance more efficiently.

Gray, however, draws more extensive comparisons between Eysenck's theory and his own than those already noted. He argues that a critical element in the development of Eysenck's theory was his attempt to answer two related questions. "(1) What are the psychological and/or physiological variables which result in the higher suseptibility of individuals with higher degrees of neuroticism <u>both</u> to dysthymic disorders and to commission of psychopathic offences? (2) What are the psychological and/or physiological variables which differentiate neurotic individuals along the dimension of introversion-extraversion (which is quite independent of the dimension of neuroticism) and which causes the <u>introverted</u> neurotic to be susceptible to the <u>dysthymic</u> disorders, but the <u>extroverted</u> neurotic to display anti-social behaviour of a <u>psychopathic</u>

kind?" (Gray, 1970, pp.249-250).

Gray (1970) summarizes Eysenck's solutions to these problems in the following manner: (1) dysthymics are over-socialized, while psychopaths are under-socialized; introverts are too conscientious, while extraverts are not conscientious enough. The conscience is considered to be "a cluster of classically conditioned fear reactions" (Gray, 1970, p.251). (2) Introverts are thought to condition better than extraverts; while neuroticism functions as a drive to facilitate conditioning; and because neuroticism is a dimension of susceptibility to emotionality, "it is the combination of strong emotions with the neurotic introvert's over-socialized conscience which leads him into hospital; and correspondingly the combination of strong emotions with the lack of a conscience which leads the extraverted neurotic into trouble with the law". (Gray, 1970, p.251).

Gray's main objection to these proposed solutions is that introverts are not, correctly, held to be more conditionable than extraverts, but as has already been discussed, are expected to condition more efficiently only under certain conditions, which he terms under-arousing. Eysenck talks of conditions in which inhibition is most easily developed, however, these two forms of expression amount to the same thing if we regard arousal as being synonomous with excitation-inhibition balance. Gray goes on to remark that "since there is no reason to suppose that parental conditioning techniques are more often under-arousing than over-arousing, there is equally no reason to predict the over-socialization of the introvert which is critical to the whole of Eysenck's theoretical super structure." (1970, p.255).

Accepting Eysenck's contention that socialization, or a conscience, consists of a cluster of fear reactions, Gray

LEVEL		
1.	SOCIO-PSYCHIATRIC	Introverts: Dysthymic Disorders Extraverts: Psychopathic Behaviours
2.	SOCIALIZATION	In Introverts: Good In Extraverts: Poor
3:	CONDITIONING OF FEAR	In Introverts: Good In Extraverts: Poor
4:	CONDITIONABILITY	In Introverts: Good In Extraverts: Poor
5:	AROUSABILITY	In Introverts: High In Extraverts: Low
6:	PHYSIOLOGICAL	ARAS In Introverts: High Activity In Extraverts: Low Activity

FIG: 5:11(a) BASIC STRUCTURE OF EYSENCK'S THEORY INTROVERSION-EXTRAVERSION
(Taken from Gray (1972), p.197)

LEVEL		
1.	SOCIAL-PSYCHIATRIC	Introverts: Dysthymic Disorders Extraverts: Psychopathic Behaviours
2.	SOCIALIZATION	In Introverts: Good In Extraverts: Poor
3.	CONDITIONING OF FEAR	In Introverts: Good In Extraverts: Poor
4.	SUSCEPTIBILITY to Punishment	In Introverts: High In Extraverts: Low
5.	AROUSABILITY	In Introverts: High In Extraverts: Low
6.	PHYSIOLOGICAL	Feedback Loop comprising ARAS Frontal Cortex, Septal Area and Hippocampus In Introverts: High Activity In Extraverts: Low Activity

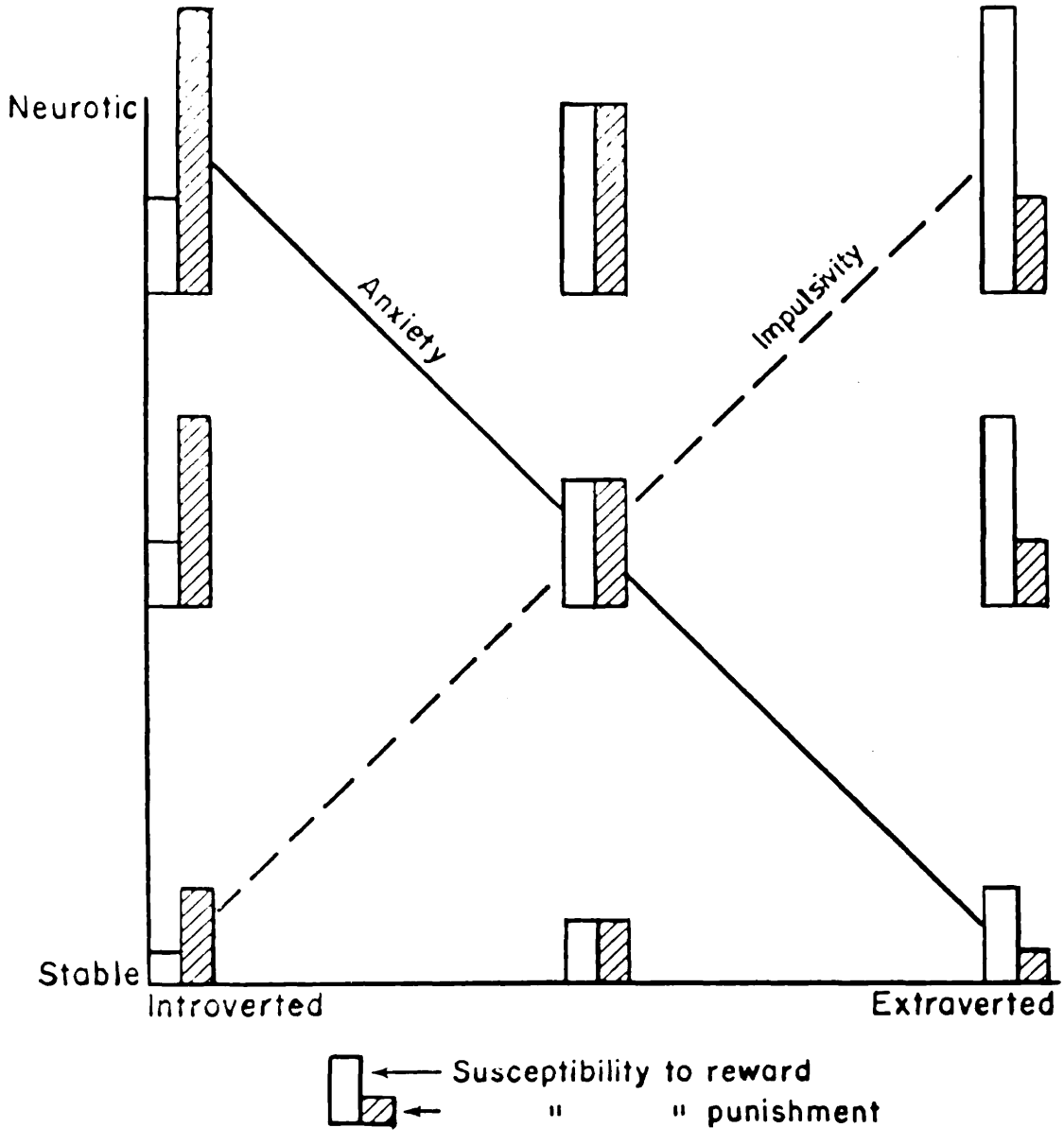
FIG: 5:11(b) GRAY'S PROPOSED MODIFICATION OF EYSENCK'S THEORY OF INTROVERSION-EXTRAVERSION
(Taken from Gray (1972) p.200).

suggests that we ought not to regard introverts as generally more efficient at conditioning, but simply 'better' at conditioning fear.

This solution, he feels, also offers an explanation of the Spence-Taylor anxiety data which suggest that relatively threatening situations are optimal for the conditioning of high anxiety (HA) subjects, for these, as has already been observed, tend to be introverted neurotics as classified by Eysenck's dimensions. Thus introverted neurotics can be expected to be more prone to disorders displaying an excess of fear, while extraverted neurotics should be more susceptible to those characterized by a lack of fear. Gray has offered these two schematic representations fig 5:11a of Eysenck's model and Fig. 5:11b of his modification of that model.

Turning now to the dimension of neuroticism, one of the most evident, and ultimately most important, differences between the two theories is the manner in which this dimension is construed. For Eysenck neuroticism or emotionality, has the properties of a drive, while Gray has defined emotions "those(hypothetical) states in the nervous system which are produced by reinforcing events or by stimuli which have in the subjects previous experience been followed by reinforcing events." (1972a, pp.81-82). Now the events usually associated with emotionality are stressful. In the description of neuroticism offered earlier Jones, (1960) indicated that the more neurotic person could be expected to have a lower tolerance both for physical pain and for frustration, and he went on to conclude that "the high drive of neurotics is aroused only in situations of threat", and not necessarily in appetitive situations. If this were the case then for Gray, neuroticism and introversion would at least be indistinguishable, if not identical. Thus another

FIG: 5:12 Proposed relationship of (a) susceptibility to signals of reward and susceptibility to signals of punishment of (b) the dimensions of introversion-extraversion and neuroticism. The dimensions of anxiety and impusivity (diagonals) represent the steepest rates of increase in susceptibility to signals of punishment and reward respectively. (Taken from Gray, 1972; p.102).


Neurotic
Anxiety
Impulsivity
Stable
Introverted
Extraverted
Susceptibility to reward
" " punishment

modification of Eysenck's theory is necessitated. Gray accepts Eysenck's description of the neurotic as being generally more emotional, but, in line with his own definition of emotionality, interprets this as meaning that the neurotic is more sensitive to cues of reinforcement, both classes of reinforcement. It is worth noting in passing that this view has other implications, for example neuroticism will be less closely linked to activity in the ANS, but will reflect the level of activity in the reward and punishment systems, and the thresholds of these systems.

The net result of these proposals is the proposal that Eysenck's two dimensions should be rotated by 45^{o}, as shown in Figure 5:12. The anxiety dimension running from Eysenck's stable-extravert to neurotic introvert quadrant is a dimension of increasing relative, (and absolute), sensitivity to cues of punishment, the impulsivity dimension, running from Eysenck's stable-introvert to neurotic-extravert quadrant is a dimension of increasing relative, (and absolute), sensitivity to cues of reward. This dimension is, of course, quite distinct from, although presumably expected to correlate with, Eysenck's trait of impulsivity which is independent of neuroticism.

Thus the neurotic is sensitive to cues of both classes of reinforcer, while "the extraversion score reflects the relative balance of sensitivity to signals of punishment and reward, respectively: an individual relatively more sensitive to signals of punishment (or non-reward) than to signals of reward (or non-punishment) is introverted; one relatively more sensitive to signals of reward than to signals of punishment is extraverted" (Gray, 1972, p.102).

It is evident that Gray is assumming (1) that for the introvert there is greater activity in the 'stop' system than in the 'approach' system: (2) that this balance is

reversed for the extravert. However, if we re-examine the arguments presented earlier we find that conditions which reduce activity in the 'stop' system increase extraversion, from which it was concluded that the introvert has a relatively higher level activity in the 'stop' system than does the extravert. No evidence is presented to demonstrate that the extravert has a relatively higher level of activity in the 'approach' system either than he has in the 'stop' system or than the introvert has in the approach system.

Even allowing for the fact of mutual inhibition of the two systems there is no necessity to postulate the complete cross over Gray presents us with. Indeed there is virtually an infinite number of relationships between the two dimensions which could be postulated without having to propose this complete cross over. Indeed it would not be too difficult to make out a, speculative, case for it to be advantageous to always have lower threshold and greater activity on the punishment side.

I think it would be fair to say that the jump from the experimental animal data to the original model is somewhat conjectural, but the jump from what human data there is available to the application of the model to human personality is highly speculative.

5:6 SOME OBSERVATIONS

One would like at this stage to review the experimental data collected both specifically to test Gray's hypotheses and from human subjects, but there is, as yet, little such data available. Two experiments have been reported however, (Nicholson and Gray, 1971, 1972,) both dealing with the peak shift and behavioural contrast phenomena, which Gray considers to be frustration effects. In the first of these experiments data from only four children are reported, three of the seven

children tested failing to learn the initial discrimination. While it was found that the children did display the behavioural contrast effect, the evidence for peak shift was unclear, with the children showing the most efficient discrimination showing no evidence of it. No measures of personality are reported in this first study. However in the second report (1972) the Junior EPI was used. The authors express some dissatisfaction with the neuroticism scale of this instrument with the result that they base their conclusions, with respect to neuroticism on teachers' ratings. In support of Gray's contention that both behavioural contrast and peak shift reflect sensitivity to frustration and hence are relevant to Gray's theory they report a correlation between measures of the two phenomena of .66, but this is based on scores of only 10 of the available 14 subjects. The first order-correlations between the measures of personality and measures of behaviour did not yield any significant relationships:

		Behavioural Contrast	Peak Shift
Teachers'	Rating N	0.36	0.37
J.E.P.I.	N	0.31	-0.29
J.E.P.I.	E	-0.31	-0.35

TABLE 5:1 Correlations between measures of personality and Behavioural Contrast + Peak Shift.

Some significant relationships were discovered, however, when a series of partial correlations was calculated. These relationships appear to be stronger for the neuroticism dimension than for the extraversion dimension. However, the authors interpret the results as supporting their hypotheses. In view of the relatively large number of variables collected, the variety of partial correlations performed, the fact that peak shift was calculated in an

original manner, that the data were transformed, that only a small number of subjects was employed (of 17 subjects tested some analyses included data from 14, some from 10 subjects, 3 subjects were dropped from all the main analyses), and also bearing in mind the rather complex nature of the data obtained, a firm conclusion that these data really do support Gray's hypothesis must attend on either a replication of the experiment or further experimentation.

If there are few studies, on the one hand, which have attempted to test hypotheses drawn from Gray's theory then there is, on the other hand, also a paucity of literature attempting a critical evaluation. Passingham (1970) has offered some criticisms of the theory which are worthy of note. He complains that Gray does not offer sufficient anatomical or physiological evidence in form of the various feedback loops he proposes. As to the effect of frontal lesions, which is so important in linking the animal work with human personality dimensions for Gray, Passingham has objected these lesions were obviously not planned to provide data on the effects of such treatments on the extraversion-introversion dimension with the result that the work is unsystematic, methods of lesioning differ widely from study to study and the behavioural tests employed, on the basis of which it is concluded extraversion increases after such lesions, often bear a questionable relationship with Eysenck's extraversion dimension. Similarly, he points out, widely different types of lesions have been studied with the result that it is not clear that whatever behavioural changes do occur after such lesions are due to lesions of the orbital frontal cortex, indeed there is some evidence that dorso and ventro-lateral lesions are more likely to produce perseveration.

He also questions whether, given our present state of ignorance, it is safe to assume that the septo-hippocampal system performs the same function in man as it does in rats. He concludes: "It can only be concluded that, even where the evidence reviewed in this paper provides partial or suggestive support for the theory of Gray (1970), it is by no means conclusive. This review suggests that Gray's (1970) theory as to the neurological basis of introversion-extraversion has not yet been adequately tested or verified". (1970, p.364).

Besides highlighting the problematic nature of the neurological data, at least with reference to humans, Passingham also made another point, closely allied to one made above and that is that despite the fact that according to Gray's theory neurotics ought to be more sensitive to all cues of reinforcement, there does not appear to be any evidence that neurotics are more sensitive to cues of reward. Indeed one might add that this is hardly a trait one would associate with the typical characture of the neurotic.

One would be reluctant to contest Passingham's conclusion; what Gray has done is to assemble a substantial amount of data and to relate these in an original and interesting way. Looked at closely no one area of data presented seems to lead to one unequivocal solution. He relies much more on the weight of evidence. The later parts of the theory, those referring to man, and particularly the proposed modification of Eysenck's theory are of most interest in the present context, but are the most speculative, having the least supportive evidence.

The theory places a strong emphasis on conditioning, both instrumental and classical, and Gray obviously regards it as also providing nomothetic descriptions, hence his explicit comparison of this theory with Eysenck's. For example,

one might be tempted to characterize Gray's introvert as the type of person who is sensitive to social cues, he knows when to say "thank you" and how to dress appropriately, but is normally somewhat retiring and certainly does not search out the limelight. He is afraid of failing. His extravert is poorly socialized, but outgoing and friendly, likes to be the centre of attraction and to succeed. Though these are easily recognizable 'types' are they correctly drawn from Gray's theory? The most obvious prediction drawn from the theory is that the introvert should condition passive avoidance relatively more easily than approach or active avoidance. The bulk of the data, however, on which the model is built is between - group data, while this is a within-individual comparison. This aside, it is evident that much of socialized behaviour, like two 'innocent' examples in the characterization of the introvert, is not passive, but active avoidance, and according to the model it is the extravert who is supposed to display the superiority in this department. Classical conditioning, it is hypothesized, is carried out at the level of the amygdala, a level at which no difference is hypothesized between the introvert and the extravert. The relating of these CSs to instrumental reponses is assummed to be carried out at the level of the reward and punishment mechanisms. Thus introverts and extraverts might be expected to form the CS-UCS(aversive) connection equally well but their talents lie in being able to formulate different types of instrumental responses to cope with the situation, with differential ease.

This does not match our original descriptions of the 'typical' introvert and extravert, it now seems that these must be modified to describing the introvert as a recluse and the extravert as friendly and well socialized. This change does not necessarily cause any great difficulty, indeed

it could be taken as yet another example of looking for patterns of behaviour which do not, in fact, exist in nature.

However if the above analysis is correct and introverts are passive avoiders and extraverts active avoiders what are we to make of Gray's claim that most individuals displaying obsessive-compulsive disorders will be neurotic introverts? Surely these are active avoidance activities. It appears Gray would agree, talking of obsessional hand washing he says: "Should we look for any more recondite explanation of this behaviour than that the patient has acquired, through the vagaries of conditioning and learning an active avoidance response not different in principle from the kind that carries a rat over a hurdle to avoid a potential electric shock? (1971, p.237).

What is to be concluded from this? Part of the problem is that Gray does not adequately distinguish between the classical and instrumental aspects of these situations in relation to his model of personality functioning. He does in his book "Elements of a Two-Process Theory of Learning" (1975a) spend a considerable amount of time discussing the distinction between these two types of conditioning and how they might both be present in avoidance learning, but he does not relate these problems to his model of personality. The answer may well be that when extraverts do condition fear they will learn an active avoidance response relatively more efficiently. The extravert, however, is less likely to condition fear, perhaps because the introvert being relatively more aroused will find more stimuli reaching aversive levels, with the result that he will attempt to escape more often than the extravert employing both active and passive avoidance responses. After all, the difference between the two systems is supposed to be a relative and not an absolute one.

Various other criticisms could be leveled against the theory, but once more the central point of the observations made is that in some situations two apparently contradictory hypotheses may be drawn from the theory. Alternatively results may be explained post hoc as having confirmed, or as being interpretable in terms of some particular aspect of the theory. This is obviously an unsatisfactory state of affairs. In the experiments which follow it will be assumed that as mildly aversive stimuli will be employed, and assumming that the higher arousal of the introvert will facilitate the conditioning of fear it will be the introvert who will learn the avoidance response, both active and passive, more efficiently. On the other hand if a stronger aversive stimulus were used one might predict, as was done earlier that the extravert would condition active while the introvert would condition passive avoidance more efficiently.

The various observations and criticisms of the theory which have been made while outlining it are intended to be cautionary and not the basis for out right rejection. The theory is virtually untested and in consequence it is hardly surprising that it exhibits a certain 'vagueness' and lack of precision in places. On the other hand there is a certain plausibility about it; it is easy to picture some people as interested primarily in rewards and others primarily in avoiding punishment. However, as Mischel remarked with respect to trait theories in general they have a certain plausibility about them, but in practice they have not proved to be useful, so any final evaluation of Gray's theory must wait until hypotheses drawn from it have been tested and the theory as a whole is not so "innocent of contact..... with the harsh world of experimental fact".

CHAPTER VI: EYSENCK, GRAY AND MISCHEL

It was suggested earlier that if Eysenck's theory is to prove itself useful in linking the nomothetic and idiographic approaches in a single theoretical structure, then his introversion-extraversion dimension must be demonstrated to be a general factor of conditionability. This, it was concluded, in Chapter IV, has not yet been done; hypotheses drawn from the theory being subject both to confirmation and frequent disconfirmation. Some of these "disconfirmations" may be challenged on the grounds that they used inappropriate parameters. Gray, however, has gone a step further than reporting disconfirmating data and suggested that Eysenck's I - E dimension cannot be the factor of conditionability, because there are, according to data reviewed by him, two conditionability dimensions; one related to positive reinforcement, the other to negative reinforcement. Unfortunately there is little evidence from studies of conditioning with humans which permits a realistic evaluation of Gray's claims, though there are aspects of the theory he puts forward as an alternative to Eysenck's which are not entirely satisfactory. However, if Gray is correct, in general, if not in detail, it would mean that Eysenck's theory would be unsuitable for the task required of it here. Gray's model may, of course, itself, prove to be an adequate substitute.

There is a certain attraction in bringing together the approach to behavioural organization outlined by Mischel and Bandura with Gray's model of personality. While the two former authors plead for a learning theory approach to

personality, Gray attempts to deduce a model of personality functioning from work on animal learning. Eysenck, by contrasts, starts with a model of personality from which he deduces differences in learning ability. It is true that whereas Mischel and Bandura stop at the stage of describing how learning a behavioural repertoire might take place, and suggesting factors which might influence this learning, Gray couches his model in nomothetic generalizations and suggests that whole groups of people will be expected to behave in certain ways. Nevertheless, the addition of some of Gray's suggestions to Mischel's approach to behavioural organization may be seen as adding both variety and strength.

Mischel has argued that consistency may be explained without recourse to unobservable traits, but simply on the basis of generalization, Gray might provide a clue to what is generalized. If people are differentially sensitive, as he suggests, we might expect individuals to notice certain cues more readily than others and in consequence produce certain classes of responses more often than others. There is no need either to claim individuals are absolutely consistent, or deny that learning constitutes an important determinant of behaviour. Nor does all individuality need to be explained in terms of differential learning history, for two people to behave in exactly the same way they would need to have not only identical learning histories, but also identical nervous systems, or at least functionally identical interactions of these two. To take an example from Mischel (1968), he argues that it is not surprising that a child does not show consistent behaviour if, for example, he is rewarded at school for "being near" and "showing affection" while at home the same behaviours are discouraged. It seems probable that some children will behave "appropriately" and respond differently

at home and at school. However, many children may not, infact, display this differentiation. Often one child may ignore the punishment and persist in demonstrations of affection at home, while another, more sensitive to cues of punishment according to Gray's model, will shy away from demonstrations of affection even though rewarded for them at school.

One might even view Gray as attempting to put forward a model of Bandura's reciprocal determinism. According to Gray's model behaviour produces an effect on the environment and also sets us an expectation in the individual; changes in the environment are seen as modifying expectations and influencing the individual's experiencial state, both of which, of course, will affect his future behaviour - and the situation. One might equally well start from the situation which provides cues, which set up expectations and initiate behaviour. Nor is this approach at odds with Mischel's speculations about strong and weak situations, for the hypothesized differences between individuals is one of relative differential sensitivity, not absolute insensitivity on one side. Thus if, as Mischel claims, as a situation becomes "stronger" more individuals will behave in a similar way, Gray would predict the same but indicate that if the cue is a cue of punishment then those individuals more sensitive to cues of punishment will show the behaviour earlier than those relatively more sensitive to reward. In fact one might extend the point and suggest that one might begin to suspect abnormality if, amongst other things, the level of punishment required to dissuade an individual from persuing a particular course of action has to be uncommonly high, or rewards have to be at an extreme level before he is induced to behave in a particular manner. Gray would predict that these two abnormalities would be most likely to occur

in different individuals

It would seem necessary, however, to turn from speculation for no matter how persuasively these approaches may be fitted together before one can go any further one must have some empirical data on the basis of which these suggestions may be evaluated. One would like to know whether or not introverts do always condition better than extraverts, given the appropriate parameters, or whether the type of reinforcement does determine which group shows superior conditionability; whether individuals show different patterns of behaviour in response to cues of reward and punishment when they are all presented with the same information, or only when they have to learn these relationships themselves. In general one is interested in knowing which of these two theories is correct, when yielding different predictions especially with respect to conditioning, or whether they are both incorrect. It was to provide information relevant to these and related questions that the experiments to be reported here were performed.

CHAPTER VII: EXPERIMENT 1

7:1 ABSTRACT

The purpose of this experiment was to compare the predictive value of Gray's and Eysenck's theory of personality. Predictions were also drawn from Spence's theory. A modified Taffel-type verbal conditioning paradigm was employed.

As no clear conditioning effect was evident it is impossible to draw any firm conclusions with respect to the three theories. The results do, however, provide some support for the predictions drawn from Eysenck's theory. Some of the predictions from Gray's theory with respect to the interaction of extraversion and the type of reinforcement employed also received weak support, but there was no consistent support for hypotheses drawn from this theory.

The LN and HN groups were found to differ significantly in their initial level of responding, this introduced a further complication in attempting to interpret the data. However, contrary to the predictions of both Spence and Gray it was the LN group who appeared to show some increase in the use of the CR with reinforcement. It was suggested that this might be interpreted as supporting Roessler's view of Ego-strength, and that any future replication of the experiment might usefully control for Ego-strength in order that Roessler's predictions might be properly put to the test.

7:2 INTRODUCTION

It has already been pointed out in some detail that the theories of Gray and Eysenck offer different predictions with regard to the relationship between personality and conditionability. It has also been remarked that while on the one hand hypotheses drawn from Eysenck's theory have not received overwhelming confirmation, on the other hand many of the studies purporting to test Eysenck's theory are of questionable methodological validity. The present experiment attempts not only to put Eysenck's predictions, with regard to the relationship between the introversion-extraversion dimension and conditionability to the test once more, but also to compare these predictions with some drawn from Gray's theory.

Verbal Conditioning

(A) Extraversion-Introversion

Eysenck (1959) suggested the possibility of using verbal conditioning as a test of his theory, and went on to report that, as predicted by the theory, the more introverted subjects displayed superior conditionability. He indicated, however, that the results of this experiment need to be treated with some caution as there appeared to be an initial, significant, difference in the preference of the two groups to use the designated CR, verbs of muscular activity, even before reinforcement was introduced.

In two subsequent studies, Johns and Quay (1962) and Quay and Hunt (1965), results were reported which were interpreted as supporting Eysenck's hypothesis. Quay and Hunt (1965) report a correlations of -.25 between conditionability and extraversion as measured by the MPI. Once more, however, the results of the experiment proved difficult to interpret because of a differential rate of emission of the designated

CR prior to reinforcement. Indeed Person and Person (1965) have argued that the results of the Johns and Quay's experiment should be interpreted in the direction opposite to that indicated by those authors. Weight was added to Person and Person's criticism when Bryan and Kapche (1967) failed to replicate Johns and Quay's findings. McDonell and Inglis (1962), employing the MPI, whereas both Johns and Quay, and Bryan had used the Quay-Peterson Inventory (1965), also failed to find any relationship between the introversion-extraversion dimension and conditionability.

Gelfand and Winder (1961) further added to the confusion when they reported that dysthymics displayed superior verbal conditioning to hysterics, which is as Eysenck would predict, but that Guilford's R, used as a measure of extraversion, failed to distinguish between the two clinical groups. In consequence of this latter finding they went on to conclude that:"The findings do not support the relationship between extraversion and resistance to conditionability postulated by Franks (1956) and Eysenck (1955)". (1961, p.689).

Goodstein (1967) also employed Guilford's R and failed to find any relationship between introversion-extraversion and verbal conditioning, as did Das and Mitra (1967) using the MPI, and Languani (1968) using the EPI.

Costello (1967) moved from the usual Taffel type paradigm where the subject is supplied with a verb and series of pronouns and required to respond with a sentence containing the verb and one of the pronouns, and reinforced, usually by the experimenter saying "good", if he uses the designated pronoun(s) usually "I" and "We". Instead Costello attempted to condition the "connotive meaning of words" and reported that introverts did display superior conditionability.

Knowles (1963), however, reported conflicting results,

in one experiment suceeding in conditioning the pronouns "I" - "we", but not "he" - "they", while in the second experiment failing to find any evidence of conditioning of either pair of pronouns. There was no evidence of a relationship between personality and conditioning in either experiment. Beech and Adler's (1963) results are little more comforting for Eysenck. These authors used schizophrenic, neurotic and depressive patients and "normals". All subjects completed the MPI. They report a correlation between introversion-extraversion and verbal conditioning of -.64, but this was true only for the depressive group. Similarly they report a correlation of -.54 between neuroticism and conditioning, but in this case it was true only for the "normal" group.

Eysenck does receive some limited support from Gidwani (1971) who used children as subjects and "smarties" as reinforcers. In the extinction phase of the experiment, for those subjects classified as being unaware of the reinforcement contingencies, the differences in emission of the CR was aspredicted by Eysenck's theory, the extraverted group showing the more rapid decline in its use. Eysenck (1967) reported a study by Jawanda (1966) which he describes as the most extensive and best controlled study to that time. Jawanda employed subjects belonging to three age groups: 21-25, 36-40, 56-60 years, and four personality groups: Neurotic extraverts (HNHE), stable extraverts (LNHE), neurotic introverts (HNLE) and stable introverts (LNLE). Subjects were assigned to personality groups on the basis of their MPI scores. He employed the usual Taffel procedure. For all age groups the LNLE groups were found to produce the largest number of CRs, the HNHE group the least and the other two groups falling midway between those two.

While this study does appear to support Eysenck's

predictions, given the variability of results relating verbal conditioning to the introversion-extraversion dimension it would be fool hardy to place too much importance on this one study alone. However, it must also be remarked that not all of these studies are easy to interpret, in some studies there appeared to be a differential tendency for groups to use the CR before reinforcement began, in others either clinical groups were employed or else psychometric testsother than Eysenck's own were used. Almost all these studies used 100% reinforcement and as pointed out earlier, if parameters appropriate to the theory are not employed it becomes impossible to draw any meaningful conclusions.

If Eysenck's predictions have fared badly, Gray's have fared even less well, for not one study reports superior conditioning by the extravert group although all use positive reinforcement in an operant reinforcement situation.

(B) Anxiety

Turning now to the second personality dimension hypothesized as being of importance in determining conditioning, anxiety, this is of less importance for Eysenck's theory, but, of course, of some importance to both Spence and Gray. Spence, it has often been assummed, would predict that anxiety ought to correlate positively with verbal conditioning. However, it will be recalled from Chapter 4 that he lays some stress on the importance of employing an anxiety provoking situation, and the usual verbal conditioning procedure can hardly be regarded as that. The result of this is that it is not immediately evident whether one ought to predict no systematic relationship between anxiety and verbal conditioning. There is similar confusion over Gray's position, while he would predict that

neurotics, given their greater sensitivity to cues of reinforcement, ought to condition better than stables, it has been reported that anxiety correlates both positively with neuroticism and negatively with extraversion. Thus where a negative reinforcement is employed we can confidently conclude that Gray would predict that the more highly anxious (HA) subject would condition more efficiently than the less anxious (LA) subjects. However most verbal conditioning studies employ a positive reinforcer and so it will depend on the precise composition of the LA group in any particular experiment whether or not the HA group is seen to condition more efficiently. Put another way, to test Grays predictions adequately, independent measures of both extraversion (E) and neuroticism (N) are necessary.

Returning to Jawanda 's study we find him concluding, contrary to Gray's prediction that "subjects scoring low on neuroticism develop verbal conditioning better than their counterparts scoring high on neuroticism" (quoted by Eysenck, 1967, p.125). As already noted the group one would expect to find producing the most CRs, according to Gray's theory the HNHE group, in fact produced the fewest; the LNLE group producing the most.

Few studies, however, have employed measures of both E and N, and ever since Taffel's (1955) original study the Taylor Manifest Anxiety scale (MAS) has been the favoured instrument. Taffel, himself, reported a positive relationship between MAs and verbal conditioning, as did Cambell (1960) and Rosenblum (1960). Buss and Geruoy (1958), however, failed to replicate Taffel's findings and reported results in the opposite direction, with LA subjects showing better conditioning than HA subjects. Jantz (1960) Vidulich (1963) have also reported a negative relationship

between anxiety and verbal conditioning.

Patterson, Helper and Willcott (1960) have complicated the issue somewhat by on the one hand reporting superior conditioning by the LA group but interpreting this as being in line with the Spence-Hull hypothesis. They argue that on the basis of Hull's theory in a simple learning situation, such as the Taffel type verbal conditioning procedure, a higher level of anxiety facilitates conditioning. However, as the response to become conditioned becomes more complex, as in the Kent-Rosanoff word association test which they used, higher levels of anxiety become detrimental with the result that LA subjects should condition more efficiently than HA subjects.

The difficulty in achieving replicable results is underlined by the fact that not only do some studies report a positive relationship while others report a negative relationship between anxiety and verbal condition, but a number of studies have failed to find any relationship at all between these two variables (e.g. Daily, 1953; Zadek, 1959; Maltarazzo et al, 1960; Ebner, 1961; Oakes, 1963).

After reviewing studies which had employed anxiety as a variable Sarason (1960) concluded that it is difficult to achieve any replicable results where anxiety is concerned, and in this respect at least the relationship between anxiety and verbal conditioning appears to be running true to form.

(C) Reinforcement

(i) Type: One of the major distinguishing features of Gray's theory is the importance he places on the type of reinforcement employed and the manner in which this is expected to interact with personality groupings. There do not appear to be any studies in the verbal conditioning literature which

report data which can be used as evidence either for or against this proposition. Several studies have employed either aversive, or compared aversive with appetitive reinforcement (e.g. Buss, Weiner and Buss, 1954; Rotberg, 1959; Das and Mitra, 1962). Das and Mitra (1962) even recorded the subject's E, N, intelligence and rigidity scores; they report that no personality variable was found to relate significantly to conditionability, though they do not report whether there was any tendency towards an interaction between reinforcement type and personality type.

(ii) Schedule: As with the effects of different types of reinforcement there do not appear to be any studies relating personality, verbal conditioning and reinforcement schedule. There are some studies, however, relating the second and third of these variables. Kanfer (1954) using the autokinetic effect compared 100%, 67% and 50% reinforcement schedules of reinforcement; both reinforcement and responses were verbal. He reports that in the acquisition phase partial reinforcement groups required more trials, but fewer reinforcements to reach a criterion level of responding. In the extinction phase those subjects who had been given continuous reinforcement showed greater initial resistance but later showed a more rapid decline in use of the CR than those who had received partial reinforcement

(iii) CS-UCS Interval: Again investigation of this parameter appears to have been neglected with reference to verbal conditioning. Beech and Adler (1963) did have their subjects pause after the first word of the sentence, which had to be a pronoun, and administered reinforcement where appropriate. Though this procedure is more in line with the requirements of Eysenck's theory than the usual method of reinforcing at the end of the sentence, as already reported,

their results were not strongly in Eysenck's favour.

(D) The Pronoun

Though not of immediate theoretical interest to any of the theories under consideration here, the work of Gidwani (1971) strongly suggests that one other variable, the pronoun to be reinforced, must be given some consideration. Gidwani (1971) reports that when she employed the normal method of reinforcing both "I" and "we" only the former was found to increase in frequency. In a subsequent experiment she discovered that even in a non-reinforced control group the frequency of the use of "I" increased across trials. This increase was significant, though the increase in frequency was not as great for this group as for the experimental group. In a further experiment she failed to find any evidence of conditioning of the pronouns "we", "she", "he", "they".

Gidwani is not alone in experiencing this problem. Gelder (1968) reports that when reinforcing both "I" and "we" only the former was found to increase significnalty. Knowles (1963) reported success in conditioning "I/we" but not "he/they", although he failed to replicate these results, failing, in the second experiment, to obtain any evidence of conditioning of either pair of pronouns.

Beech and Adler (1963), on the other hand, do report some success in conditioning the pronoun "we". In this experiment, however, when subjects were unaware of the reinforcement contingencies only the schizophrenic group displayed any evidence of conditioning. Leftwich and Nawas (1969) also report some success, this time in conditioning the pronouns "he/we". Conditioning was only evident, however, for those subjects aware of the reinforcement contingencies.

In view of the strong possibility of obtaining a pseudo-conditioning effect, especially if the pronoun "I" were employed as the CR, it was decided to use three pronouns, randomly across subjects, as the CR in the present experiment. On the basis of data taken from Gidwani (1971) the pronouns "he", "she" and "they" were chosen as these appeard to have approximately equal probability of occurence in a non-reinforced situation.

TABLE 7:1

I	They	She	He	We	You	
4.93	3.83	3.74	3.28	2.81	1.11	Experiment 1
5.25	3.85	3.77	3.52	3.05	.75	Experiment 2

Mean number of times pronoun is used over a block of 20 unreinforced trials (data taken from Gidwani 1971).

It was hoped that by randomly allocating subjects to three groups, each group to have a different pronoun reinforced, it might be possible to avoid spurious effects due to the preference of particular personality groups for particular pronouns.

7:3 HYPOTHESES

The aim of the present experiment is, of course, to compare predictions drawn from several theories as to the differential conditionability of individuals located at different points on generalized personality dimensions thought to be of relevance. According to Eysenck's theory the higher cortical excitation and lower cortical inhibition of the introvert

ought to favour superior conditioning of this group, while the faster development of cortical inhibition in the extravert ought to favour a more rapid extinction effect, given that the appropriate parameters are employed. This then is the first hypothesis:

Hypothesis 1:1 The introverted group will show superior conditioning, while the extraverted group will show a more rapid extinction.

Gray, on the other hand, would not predict a significant extraversion effect, the superior conditioning of the introverts when faced with negative reinforcement and the superior conditioning of the extravert when offered positive reinforcement cancelling each other out. One might predict, instead, on the basis of Gray's theory, that extraversion would interact with type of reinforcement. This then is the second hypothesis:

Hypothesis 1:2 There will be an interaction between extraversion and type of reinforcement due to the superior conditioning of the extraverted group in the reward condition and the introverted group in the punishment condition.

Gray also envisages the neuroticism dimension as a dimension of general sensitivity to cues of reinforcement, which leads to two further predictions:

Hypothesis 1:3 The neurotic group will demonstrate a superior conditionability as compared with the stable group (i.e. low N scorers).

Hypothesis 1:4 There will be an extraversion x neuroticism x type of reinforcement interaction, with the HNHE group employing the CR most in the reward condition and the HNLE group using the non-punished pronoun most in the punishment condition.

It is often assummed that Spence would predict that anxiety ought to be positively related to verbal conditioning, but as already noted, this is not necessarily so, because of the importance he places on anxiety provoking conditions. As the verbal conditioning task cannot be classified as a complex one there does not appear to be any justification for the contention that negative relationship between verbal conditioning and anxiety ought to be regarded as supporting Spence's position. In the present experiment, then, confirmation of hypothesis 3 may be regarded as supporting Spence's theory, though if no significant difference is found between these two groups it cannot be regarded as a "strong" disconfirmation of the theory as no effort will be made to provoke anxiety in the subject.

All these hypotheses rest upon one assumption, that conditioning does indeed take place. As all the theories from which hypotheses have been drawn predict differential conditioning, rather than that one group will condition while another will not, one further hypothesis may be added:

Hypothesis 1:5 There will be a significant trials effect and a significant linear trend over trials. It is to be expected this effect will be evident for each of the other main effects and interactions which have been indicated as being of theoretical significance.

7:4 METHOD

Design

The independent variables in this experiment were: Introversion-extraversion, neuroticism-stability, reward-punishment and seven blocks of twenty trials, yielding a 2 x 2 x 2 x 7 design with repeated measures in one variable Reinforcement: In the "positive", or reward, condition the

experimenter said : "mmmh" (a noise of approval), accompanied by a slight nod of the head after each response which was to be rewarded. In the "negative", or punishment, condition reinforcement consisted of the experimenter saying: "tut" (a noise of disapproval), accompanied by a slight shake of the head.

Schedule: A fixed ratio 50% reinforcement schedule was employed, that is, every other time the CR was used it was reinforced as was appropriate to the condition.

Trials: Each subject was presented with 140 cards sequentially. He/she was required to compose a phrase in response to each card. Trials were divided into blocks of twenty, with the first block serving to establish the operant level of responding, and so going unreinforced. The next four blocks (blocks 2 - 5) were reinforced, while the final two blocks (blocks 6 & 7) were again unreinforced and served as an extinction phase. There was no pause between blocks of trials and so the subject was unaware of this division.

The CR: The response to be reinforced was a pronoun. Three pronouns, "he", "she" and "they" were used randomly across subjects, of course any one subject had only one pronoun reinforced.

Task: The subject was presented with a card and required to respond with a two word phrase in the form pronoun-verb. Each card had printed on it the verb to be used in the response, and three personal pronouns, of which either the first or third had to be used as part of the response, i.e. the second, or middle pronoun was not to be used as part of the response.

Materials: Six sets of 6" x 4" white cards, 140 cards to a set were prepared. Each card had a verb in the centre and three pronouns underneath the verb. The verbs were neutrally

toned past tense verbs taken from Dixon and Dixon (1964) and Binder, McConnell and Sjoholm (1957). Responses were recorded on a prepared score sheet.

Personality measurement: Extraversion and neuroticism were defined by Eysenck and Eysenck's (1964) Eysenck Personality Inventory (EPI). Subjects scoring 11 or over on the E scale were classified as extraverts and those scoring 10 or less as introverts. Similarly, subjects scoring 11 or over on the N scale were classified as neurotics for the purposes of this experiment and those scoring 10 or less as stables.

Procedure

60 subjects were tested as part of this experiment, but 13 were subsequently discarded when it was discovered that either they had scored 5 or more on the EPI lie scale, or that English was not their first language. Two further subjects were dropped as they knew of the Taffel experiment and so were not naive as to the purpose of the experiment. It was then decided to drop a further five records in order to create equal cell frequencies. Records to be dropped were chosen in a random manner, with the constraint that equal cell frequencies ensued.

Subjects were volunteers, university students, 16 males and 24 females, all with English as their first language, They were randomly assigned to either the reward or punishment conditions. For each subject one of the six available sets of cards was chosen,again at random, and finally one pronoun was chosen to be reinforced.

Each subject completed the EPI and immediately on finishing this was instructed as follows:

"There are 141 cards here. Each card is of this format (a sample card was shown on which there appeared the verb "thanked", plus the pronouns to be used in the experiment).

There will be a verb in block capitals at the top and three personal pronouns underneath. What I want you to do is to make a phrase using only the verb and either the pronoun in the first or third position (these were indicated while speaking). So it will be a two word phrase in the form pronoun-verb. For example if you were given this card you could say either..... (the two possible phrases were given). Do you understand?"

Each card contained a different verb (cf. appendix for list of verbs used). The pack of cards was placed in front of the subject and one card was handed to him/her at approximately four second intervals. The whole task took approximately 10 minutes. Each response was recorded on a prepared scoring sheet before the next card was handed to the subject.

The experimenter sat facing the subject, but the subject was unable to see the scoring sheet. The experimenter kept his eyes fixed on the scoring sheet or the cards in order to restrict communication to that required by the experiment i.e. the appropriate noises and gestures. After the last card had been presented to the subject the experimenter said: "I would like to ask you some questions now." He then proceeded to ask a series of questions taken from Gidwani (1971) designed to assess the subject's awareness of the experimental contingencies. (Cf. appendix for questions).

Six packs of cards were available to the experimenter. Each pack contained all 140 verbs, but there are six possible combinations of the three pronouns. These were organized so as to give three choice situations: "he" vs "they," "he vs "she," "she" vs "they". For each of these situations there were two packs available, each pack made up of 50% of the cards with e.g. "he" in the first position and "they" in the

third position and the converse for the other 50% of cards. The second pack was the complement of this. Within a pack cards were shuffled so as to yield equal probability of either pronoun occuring in the first position on any card. Thus for each subject the experimenter chose first one of the six packs and then one of the two pronouns at random.

Though the subject completed the EPI before the conditioning phase of the experiment began, these were not scored until sometime later, and so the experimenter was unaware during conditioning of the personality group to which the subject would be assigned.

7:4 Results

The mean scores on the E and N scales of the present sample are comparable to those reported by Eysenck and Eysenck (1964); here the mean E score was 11.1, and N 10.6, as compared to values of 11.095 and 10.006 reported by Eysenck.

In order that data from the reward and punishment conditions might be statistically comparable a subject's score in the reward condition was simply a count of the number of times he/she used the reinforced pronoun in a block of 20 consecutive trials. In the punishment condition the subject's (S) score was a count of the number of times the unreinforced pronoun was used. That is, in both conditions it was the number of times the pronoun, which was expected to increase in frequency, was used which provided the data for the analysis. It was in order to be able to analyse the data in this way that the subject was given the choice of only two pronouns, rather than three or more as is usual.

Conditioning

As the experiment was designed to investigate the

TABLE 7:2. ANALYSIS OF VARIANCE USING RAW DATA

Source	SS	DF	MS	F	Sig.
Between					
Type of Reinforcement (R)	70.805	1	70.805	3.583	N.S.
Extraversion (E)	61.605	1	61.605	3.118	"
Neuroticism (N)	117.045	1	117.045	5.923	$p < .05$
R x E	0.605	1	0.605	< 1	N.S.
R x N	4.805	1	4.805	< 1	"
E x N	0.245	1	0.245	< 1	"
R x E x N	0.125	1	0.125	< 1	"
Residual	632.32	32	22.758		
Within					
Trials (T)	9.28	4	2.32	1.184	"
Linear trend	2.89	1	2.89	1.474	"
T x R	1.92	4	0.48	< 1	"
T x E	0.12	4	0.03	< 1	"
T x N	15.68	4	3.92	2.0	"
T x R x E	8.92	4	2.23	1.138	"
T x R x N	8.92	4	2.23	1.138	"
T x E x N	4.08	4	1.02	< 1	"
T x R x E x N	15.8	4	2.015	2.015	"
Residual	250.88	128	1.96		
Total	1203.155	199			

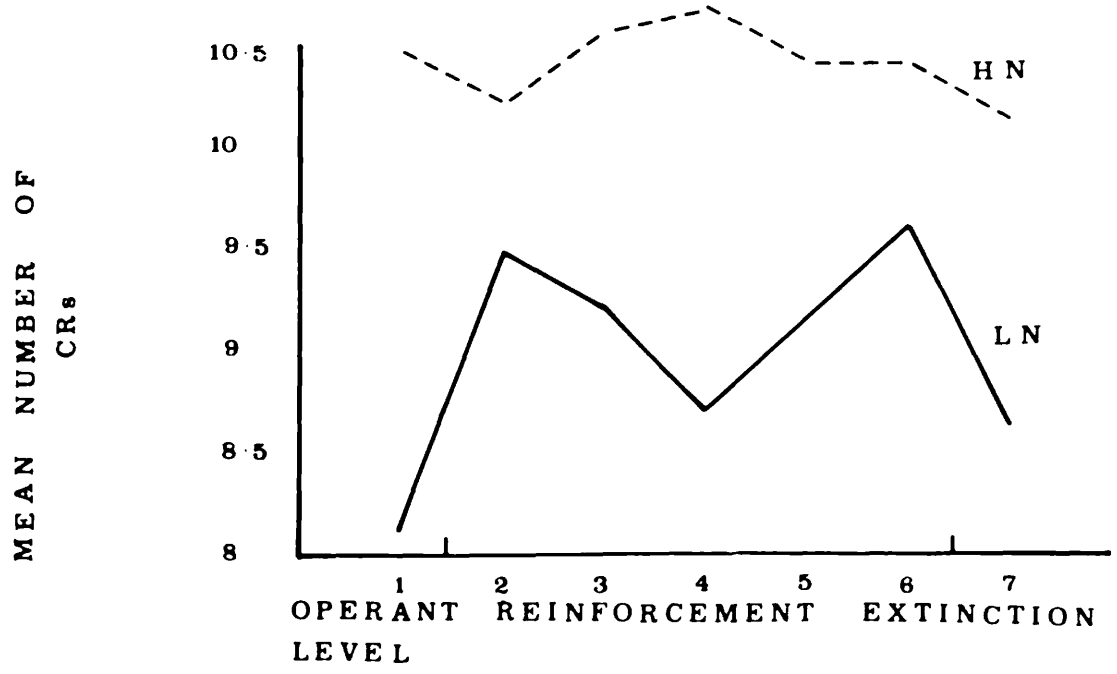


FIG.7:1 COMPARISON OF LN AND HN GROUPS DURING VERBAL CONDITIONING

(RAW DATA)

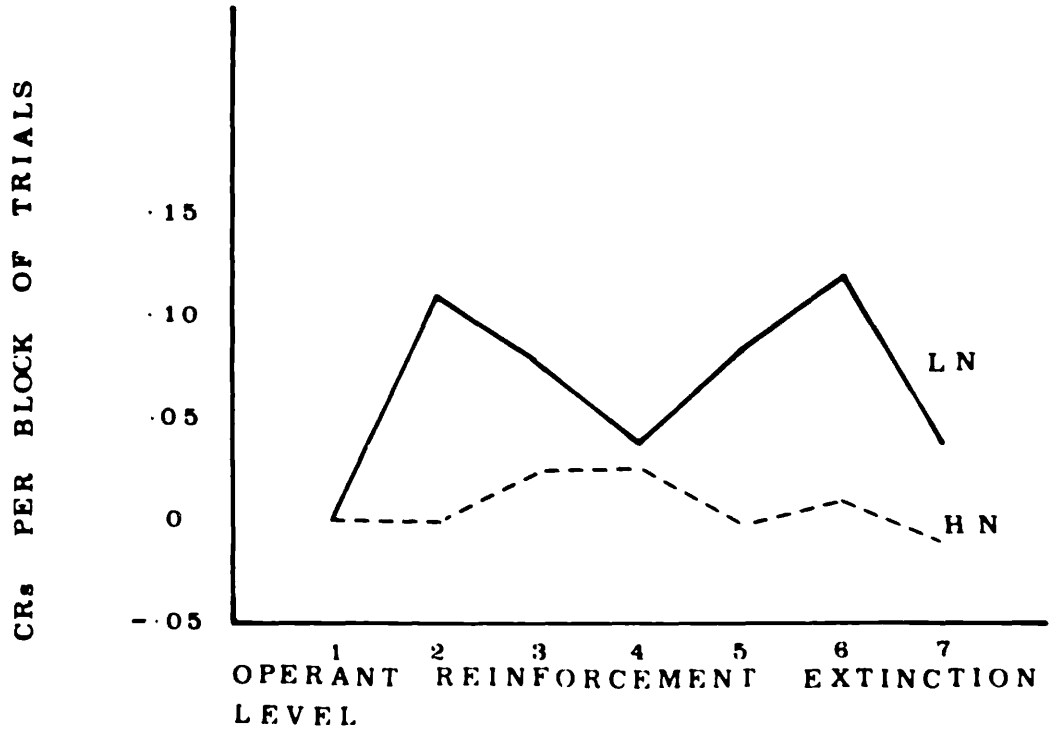


FIG.7:2 COMPARISON OF LN AND HN GROUP DURING VERBAL CONDITIONING

(CORRECTED DATA)

relationship between personality and conditioning it is of more than passing interest to know whether or not conditioning did take place over the sample as a whole. Hypothesis 1:5 predicted that there would be a trials effect, and a significant linear trend across trials, but as can be seen from Table 7:2, which presents the results of the analysis of variance of data drawn from the operant and reinforcement phases of the experiment, this hypothesis has not been confirmed. Though this result constitutes something of a set back it need not be totally damning. The various theorists considered have predicted differential conditionability of various personality groups and so it is possible that a conditioning effect will be evident in at least one of these.

Anxiety

Hypothesis 1:3 stated that HN subjects would show superior conditioning as compared to LN Ss. It can be seen from Table 7:2 that there is a significant difference between these two groups. However Fig. 7:1 suggests that even here all is not well. The HN group uses the CR more than the LN group in each block of trials, but there is no evidence of the increase in the frequency of the CR across trials one might expect if conditioning were taking place. We appear to have come across the same problem as that reported by both Eysenck (1959) and Johns and Quay (1962) with one group using the CR significantly more than the other even before reinforcement is introduced.

With respect to the Johns and Quay's experiment Person and Person (1965) have suggested that a ceiling effect might be obscuring the conditioning effect. Unfortunately they do not suggest a suitable method of correcting for this when faced with this type of data. In an attempt, therefore, to reduce the influence of any ceiling effects, scores deviating

TABLE 7:3: ANALYSIS OF VARIANCE USING CORRECTED DATA

Source	SS	DF	MS	F	Sig.
Between					
Type of reinforcement (R)	0.00146	1	0.00146	<1	N.S.
Extraversion (E)	0.00925	1	0.00925	<1	"
Neuroticism (N)	0.14258	1	0.14258	2.049	"
R x E	0.00106	1	0.00106	<1	"
R x N	0.00205	1	0.00205	<1	"
E x N	0.02205	1	0.02205	<1	"
R x E x N	0.03809	1	0.03809	<1	"
Residual	2.22668	32	0.06958		
Within					
Trials (T)	0.07282	4	0.01821	1.073	N.S.
Linear trend	0.01221	1	0.01221	<1	"
T x R	0.01403	4	0.00351	<1	"
T x E	0.00332	4	0.00085	<1	"
T x N	0.08858	4	0.02214	1.305	"
T x R x E	0.05518	4	0.01379	<1	"
T x R x N	0.07992	4	0.01998	1.177	"
T x E x N	0.04551	4	0.01138	<1	"
T x R x E x N	0.14577	4	0.03644	2.147	"
Residual	2.17244	128	0.01697		
Total	5.12077	199			

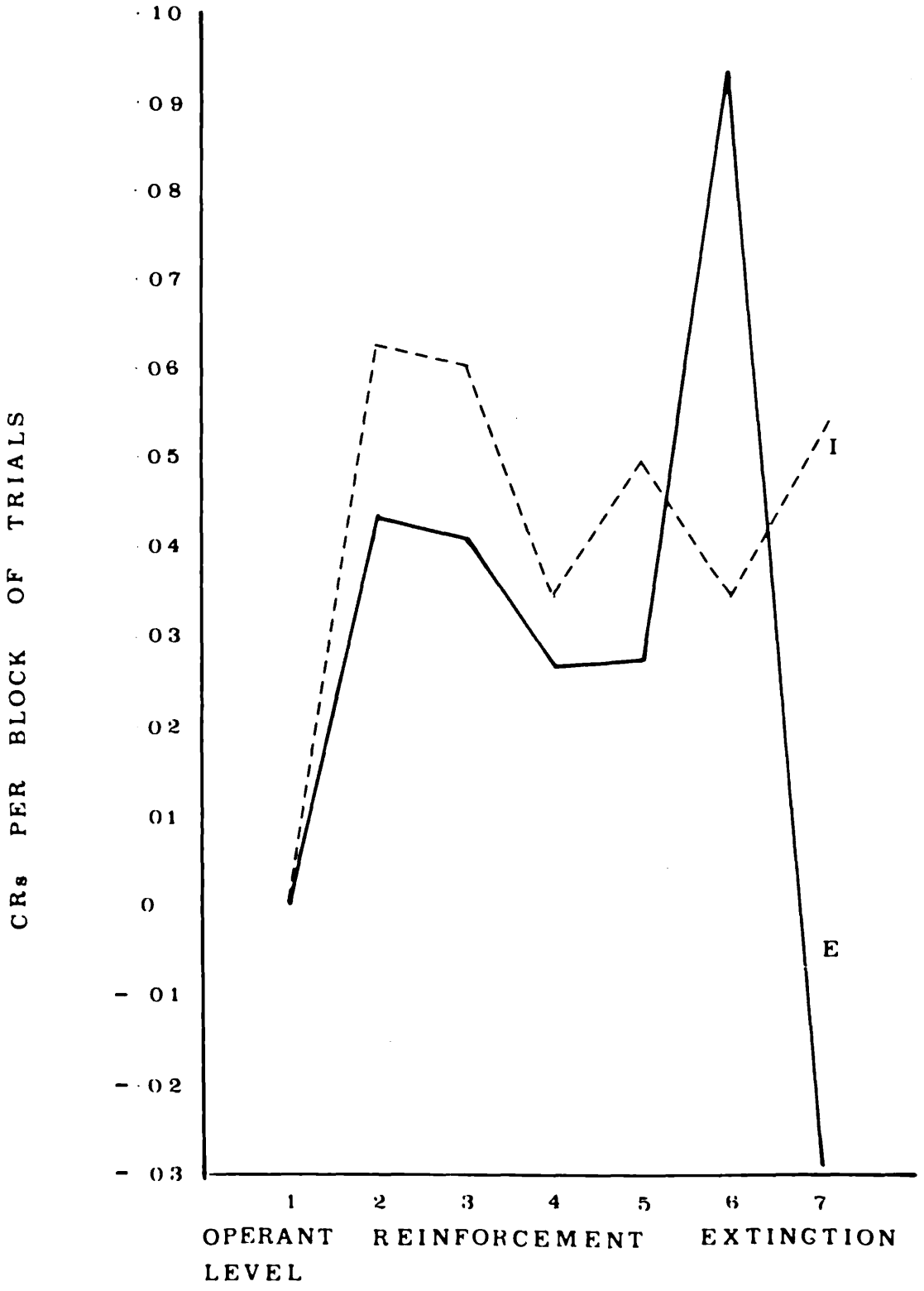


FIG. 7.3 COMPARISON OF INTROVERTS AND EXTRAVERTS DURING VERBAL CONDITIONING (CORRECTED DATA)

from the operant level of responding in a positive direction were transformed according to the following formula:

$$\frac{Xr - Xo}{Xmax - Xo}$$

Scores deviating from the operant level in a negative direction were transformed according to the formula:

$$\frac{Xr - Xo}{Xo}$$

(Where Xo = operant level of responding; Xr = the number of times the CR is employed in a subsequent block of trials; Xmax = the maximum number of times the CR could be used in a block of trials (here 20)).

Scores corrected in this way provided the data for another analysis of variance, the results of which are summarized in Table 7:3. It can be seen that the neuroticism effect is no longer significant, nor is the neuroticism by trials interaction. In fact, when the graphs for the stable and neurotic groups are compared (cf Fig. 7:2) it seems to be the LN group who show some improvement, rather than the HN group. The HN group initially showing a slight decrease in the use of the CR. This negative relationship between frequency of using the CR and N cannot be held to support the models of either Spence or Gray, though it does conform to the pattern of results reported by Jawanda (1966).

Eysenck - Extraversion

Hypothesis 1:1 predicted that it would be the introverted group who would show superior conditioning, however, as can be seen from Table 7:3 neither the extraversion main effect, nor the extraversion x trials interaction is found to be significant. Figure 7:3 indicates that at least the results are in the direction predicted by Eysenck, with the

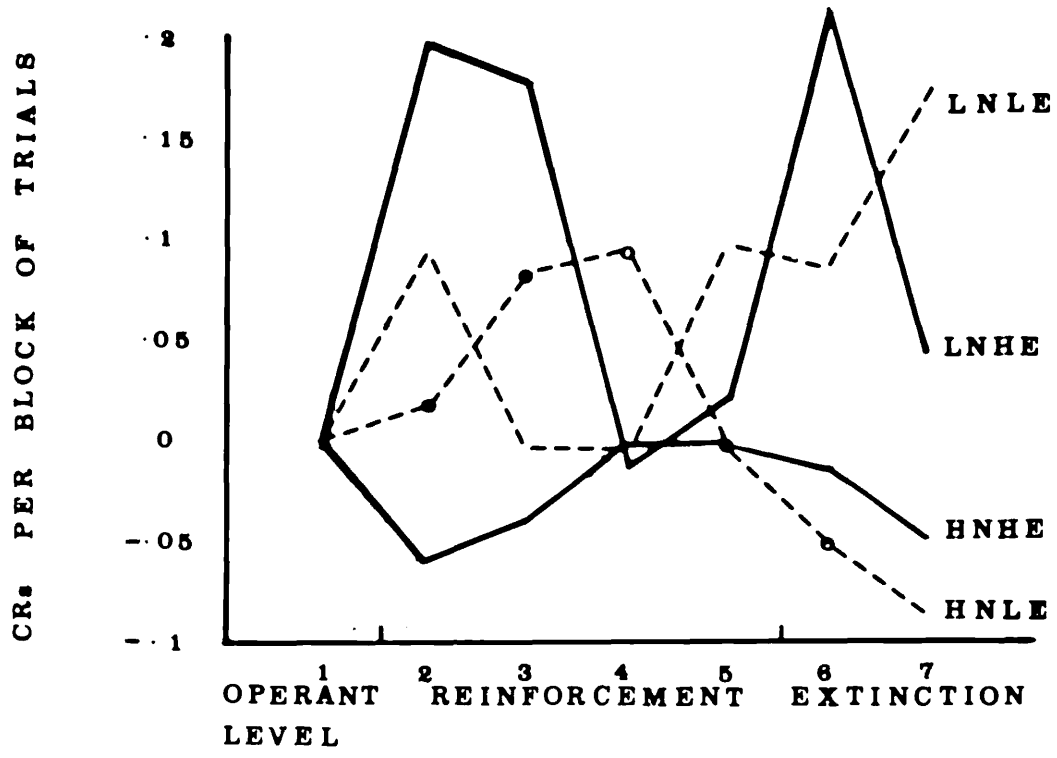


FIG. 7:4 VERBAL CONDITIONING WITH POSITIVE REINFORCEMENT AS A FUNCTION OF EXTRAVERSION AND NEUROTICISM (CORRECTED DATA)

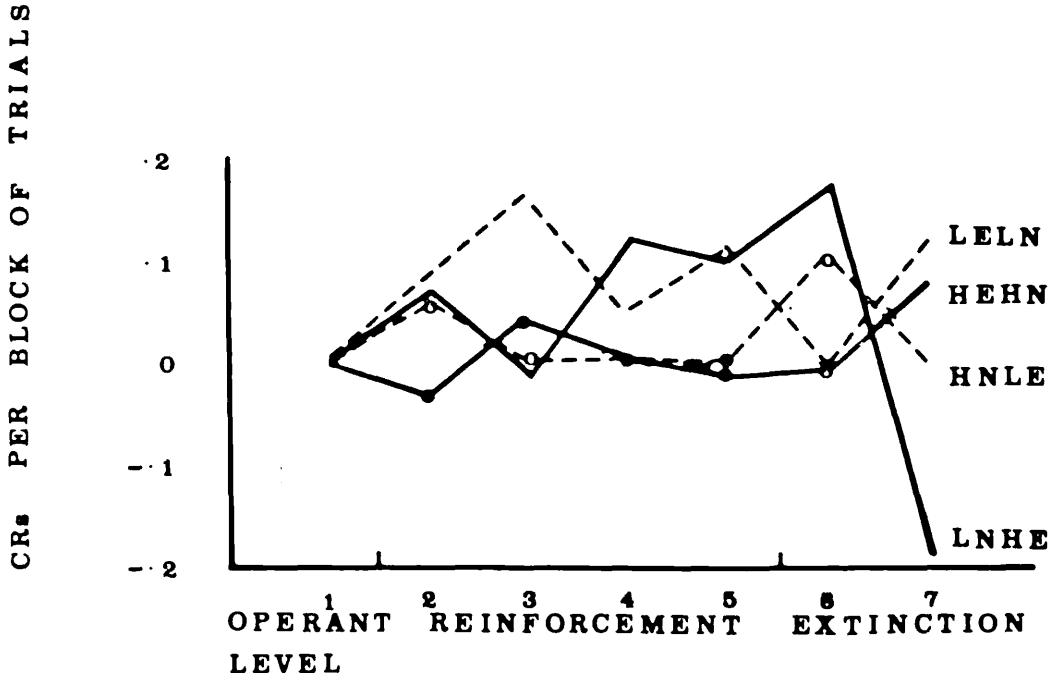


FIG. 7:5 VERBAL CONDITIONING WITH NEGATIVE REINFORCEMENT AS A FUNCTION OF EXTRAVERSION AND NEUROTICISM (CORRECTED DATA)

introverted group employing the CR more than the extraverted group on each block of reinforced trails. It is also the introverted group who show the greater initial increase when reinforcement is introduced, but this increase is not significant.

Eysenck's theory also predicts that the extraverted group ought to show a more rapid extinction, again the results are in the direction predicted. Too much cannot be made of this finding, however, because, as can be seen from figure 7:3 the extraverted group actually shows an increase in the use of the CR in the first extinction block of trials; the effect does not reach the arbitrary level of statistical significance. Most importantly, however, one cannot speak of an extinction effect with any conviction where no conditioning effect has been clearly demonstrated.

Gray - Extraversion, Nueroticism and Reinforcement

It has already been remarked that Gray's predictions with regard to the neuroticism dimension have not been supported, once the data have been corrected for initial difference between the groups in responding, and as can be seen from table 7:3 his predictions with regard to the interactions between extraversion, neuroticism and the type of reinforcement employed have fared no better. In line with the predictions of the theory the extraverted group do show the greatest increase in using the CR in reward condition, while it is the introverted group who show the greatest increase during reinforcement in the punishment (cf. Figs, 7:4, 7:5). Even here, however, as though pouring salt into the wound, the results are not unequivocally in the direction predicted by Gray.

It is the LN groups which show the improvement and

TABLE 7:4 ANALYSIS OF VARIANCE INCLUDING DATA FROM BOTH ACQUISITION AND EXTINCTION PHASES. (Uncorrected data)

Source	SS	DF	MS	F	Sig.
Between					
Type of Reinforcement (R)	100.800	1	100.800	4.043	$p < .1$
Extraversion (E)	84.700	1	84.700	3.397	$p < .1$
Neuroticism (N)	142.857	1	142.857	5.730	$p < .05$
R x E	2.057	1	2.057	< 1	N.S.
R x N	0.014	1	0.014	< 1	N.S.
E x N	0.229	1	0.229	< 1	"
R x E x N	0.014	1	0.014	< 1	"
Residual	797.771	32	24.930		
Within					
Trials (T)	15.936	6	2.656	1.216	N.S.
Linear trend	0.700	1	0.700	< 1	"
T x R	1.95	6	0.325	< 1	"
T x E	12.150	6	2.025	< 1	"
T x N	20.293	6	3.382	1.548	"
T x R x E	9.563	6	1.599	< 1	"
T x R x N	28.336	6	4.723	2.162	$p < .05$
T x E x N	39.421	6	6.570	3.008	$p < .01$
T x R x E x N	22.036	6	3.673	1.681	N.S.
Residual	419.429	192	2.185		
Total	1697.586	279			

TABLE 7:5: ANALYSIS OF VARIANCE INCLUDING DATA FROM BOTH ACQUISTION AND EXTINCTION PHASES. (Corrected data)

Source	SS	DF	MS	F	Sig.
Between					
Type of Reinforcement (R)	0.00054	1	0.00054	<1	N.S.
Extraversion (E)	0.01170	1	0.01170	<1	"
Neuroticism (N)	0.26476	1	0.26476	2.863	"
R x E	0.00903	1	0.00903	<1	N.S.
R x N	0.03960	1	0.03960	<1	"
E x N	0.00506	1	0.00506	<1	"
R x E x N	0.02942	1	0.02942	<1	"
Residual	2.95901	32	0.09247		
Within					
Trials (T)	0.12724	6	0.02121	1.106	N.S.
Linear trend	0.00255	1	0.00255	<1	"
T x R	0.01861	6	0.00310	<1	"
T x E	0.10011	6	0.01668	<1	"
T x N	0.11809	6	0.01968	1.027	"
T x R x E	0.06409	6	0.01068	<1	"
T x R x N	0.25219	6	0.04203	2.192	$p < .05$
T x E x N	0.34433	6	0.05739	2.993	$p < .01$
T x R x E x N	0.20844	6	0.03474	1.583	N.S.
Residual	3.68115	192	0.01917		
Total	8.23337	279			

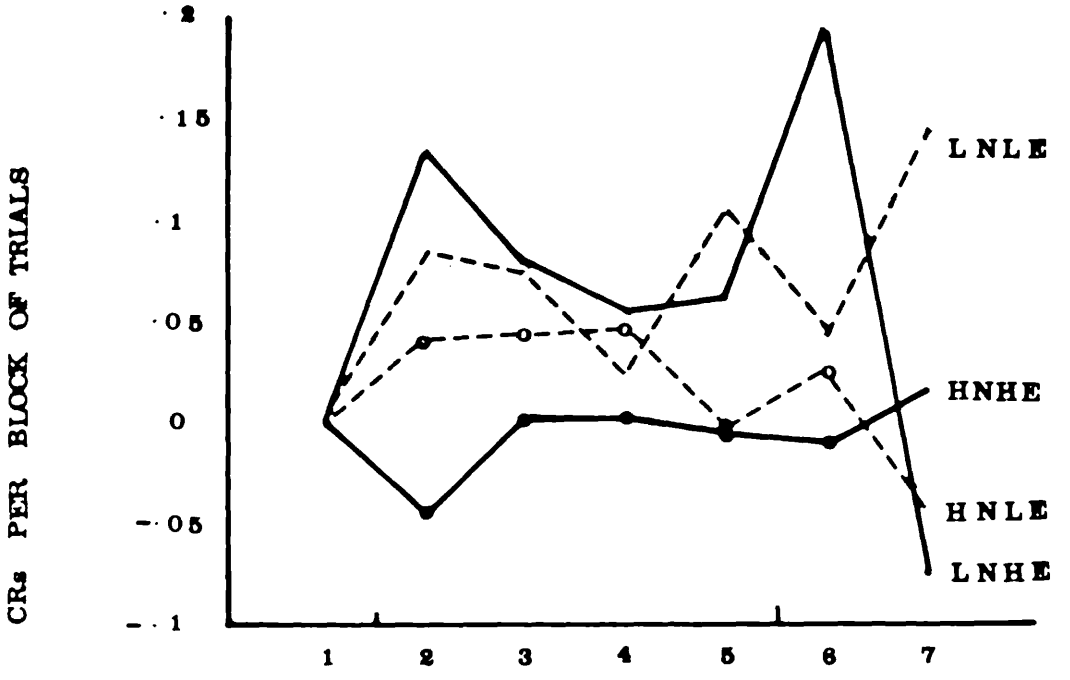


FIG. 7:6 VERBAL CONDITIONING AS A FUNCTION OF EXTRAVERSION AND NEUROTICISM (CORRECTED DATA)

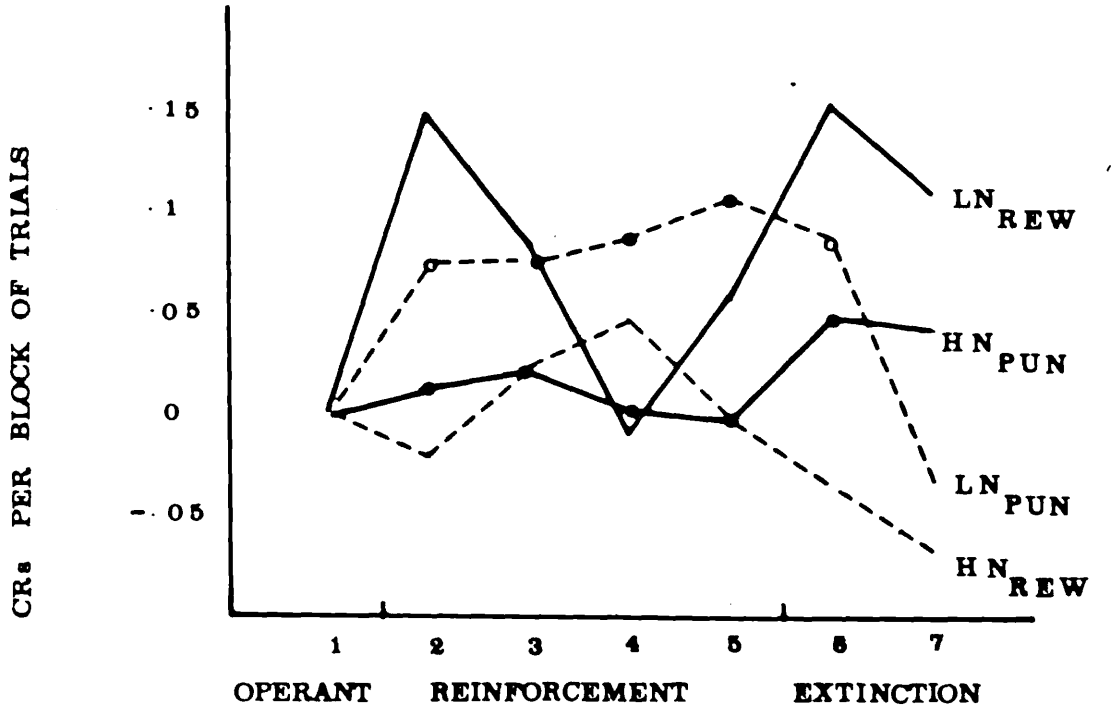


FIG. 7:7 VERBAL CONDITIONING AS A FUNCTION OF NEUROTICISM AND TYPE OF REINFORCEMENT (CORRECTED DATA

and not the HN groups as might be expected from the theory. Indeed the HNHE group shows no evidence of conditioning in either condition.

The same situation exists in the extinction phase with some trends interpretable as being in the direction predicted by Gray, e.g. the dramatic switch, by the LNHE group, to the previously punished pronoun (non punishment being equal to reward), but the results lack an over-all consistency which might persuade one that the predictions of the theory had been, to some degree, supported.

When the data from both the acquisition and extinction blocks are included in the same analysis two interactions do emerge as being significant (cf Tables 7:4 and 7:5), the neuroticism x type of reinforcement x trials interaction and the extraversion x neuroticism x trials interaction. As neither of these is significant when the acquisition data alone is analysed these results must be due either to a contrast between the acquisition and extinction phases, or because of differential behaviour of the groups in the extinction phase. The T x E x N interaction seems to be largely due to the dramatic change in behaviour of the LNHE group in the extinction phase, this group showing a marked decrease in the use of the CR while the LNLE group displayed an increase in use of the CR over the two extinction blocks (cf. Fig 7:6). However, even this finding can be "explained away" when it is noted that the dramatic change of the LNHE group is due to a substantial increase in the use of the CR in the first extinction period in the reward condition, and a decrease in the second extinction period of the punishment condition. (cf Figs 7:4 and 7:5)

The T x R x N interaction appears to be due to differential behaviour of the groups in the acquisition and

extinction phases. As can be seen from Figure 7:7 the LN group in the reward condition actually increases its output of the CR in the first extinction block and then decreases it in the second. It is almost as though they were searching for the withdrawn reinforcement when it is first withdrawn, and then, on realizing it is being withheld returning to something like their operant level of responding.

It the punishment condition they behave quite differently, after showing a steady improvement throughout the acquisition trials they display a tentative decrease in the first extinction block and then on discovering that the punishment has ceased they slump back to their operant level.

The LN group show a substantial increase in both conditions while the HN group display no evidence of conditioning in either condition. In the punishment extinction period they show a slight increase, while in the reward ondition they show a slight decrease.

Several X^2 (Chi squares) were calculated. There appears to be no significant difference in the frequency with which any of the three pronouns was designated as the CR for either the HN - LN groups (X^2 = .72) or the HE - LE groups (X^2 = 1.3). Nor can the differences in performance of the HN - LN groups be attributed to the fact that one group was more "aware" than the other, at least as indicated by Gidwani's technique. Only five subjects were able to state that a particular pronoun was being reinforced. 17 Ss remarked that the experimenter was doing anything at all other than handing over cards and ticking the record sheet. There was no significant difference even in this limited "awareness" between the HN and LN groups.

7:6 DISCUSSION

The purpose of the present experiment was to compare the differential conditionability of various personality groups. In view of the fact that there was no clear conditioning effect the relevance of the present results to the various theories under consideration is questionable. Why no conditioning effect is evident is not immediately obvious considering that such a well tried technique was employed. One possibility, however, which does suggest itself is that the pronouns "he", "she" and "they" were employed as opposed to the more usual "I" and "we". As reported in the introduction Gidwani (1971), Gelder (1968), Knowles (1963), Beech and Adler (1963) and Nawas (1969) have all experienced difficulties in conditioning pronouns other than "I". These findings, together with the results of the present experiment and Gidwani's finding that the frequency of use of the pronoun "I" increases over blocks of trials whether or not it is reinforced, induce a sense of disquiet. Why should only one pronoun be conditionable? Are verbal conditioning studies really demonstrating conditioning?

Another finding which advises caution in attempting to interpret the results of the present experiment is that the two neuroticism groups were found to differ significantly in the use of the pronoun designated as the CR even before reinforcement was introduced. If the results are accepted at face value then it must be reported that the HN group used the CR more than the LN group in each block of reinforced trials. This finding is consistant with the predictions drawn from the theories of Spence and Gray. The data represented in Fig. 7:1 do not suggest, however, that there is any increase in the frequency of the CR over trials, for the HN group, as one would expect if conditioning

really were taking place. On the other hand if it is argued that the conditioning effect cannot be deciphered because of a ceiling effect and the data are "corrected" to take account of this then it appears to be the LN group who display the superior performance. This is contrary to the predictions of Spence and Gray, but, one might argue, support the contentions of Roessler (1963, 1965, 1966, 1967, 1970, 1973).

Roessler regards adjustment as a process of "reality testing", or "the ability to accurately appraise the nature and intensity of stimuli", combined with appropriate responding. HN, or more accurately, as Roessler prefers to use Ego-Strength (Es), low Es, individuals are less likely than high Es individuals to display this accuracy of perception and appropriate responding. Roessler (1972) reports a correlation between Es and the EPI N scale of -.72 (p .001), and of -.76 ($p < .001$) between Es and the MAS and so it might be suggested that the differential performance of the N groups in the present experiment reflects the more appropriate responding of the high Es individuals reported by Roessler. One might even interpret the significant T x R x N interaction as supporting Roessler's thesis. This interaction is due largely to the differential behaviour of the LN group in the reward and punishment conditions which might be regarded as reflecting a more accurate perception of reality by this group.

These suggestions are, of course, post hoc, and given the ambiguous state of the data, the dearth of significant results, and the fact that N and not Es was recorded, must be treated with a healthy caution, but at least they do suggest that hypotheses drawn from Roessler's theory might, fruitfully, be taken into account in future experiments.

Returning from speculation about theories which might fit the data to ones, consideration of which, generated the data, it is difficult to provide a convincing case claiming that any support has been found for the theories of Gray and Spence. Although the behaviour of some groups occasionally shows the trends predicted by Gray, an overall consistency in the results which might persuade one that there was some substance to the theory, despite the non-significant results, is missing. For example, the prediction of differential behaviour is more clear cut with respect to the reward than the punishment condition, and in accord with the theory it is the extraverted group who display the greater improvement, but this is true only for the LN group, the HNHE group, who should display the greatest improvement, shows an initial decrease in use of the CR and never on any block of trials surpasses its operant level of responding.

Nor does the theory explain why when positive reinforcement is withheld (extinction), a procedure which according to the theory is equivalent to punishment, the LNLE group, a group hypothesized as being differentially more sensitive to cues of punishment, should show an increase in use of the CR. (cf. Fig. 7:4).

Eysenck's theory on the other hand emerges from the experiment in a much more favourable light, despite the fact that the differences between the E and I groups is never significant it can be seen from Fig. 7:3 that the results are in the direction predicted by Eysenck, the Is always using the CR more during reinforcement, and the Es showing a steeper extinction.

Since this experiment was completed two other experiments have been reported, Gupta (1976) and Gupta and Nagpal (1978), both using the Taffel procedure to test

hypotheses drawn from Gray's theory. In both experiments they predict that extraverts, or Ss designated as impulsive or social, would display superior conditioning in the reward condition and introverts, or the less impulsive and less social Ss, would show superior conditioning in the punishment conditioning. In the first experiment four reinforcers were used: the word "good" and a buzzer, both designated as positive reinforcers, and the word "bad" and an electric shock, both designated as negative reinforcers. In the second experiment only three reinforcers were used: "good", buzzer and the electric shock.

In the first experiment all Ss were male while in the second all were female. Five pronouns: "he", "you","they", "I" and "we" were used. "I" and "we" were reinforced.. A 100% reinforcement schedule was used and reinforcement was adminstered at the end of the sentence, so the CS - UCS interval was long as compared to the present experiment. In both experiments the results are interpreted as supporting Gray's theory. There are, however, some features of the experiments which suggest that one ought to be cautious, in interpreting the results. From the descriptions of the methodology in both experiments it appears that data have been used in the analyses which are not, strictly speaking, statistically comparable. When a positive reinforcer was used the frequency of the CR increased, while when a negative reinforcer was employed this frequency decreased. If one were interested in demonstrating this one might include all scores in the analysis with the appropriate sign. However, when one is interested in the degree of conditioning, how much behaviour changes due to reinforcement, the magnitude of change is of primary importance, not the direction. With this in mind it is unclear whether or not the Ex type of

reinforcement interaction would have been significant if only the magnitude of change in the frequency of the CR had been considered in the analyses of data from these experiments. Although it must be noted that the type of reinforcement main effect was not significant. When one considers the reported results it is found that the extraverted group displays the greatest change in behaviour in both conditions, and that an extraverted group with a male experimenter displays the least change in behaviour in the reward condition, and an introverted group with a male experimenter the least conditioning in the punishment condition i.e. these results are in the direction opposite to those predicted. With the female experimenter the results reported are in the predicted direction with extraverts conditioning more efficiently than introverts in the reward condition, and introverts more efficient conditioning than extraverts in the punishment condition. Gupta suggests that the differential effect produced by the sex of the experimenter might be due to the fact that approval from a young woman is more reinforcing for a young man than is approval from another man. While this may be true, it does not explain why introverts display superior conditioning to extraverts in the reward condition when the experimenter is a male, and why the extravert displays superior conditionability in the punishment condition.

In the second experiment, though the same objection may be put with regard to the method of statistical analysis adopted, the results do appear to be in the direction predicted by Gray. In this experiment the sex of the Ss is reported, female, but not of the experimenter. One might point to the fact that the parameters specified by Eysenck as being relevant were not employed here, or to the fact that the

pronouns "I" and "we" were used as the CR, data were produced only for the two pronouns jointly, not for each separately, and some reservation has already been expressed with regard to the use of the pronoun "I" in this context, but it seems unlikely that either of these factors is totally responsible on its own for the results that have emerged. While on one hand it must be acknowledged that some support for the Gray model does emerge from the Gupta studies, even there the results are inconsistent, results sometimes being reported in the direction opposite to those predicted by the theory. It does not bode well for the theory when such variable results are reported.

With respect to the present experiment one might complain that with only two pronouns available, and being restricted to two word phrases, the S found the task boring and so rather than constantly repeating one pronoun he/she changed from time to time, these fluctuations obscuring any conditioning effect which may have been present.

An alternative explanation of the results, it might be argued, is that they support the contention that no conditioning can take place in humans unless the S is aware of the reinforcement contingencies obtaining at the time. In the present experiment it appears that Ss were not aware of the reinforcement contingencies.

These, however, are untested hypotheses and further data would be needed before they could be accepted or rejected. In a similar vein, one might consider whether or not the reinforcements necessarily belong to the categories to which they were allotted, or whether there was any consistency across subjects in this respect. One S when asked if she had noticed the experimenter doing anything during the experiment replied that he had been making "doctor-like noises", which she

found quite the opposite of rewarding. This Ss belonged to the HNLE group, the group Gray assumes to be differentially most sensitive to cues of punishment. One might speculate that this group found any observation of their behaviour unpleasant. If this were true one would expect them to condition especially well in the punishment condition. They did not.

Mandler and Kaplan (1956) concluded from their study that: "in human verbal learning, the subject's subjective evaluation of the reinforcing stimulus may provide an independent measure of the reinforcing value of a verbal reinforcer." Ss who evaluated the "mmmh" of the experimenter positively conditioned, where as those who evaluated it negatively did not. Here again, however, without additional information it is impossible to evaluate to what extent this effect influenced the results of the present study.

In summary then, as no significant conditioning effect was evident one cannot argue that the results of this experiment strongly support, or disconfirm any of the predictions drawn from the theories of Eysenck, Gray or Spence. The situation is made even more complex by the fact that the HN group used the pronoun designated as the CR significantly more often than the LN group during the operant period, even before reinforcement was introduced. Despite this, however, the experiment might be regarded as offering weak support for Eysenck, as the introverted group showed the more pronounced improvement during reinforcement, while the extraverted group displayed the more pronounced extinction effect.

In favour of Gray's theory it was observed that the LNHE group displayed greater improvement than the LNLE during positive reinforcement, and the LNLE out did the

LNHE during negative reinforcement, but the HN groups did not display the behaviour expected of them on the basis of the theory, in either reinforcement condition. No evidence of conditioning was found for the HN group, overall, but the LN group showed a clear improvement, a finding contrary to the predictions of both Gray and Spence.

CHAPTER VIII: EXPERIMENT II

8:1 ABSTRACT

The present experiment was a modification of one by Ehlers (1963) which concluded that recognition reaction times could be altered by a conditioning procedure. Introverts were found to be the better conditioners.

In the present experiment an appetitive condition as well as an aversive condition was employed. A significant conditioning effect was found, but despite some results being in the direction predicted by the theories of Eysenck, Gray and Spence, none of these theories received any clear support.

Introverts were found both to show the more rapid extinction of both the "sensitization" and "perceptual defence" effects, and to be more likely to become aware of the reinforcement contingencies.

With respect to self-rated consistency it seems more likely that this is a trait specific, rather than a generalized, factor.

8:2 INTRODUCTION

Given the failure of experiment 1 to produce any clear-cut results with respect to the hypotheses that different personality groups will be found to show differential conditionability it was decided to put these hypotheses to the test once more, but this time using a different paradigm.

Eysenck (1967) reports a study by Ehlers (1963) in which four groups of nonsense syllables, each characterized by containing a specific letter, were associated with different types of stimuli: painful electric shock, a visual stimulus, an auditory stimulus, with a fourth group serving as a control group and not being associated with any stimulus. After this association phase the syllables were presented once more but this time unaccompanied, while "tachistoscopic reaction times measured to correct recognition of the stimuli" (Eysenck, 1967, p.150) were recorded.

As predicted those syllables associated with the painful shock were found to have the highest recognition thresholds. Subjects had been classified into personality groups on the basis of their EPI scores and Eysenck reports that although the HN group had higher recognition thresholds than the LN group for all four groups of syllables when one considers the difference between recognition times for the shock and non-shock associated syllables there was no significant difference between the LN and HN groups. However, "introverts, as compared to Extraverts, showed a significant increase in recognition time for shock over non-shock stimuli; the extraverts, in fact, showed no increase for the shock stimuli." (Eysenck, 1967, p.150).

Eysenck interprets these results as demonstrating on one hand the superior conditionability of the introverted group, and on the other the relative mildness of the stress

produced in the conditioning situation. These findings may be seen as supporting the hypotheses of Brown (1961) and Inglis (1960). Inglis (1960) while reviewing work on perceptual defence (PD) had noted that an interaction was to be expected between "situations of differential stress and differential individual susceptibility to stress." (p.264).

Brown(1961), on the basis of his extensive review of the PD literature, offered the following hypothesis: "That recognition thresholds at first rise with increases in stimulus emotionality; and that the amount of stimulus emotionality required to bring recognition thresholds to their peak is directly related to the degree of extraversion of the subject."

Both these authors postulate an inverted U - shaped function in an attempt to account for the apparently conflicting results reported by different authors. Many experiments on perceptual defence (PD) had reported an increase in recognition thresholds e.g. Bootzin and Natsoulas (1965), De Lucia and Stangner (1953), Ehlers (1963), Eriksen (1952), Eriksen and Brown (1956), Kates and Klein (1954), Kissin et al (1957), Kogan (1956), McGinnies (1949), McGinnies and Sherman (1952), Matthews and Wertheimer (1958), Ruiz & Krauss (1968), Sauber (1971), Zajonc (1962), others have reported lowered thresholds e.g. Greenbaum (1956), Hatfield (1959), Osler & Lewinsohn (1954), Postman, Bronson, & Gropper (1953), Spence (1956). Still other experiments have reported both a rise and fall in thresholds e.g. Adcock and Mangan (1970), Brown (1961), Bruner & Postman (1947), Dodwell (1964), Lazarus, Eriksen and Fonda (1951), Lysak (1954), Reece (1954) Rosen (1954), Spence (1957).

Indeed, even in the first study to use the term "perceptual defence" (Bruner and Postman, 1947) the authors postulated that two processes were necessary to account for

the PD phenomenon:

(i) Perceptual defence - "With increase in emotionality of stimuli, recognition may lead to an anxiety and is to be avoided as long as possible. For some of our subjects, this type of perceptual defence became increasingly evident over the total range of emotionality." (p.74).

(ii) Sensitization - "In situations which are highly threatening and highly exacting, the most adaptive perceptual response is frequently the one which takes most vigilant account of 'reality'." (p.76).

Both Brown and Inglis regard the dimension of extraversion-introversion as well as the neuroticism dimension as of some importance to the PD phenomenon. Brown (1961), when attempting to test his hypothesis did find the predicted curvilinear relationship between the threshold of recognition and stimulus emotionality, but this relationship held for female subjects only, male subjects showing the ascending phase of the curve only. Only LN subjects displayed the inverted U - shaped relationship, though, in line with the hypothesis, in the group which did show the relationship the LE subjects reached peak recognition threshold earlier than HE subjects.

Dodwell (1964) used the EPI to classify his subjects, and he reports that at low levels of stress the introverted group displays "defence" and the extraverted group "vigilance". At higher levels of stress these roles were reversed.

Anxiety has, of course, also been related to PD. Greenbaum (1956), for example, reported that high MAS scorers had lowered thresholds for hostile faces. Osler and Lewinsohn (1954) found that high MAS scorers had lowered thresholds for "unacceptable words". Spence (1956) reported a significant correlation between the lowering of thresholds and state anxiety.

Other personality dimensions have also been considered: Matthews and Wertheimer (1958) found that hysterics (high scorers on the MMPI Hy scale) showed PD while psychasthenics (high scorers on the MMPI Pt scale) did not; Bootzin and Natsoulas (1965) report that hysterics showed a generalized PD effect while other groups showed it only to anxiety producing words, Eriksen and Brown (1956) found that low Pt scorers showed the more marked PD effect.

Kates and Klein (1954) employed the California F scale and found that high scorers were significantly slower to recognize "charged" words. Similarly Kogan (1956), again using the California F scale, found his results, "clearly indicated the presence of an inverse relationship between authoritarian attitudes and the capacity to identify highly emotional stimuli". Sauber (1971) looked at Locus of Control and found externals to be more defensive.

However the PD phenomenon has not gone unchallenged Howes and Solomon (1950), criticizing an experiment by McGinnies (1949) offered two alternative explanations of the results of that experiment, (i) that the findings reflected differential familiarity with the words employed in the different conditions, that is, the PD phenomenon might simply be explained by claiming that less familiar words are harder to recognise; (ii) that what was being observed was not <u>perceptual</u> defence but simply <u>response</u> suppression, the subject is more cautious, perhaps due to embarrassment, about responding with emotional or taboo words.

Many ingenious experiments were devised to obviate these problems, but one attempted solution, in particular, is of interest here. A number of authors have used nonsense syllables or neutral words and associated these with emotional stimuli, usually electric shock. Hatfield (1959), for

example used five letter disyllables and shock, and found a lowering of recognition threshold. Lysak (1954) again using syllables and shock found some evidence for both defence and for sensitization. Rosen (1954) entrained shock to nonsense syllables and concluded that when correct identification of a stimulus ended an associated punishment then sensitization increased. If, however, punishment could not be avoided by a correct response then perception was disturbed.

Along similar lines Reece (1954) used a paired associate learning task and five groups: (i) predictable shock (the same nonsense syllables were associated with shock on each trial, and shock was escapable by reporting the syllable correctly), (ii) unpredictable but escapable shock (random pairings of syllables and shock), (iii) predictable inescapable shock, (iv) unpredictable-inescapable shock, (v) a no-shock control group. The escape groups were found to have the lowest thresholds. Reece also found that subjects in the non-escape groups were significantly more inclined to view shock as a punishment. He goes on to suggest that perceptual defence will be more evident when the subject has no learned escape response available, that is, escape from the noxious stimulus will be viewed as a reward and one can expect low thresholds with reward, as found in his experiment.

As already mentioned Ehlers (1963) also successfully conditioned the perceptual defence phenomenon and in addition also demonstrated the importance of the personality variable, extraversion.

8:3: HYPOTHESES

The present experiment may be considered as a follow up of Ehler's study with some modification to permit the comparison of predictions drawn from the theories of Gray

and Eysenck. Previous studies appear to have demonstrated that the perceptual defence phenomenon may be conditioned, even if it is not legitimate to regard it as always being a conditioned phenomenon. In addition to the raised thresholds associated with mildly aversive situations it appears that thresholds may also be lowered either in the face of strongly aversive stimuli or of rewarding stimuli, that is, there appears to be a process which enables one to ignore that which is annoying, but which makes one aware of that which is either dangerous or rewarding.

The present experiment employs both a positive and a negative reinforcer, and we would expect on the basis of previous experimental results that lowered thresholds will be associated with the positive reinforcer and raised thresholds with the negative reinforcer. This then is the first hypothesis:

Hypothesis 2:1 There will be a significant difference between the recognition thresholds of those syllables associated with positive reinforcement and those associated with negative reinforcement, reaction times of the syllables associated with the positive reinforcer being the faster.

Of course, turning to the personality variables, on the basis of Eysenck's theory it is to be expected that the introverted group will show the superior conditioning as compared with the extraverted group. This then gives us the second hypothesis:

Hypothesis 2:2 There will be a significant extraversion x type of reinforcement interaction when reaction time scores are compared, the introverts showing superior conditioning in both the positive and negative reinforcement conditions. When the difference between the reaction times in the positive and negative conditions are used as scores for analysis it is predicted that the introverted group will show a greater

difference between these two conditions than will the extraverted group.

Gray, with his model of differential sensitivity to cues of reinforcement, would predict a rather different pattern of results:

Hypothesis 2:3 The introverted group will be expected to show the slowest reaction times in the negative reinforcement condition, while the extraverts will be expected to show the fastest reaction times in the reward condition.

Gray also considers the neuroticism dimension to be of importance as a dimension of general sensitivity to cues of reinforcement, with this in mind it is predicted:

Hypothesis 2:4 The HN group will show faster reaction times than the LN group in the reward condition and slower reaction times than the LN group in the negative reinforcement condition.

For the reasons given in Experiment 1 if hypothesis 2:4 is confirmed this will be taken as supporting Spence's position.

On the basis of Gray's theory it is also predicted:

Hypothesis 2:5 There will be an extraversion x neuroticism x type of reinforcement interaction, with the HNHE group showing the fastest reaction times in the reward condition and the HNLE group showing the slowest reaction times in the negative reinforcement condition.

With respect to the recognition stage of the experiment as no reinforcement is available during this phase it may be regarded as an extinction phase. On this basis it may be predicted that if Eysenck's theory is correct:

Hypothesis 2:6 The extraverted group will display the steeper extinction trends in both "reward" and "punishment" conditions.

However, on the basis of Gray's theory it is predicted:

Hypothesis 2:7 The extraverted group will show the steeper extinction trend in the punishment condition, (CS -$\overline{\text{Pun}}$ = CS-Rew), while the introverted group will show the more rapid extinction in the reward condition (CS-$\overline{\text{Rew}}$ = CS-Pun)

8:4 METHOD

Design

The independent variables used in this experiment were: two levels of extraversion, and two levels of neuroticism, the between subject variables, and three types of reinforcement, positive, neutral and negative, providing a within subject variable. A 2 x 2 x 3 design was employed. The dependent variable was the tachistoscopic reaction time to correct recognition of a three letter syllable.

The experiment was performed in two stages: a conditioning stage and a recognition stage.

Stage 1: In the first phase of the experiment a classical conditioning procedure was employed. Reinforcement was associated with three letter syllables on a variable ratio schedule, the CS overlapping in presentation with the UCS. The reinforcement was not contingent on any response from the subject.

Three sets of nonsense syllables were used, all taken from either Archer (1960) or Noble (1960). Each set was characterized by its central letter, which was a vowel. Three vowels were used, I, U, E, hence three sets of syllables. These vowels were associated with either a positive reinforcer, a 2p coin, a negative reinforcer, white noise at 75dbs for 500 msec.s, or a neutral "reinforcer", the noise of the feedback between two speakers, again at 75dbs for 500 msec.s. A random 75% reinforcement schedule was employed. Each subject performed under all three

conditions though the relationship between vowels and reinforcers was randomized across subjects.

It will be seen that an effort has been made to employ those parameters Eysenck regards as being of importance if one is to demonstrate the superior conditionability of the introvert: partial reinforcement, a weak UCS, a short CS - UCS interval, here there was 350 msec.s interval between the on-set of the CS and the on-set of the UCS, and they overlapped for 500 msec.s. Discriminant learning was also used.

Stage II: Nonsense syllables were again presented tachistoscopically, but this time the S was required to press a switch as quickly as possible, (this both stopped a timer, thereby yielding a reaction time (RT) score, and changed the tachistiscope field, thereby preventing the S from reading the syllable after an RT was recorded). The S was required to call out the nonsense syllable at the same time as pressing the switch. It was these RT's which provided the data for analysis.

Personality Measurement

Ss were classified as extraverted or introverted, and of high or low neuroticism on the basis of their EPI (form A) scores. They also completed a self rating questionnaire in which they rated themselves on a nine point scale for each of the three traits: Extraversion (sociability), impulsivity, and neuroticism. Preceding each scale was a brief description of the trait and following each rating scale was another nine point scale with the word "sometimes" printed at one end and "always" at the other. The direction of this scale was varied.

Ss were asked to use this consistency scale to rate how typical they felt the rating they had given of themselves

on the trait scale was of their behaviour. The idea of using these self rating scales was that on the one hand the consistency ratings might throw some light on the issue of whether consistency is trait specific or more generalized. On the other hand one might perform an analysis of data drawn only from those subjects who rated themselves as highly consistent, hypothesizing that even if the statements of trait theorists are not generally applicable they might never-the-less be true of this subgroup. It was for this reason that a description of the trait, as conceived of in Eysenck's theory, was given before each rating scale, rather than follow the suggestion that people will be found to be consistent with respect to dimensions, and clusters of situations, of relevance to them. Though in some ways this method of global self-rating must be nearer to this ideal than the traditional questionnaire.

As events turned out, however, there were too few observations in some cells for this latter analysis to be performed meaningfully, (cf. appendix for copy of Questionnaire).

APPARATUS

Syllables were presented tachistoscopically. The length of exposure was controlled by a digit timer. This also controlled the timing of the on-set and off-set of two cassette recorders which provided the noise stimuli. Additional circuitry permitted the experimenter to select, on each trial, which, if either, of the two noises would be heard by the S. Subjects wore headphones throughout the experiment, and it was through these that the noise stimuli were delivered. As mentioned above still further circuitry caused a digit timer to stop and the tachistiscope field to change whenever the S pressed the switch positioned in front of him/her.

Pilot Studies

Two pilot studies were conducted in preparation for the present experiment. The design of the first of these was substantially the same as that of the present experiment, it brought two potential problems to light, however.
(i) It was found that with 12 presentations of the CS on a 75% reinforcement schedule, yielding 9 reinforced trials, the conditioning trend was not clear. It was, therefore, decided to increase the number of presentations to 20 i.e. 15 CS - UCS pairings.
(ii) It was also found that the noise used as the aversive stimulus, the sound of a whining drill, was not found to be particularly unpleasant by the subjects. In an attempt to find a noise stimulus which could serve as an unpleasant stimulus 7 different sounds were recorded and played, in random order, twice each to 40 subjects. Noises were presented at 75 dbs for 500 msec.s over headphones i.e. exactly as they would be heard in the experiment proper. Subjects were then asked to rate each noise. They were presented with a line marked "very pleasant" at one end, and "very unpleasant" at the other, with the mid-point of the line marked "neutral". The line was 15 cm.s long. Subjects were asked to put a mark somewhere on the line indicating how pleasant or unpleasant they found the noise. The distance of these points from the end of the scale was measured to one decimal place. These measures consititued the data for the analysis. Each noise was rated on a separate scale. Two noises were then selected, (a) the noise most consistently and significantly rated as unpleasant - white noise; (b) the noise which had a mean rating nearest to the mid-point of the scale - this was the noise of the feed back between two speakers.

These two noises plus the original "aversive" noise

were then played to a further 19 subjects. The procedure was the same as before. White noise was found to be rated as significantly more unpleasant than the other two noises, which were not found to be significantly different from each other. There were no interactions between ratings for the noises and the personality dimensions measured by the EPI.

Procedure

55 subjects were originally used in this experiment; two of these had to be disgarded from the analysis because of errors which occured during the conditioning phase of the experiment. One further subject was excluded from the analysis. She made 18 errors in the recognition phase of the experiment, whereas most subjects made none at all, and her mean RT was substantially slower than that of the other subjects. She was also the oldest subject used by some years.

Both male and female volunteer, university students took part in the experiment. Each subject completed an EPI (form A) and a self rating scale. One of these was completed before the experiment and the other after. The order was randomized across subjects.

In the first phase of the experiment the subject was seated in front of a tachistoscope, wearing headphones and with his/her right hand lying open on the table, alongside the tachistoscope. Nonsense syllables of all three types were presented to the S in random order, and the appropriate stimulus paired with it. In this phase of the experiment the subject was presented with 60 nonsense syllables, 20 of each type, 75% of each type being reinforced as was appropriate. The type of reinforcement paired with each type of syllable was randomized across subjects.

350 msec.s after the syllable appeared in the tachistoscope the noise appropriate to that condition commenced,

this noise continued for 500 msec.s. At the end of this 500 msec.s the tachistoscope field changed to a visual noise field and the noise ceased. Thus the syllable was visible for 850 msec.s, and the noise and syllable overlapped by 500 msec.s.

On the positive reinforcement trials the experimenter dropped a 2p coin into the hand of the S approximately 350 msec.s after the syllable became visible. Naturally this procedure was not nearly so accurate as the electronically timed onset of the noise in the negative reinforcement condition, but it is unlikely that the CS-UCS onset interval was ever greater than 500 msec.s. As in the negative reinforcement condition the syllable remained visible for 850 msec.s in all.

On non-reinforced trials no noise was played nor coin given, only the syllable was seen for 850 msec.s. Instructions to the S were as follows: "I'd like you to look into the tachistoscope where you will see a number of cards flashed up. These cards will have three letters on them, always in the order consonant-vowel-consonant.

These letters will sometimes be accompanied by a noise which other people have rated as unpleasant, sometimes by a noise that other people have rated as neutral, neither pleasant nor unpleasant. And from time to time I will drop a 2p coin into your hand - you can keep all the coins you get. Any questions?"

Most subjects asked what they had to do and were assured that at this stage they were required to do nothing. Some subjects asked if they would be expected to remember the letters, but were only told, as were all subjects, that there would be a second part to the experiment and this would be explained to them when they got to it.

In the second phase of the experiment 10 cards of each type were presented. In two of the groups, those characterized by the letters "I" and "O", none of these syllables had been seen by the S. before. By accident, however, one card in the "E" group appeared in both stages one and two. However, as the relationship between type of syllable and type of reinforcement was randomized across subjects it seems unlikely that this could have influenced the results of the experiment.

Ss were required to make a reaction time response to the syllables by pressing a switch positioned in front of them. This switch both stopped the timer and changed the tachistoscope field. This prevented the S from reading the card after he/she had stopped the timer. Ss also had to call out the three letters of the syllable.

In order to make the two phases as similar as possible the S was not told that there would be no more reinforcement and was required to continue wearing the headphones during the second part of the experiment.

Ss were given a 2 - 3 min. rest between the two phases of the experiment, during which time the experimenter prepared the second set of cards, the scoring sheet etc.

The instructions for the second part of the experiment were as follows:

"I'm going to present you with another series of cards. Like the first set of cards each card will have three letters on it. As soon as you can tell me what these three letters are I want you to press this switch and say them aloud. I want you to make your response as quickly as possible, but be sure that you get the letters right. Are there any questions?

I will say 'ready' just before each syllable is

presented so you will know when to expect each card."

The RT to each syllable was recorded. In those cases where the syllable was incorrectly reported it was presented again at the end of the series. However there were only 14 of these out of 1560 responses.

After all the cards had been shown the subject was asked the following series of questions:

1. What did you think was happening?
2. Did you notice any consistent relationships?
3. Did you notice anything consistent happening when certain types of cards were shown?
4. Did you notice anything particular in the relationship between the letters, noises and coins?

Ss were classified as "aware" or"unaware" on the basis of these questions. Most subjects who were aware of what was "going on" correctly reported the reinforcement contingencies in response to question one.

Ss were also asked which of the two noises they found less pleasant. Most Ss found the white noise less pleasant but several found the feedback noise less pleasant and so for these Ss this was regarded as the negative reinforcement condition.

8:5 RESULTS

For the purposes of the present experiment Ss scoring 11 or more on the E or N scale of the EPI were classified as extraverted or neurotic, respectively. Those scoring 10 or less were classified as introverted or stable. The means and standard deviations for the groups were as follows:

TABLE 8:1

		Mean	S.D.
Extraverts	Scores on E	13.71	2.56
Introverts		6.04	2.44
Neurotics	Scores on N	15.15	3.13
Stables		6.46	2.94

S were also classified as extraverted or introverted, neurotic or stable, impulsive or non-impulsive on the basis of their own self-ratings. Those scoring 10 or over were regarded as extraverted (mean = 9.65). Those scoring 6 or over on the impulsivity scale as implusive (mean = 4.58), and those scoring 6 or over on the neuroticism scale as neurotic (mean = 4.77).

Because syllables were presented in a random order in the second phase of the experiment it was possible that while the first occurrence of a syllable associated with e.g. the positive reinforcer might be on the first RT trial, while the first occurrence of a syllable associated with a non-appetitive reinforcer might have been on the third RT trial, or even later. In an attempt to reduce any effect position might have had on the results of the experiment the mean of two consecutive trials in a condition was used as the S's score. This had the effect of reducing each Ss scores from 10 to 5 per condition.

Before turning to the testing of the hypotheses set out earlier one other effect was looked at. Several studies (e.g. Morgan 1967; Morgan and Farmer, 1967; Zharov and Yemolayer-Tomina, 1972) have reported significantly faster reaction times for extraverts as compared to introverts, As can be seen the same difference in RTs between these two

TABLE 8:2 ANALYSIS OF VARIANCE OF REACTION TIMES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES.

Source	D.F.	M.S.	F	P
Extraversion (E)	1	42639490.294	12.237	0.001
Neuroticism (N)	1	4970276.594	1.426	0.236
E x N	1	11562890.798	3.318	0.071
Residual	48	3484379.734		
Total	51	4129337.535		

TABLE 8:3 ANALYSIS OF VARIANCE OF REACTION TIMES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS.

Source	D.F.	M.S.	F	P
Extraversion (E)	1	21520455.955	5.473	0.022
Neuroticism (N)	1	1702637.928	0.433	N.S.
E x N	1	7469367.285	1.900	0.171
Residual	48	3932060.378		
Total	51	4129337.535		

TABLE 8:4 ANALYSIS OF VARIANCE OF REACTION TIMES. Ss ALLOCATED ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES. ("Aware" Ss only)

Source	D.F.	M.S.	F	P
Extraversion (E)	1	15546692.001	4.362	0.044
Neuroticism (N)	1	4908864.282	1.377	0.250
E x N	1	4195820.419	1.177	0.288
Residual	26	3563757.380		
Total	29	3771772.279		

TABLE 8:5 ANALYSIS OF VARIANCE OF REACTION TIMES. Ss ALLOCATED PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS. ("Aware" Ss only)

Source	D.F.	M.S.	F	P
Extraversion (E)	1	14680568.955	4.210	0.048
Neuroticism (N)	1	825270.022	0.237	N.S.
E x N	1	1275572.508	0.366	N.S.
Residual	26	3486910.631		
Total	29	3771772.279		

TABLE 8:6 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES.

Source	S.S.	D.F.	M.S.	F	P
Extraversion (E)	133.852	1	133,852	2.579	N.S.
Neuroticism (N)	8.925	1	8.925	0.172	N.S.
E x N	31.926	1	31.926	0.615	N.S.
Residual	2490.951	48	51.895		
Total	2630.244	51			

TABLE 8:7 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS.

Source	S.S.	D.F.	M.S.	F	P
Extraversion (E)	229.169	1	229.169	4.59	0.035
Neuroticism (N)	11.728	1	11.728	0.235	N.S.
E x N	75.209	1	75.209	1.507	0.224
Residual	2396.297	48	49.923		
Total	2632.902	51			

TABLE 8:8 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF THEIR EPI SCORES. ("Aware" Ss only)

Source	S.S.	D.F.	M.S.	F	P
Extraversion (E)	151.607	1	151.607	2.405	.130
Neuroticism (N)	39.509	1	39.509	.627	N.S.
E x N	82.782	1	82.782	1.313	.261
Residual	1638.862	26	63.033		
Total	1795.043	29	61.898		

TABLE 8:9 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS. ("Aware" Ss only)

Source	S.S.	D.F.	M.S.	F	P
Extraversion (E)	383.305	1	383.305	7.134	.012
Neuroticism (N)	52.372	1	52.372	.975	N.S.
E x N	118.342	1	118.342	2.203	.146
Residual	1396.897	26	53.727		
Total	1795.334	29	61.908		

groups has been found here (Tables 8:2 - 8:5). The data for these analyses are the sum of the five scores in each of the three conditions (i.e. the sum of 15 scores) for each S; that is, it is the same as the main effect for extraversion in a 4 way analysis of variance (extraversion x neuroticism x type of reinforcement x trials) which could have been performed on this data.

Because of this significant difference between the two groups it was felt that a "floor effect" might well be affecting the results and, if it were not corrected for, might well yield spuriously significant results especially when the differences between the RTs to syllables associated with appetitive and aversive stimuli were compared (difference scores). For this reason the scores were transformed in the following manner: The fastest RT response of each subject was subtracted from each of his other scores. As each score was the mean of two consecutive RT responses this procedure did not yield any zero scores. It was the natural log.of these differences which was then used in subsequent analyses. The transformation may be described by the following formula:

$$\log (X - X \min)$$

(Where X = the mean of two consecutive RTs belonging to the same reinforcement condition; X min = the fastest RT for the S.)

The success of this transformation can be judged, at least in part, by the fact that when the analyses summarized in Tables 8:2 and 8:4 were performed using data transformed in the manner described above, the significant extraversion effect is no longer evident at least when Ss were allocated to cells on the basis of their EPI scores (Tables 8:6 and 8:8). There is still a significant difference between the RTs of the introverted and extraverted Ss when they are allocated to cells

TABLE 8:10 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, FIRST SCORES PER CONDITION. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES

Source	S.S.	DF	M.S.	F	P
Between		51			
Extraversion (E)	1.1686	1	1.1686	1.01784	N.S.
Neuroticism (N)	0.4443	1	0.4443	<1	N.S.
E x N	0.5005	1	0.5005	<1	N.S.
Ss within groups	55.1096	48	1.14812		
Within		52			
Type of Reinforcement (R)	2.357	1	2.357	6.2457	$p<.05$
E x R	0.0576	1	0.0576	<1	N.S.
N x R	0.3581	1	0.3581	<1	N.S.
E x N x R	0.03042	1	0.03042	<1	N.S.
R x Ss within groups	18.11407	48	0.37738		
Total	77.530701				

TABLE 8:11 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, FIRST SCORES PER CONDITION. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS.

Source	S.S.	DF	M.S.	F	P
Between		51			
Extraversion (E)	0.384	1	0.384	<1	N.S.
Neuroticism (N)	2.1342	1	2.1342	1.888	N.S.
E x N	0.322	1	0.322	<1	N.S.
Ss within groups	54.2563	48	1.13034		
Within		52			
Type of Reinforcement (R)	2.4681	1	2.4681	6.7176	$p<.05$
E x R	0.0989	1	0.0989	<1	N.S.
N x R	0.0853	1	0.0853	<1	N.S.
E x N x R	0.7391	1	0.7391	2.0117	N.S.
R x Ss within groups	17.6355	48	0.36741		
Total	77,53291	103			

TABLE 8:12 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, FIRST SCORES PER CONDITION. Ss ALLOCATED TO PERSONALITY GROUPS ON BASIS OF EPI SCORES. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Between		29			
Extraversion (E)	0.3122	1	0.3122	< 1	N.S.
Neuroticism (N)	0.8886	1	0.8886	< 1	N.S.
E x N	0.025	1	0.025	< 1	N.S.
Ss within groups	39.14	26	1.5054		
Within		30			
Type of Reinforcement (R)	0.7631	1	0.7631	3.06	$p < .05$
E x R	0.1595	1	0.1595	< 1	N.S.
N x R	0.0784	1	0.0784	< 1	N.S.
E x N x R	0.0416	1	0.0416	< 1	N.S.
R x Ss within groups	6.48567	26	0.2494		
Total	47,8765	59			

TABLE 8: 13 ANALYSIS OF VARIANCE OF TRANSFORMED DATA, FIRST SCORES PER CONDITION. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Between		29			
Extraversion (E)	0.0872	1	0.0872	< 1	N.S.
Neuroticism (N)	1.5376	1	1.5376	1.032	N.S.
E x N	0.1322	1	0.1322	< 1	N.S.
Ss within groups	38.745	26	1.4902		
Within		30			
Type of Reinforcement (R)	1.0413	1	1.0413	4.642	$p < .05$
E x R	0.2546	1	0.2546	< 1	N.S.
N x R	0.0035	1	0.0035	< 1	N.S.
E x N x R	0.5047	1	0.5047	2.25	N.S.
R x SS within groups	5.8311	26	0.2243		
TOTAL	47.8766	59			

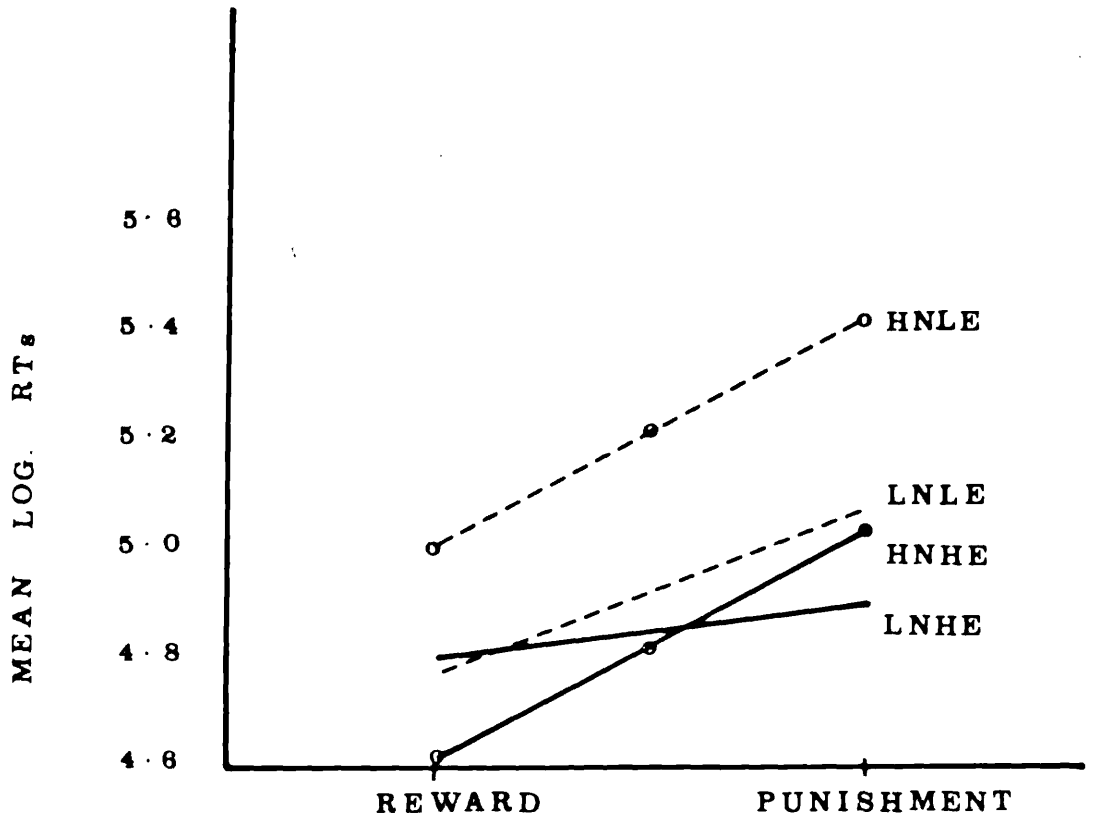


FIG. 8:1 REWARD AND PUNISHMENT MEAN LOG. RTs FOR EPI PERSONALITY GROUPS

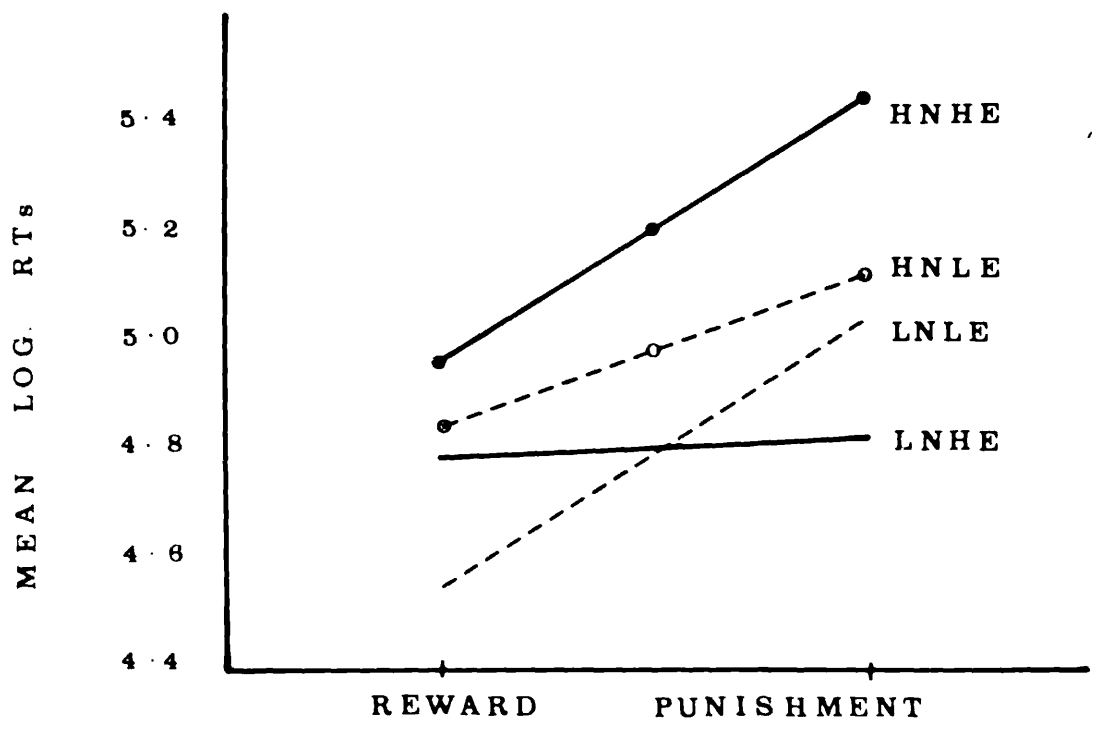


FIG. 8:2 REWARD AND PUNISHMENT MEAN LOG. RTs FOR SELF-RATED PERSONALITY GROUPS

on the basis of their self-ratings (Tables 8:7 and 8:9), however.

Conditioning

Hypothesis 2:1 predicted that there would be a significant difference between the RTs of syllables associated with the positive reinforcer, and those associated with the negative reinforcer. This really amounts to the statement that conditioning will take place, and as such the confirmation of this hypothesis is of central importance to the interpretation of the results of this experiment. As can be seen (Tables 8:10 - 8:13), a significant difference has been found between these two sets of RTs as predicted, and so it can be concluded that conditioning has taken place. The data used in these analyses are the transformed scores for the first recognition trials. It was decided only to use these first scores rather than all five scores per condition, as these should reflect the conditioning effect most clearly, the sum of all five scores would reflect the extinction trends as well.

Eysenck - Extraversion

The second hypothesis for this experiment was drawn from Eysenck's theory of personality and predicted that there would be a significant introversion-extraversion x type of reinforcement interaction. There is no evidence of this, however, in Tables 8:10 - 8:13.

When Fig 8:1 is considered, however, it is obvious that the introverted group has slower mean log. RTs than the extraverted, which suggests, in its turn, that "difference" scores, (log. RTs to syllables associated with negative reinforcement minus log. RTs to syllables associated with positive reinforcers), might throw some light on the differential conditionability of the various personality groups, by yielding scores relatively uncontaminated by the differences in RTs between groups.

TABLE 8:14 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES.

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	0.267	1	0.267	0.351	N.S.
Neuroticism (N)	0.732	1	0.732	0.965	N.S.
E x N	0.077	1	0.077	0.102	N.S.
Residual	36.448	48	0.759		
Total	37.525	51			

TABLE 8:15 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS.

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	1.553	1	1.553	2.105	0.150
Neuroticism (N)	1.396	1	1.396	1.893	0.172
E x N	1.507	1	1.507	2.043	0.156
Residual	35.400	48	0.738		
Total	37.525	51			

TABLE 8:16 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	0.130	1	0.130	0.261	N.S.
Neuroticism (N)	0.194	1	0.194	0.389	N.S.
E x N	0.081	1	0.081	0.161	N.S.
Residual	12.988	26	0.500		
Total	13.240	29			

TABLE 8:17 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	0.046	1	0.046	0.103	N.S.
Neuroticism (N)	0.333	1	0.333	0.742	N.S.
E x N	1.012	1	1.012	2.253	0.142
Residual	11.675	26	0.449		
Total	13.240	29			

TABLE 8:18 ANALYSIS OF VARIANCE OF THE CONTRASTING LINEAR TRENDS FOR REWARD AND PUNISHMENT CONDITIONS. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	23.280	1	23.280	3.872	0.052
Neuroticism (N)	14.593	1	14.593	2.427	0.122
E x N	6.177	1	6.177	1.027	0.317
Residual	288.585	48	6.012		
Total	320.270	51			

TABLE 8:19 ANALYSIS OF VARIANCE OF THE CONSTRASTING LINEAR TRENDS FOR REWARD AND PUNISHMENT CONDITIONS. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	2.534	1	2.534	0.459	N.S.
Neuroticism (N)	0.380	1	0.380	0.069	N.S.
E x N	0.000	1	0.000	0.000	N.S.
Residual	143.523	26	5.520		
Total	148.623	29			

Using this data one would expect a significant introversion-extraversion difference if Eysenck's theory was to receive any support. Tables 8:14 - 8:17 suggest that this is not to be.

Neuroticism

On the basis of the theories of both Gray and Spence it was predicted that there ought to be an N x type of reinforcement effect, but, as with Eysenck's predictions, none of the analyses (Tables 8:10 - 8:17) suggest that there is any support at all for this prediction in the present experiment.

Gray - E, N and type of Reinforcement

It has already been observed that no signifcant introversion-extraversion effects, or extraversion x type of reinforcements interactions have emerged from the analyses. This together with the lack of significant results associated with the neuroticism dimension suggests that little support can be derived from the present experiment in favour of Gray's hypothesis. Turning to the 3 way interaction, and Fig. 8:1, it can be seen that as predicted it is the HNLE group who show the longest RTs in the punishment condition, and the HNHE group who have the shortest RTs in the reward condition. There is no suggestion in Tables 8:10, 8:12, 8:14, or 8:16 that this arrangement of groups might not be entirely due to chance factors.

The relevant interactions in Tables 8:11, 8:13, 8:15 and 8:17 do, however, approach a more acceptable level of significance, but, as Fig. 8:2 indicates, the groups no longer organize themselves into a pattern nearly so favourable to Gray's speculation. The difference between these two figures

and their complementary analyses is that in the former subjects are allocated to personality groups on the basis of their EPI scores, while in the latter they are allocated on the basis of their own self-ratings. It is the former, therefore, which must be regarded as the more sensitive test of the hypotheses drawn from the theories of both Gray and Eysenck and though it can be pointed out that the HN groups display steeper slopes than the LN groups (cf. Fig. 8:1) indicating a greater differentiation between the two types of syllables as benefits the group hypothesized as being generally more sensitive to cues of reinforcement, and as already pointed out there is some indication that the groups organize themselves as would be predicted on the basis of Gray's theory, nevertheless it would be foolish to stress this very tentative support for Gray in face of the non-significance of the results.

When subjects are allocated to personality groups on the basis of their self-ratings the results do appear to be approaching a more "respectable" level of probability, but the pattern of results no longer favours Gray. It is the HNHE group which now displays the slowest RT in the punishment condition, and the LNLE group which displays the fastest RTs in the reward condition. It can be seen from Fig. 8:2 that the tendency towards a significant three way interaction is due almost entirely to the lack of differentiation between the two conditions by the LNHE group. Indeed if only the stable (LN) groups are considered the differences between the introverted and extraverted groups are exactly as one would predict on the basis of Eysenck's theory.

Footnote Apart from those analyses looking at the overall differences in RTs between groups data from the control condition has not been used in any analysis. The reason for this can be seen from Fig. 8:3. The graph described by the control condition follows the same path as that followed by the punishment condition. This may be because there was no aversive conditioning taking place, but the general trend of the graph does not suggest that this is so. This similarity may have been due either to (a) generalization, both were noises, or (b) because, as Gray suggests those stimuli associated with not receiving an expected reward may well themselves function as aversive stimuli.

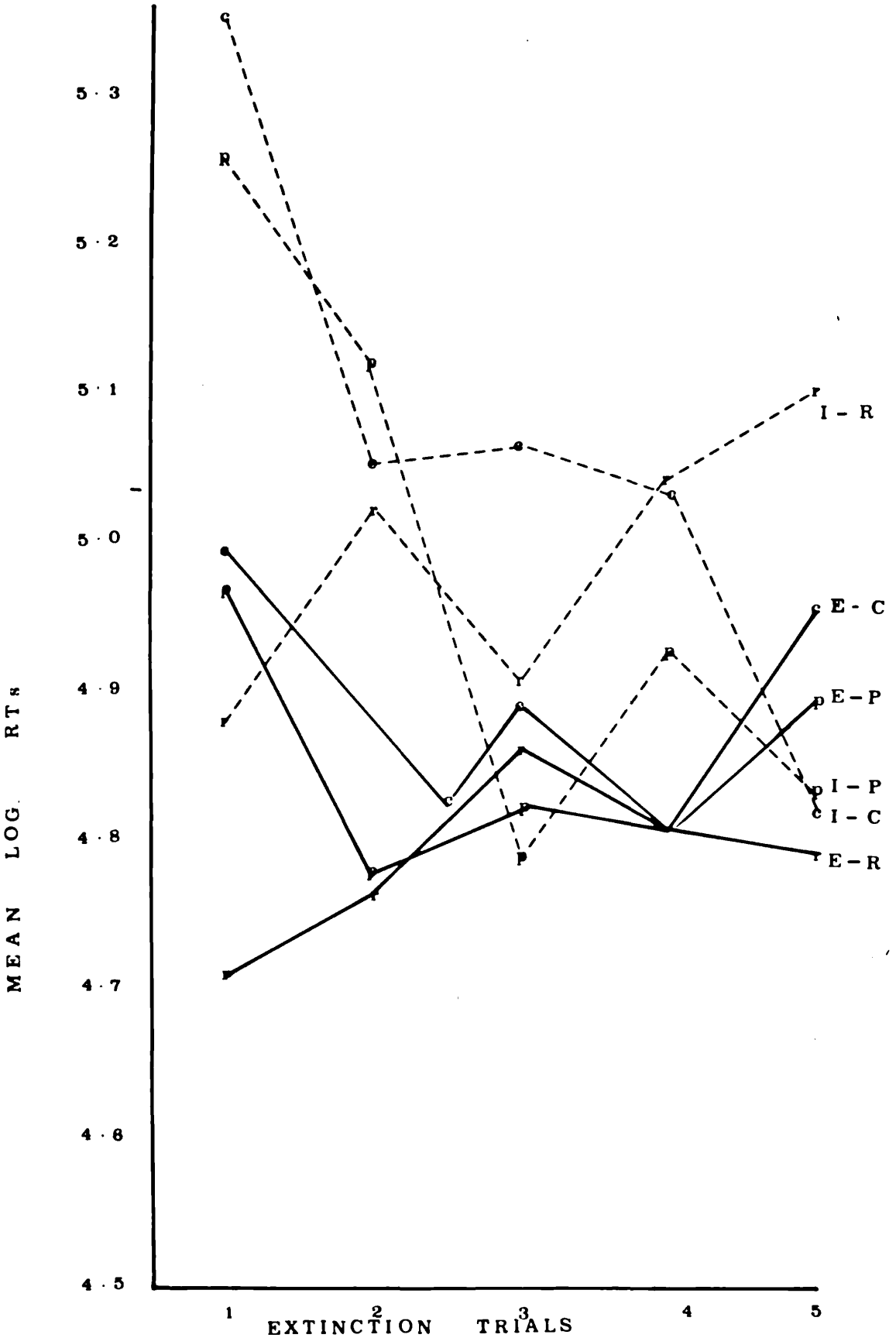


FIG. 8:3 EXTINCTION AS A FUNCTION OF INTROVERSION-EXTRAVERSION AND TYPE OF REINFORCEMENT

(P= PUNISHMENT, R = REWARD, C = CONTROL)

Extinction

As no reinforcement, either positive or negative, was available during the 30 recognition trials this second stage of the experiment can be regarded as an extinction phase. Again different predictions can be drawn from the theories of Eysenck and Gray. Hypothesis 2:6 suggests that in both the positive and negative reinforcement conditions the extraverted group will show the faster extinction, and this pattern of results will be seen as supporting Eysenck's theory. Hypothesis 2:7, on the other hand, drawn from Gray's theory, predicts a more rapid extinction by the extraverted group only in the punishment condition, while the introverted group will be expected to show the faster extinction in the reward condition.

Table 8:18 summarizes the results of the contrasts between the linear trends of RTs to "rewarded" and "punished" syllables over the extinction trials. As can be seen the expected significant extraversion effect is, indeed, evident. For the introverted group there is a general trend for the RTs to get slower across trials in the reward condition, while the trend is in the opposite direction for the punishment condition (cf. Fig. 8:3), clear extinction effects. Eventually the RTs for the two types of syllables should settle down to the same level, but here they appear to have over shot that level. This might well be a rebound effect; or, on the other hand, it might be regarded as an example of Gray's equivalences of reinforcers, non punishment being the same as reward, and the absence of an expected reward being equivalent to a punishment.

These extinction trends are much flatter for the extraverted group, than they are for the introverted group. The effect, then, though it reaches an acceptible level of

TABLE 8:19a ANALYSIS OF VARIANCE OF THE CONTRASTING LINEAR TRENDS FOR REWARD AND PUNISHMENT CONDITIONS. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES. (Extreme scores only)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	8.484	1	8.484	1.222	N.S.
Neuroticism (N)	9.123	1	9.123	1.314	N.S.
E x N	3.059	1	3.059	0.44	N.S.
Residual	159.719	23	6.944		
Total	180.385				

TABLE 8:20 ANALYSIS OF VARIANCE OF THE CONTRASTING LINEAR TRENDS FOR REWARD AND PUNISHMENT CONDITIONS. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS. ("Aware" Ss only)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	10.805	1	10.805	2.210	0.146
Neuroticism (N)	0.023	1	0.023	0.005	N.S.
E x N	0.018	1	0.018	0.004	N.S.
Residual	127.140	26	4.890		
Total	148.624	29			

TABLE 8:21 ANALYSIS OF VARIANCE OF THE CONTRASTING LINEAR TRENDS FOR REWARD AND PUNISHMENT CONDITIONS. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF? RATINGS.

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	2.534	1	2.534	0.387	N.S.
Neuroticism (N)	5.588	1	5.588	0.854	N.S.
E x N	4.869	1	4.869	0.744	N.S.
Residual	313.991	48	6.541		
Total	320.269	51			

significance is not as was predicted by either theory. One might of course attempt to argue either that the results may be construed as supporting Eysenck, the extinction trends reflecting the fact that the introverted group reached a level of conditioning beyond that attained by the extraverted group. This seems unlikely, however, in view of the fact that there was no evidence of differential conditioning in the earlier analyses. On the other hand one might simply argue that, contrary to Eysenck's hypothesis, introverts show a more rapid extinction of CR than do extraverts.

This effect is not found to be significant, however, in Tables 8:19 - 8:21, where Ss are allocated to groups on the basis of their self-ratings, or the analysis is limited to the data from either Ss aware of the reinforcement contingencies, or are more than half a standard deviation above or below the mean on the E and N scales.

Awareness

It was suggested in the first experiment that the lack of any evidence of conditioning might be construed as being related to the fact that few Ss became aware of the reinforcement contingencies. It was, therefore, thought worthwhile here to look at the effect of awareness on conditioning, and the distribution of "aware" Ss across the personality groups. A series of Chi square analyses were calculated. No significant difference was found in the proportion of HN and LN Ss who were classified as "aware". Nor was any significant difference found when the LNHE and HNLE groups were combined and compared with the HNHE and LNLE groups, (reflecting the E x N interaction). A significantly larger proportion of the introverted, than of the extraverted group, irrespective of whether the EPI scores on self-ratings were used as the basis of classification, were found to be

TABLE 8:22 SUMMARY OF ANALYSES OF VARIANCE

	S's Allocated to Groups on the Basis of EIP			S's Allocated to Groups on the Basis of Self-Rating			'Aware' vs 'Unaware' S
	All Ss	'Aware' Ss	'Extreme' Ss	All Ss	'Aware' Ss	Impulsive	
1 First Trial (3 way) (log.data)	RT for P R $p < .025$ (Table 8:10)	RT for P R $p < .05$ (Table 8:12)		RT for P R $p < .025$ (Table 8:11)	RT for P R $p < .025$ (Table 8:13)		
2. First Trial Difference Score (P - R) (log.data)	N.S. (Table 8:14)	N.S. (Table 8:15)	N.S.	N.S. (Table 8:15)	N.S. (Table 8:17)	N.S.	N.S.
3 R + P + C (Raw data)	E,p .001 E x N, p = .071 (Table 8:2)	E,p = .044 (Table 8:4)	N.S. (E,p = .119)	E,p = .022 (Table 8:3)	E,p = .048 (Table 8:5)	N.S.	N.S.
4 R + P + C (log.data)	N.S. E,p = .111 (Table 8:6)	N.S. (Table 8:8)	N.S. (p = .132)	E,p = .035 (Table 8:7)	E,p = 0.012 (Table 8:9)	N.S.	N.S.
5 R vs P (log.data) (data from all extinction trials)	N.S.	N,p = .06	N.S.	N.S.	N.S.	N.S.	N.S.
6 R vs P Linear Trend	E,p = .052 (Table 8:18)	N.S. (Table 8:19)	N.S. (Table 8:19a)	N.S. (Table 8:21)	N.S. E p = .146 (Table 8:20)	N.S.	N.S.
7 R vs P Quadritic Trend	N p = .068	N.S.	N.S.	N p = .08	N.S.	N.S.	N.S.

aware of the existing reinforcement contingencies. There was no evidence of any difference in performance, however, between the "aware" and "unaware" Ss, (cf. summary Table 8:22).

Correlations

To this point all the results which have been reported have been looking for significant differences between groups, but questions about differential conditionability and personality dimensions may also be asked the other way round - to what extent are measures of personality related to indices of conditioning?

Two sets of correlations may be seen as of importance (i) correlations between personality variables and indices of conditioning; (ii) the inter-correlations of conditioning indices. The second of these is, in many ways, the more important. If indices of conditioning do not correlate with each other significantly then any theory which postulates a single general factor of conditionability must be wrong. The present, experiment, however, although it uses three indices of conditioning, cannot really shed any light on the question of whether there is, or is not, a general factor of conditionability because, on one hand, the correlations between the indices reflect mainly the common source of the data, RTs, from which they are drawn, and on the other the "difference" index is obviously not entirely independent of the other two indices, RTs to reward syllables and RTs to punished syllables. Table 8:23 may be summarized by saying that Ss who have fast RTs in one condition also tend to have fast RTs in the other, and Ss who show the largest differences tend to have fast RTs in the reward condition, and slow RTs in the punishment condition.

TABLE 8:24 CORRELATION MATRIX OF VARIABLES EMPLOYED IN EXPERIMENT 2

	1	2	3	4	5	6	7	8	9	10	11
1 Extraversion (EPI)	1.00										
2 Neuroticism (EPI)	-.1894 (p = .179)	1.00									
3 Extraversion (Self-rating)	.7242** (.001)	-.1078 (.447)	1.00								
4 Consistency of Extra	.1105 (p=.436)	-.2756* (.048)	.1562 (.269)	1.00							
5 Impulsivity (Self-rated)	.4479** (p=.001)	.0614 (.666)	.3118* (.025)	-.2382 (.09)	1.00						
6 Consistency of Imp.	-.1286 (.364)	.0434 (.761	-.177 (.210)	.1578 (.264)	-.4062** (.003)	1.00					
7 Neuroticism (Self-rated)	-.219 (.119)	.7441** (.001)	-.316* (.023)	-.3952** (.004)	.0935 (.51)	.1 91 (.176)	1.00				
8 Consistency of Neuro	.1 549 (.273)	0.1998 (.156)	.0173 (.903)	.0891 (.531)	-.150 (.27)	.0332 (.816)	-.3792** (.006)	1.00			
9 Difference (P - R)	-.0329 (.817)	.167 (.237)	.0398 (.78)	.0261 (.855)	-.0093 (.948)	.3064* (.028)	.101 (.477)	-.3036* (.029)	1.00		
10 RT to Reward	.1152 (.417)	.0156 (.913)	.0061 (.966)	-.2568 (.067)	.1553 (.272)	-.3071* (.077)	.0753 (.596)	-.0368 (.796)	-.5188** (.001)	1.00	
11 RT to Punishment	-.1164 (.412)	.2228 (.113)	.117 (.409)	-.1654 (.242)	.0672 (.636)	.0118 (.934)	.1924 (.172)	-.3642** (.008)	.3982** (.004)	.5096** (.001)	1.00

TABLE 8:23 CORRELATIONS BETWEEN INDICES OF CONDITIONING

1.	Difference Scores	1.00		
2.	RTs to "reward" syllables	-.5188 (p .001)	1.00	
3.	RTs for "punished" syllables	.3982 (p=.004)	.5096 (p<.001)	1.00
		1	2	3

As can be seen from Table 8:24 none of the correlations between the personality dimension and either the RTs or the "difference" score turned out to be significant. Significant correlations were found between some of the consistency measures and the condition indices, however.

TABLE 8:25 RATED CONSISTENCY OF IMPULSIVITY

"Difference" Score	.3064 (p=.028)
RT to "Reward" CS	-.3071 (p=.027)
RT to "Punishment" CS	.0118 (N.S.)

(Correlations between rated consistency of Impulsivity and indices of conditioning).

The correlations reported in Table 8:25 suggest that individuals who rate themselves as consistent with respect to their level of impulsivity also tend to be good conditioners, particularly with respect to appetitive reinforcement

TABLE 8:26

	Rated Consistency of N
"Difference" Scores	-.3036 (p=.029)
RT to "Reward" CS	-.0368 (N.S.)
RT to "Punishment" CS	-.3642 (p=.008)

(Correlations between rated consistency of Neuroticism and indices of conditioning)

These results (TABLE 8:26), in their turn, suggest that those individuals who rate themselves as inconsistent with respect to their level of neuroticism tend to be the better conditioners, particularly with respect to aversive reinforcement.

Rated consistency on the extraversion (sociability) scale was found to correlate significantly and negatively (-.395, p=.004) with rated neuroticism and with the EPI N scale (-.2756, p=.048). Similarly self-rated consistency for neuroticism was found also to correlate significantly and negatively (-.3792, p=.006) with self-rated neuroticism, though not with the EPI N scale (-.1998, p=.156). Correlations, of course, do not imply any direction of causality and so it is unclear whether these relationships indicate whether inconsistency is seen as an aspect of neuroticism, when people are rating themselves on this dimension, or whether people who see themselves as neurotic also see themselves as inconsistent.

It is interesting to note, too, the high, though far from perfect, correlations between the questionnaire measures and the self-ratings. For extraversion there was a correlation of .724 ($p < .001$) between the EPI measure and self-rated extraversion (sociability) and a correlation of .448 ($p < .001$) between EPI E and self-rated impulsivity. For the neuroticism dimension the correlation between the EPI measure and the self-rating was .744 ($p < .001$).

With respect to the three consistency measures no evidence of a general factor emerged, none of these self-ratings correlating significantly with any of the others, as can be seen from Table 8:27.

TABLE 8:27

1.	Rated Consistency of Extraversion	1.00		
2.	Rated Consistency of Impulsivity	.1578 (N.S.)	1.00	
3.	Rated Consistency of Neuroticism	.0891 (N.S.)	.03321 (N.S.)	1.00
		1	2	3

8:6 DISCUSSION

The first, and in many ways the most important, point to be made about this experiment is that in as far as it set out to elicit longer RTs to one set of syllables than another on the basis of the reinforcements associated with them, it succeeded. It was found that those syllables characterized by having the same central vowel as the syllables associated with the unpleasant noise yielded significantly slower RTs to recognition than did the syllables characterized by the vowel associated with 2p coins. Apart from this reinforcement history there appears to be no obvious reason to expect these syllables to give rise to different RTs, especially as all three vowels were randomly assigned to all reinforcement conditions across Ss. Given that the experiment was successful in its attempt to condition differential perception, or at least responding, there was every reason to expect the differential conditionability of the personality groups predicted by the trait theorists.

It was predicted on the basis of Eysenck's theory, of course, that the introverted group would display the superior conditionability. No significant differences were found, however, between the introverted and extraverted groups in conditioning. Nor were any of the correlations between

the extraversion dimension, whether measures were taken from the EPI or self-ratings, and the indices of conditioning significant. This finding must be regarded as a serious set back for the Eysenckian theory when it is considered that there is both clear evidence of conditioning, and that parameters, thought to favour the superior conditionability of the introverted group, were employed.

In Eysenck's favour one might point to the fact that it is an extraverted group, the INHE group, which shows the least differentiation between conditions (cf. Fig. 8:1 and 8:2), and when Ss are allocated to personality groups on the basis of their self-ratings, for the stable groups at least, it appears that the introverted groups do display the superior conditioning (cf. Fig. 8:2). In fact there appears to be very little evidence of any conditioning at all on the part of the extraverted group.

When Ss are allocated to personality groups on the basis of their EPI scores (Fig. 8:1) both introverted groups display slower RTs in the aversive condition than the extraverted groups do. This arrangement of groups is, of course, as Eysenck would predict. This is of some interest. It has already been remarked that most experiments purporting to put Eysenck's theory to the test have employed an aversive stimulus, puffs of air and electric shocks being the favourite. Here again when an aversive stimulus is used as reinforcement the results, even though non-significant statistically, are in the direction predicted by Eysenck. The theory does not receive such favourable support when an appetitive reinforcer is employed, however. One might speculate as to the meaning of such findings. Is it that Eysenck's theory is true only with respect to aversive reinforcement? Ought these results to be interpreted as supporting Gray who does predict that

introverts will display superior conditioning only to aversive reinforcement? To this last point one might also add the observation that the evidence Gray assembles when presenting his theory is more convincing with respect to the relationship between introversion and passive avoidance than it is with respect to the relationship between extraversion and the conditioning of appetitive behaviour.

Looking at the problem from an altogether different angle one might argue that the mean RT of the introverted group is slower in both conditions than the mean RT of the extraverted group. The slower RTs of the introverted groups in the punishment condition, may, therefore, be regarded as a manifestation of the more generalized situation, that introverts have slower RTs than extraverts, and nothing at all to do with differential conditionability.

Two things, however, argue against this last point being the whole explanation, on one hand the transformation of the data does appear to have had some success, (compare Tables 8:2 and 8:6); on the other hand the introverted group in the extinction phase showsa substantial increase in the speed of their RTs.

Turning to the extinction phase, here Eysenck's theory is quite explicit and predicts that it is the extraverted groups who ought to show the more rapid extinction. A significant extinction effect did emerge, but Fig. 8:3 shows clearly that it is the introverted and not the extraverted groups who display the more rapid extinction. A finding quite contrary to the prediction made. One might have argued that no differential conditioning effect was observed because all groups reached an asymtotic level, but if this were true the differential extinction effects ought to have been even more pronounced.

Another finding might be introduced at this point which might help explain the more rapid extinction of the introverted group, even if Eysenck's theory is correct. A larger proportion of the introverted group than of the extraverted group was found to be aware of the reinforcement contingencies. The effect of this 'awareness', it might be argued, was that the two groups of Ss were not, in fact, in exactly the same situation. The 'aware' Ss were not responding to the CS alone, but also to the 'correct' construction of the situation. The result of this might well have been that when reinforcements were withdrawn the 'aware' Ss would be able, with the aid of the information available to them to return, rapidly, to their normal, pre-conditioning, level of responding. No doubt the 'unaware' Ss also had some personal construction of the situation, but as it was not the 'correct' one it would not necessarily facilitate a rapid return to a normal level of responding.

One might even wish to construe the fact that proportionally more introverts became aware of the reinforcement contingencies as support for Eysenck's theory. To do this one would have to argue that it is only after the CR has been established that an individual becomes aware of the reinforcement contingencies.

On the other hand it might be argued that the reverse is true and behaviour changes only after some degree of 'awareness' is attained.

These view points belong, of course, to the learning with/without awareness controversy, and as the present experiment provides no data on the basis of which one might decide between these two alternatives extended speculation here would be fruitless.

These speculations must be tempered, however, by the

knowledge that no significant differences were found in the comparisons of Ss classified as 'aware'and 'unaware'. (cf. Table 8:21).

With respect to Eysenck's theory then, despite the fact that appropriate parameters were employed and the fact that certain previous findings were confirmed, the faster RTs of the HE groups, suggesting that this sample was not yielding 'abnormal' results, none of the hypotheses drawn from the theory were confirmed to a significant degree. However, certain features of the data are in the direction predicted by Eysenck, while others may be, arguable, interpreted as supporting his position, though they were not predicted before hand. It might, therefore, be wiser to claim rather that Eysenck's theory receives no strong support from the present experiment, rather than that it has been disconfirmed.

There was little which could be interpreted as supporting Gray in the first experiment, and the situation seems to be much the same in this second experiment. Despite the fact that this experiment might be viewed as a more appropriate test of Gray's theory, employing as it does a passive avoidance situation, none of the hypotheses drawn from Gray's theory were confirmed to a significant degree. In favour of Gray's theory, however, it must be pointed out that the neurotic groups did show the greatest differentiation between conditions (Fig. 8:1) as one would expect from the group hypothesized as being generally more sensitive to cues of reinforcement.

The HNLE group was found to have the slowest RT in the punishment condition and the HNHE the fastest RT in the reward condition, which is again as the Grayian model predicts, but these differences failed to reach significance. On the other hand when we look at the trends over extinction it is

difficult to find any evidence which might be called in support of Gray's theory. Given both the non-significance of the results and this lack of consistency one cannot argue too strongly that the results are not due simply to chance factors. No evidence, then, has been found which clearly supports Gray's position.

As with Gray's theory no real support can be claimed to have been found, in the present experiment, in favour of Spence's theory. Similarly, although Ego Strength was not measured in the present experiment, it is not obvious how these results might be used to support the theory of Roessler.

At this point it seems worth suggesting that it might be rather too simplistic to relate one or more personality variables to "conditioning in general". This harks back to a suggestion made earlier when discussing Gray's theory. There it was pointed out that the theory appeared to claim that classical conditioning takes place at the level of the amygdala, at a level where no difference was hypothesized between the introvert and extravert. Instrumental conditioning, however, seemed to be thought to take place at the level of the reward and punishment mechanisms. In an attempt to fit in these features of the theory with certain observations made by Gray it was suggested that extraverts are less likely to condition fear than introverts, perhaps because being less highly aroused fewer stimuli would reach an aversive level for them. The point of this speculation was that differences might emerge in operant conditioning because, at least for aversive stimuli, differences might also be present in the classical conditioning which preceded it. In view of the fact that no differences have emerged between the introverted and extraverted groups

in the present classical conditioning situation it might be fruitful to see if the differences hypothesized by Gray do in fact emerge in an operant conditioning situation. It might then be speculated, if Gray's hypotheses are confirmed, that Gray's theory is limited to operant learning situations. Whereas Eysenck's theory, which fared somewhat better in the present experiment and has most often been tested in classical conditioning situations, makes statements relevant, primarily, to classical situations.

Along similar lines it may be suggested that classical conditioning is relatively less stress inducing than operant conditioning, or perhaps induces a different kind of stress, as the S does not have to learn and perform a task and is not in a position of such obvious evaluation. Thus is might be predicted that Eysenck's theory is more relevant to low stress classical conditioning, Spence's to high stress classical conditioning and Roessler's to operant conditioning situations. In this scheme of things all the theories might be correct, but only in a limited number of situations. If this were true it would necessitate a change in research strategy from simply comparing predictions drawn from different theories to looking at predictions drawn from a theory in a number of situations.

Turning now to the consistency issue one might expect the less consistent individuals to be more easily conditioned and this is the way things appear to be at least with respect to the neuroticism dimension. Individuals who rate themselves as inconsistent with respect to their level of neuroticism are also found to show superior conditionability, especially with respect to aversive reinforcement, which appears to make some sense. However, the situation is just the reverse with respect to impulsivity. Individuals who rate themselves as

consistent with respect to their level of impulsivity were found to be more easily conditioned, especially with relation to appetitive reinforcement. Rated consistency of extraversion (sociability) was not found to correlate with any of the indices of conditioning. Before speculating about what a pattern of results such as these might mean it might be useful to wait and see if they are replicated. What does appear to be true of the consistency ratings in the present experiment is that they show a considerable independence of each other and would appear to support the suggestion that, in as far as consistency might be regarded as a factor in its own right, it must be viewed as trait specific, rather than one generalized factor.

Of course, rated consistency was looked at here rather than the consistency of actual behaviour and this appears to be closely linked, at least for the present sample, to the construct of neuroticism. Rated consistency of neuroticism and rated consistency of extraversion (sociability), though not rated consistency of impulsivity, were both found to correlate with self rated neuroticism. Being a correlation it is not clear, however, whether Ss tended to rate themselves as being more neurotic because they saw themselves as inconsistent, or as inconsistent because they were neurotic. Though these results seem to accurately reflect the commonly held view of the neurotic, inconsistency does not form part of Eysenck's construct of neuroticism.

Similarly though Eysenck was at pains to remove any correlation between the E and N scales of the EPI, the correlation for the present sample was -.19(N.S.). When subjects rated themselves a correlation of -.316($p < .025$) between self rated extraversion and self rated neuroticism emerged, suggesting that at least in the "common view" an

individual who is introverted is also likely to be somewhat neurotic.

In summary, then, despite the fact that a clear and significant difference emerged in RTs to recognition of nonsense syllables due to differential reinforcement none of the predictions drawn from Eysenck's, Gray's or Spence's theory were confirmed to a significant degree. Some results were found to be in the direction predicted by the theories of Gray and Eysenck, but, particularly with respect to Gray's theory, these results lacked the consistency to suggest that they might be due to other than chance factors.

It was suggested that predictions drawn from these theories might have a more limited usefulness than is at present implied by the theories themselves.

There were some significant differences, however, between the personality groups. The introverted groups, as compared to the extraverted group was found to show a more rapid extinction and to be generally more aware of the reinforcement contingencies.

The results also suggest that inconsistency might generally be viewed as an aspect of neuroticism, but despite this there is no evidence, as least when people are asked to rate their consistency, that the consistency is a single generalized factor.

CHAPTER IX: EXPERIMENT 3

9:1 ABSTRACT

Predictions were drawn from the theories of Eysenck, Spence, Roessler and Gray and tested in an operant conditioning situation in the present experiment. This type of conditioning was used in order to permit the suggestion made in the previous experiment, that different personality dimensions might be relevant to different conditioning situations, to be put to the test.

As the predictions drawn from Eysenck's theory, that introverts condition more efficiently than extraverts, were confirmed it was concluded that the above suggestions had not been supported.

The theories of Gray, Spence and Roessler received no support. The low ego-strength, and the high neuroticism groups were found to use the response key to a greater extent than the other groups. It was suggested that this might well reflect a difficulty on the part of these individuals in inhibiting responses.

Two findings reported in the previous experiment failed to be replicated here: rated consistency was not found to correlate with rated neuroticism; consistency ratings were found to correlate moderately well with each other.

Extraversion was found to correlate with the indices of conditioning, these indices correlated only poorly with each other, it was therefore questioned whether it was reasonable to assume that there is a general factor of conditionability.

9:2 INTRODUCTION

It was proposed in experiment 2 that perhaps those theories which relate a personality dimension to conditionability might be wrong not so much in kind as in degree. That is, certain personality traits might favour conditioning in certain situations. Now this is a somewhat disappointing conclusion to come to in the present context, where it has been suggested that if a personality trait could be identified which was related to a general factor of conditionability then it might be possible to weld the idiographic and nomothetic approaches to personality study into a single, and ultimately more useful approach.

The present speculations should not be taken as a counsel of despair, however. While on one hand there may be several factors of conditionability, all of approximately equal importance, on the other there may be a single, major, general factor of conditionability with several minor factors, each with some relationship to a personality trait, contributing relatively minor amounts of observed variance.

The other point worth considering, before speculation runs riot, is that as yet these suggestions are untested hypotheses. In the first experiment there appeared to be some evidence that the stable (LN) Ss displayed superior conditionability, but it was felt that the results of this experiment must be treated with extreme caution, particularly in view of the fact that the LN and HN groups were found to be behaving significantly differently before reinforcement was introduced.

In the second experiment it was Eysenck's theory which fared best, or, more accurately, least badly. Thought none of the predictions drawn from this theory received significant support, some results were in the direction predicted by the theory, while those results which were statistically significant were

interpretable, though not unequivocally so, as supporting the theory.

The purpose of this third experiment is to put the earlier speculation to the test by employing an experimental design which is, sufficently similar to experiment 2 to permit comparison of the two sets of results but which employs an operant conditioning technique as opposed to the classical conditioning paradigm used in experiment 2.

In view of the suggestions being made here it is note worthy that Spence and Spence (1966) presented the following Table (Table 9:1) comparing high and low anxiety Ss in conditioning studies in which a masking task had been employed.

TABLE 9:1 STUDIES OF PERFORMANCE OF LA AND HA SCALES S WHEN A MASKING TASK HAS BEEN USED.

Study	N	Difference in % CRs (H - L)
1. Spence & Rutledge (1964)	17	- 2.2
2. Homzie (1964)	48	-1.5
3. Spence (1966)	48	2.8
4. Spence (Unpublished)	48	4.3
5. Spence (Unpublished)	28	11.3
6. Spence (Unpublished)	37	-9.8

None of these studies produced significant results, and the fact that in three out of the six studies the LA group displayed better conditioning than the HA groups puts things pretty much at chance level. What is noteworthy about these studies is simply that the inclusion of a masking task in the experimental design results in the disapperance of the impressive superiority, in conditioning, of the HA groups, so often reported by Spence.

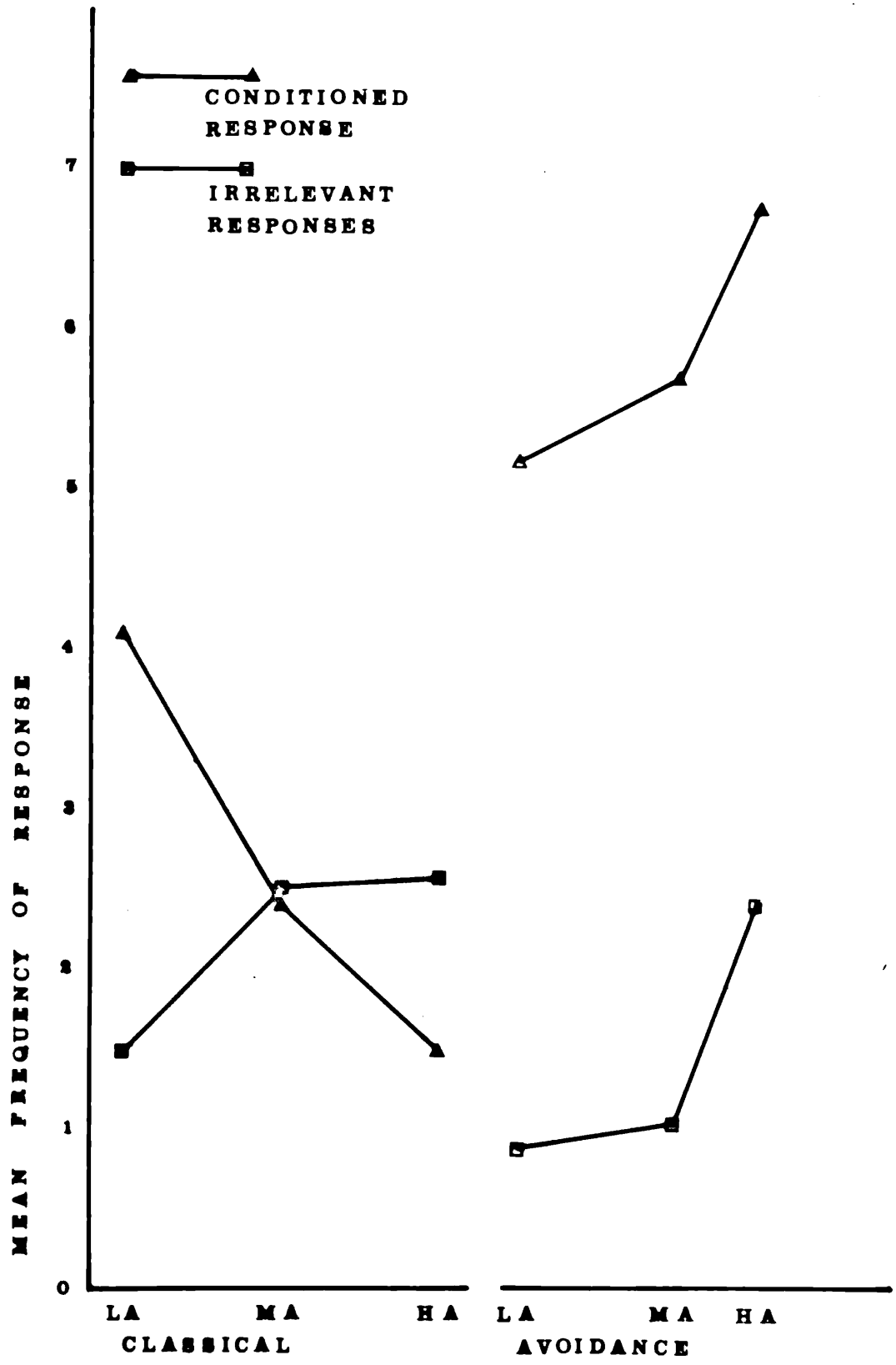


FIG. 9:1a

PERFORMANCE OF HIGH, MEDIUM AND LOW ANXIETY GROUPS UNDER CLASSICAL AND AVOIDANCE CONDITIONING

FROM ELIAS 1965, p. 115

A study by Elias (1965) might also be regarded as relevant here. Elias, using the MAS as a basis for categorizing Ss, compared a classical with an operant conditioning situations. He compared three levels of anxiety, HA, MA, LA, with two intensities of electric shock, 30 - 70 A versus 75 - 125 A. For half the Ss shock always followed the CS, a buzzer, (classical conditioning); for the rest of the Ss the CR resulted in avoiding the shock (avoidance conditioning). In both the experiments reported avoidance conditioning proved to be superior to classical conditioning. In the first experiment HA Ss showed the highest level of conditioning and LA Ss the lowest under low intensity shock conditions. The position was reversed for the high intensity shock condition. This result was not replicated in the second experiment, however.

In the second experiment an interesting finding was reported. It may be recalled that in Chapter One there was some discussion of the fact that individuals may appear inconsistent because they are focusing on different aspects of the situation to those considered by the observer. In Chapter Two it was commented that the actors and the observer's constructs might not overlap and "to the extent that they do not the individual will appear, from the observer's point of view, inconsistent, or to have failed to learn a particular response".

In his second experiment Elias recorded not only CRs, as is customary, but also the number of irrelevant responses emitted by the S. The interesting point is that in the classical conditioning situation it is the LA group which produces both most CRs and fewest irrelevant responses. In the operant conditioning situation the irrelevant responses are quickly extinguished for the LA group, but go on increasing in frequency for the HA group. (cf. Fig. 9:1), despite this the HA group also produce more CRs, though not significantly

more, than the LA group.

Elias comments on his results as follows: "These data suggest that the function of drive is to energize general defence responses, while UCS avoidance acts to selectively reinforce a particular response. In classical conditioning, where shock could not be avoided, finger withdrawal and irrelevant responses were acquired at about the same rate and reached the same level. In avoidance conditioning, where shock was avoided by a finger withdrawal, the finger-withdrawal response showed a progressive acquisition throughout conditioning as irrelevant responding extinguished.

Interference of irrelevant responses with the conditioning of finger withdrawal may explain the relatively poor performance of the classical conditioning group". (Elias, 1965, p.115).

It seems then that the situation may be still more complex than initially suggested; not only may personality factors contribute some variance, even if not all, to conditioning, but if the response acquired is not the one designated as the CR then good conditioners may well be classified as poor conditioners, especially if the "irrelevant CR" interferes in some way with the performance of the CR.

On the basis of Elias' results, however, it seems that such interference is less likely to take place in operant than in classical conditioning, perhaps because the CR in operant conditioning has a functional value and so all operant conditioning situations involve discrimination learning.

There seems little point in further speculation here until data are available which might be called in support of, or against, any hypotheses made. The present experiment, then, is similar to experiment two in that it employs nonsense syllables paired with two types of reinforcement, appetitive

and aversive. It is different from the previous experiment in that reinforcement is contingent on certain responses being made or withheld. The CR to avoid punishment was to press a response key; the CS was a three letter nonsense syllable characterized by the letter "I" ("U" for half the Ss).

The CR to gain the positive reinforcement was to avoid pressing the key; the CS was a three letter nonsense syllable characterized by the letter "U" ("I" for half the Ss)

Positive reinforcement was gaining 2p, negative reinforcement losing 2p.

9:3 HYPOTHESES

The hypotheses here were substantially the same as those in experiment two:

Hypothesis 3:1 There will be a significant conditioning effect, reflected in there being significantly more successful presses of the reponse key to the aversive CS.

Hypothesis 3:2 On the basis of Eysenck's theory it is predicted that there will be an introversion-extraversion x type of reinforcement interaction.

It is predicted that the results will indicate that the introverts will show superior conditioning.

Hypothesis 3:3 On the basis of Gray's theory it is predicted that

(i) there will be a significant introversion-extraversion x type of reinforcement interaction; introverts showing the superior conditioning in the punishment condition, extraverts in the reward condition.

(ii) There will be a neuroticism x type of reinforcement interaction, the HN group displaying the superior conditioning.

(iii) there will be an introversion - extraversion x neuroticism x type of reinforcement interaction.

Hypothesis 3:4 On the basis of Roessler's theory it is predicted that there will be a significant Es x type of reinforcement effect, with high Es scorers displaying the superior conditioning.

9:4 METHOD

Design

Subjects completed the EPI (form A), giving measures of extraversion and neuroticism, a self-rating scale, giving ratings of extraversion (sociability), impulsivity, neuroticism and consistency for each of these traits, and the Barron's Ego-Strength (Es) scale from the MMPI. These, together with two types of reinforcement, appetitive and aversive, were employed as the independent variables in the various analyses performed in the present experiment.

An operant conditioning paradigm was employed. The experimental situation was so designed that 500 msecs after a nonsense syllable was projected on to a screen, clearly visible to the S, the number shown on one of two counters, situated immediately in front of the S, increased by one. The S was told that he/she would receive 2p for each point on the left hand counter (appetitive reinforce), but lose 2p for each point on the right hand counter (aversive reinforcer).

60 nonsense syllables were presented. Partial reinforcement was used, 23 syllables out of 30 per condition being reinforced (76.67%). Reinforcement was random, and syllables were presented in random order.

Two sets of syllables were used, one characterized by the central vowel I, the other by the vowel U. These syllables had been used in experiment 2. The association between these vowels and reinforcement was random across Ss.

The S was able to prevent the number on the counters

*1. An outline of Roessler's theory is provided in Appendix I

increasing by pressing a key within 500 msec.s of the syllable appearing. The syllable was visible to the S for one second, both counters were permanently visible.

A number of scores were obtained from each S:

(i) The number on the positive counter at the end of the experiment i.e. the amount the S gained. ("Positive" or "Reward" score).

(ii) The number on the negative counter at the end of the experiment i.e. the amount the S lost. ("Negative" or "Punishment"score).

(iii) The Ss "score" - the number on the positive counter minus the number on the negative counter. ("Difference" score)

(iv) The number of times the S pressed the key.

Apparatus

As mentioned above introversion - extraversion and neuroticism were measured by the EPI (form A), and Es by Barron's (1956) scale from the MMPI. The self rating scale was the same as that described in experiment 2.

Syllables were projected on to a screen by a carousel projector. The image of a letter on the screen measured11.5 cms x 2.5 cms. The screen was 127 cms from the S, the digital counters 52 cms from the S.

The experimenter controlled which, if either, of the two counters would show an increase on any particular trial. He also had control of the interval between syllables. Additional circuitry made it possible for the S to prevent the number on the counters from increasing, by pressing a key within the first 500 msec.s that the syllable was visible. Pressing the key after this had no effect.

Procedure

53 volunteer Ss took part in the experiment. All Ss were students or academic staff of the university. No S who had taken part in experiment 2 was used as a subject in this experiment. It was decided to exclude these potential Ss because it was felt that the designs of the two experiments were similar enough to facilitate awareness of the reinforcement contingencies. This might have distorted any results obtained, it would certainly limit their generality.

As in the previous experiment Ss scoring 11 or more on the EPI E or N scales were classified as extraverted (HE) or neurotic (HN), as appropriate, for the purpose of analysis. Those scoring 10 or less were classified as introverted (LE) or stable (LN), as appropriate.

Three Es groups were used, high Es (HEs) Ss scoring 54 or more on the Barron Es scale, medium Es (MEs) Ss scoring 40 - 53, and low Es Ss scoring less than 40.

Again as in the previous experiment Ss scoring 10 or more on the relevant rating scales were classified as extraverted, those giving a rating of 6 or more on the impulsivity scale were classified as impulsive, and those giving a rating of 6 or more on the neuroticism scale as neurotic.

Three S were omitted from the analyses because they had scores of over 5 on the EPI lie scale. Four further S had also to be omitted from the analyses due to an error in reinforcement, such that on some trials the wrong reinforcer was paired with the syllable.

Ss completed an EPI and an Es scale, one before and one after the experiment, (the order was randomized across Ss), and a self rating scale, again completed either before or after the experiment with the order randomized across Ss.

These were not scored until sometime later and so,

during the experiment, the experimenter had no idea to which personality category the S would be allocated.

After completing the personality questionnaire the S was given a typed sheet containing the following instructions.

"Instructions

What you will be doing in this experiment is playing a game, and the object of the game will be to earn as much money as possible.

This is what will happen: a 3 letter syllable will be projected on to the screen; half a second later, while the 3 letters are still visible, the number on one or other of the two counters will increase by 1 unless you press the key. If you press the key neither counter will increase. There will also be times when even though you do not press the key neither counter will increase.

You will receive 2p for each point on the LEFT hand counter, but, (the catch), you will LOSE 2p for each point on the RIGHT hand counter. That is you will get 2p for each point remaining when the number on the RIGHT hand counter is taken from the number on the LEFT hand counter

E.G:

LEFT	RIGHT
00025	00012

in this case you would get 25 - 12 = 13 x 2p, that is 26p. Any questions?"

The key and counters were pointed out to the S so as to avoid any confusion. The S was allowed to place the key wherever he found it most comfortable. By looking straight ahead both screen and counters were clearly visible to the S.

Three syllables not used in the experiment proper, YAD, XXX, HOT, were used to demonstrate the procedure. Each S was allowed to practice until he felt confident enough to

start the experiment. A minimum of 10 practice trials were given.

The pattern on which the counters increased or failed to increase during the practice trials was random and bore no relationship to the reinforcement contingencies of the experiment proper. The S was informed of this.

Immediately before the first syllable of the experiment proper was shown the experimenter said to the S, "Are you sure you understand what you have to do? You have to devise a strategy, or discern a pattern which will allow you to predict which counter will increase, and decide whether or not to press the Key and stop the counters from increasing."

60 syllables were then presented in random order. The inter trial interval was random in length with a mean value of approximately 3.5 secs.

The experimenter recorded on each trial whether or not the S had pressed the key, but, because the counters were not visible to him, he was unable to record whether or not the response had been "successful" i.e. whether it had been made within the initial 500 msec.s and so prevent the counter from increasing. At the conclusion of the experiment he recorded the numbers on the two counters and paid the S his/her "winnings". On several occasions he had to refuse to take money from Ss who had so entered into the spirit of things that they wanted to pay the experimenter because they had "lost".

At the end of the experiment the S was asked a series of questions similar to those asked in the two previous experiments, and again used to classify Ss as "aware" or "unaware".

1. "What did you think was going on during the experiment?"

This question elicited a correct description of the reinforcement contingencies from almost all Ss who were

TABLE 9:2 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES

Source	S.S.	DF	M.S.	F	P
Between		45			
Extraversion (E)	0.198	1	0.198	<1	N.S.
Neuroticism (N)	5.847	1	5.847	<1	"
E x N	0.595	1	0.595	<1	"
Ss within groups	448.81	42	10.686		
Within		46			
Type of reinforcement (R)	408.49	1	408.49	16.352	.001
E x R	39.768	1	39.768	1.592	N.S.
N x R	10.521	1	10.521	<1	"
E x N x R	3.153	1	3.153	<1	"
R x Ss within groups	1049.19	42	24.981		
Total		91			

TABLE 9:3 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES. ("EXTREME" scores only)

Source	S.S.	DF	M.S.	F	P
Between		23			
Extraversion (E)	5.361	1	5.361	<1	N.S.
Neuroticism (N)	15.73	1	15.73	2.337	"
E x N	0.037	1	0.037	<1	"
Ss within groups	135.207	20	6.76		
Within		24			
Type of reinforcement (R)	313.618	1	313.618	11.767	.005
E x R	74.792	1	74.792	2.806	N.S.
N x R	26.313	1	26.313	<1	"
E x N x R	44.292	1	44.292	1.662	"
R x Ss within groups	533.068	20	26.653		
Total		47			

TABLE 9:4 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS

Source	S.S.	DF	M.S.	F	P
Between		45			
Extraversion (E)	0.36	1	0.36	<1	
Neuroticism (N)	9.64	1	9.64	<1	
E x N	36.54	1	36.54	3.78	
Ss within Groups	405.63	42	9.66		
Within					
Type of Reinforcement (R)	367.78	1	367.78	13.48	$<.005$
E x R	0	1	0	<1	
N x R	30.82	1	30.82	1.13	
E x N x R	2.18	1	2.18	<1	
R x Ss within groups	1145.63	42	27.28		
Total		91			

TABLE 9:5 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS ON IMPULSIVITY AND NEUROTICISM

Source	S.S.	DF	M.S.	F	P
Between		45			
Impulsivity (I)	1.09	1	1.09	<1	
Neuroticism (N)	7.91	1	7.91	<1	
I x N	38.36	1	38.36	4.1	$<.05$
Ss within groups	404.47	42	9.63		
Within		46			
Type of Reinforcement (R)	368.6	1	368.6	13.6	$<.005$
I x R	0.45	1	0.45	<1	
N x R	32.27	1	32.27	1.19	
I x N x R	8.00	1	8.00	<1	
R x Ss within groups	1138.7	42	27.11		
Total		91			

TABLE 9:6 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS, WITH THOSE USING THE MID POINT OF THE SCALE (5) OMITTED

Source	S.S.	DF	M.S.	F	P
Between		28			
Impulsivity	6.35	1	6.35	< 1	
Neuroticism	1.38	1	1.38	< 1	
I x N	13.32	1	13.32	1.63	
Ss within groups	204.51	25	8.18		
Within		29			
Type of Reinforcement (R)	263.37	1	263.37	8.94	< .005
I x R	3.2	1	3.2	< 1	
N x R	2.42	1	2.42	< 1	
I x N x R	6.35	1	6.35	< 1	
R x Ss within groups	736.83	25	29.47		
Total		57			

TABLE 9: 7 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EGO-STRENGTH SCORES

Source	S.S.	DF	M.S.	F	P
Between		45			
Ego-strength	9.55	2	4.78	< 1	
Ss within groups	440.17	43	10.24		
Within		46			
Type of Reinforcement (R)	443.62	1	443.62	17.79	< .005
Es x C	78.44	2	39.22	1.57	
R x Ss within groups	1071.97	43	24.93		
Total		91			

eventually classified as "aware". However, if it did not elicit a correct description the following questions were asked.

2. "Were you aware of any pattern in what was happening in the experiment?.
3. "Did you notice any relationship between what was on the screen and what was on the counters?"

Most Ss had formed some kind of hypothesis, or tested out some strategy. If it had not been the "right" one Ss were aware of it and would add something like: "But it didn't work", or "It worked for a while and then started to go wrong", to their descriptions.

Results

The mean scores on the EPI scales for the present sample were comparable with those of the previous experiments: E 11.7 (S.D. 4.55), N 9.65 (S.D. 5.6). The mean for the Es scale was 45.09 (S.D. 8.77).

Conditioning

It seems appropriate that the first result to be reported should be that dealing with conditioning. The interpretation of the other results rests, to a considerable extent, on this one. If no conditioning had taken place there would be no reason to expect the "positive" and "negative" scores to reflect anything other than chance variations. On the other hand "positive" scores ought to be significantly larger than "negative" scores if conditioning had taken place. "Positive" scores were found to be significantly larger than "negative" scores. (cf. Tables 9:2 - 9:7).

In experiment two the subject's awareness of the prevailing reinforcement contingencies appeared to have no

TABLE 9:8 ANALYSIS OF VARIANCE COMPARING 'AWARE' WITH 'UNAWARE' Ss USING 'DIFFERENCE' SCORES (REWARD SCORE MINUS PUNISHMENT SCORE).

Source	S.S.	DF	M.S.	F	P
Awareness	1774.801	1	1774.801	133.167	< .001
Residual	586.417	44	13.328		
Total	2361.217	45			

TABLE 9:9 ANALYSIS OF VARIANCE COMPARING 'AWARE' AND 'UNAWARE' Ss IN THEIR TRENDS OF USING THE KEY IN FACE OF CUES OF REWARD

Source	S.S.	DF	M.S.	F	P
Awareness	55.00	1	55.00	8.711	< .005
Residual	277.804	44	6.314		
Total	332.804	45			

TABLE 9.10 ANALYSIS OF VARIANCE COMPARING 'AWARE' AND 'UNAWARE' Ss IN TRENDS OF RESPONDING TO CUES OF PUNISHMENT

Source	S.S.	DF	M.S.	F	P
Awareness	108.528	1	108.528	25.543	< .001
Residual	186.95	44	4.249		
Total	295.478	45	6.566		

TABLE 9:11 ANALYSIS OF VARIANCE COMPARING REWARD AND PUNISHMENT SCORES IN THE 'UNAWARE' GROUP

Source	S.S.	DF	M.S.	F	P
Between Ss	330.6	29	11.4	2.724	N.S.
Within Ss	128.0	30	4.267	1.02	N.S.
Type of Reinforcement	6.67	1	6.67	1.59	N.S.
Residual	121.33	29	4.184		
Total	458.6	59			

significant effect on the degree to which an individual conditioned. Here, however, a significant difference was found between Ss classified as "aware" and those classified as "unaware". Table 9:8 summarizes the results of an analysis of variance where the S's score, ("positive" score minus "negative" score), was used as the dependent variable. The "aware" Ss display the superior conditioning.

This measure, of course, reflects both appetitive and aversive conditioning. The same story emerges, however, if we look, instead, at the trend of responding in the reward (Table 9:9) and punishment (Table 9:10) conditions separately. The "aware" Ss still appear to show superior conditioning. In fact, when these subjects were excluded from the analysis, leaving only the "unaware" Ss, the significant conditioning effect disappeared. (Table 9:11).

At this juncture one might debate whether or not it might be desirable to exclude the "unaware" Ss from any further analyses, particularly those purporting to test hypotheses drawn from the personality theories. Whatever arguements might be offered either in favour of, or against, such an exclusion such a step proved to be, in practical terms, impossible. 16 Ss (34.78%) were classified as "aware", too few to permit the calculation of any meaningful analyses after Ss were allocated to the various personality groups.

Eysenck

As can be seen (Tables 9:2 - 9:6) the interaction between introversion - extraversion and type of reinforcement (E x R) was not found to be significant. It was this interaction one would expect to be significant if Eysenck's theory of personality were correct. The analyses summarized in Tables 9:2 to 9:6, as has already been remarked, compare "positive" with "negative" scores, thus reflecting both appetitive and

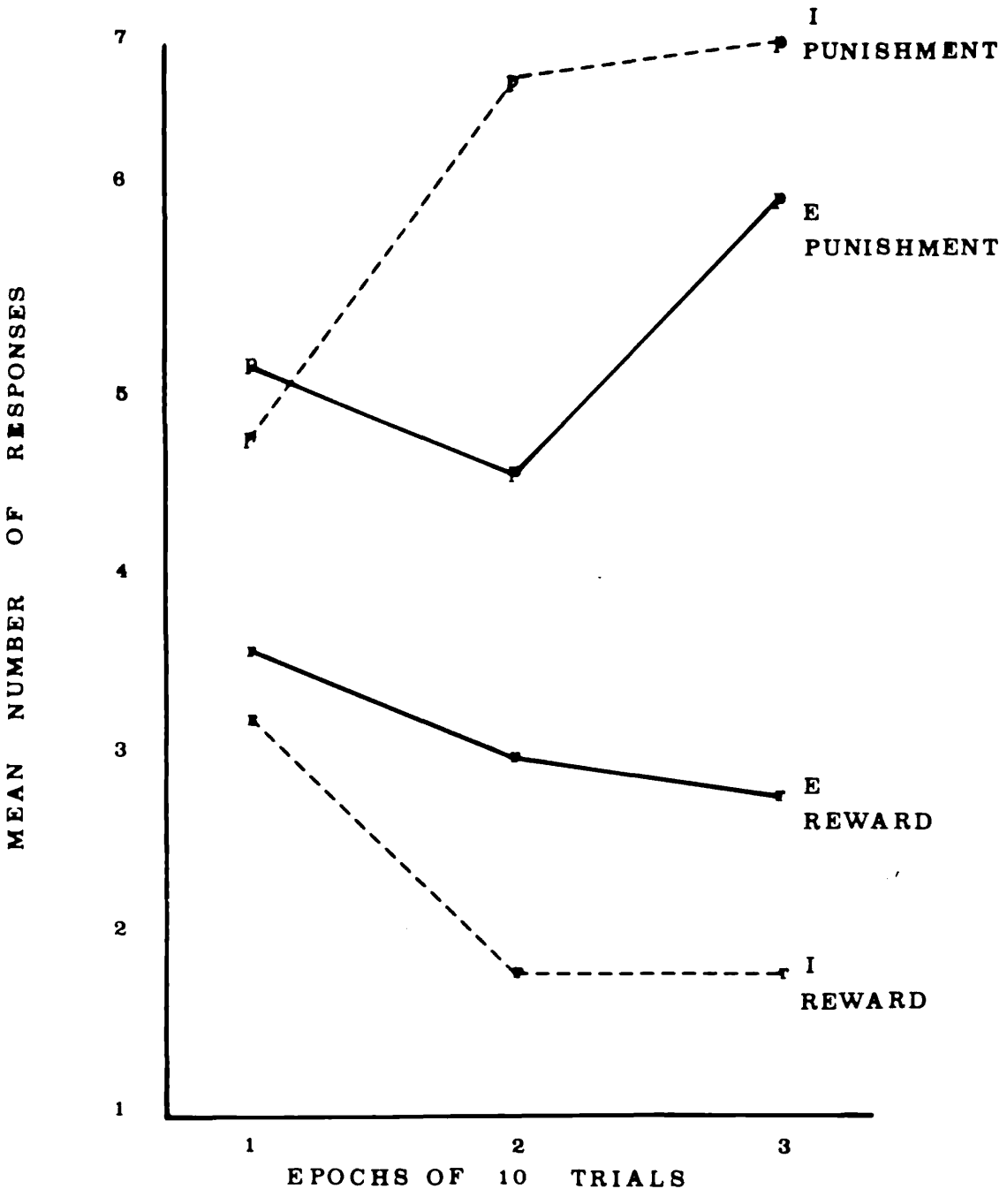


FIG. 9:1 RESPONSES PER EPOCH AS A FUNCTION OF EXTRAVERSION AND TYPE OF REINFORCEMENT

TABLE 9:12 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES ('EXTREME' SCORES ONLY)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	202.500	1	202.500	3.617	<.035*
Neuroticism (N)	147.556	1	147.556	2.635	
E x N	87.774	1	87.774	1.568	
Residual	1119.808	20	55.99		
Total	1377.833	23			

* 1 tailed

TABLE 9:13 ANALYSIS OF VARIANCE OF 'DIFFERENCE' SCORES. Ss ALLOCATED TO PERSONAL GROUPS ON THE BASIS OF EPI SCORES

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	63.626	1	63.626	1.231	N.S.
Neuroticism (N)	29.034	1	29.034	0.562	
E x N	4.355	1	4.355	0.084	
Residual	2171.238	42	51.696		
Total	2286.978	45			

TABLE 9:14 ANALYSIS OF VARIANCE OF CONTRAST IN REWARD AND PUNISHMENT TRENDS IN THE USE OF THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	52.900	1	52.900	3.457	<.05*
Neuroticism (N)	15.577	1	15.577	1.018	
E x N	17.88	1	17.88	1.168	
Residual	306.033	20	15.320		
Total	363.958	23			

* 1 tailed

aversive conditioning. When the difference between these two scores, rather than the magnitude of the scores, was used as the dependent variable then the tendency towards significance, apparent in Table 9:3, was found to be fully significant, ($p < .035$, 1 tailed). (Table 9:12). These two analyses, (summarised in Tables 9:3 & 9:12), include data only from Ss scoring half a standard deviation or more from the mean, "extreme" scorers, on the EPI scales. This had the effect of reducing the sample size from 46 to 24.

The results of these analyses were in the direction predicted by Eysenck. The greater the degree of differential responding by the S the larger this "score" will be. The means indicate that it was the introverted group which showed the greater differentiation between the two sets of cues. The group means were as follows: Introverts - 7.727

Extraverts - 3.46

When all Ss were included, though the mean scores were in the same direction the difference between the introverted and extraverted groups was no longer significant. The means were as follows: Introverts - 5.93

Extraverts - 2.871

Fig. 9:1 illustrates the differential responding of the two groups. After 10 trials both introverts and extraverts were using the response key less in the face of reward cues than in the face of punishment cues, suggesting that some conditioning had already taken place. At this stage there was little difference between the two groups in their level of responding, however. By the end of the 30 trials per condition the introverted group was responding more appropriately to both cues, least often in the reward condition and most often in the punishment condition. The fact that the introverted group displayed steeper slopes in both reward and punishment conditions

TABLE 9:15 ANALYSIS OF VARIANCE OF CONTRAST IN REWARD AND PUNISHMENT TRENDS IN THE USE OF THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	17.371	1	17.311	0.912	
Neuroticism (N)	28.396	1	28.396	1.588	
E x N	3.763	1	3.762	0.211	
Residual	750.82	42	17.877		
Total	799.304	45			

TABLE 9:16 ANALYSIS OF VARIANCE OF CONTRAST IN THE LINEAR TRENDS OF USING THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (PUNISHMENT CONDITION)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	25.737	1	25.737	3.811	<.03*
Neuroticism (N)	8.843	1	8.843	1.309	
E x N	12.617	1	12.617	1.868	
Residual	283.654	42	6.754		
Total	311.413	43			

* 1 tailed

TABLE 9:17 ANALYSIS OF VARIANCE OF CONTRAST IN THE LINEAR TRENDS OF USING THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY) PUNISHMENT CONDITION

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	36.1	1	36.1	4.318	.024*
Neuroticism (N)	7.633	1	7.633	0.913	
E x N	21.337	1	21.337	2.552	
Residual	167.208	20	8.36		
Total	204.00	23			

* 1 tailed

TABLE: 9:18 ANALYSIS OF VARIANCE OF CONTRAST IN THE LINEAR TRENDS IN THE USE OF THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES REWARD CONDITION

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	0.82	1	0.82	0.118	
Neuroticism (N)	13.772	1	13.772	1.989	
E x N	0.681	1	0.681	0.098	
Residual	290.778	42	6.923		
Total	317.935	45			

TABLE 9:19 ANALYSIS OF VARIANCE OF CONTRAST IN THE LINEAR TRENDS IN THE USE OF THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY) REWARD CONDITION

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	1.6	1	1.6	0.338	
Neuroticism (N)	1.969	1	1.969	0.415	
E x N	0.058	1	0.058	0.012	
Residual	94.8	20	4.74		
Total	103.833	23			

TABLE 9:20 ANALYSIS OF VARIANCE OF THE PUNISHMENT SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	60.143	1	60.143	4.016	$<.05$
Neuroticism (N)	41.377	1	41.377	2.3	
E x N	22.562	1	22.562	1.28	
Residual	359.5	20	17.975		
Total		23			

TABLE 9:21 ANALYSIS OF VARIANCE OF THE REWARD SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY)

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	20.06	1	20.06	1.19	
Neuroticism (N)	0.66	1	0.66	< 1	
E x N	21.72	1	21.72	1.29	
Residual	336.105	20	16.805		
Total	7899	23			

TABLE 9:23 ANALYSIS OF VARIANCE OF 'TOTAL' SCORES. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	48.188	1	48.188	2.492	
Neuroticism (N)	8.832	1	8.832	0.457	
E x N	71.651	1	71.651	3.705	0.058
Residual	812.135	42	19.337		
Total	906.804	45	20.151		

TABLE 9:24 ANALYSIS OF VARIANCE OF 'TOTAL' SCORES Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF SELF-RATINGS OF IMPULSIVITY AND NEUROTICISM

Source	S.S.	DF	M.S.	F	P
Impulsivity (I)	56.467	1	56.467	2.588	
Neuroticism (N)	11.929	1	11.929	0.547	
I x N	118.823	1	118,823	5.446	0.023
Residual	916.362	42	21.818		
Total	1068.609	45	23.747		

was reflected in the significant difference found between the introverted and extraverted groups when the two trends are constrasted, at least when the analysis is limited to "extreme" extraverts and introverts. (Table 9:14).

When the two reinforcement conditions were considered independently of each other it was found that the introverts conditioned significantly more quickly in the punishment condition, (Tables 9:16 & 9:17), but not in the reward condition, (Tables 9:18 & 9:19), though the results were in the same direction in this condition.

These analyses employing the amount of, or trends of, responding as the dependent variable reflect all responses both those which were effective, i.e. within the 500 msec. time limit, and those which were not. Effective responding is reflected in the "positive" and "negative" scores. When these two were analysed separately and data from only Ss classified as extremely introverted and extraverted were included in the analysis then we find, once more, that there is a significant difference between the introverted and extraverted groups for the punishment condition, but not for the reward condition. (Tables 9:20 & 9:21)

A similar result was reported in the previous experiment and one is tempted to suggest that Eysenck's contention, that introverts display superior conditioning, is limited to situations of aversive conditioning. However, if we consider the pattern of results, rather than what is found to be significant and what is not, we find that Fig. 9:1 seems clearly to suggest that the introverted group were displaying superior conditioning in both reward and punishment condition. The reason for the lack of significance in the reward condition seem less likely to be due to a limitation on the part of Eysenck's theory and more likely to be due to a "floor" effect.

As can be seen from the figure the mean level of responding for the introverted group has dropped to 1.8 per ten trials by the second epoch (trials 11 to 20) and remain at this level for the final epoch.

Alternatively the difference between the two conditions might be due to the subjective intensity of the two stimuli. Though the reward and punishment stimuli were objectively equivalent, the gain and loss of 2p, this does not guarantee subjective equivalence.

Taking the overall pattern of the results in to consideration it must be concluded that Eysenck's theory emerges from this experiment with considerable support. Introverts do appear to show superior conditionability as compared with extraverts.

It was earlier reported that "aware" Ss were found to condition while "unaware" Ss were not, at least not to a significant degree. Having now concluded that introverts condition more efficiently than extraverts, and bearing in mind that in experiment two it was found that there was a greater tendency for introverted Ss, than for extraverted Ss, to become aware of the prevailing reinforcement contingencies, it seemed probable that here, too, we would find this same tendency. This was not found to be the case, however. A Chi squared test was calculated but no significant difference was found in the proportion of introverts and extraverts classified as "aware". The differences were, however, substantially in the direction one might have expected (cf. TABLE 9:22)

TABLE 9:22 "AWARENESS" AS A FUNCTION OF INTROVERSION-EXTRAVERSION

	Introverts	Extraverts
Aware	7	9
Unaware	8	22

GRAY

Turning from the success of the predictions drawn from Eysenck's theory to Gray's theory the story seems to be quite different. There is little in the data from this experiment which might be construed as supporting Gray's theory. The E x R interaction, it has already been remarked, appears to support Eysenck rather than Gray. It would be flying in the face of the general trend of the results to insist that the introverted group displays significantly superior conditioning in the aversive condition, and this is as Gray would predict.

The sum of the scores on the two counters reflects the number of "successful" responses, i.e. responses made within 500 msec.s. The more often the response key was used within this time limit the smaller this "total" score. On the basis of Gray's theory it might be expected that the neurotic introvert, the HNLE, group, as it is the group hypothesized as being most sensitive to cues of punishment, would use the response key most often, and the neurotic extravert, or the stable extravert, the HNHE and the LNHE groups, use it the least.

When Ss were allocated to personality groups on the basis of their EPI scores, no significant E x N interaction was evident. This interaction was, however, found to be significant when Ss were allocated to personality groups on the basis of their own ratings (Tables 9:23 & 9:24). Despite the significance of this interaction little support can be gleaned from this result in favour of Gray's theory. As predicted the HNLE group, (and the HNLImp.) group provided the lowest score, but contrary to the predictions of the theory the highest score was provided by the LNLE, (and LNLImp.), group. (cf Tables 9:25a & b).

TABLE 25a MEAN "TOTAL SCORES FOR FOUR PERSONALITY GROUPS. Ss ALLOCATED TO GROUPS ON THE BASIS OF THEIR SELF RATINGS.

	Introversion	Extraversion
LN	32	29.45
HN	27.71	30.8

TABLE 25b MEAN "TOTAL" SCORES FOUR HIGH AND LOW, SELF RATED IMPULSIVITY AND NEUROTICISM GROUPS

	Cautious	Impulsive
LN	31.55	29.13
HN	27.33	30.7

One might challenge the interpretation of this result pointing out that Gray argues, not that the introverted group is more sensitive to punishment, but that this group is relatively more sensitive to cues of punishment. Ss in this group will, therefore, be expected to learn these cues relatively more quickly and, in consequence, respond more often in face of such cues, rather than, as suggested above, generally using the response key more often.

As the E x N x R interaction (Tables 9:2 - 9:6) was not found to be significant, however, there appears to be no support for the hypothesis that the introverted group do learn the cues of punishment more quickly than the cues of reward. They do, of course, learn to respnnd to the cues of punishment more quickly than the extraverted group.

Nevertheless the point of the analysis of "total" scores was that if Gray's theory is correct one might expect different patterns of errors from those who are differentially more sensitive to cues of reward and those differentially more sensitive to cues of punishment. The latter group might be expected to make more errors of commission, resulting in a lower "total" score; the former group more errors of omission, resulting in a larger "total" score. However, as already reported despite the fact that a significant E x N interaction was found when these scores were analysed, the pattern of results does not provide any support for the hypotheses drawn from Gray's theory.

TABLE 9:26 ANALYSIS OF VARIANCE OF THE No. OF TIMES DIFFERENT PERSONALITY GROUPS PRESS THE RESPONSE KEY. Ss ALLOCATED TO PERSONALITY GROUPS ON THE BASIS OF EPI SCORES (EXTREME SCORES ONLY).

Source	S.S.	DF	M.S.	F	P
Extraversion (E)	4.9	1	4.9	0.225	
Neuroticism (N)	142.277	1	142.277	6.544	0.018
E x N	2.199	1	2.199	0.101	
Residual	434.833	20	21.742		
Total	662	23			

TABLE 9:27 ANALYSIS OF VARIANCE OF NUMBER OF TIMES THE THREE EGO-STRENGTH GROUPS USE THE RESPONSE KEY

Source	S.S.	DF	M.S.	F	P
Ego-strength	206.464	2	103.232	2.604	$< .05$*
High Es Vs. Low Es	127.864	1	127.864	3.225	$< .05$*
Residual	1704.688	43	39.644		
Total	1911.152	45			

* 1 tailed

NEUROTICISM

In none of the analyses reported has any difference been found between LN and HN, or LEs and HEs, groups. Thus the theories of Gray, Spence and Roessler all failed to find confirmation of their hypotheses in the present experiment.

When all responses are included in the analyses, rather than only those made within the 500 msec. time limit, a significant difference was found between high and low neuroticism, and high and low Es groups. (Tables 9:26 & 27). When the analysis was limited to Ss at the extremes of the personality dimension it was found that HN Ss, and LEs Ss, used the response key significantly more often than the LN and HEs groups. The means for these groups were as follows:

HN - 28.07, LEs - 28.36

LN - 21.9 HEs - 23.57

These differences may, of course, be due to any one of a number of factors e.g. slower RTs on the part of the HN and LEs groups, maladaptive responding, slow decision making, the inability to inhibit a response. The data are not available, here, which would allow one to choose between the alternatives with any confidence. It is, however, worth noting that these results may be seen as being in line with those reported by Elias (1965) and briefly outlined in the introduction to this experiment.

CORRELATIONS

Despite the significant differences found between the introverted and extraverted groups EPI E scores were not found to correlate significantly with either the "positive" score, ($r = -.166$), or the"negative" score ($r = .0809$). These correlations are, however, in the direction predicted by Eysenck, rather than that predicted by Gray's theory.

If there were a general factor of conditionability one would expect these two indices, the "positive" and "negative" scores, to correlate substantially with each other. The correlation observed, however, was -.251 ($p < .05$, 1 tailed), which though significant can hardly be regarded as impressive.

It was found in the previous experiment that Ss who rated themselves as high on neuroticism also tended to rate themselves as inconsistent. This finding was not replicated here, as can be seen from Table 9:28

TABLE 9:28 CORRELATIONS BETWEEN EPI AND SELF RATED N AND SELF RATED CONSISTENCY

	EPI N	SELF RATED N
Rated consistency of extraversion	-.069	-.076
Rated consistency of impulsivity	-.124	-.022
Rated consistency of neuroticism	-.207	-.251

The consistency scores were not found to intercorrelate in the previous experiment and it was then suggested that, at least for self rated consistency, consistency might be regarded as being trait specific. This result was not replicated here. Self rated consistency scores were found to correlate moderately well with each other in the present experiment (cf. Table 9:29).

TABLE 9:29 CORRELATIONS BETWEEN RATINGS OF CONSISTENCY

1. Rated consistency for extraversion	1.00		
2. Rated consistency for impulsivity	.467	1.00	
3. Rated consistency for neuroticism	.45	.404	1.00
	1	2	3

($p < .006$ for all correlations)

(TABLE 9:30 provides a full correlation matrix)

TABLE 9:30 CORRELATION MATRIX OF VARIABLES USED IN EXPERIMENT 3

1. Extraversion	1.0												
2. Neuroticism	.0922 (.543)	1.0											
3. Ego-strength	-.0603 (.691)	-.5801 (.001)	1.0										
4. Reward Score	-.1663 (.270)	.0343 (.821)	-.0853 (.573)	1.0									
5. Punishment Score	.0809 (.594)	-.2262 (.131)	-.0165 (.914)	-.2507 (.093)	1.0								
6. Difference Score	-.1807 (.23)	.1545 (.306)	-.1061 (.484)	.7129 (.001)	-.7769 .001	1.0							
7. Total Score	.09 (.552)	-.1670 (.268)	.0135 (.929)	.3935 (.007)	.7046 .001	-.2843 (.056)	1.0						
8. No. of Presses	.0305 (.841)	.334 (.024)	-.2007 (.182)	-.3011 (.043)	-.5676 (.001)	.2121 .154	-.7374 (.001)	1.0					
9. Rated E	.7714 (.001)	-.0468 (.758)	.1325 (.381)	-.1412 (.35)	.0361 (.812)	-.185 .219	.0267 (.861)	.0597 (.694)	1.0				
10. Rated Consistency of E	.0692 (.648)	-.0694 (.647)	-.1886 (.21)	-.1865 (.215)	-.1308 (.387)	.0517 (.733)	-.2637 (.077)	.323 (.029)	-.0665 (.661)	1.0			
11. Rated Impulsivity	.3667 (.013)	.1049 (.488)	.0926 (.541)	-.1262 (.404)	-.1988 (.186)	.0596 (.694)	-.1622 (.282)	.1438 (.341)	.4297 (.003)	.0195 .898	1.0		
12. Rated Consistency of Impulsivity	-.2327 (.12)	-.124 (.412)	.0354 (.816)	.1559 (.301)	.0044 (.977)	.1203 (.426)	.0744 .624	.0893 (.556)	-.191 (.204)	.45 (.002)	-.2591 (.083)	1.0	
13. Rated N	-.2576 (.001)	.6252 (.038)	-.3071 .222	.1836 (.136)	-.2233 (.155)	.2136 (.783)	-.0418 (.291)	.1594 (.005)	-.4097 (.615)	-.0763 (.726)	-.0532 (.884)	-.0221	1.0
14. Rated Consistency of N	-.0043 (.978	-.2071 .168	-.0683 (.652)	-.137 (.364)	-.1599 (.389)	.136 (.368)	-.2494 (.095)	.2165 (.148)	-.0444 (.77)	.4671 (.002)	.0547 (.718)	.4042 (.006)	-.251 (.093)

TABLE 9:31: SUMMARY OF ANALYSES OF VARIANCE

Basis on which Ss were allocated to Cells / ANALYSES	EPI	EPI (Extremes)	Self-ratings	Self-rated Impulsivity	Impulsivity "Extremes"	Ego-Strength	"Aware" vs "Unaware"	"Unaware"
Raw Scores	Type of Reinforcement ($p < .001$) (Table 2)	Type of Reinforcement ($p < .001$) (Table 3	Type of Reinforcement ($p < .005$) (Table 4)	Type of Reinforcement ($p < .005$) Impulsivity x neuroticism ($p < .05$) Table 5	Type of Reinforcement ($p < .005$) (Table 6)	Type of Reinforcement ($p < .005$) (Table 7)		N.S. (Table 11)
Reward Scores		N.S.						
Punishment Scores		Extraversion ($p < .05$; 1 tailed)						
"Difference" Scores	N.S. (Table 13	Extraversion ($p = .069$; 2 tailed) (Table 12)	N.S.	N.S.	N.S.	N.S.	Awareness ($p < .001$) (Table 8)	
"Total"	N.S.	N.S. (Table 21)	Extraversion ($p = .118$) E x N ($p = .058$; 2 tailed) (Table 23)	Impulsivity ($p = .111$) I x N ($p = .023$; 2 tailed) (Table 24)	N.S.	N.S.		
"Press" Scores	N.S.	Neuroticism ($p = .018$, 2 tailed) (Table 26)	N.S.	N.S.	N.S.	Ego-Strength MEs vs LEs ($p < .05$, 1 tailed) (Table 27)		
Contrast of Reward and Punishment Trends	N.S. (Table 15)	Extraversion ($p = .075$, 2 Tailed) (Table 14)	N.S.	N.S.	N.S.	N.S.	Awareness ($p < .001$)	
Reward Trends	N.S. (Table 18)	N.S. (Table 19)	N.S.	N.S.	N.S.	N.S.	Awareness ($p < .005$) (Table 9)	
Punishment Trends	Extraversion ($p = .055$, 2 tailed) (Table 16)	Extraversion ($p = .048$ 2 tailed) (Table 17)	Extraversion ($p = .087$), E x N ($p = .082$, 2 tailed)				Awareness $p < .001$ (Table 10)	

DISCUSSION

One might regard the present experiment as testing hypotheses at three distinct levels. At the most fundamental level the aim was to produce a predictability in behaviour by systematically associating certain outcomes with specified cues - to condition a response. At the next level interest was focused on the differential success of conditioning with relation to certain personality dimensions. At the third level the pattern of this differential conditionability as compared to the pattern in the previous experiment was the focus of interest.

At the first level a clear and significant effect was observed, Ss used the response key more often within 500 msec.s of the onset of the stimulus when it was a cue of punishment than when it was a cue of reward. In other words Ss learned to gain rewards by inhibiting a response and to avoid punishments by responding. Most Ss who applied this strategy successfully were aware of the reinforcement contingencies obtaining in the present situation and as a result a significant difference was found in the behaviour of Ss classified as "aware" and those classified as "unaware". No such difference was found between Ss classified in this way in the previous experiment, it may be recalled. This difference is not so surprising, however, as in the present experiment a voluntary response was conditioned, while the perceptual defense and sensitization of the last experiment are, presumably, involuntary responses. In the present experiment the S had to make a decision on each trial whether or not to use the response key to prevent the counters increasing. If he chose not to use the key feedback was immediately available as to the appropriateness of the choice. It seems certain that if a S hit upon the correct strategy, in this situation, he

would immediately become aware of it.

It seemed, watching the Ss, that, for most of those who became aware of the reinforcement contingencies, the "correct" response was made on several trials and then, suddenly, he/she became aware of what was happening, after which no more errors were made.

This was not the universal rule, however. One S simply watched the counters and syllables for several trials, making no attempt to respond. After he had "solved the problem" he responded on every trial "correctly". This, of course, is the most appropriate way to behave, for only by refraining from responding is new information available. It is appropriate, however, only if one wishes to gain the reward, if avoiding punishment is upper most in ones mind using the key on every trial would be the most "appropriate" behaviour.

It is possible that this S should have been excluded from the analyses, but he was not. Such an exclusion would have been equivalent to accepting the proposition that conditioning is a mechanical process and occurs without awareness. It was not desired to espouse any position on the learning with/without awareness controversy. Though this S may have contributed some artifactual error in a "pure" study of conditioning. It was felt that no such artifact was present in this situation where one wants to generalize the results to the type of learning described by Bandura (1977), and Mischel (1973) and outlined in Chapter 2. If different personality groups "learn" in different ways then it seems appropriate to include all such variance, rather than to exclude it and be left with a relatively pure measure which reflects the learning strategy or processes of only a small proportion of individuals.

Turning to the next level, the differential

conditioning of various personality groups, it is Eysenck's theory which emerges with the honours. This theory predicts that, given that certain parameters are employed introverts will show superior conditionability as compared with extraverts. Significant differences were found between the introverted and extraverted groups, at least the more extreme scoring introverts and extraverts, when the difference between the "positive" and "negative" scores were compared for these two groups. These two groups also displayed significantly different trends across the 30 trials per condition in the use of the response key in the face of the two sets of syllables, cues of punishment and of reward. The differences were always in the direction predicted by the Eysenckian model. Similarly "extreme" introverts were found to have significantly lower "punishment" scores than "extreme" extraverts. Whereas the introverts had the higher score in the reward condition, though not, this time, to a significant degree.

As pointed out in the results section, once again there is a suggestion that the introverted group do condition better when an aversive stimulus is employed. It seems possible, on the basis of the present data at least, however, to distinguish between the two possibilities (a) the introverted group are more sensitive to cues of punishment than they are to cues of reward, or (b) the aversive stimulus used here is more aversive than the appetitive was rewarding. The problem was given some thought when the experiment was designed, and it was for this reason that the reward and punishment stimuli were made objectively equivalent, either the gain or loss of 2p. Yet somehow it seems that losses are more salient than gains. Perhaps the subjective experiences of gaining and losing objectively equal amounts are never equal, merely that they are "less unequal" for some

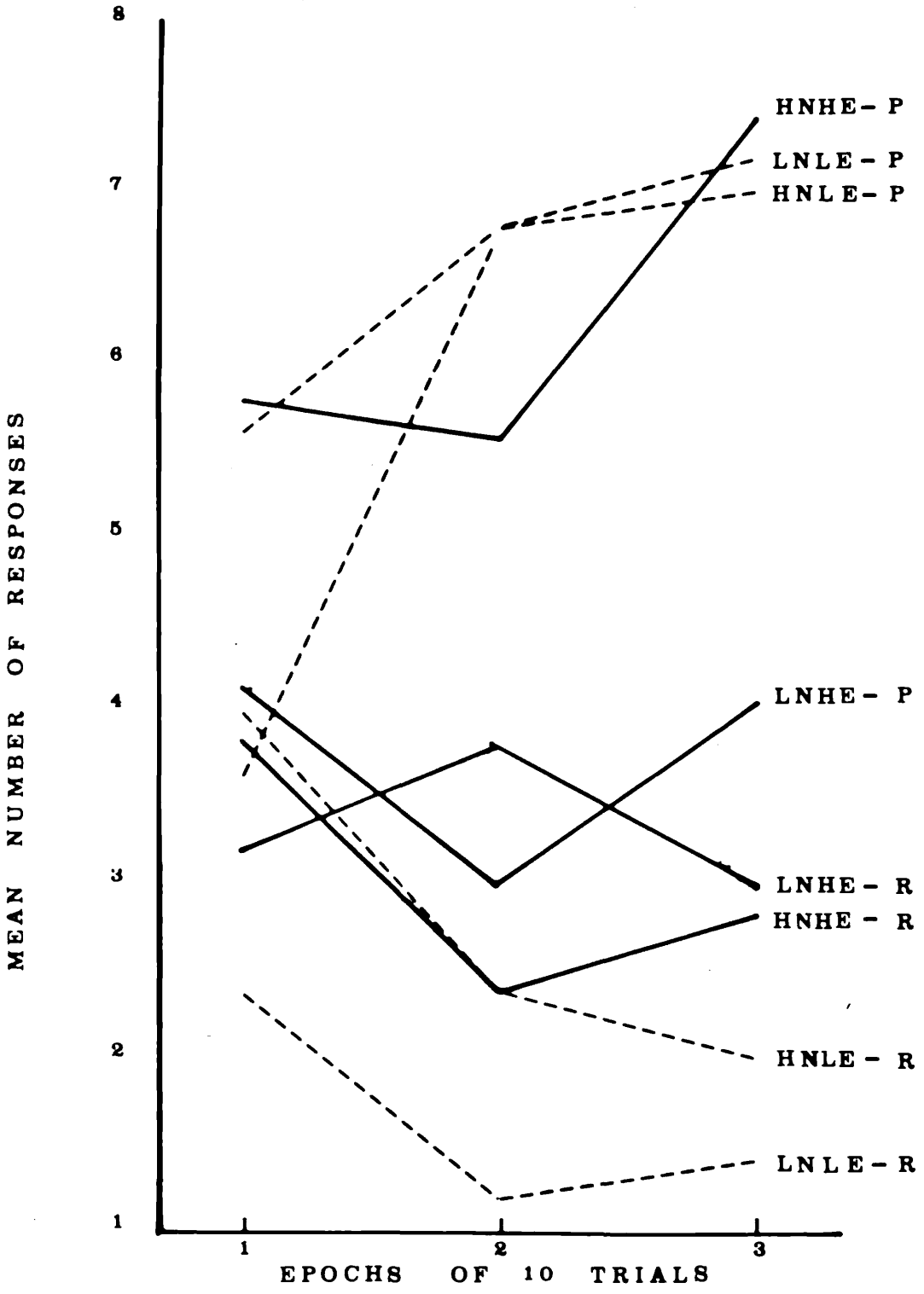


FIG. 9:2 RESPONSES PER EPOCH AS A FUNCTION OF EXTRAVERSION, NEUROTICISM AND TYPE OF REINFORCEMENT

(P = PUNISHMENT CONDITION,
R = REWARD CONDITION)

groups than for others.

I might, at this stage, be accused of being an apologist for the Eysenckian model, and attempting to obscure the fact that the results, at least partially, confirm the predictions of Gray. On the one hand we have the introverts conditioning significantly better than the extraverts in the aversive conditioning situation, as Gray's model predicts, while in the reward condition no significant difference between the groups is found, suggesting that either the extravert group is showing better conditioning, in this situation, or the introverted group worse. Whichever of these suggestions is true it may be interpreted as supporting Gray's model.

While these observations are true, in as far as they go, they do not really go far enough. It has already been noted that the real reason there is no significant differences between the introvert and extraverted groups in the reward condition might be that there is a "floor" effect distorting the results, to some degree, in this condition. (cf. Fig 9:1)

From Fig. 9:2 it can be seen that the results are not at all as Gray would predict. It is the HNHE group which uses the response key most often in face of the punishment, and the LNLE group who use it least in face of the reward cue. In as far as these data can be regarded as reflecting conditioning, as they employ responses both within and out side the 500 msec.s time limit, it appears that the LNHE group show no real evidence of conditioning in either condition, while the HNHE group conditions more effectively in the punishment, than in the reward condition. None of this is predictable on the basis of Gray's theory.

The same story appears to be told if the group scores for "reward" and "punishment" are compared:

TABLE 9:32 GROUPS MEANS FOR "REWARD" AND "PUNISHMENT" SCORES

	INTROVERTS		EXTRAVERTS	
	Reward	Punishment	Reward	Punishment
LN	19.4	11.2	15.6	16.4
HN	17.8	10.5	17.9	11.8

If individuals are responding at a chance level we would expect the score to be not significantly different from 15. If conditioning were taking place we would expect a score lower than 15 in the punishment condition, and greater than 15 in the reward condition. As can be seen in both conditions the LNHE group give mean scores of approximately 15. In the punishment condition the rank order is HNLE LNLE HNHE LNHE. In the reward condition the rank order is LNLE HNHE HNLE LNHE. These results really cannot be viewed as supporting Gray's model.

At first sight it might appear that this experiment is not quite a fair test of Gray's theory. In the aversive condition an active avoidance response is required and it was earlier pointed out that the theory clearly predicts that introverts show superior conditioning of passive avoidance. Nevertheless the introverts here show the superior conditioning, and it was argued that where mildly aversive stimuli are employed it might be justifiable to expect the introverted group to condition better than the extraverted group in both active and passive avoidance conditions.

It is the reward condition which highlights another potential weakness of the theory. From the model presented by Gray we expect the extraverted group to condition more efficiently to all cues of reward. The model assumes, however, that these rewards are obtained as the result of some activity.

The reward side of the model does not have access to the inhibitory mechanism, this is controlled by the punishment side of the model. In the present experiment, however, reward is obtained only by inhibiting a response. What is to be predicted? Is it the Ss with the differentially more sensitive reward mechanisms or those who are better at passive avoidance who display the superior conditioning in this type of situation? The data here suggests that it is the latter, in which case Grays model needs to be modified. If it is the former then the hypothesis is not confirmed. In either case Gray's model emerges from this experiment both needing modification and being more limited in its predictions than was at first thought.

In the effort to relate the findings of the present experiment to some theoretical model we must not miss a point which is emerging as a rather consistent effect that the LNHE group, the stable extraverts, seems to demonstrate especially poor conditionability. It is also note worthy that if both the theories of Eysenck and Spence were correct it would be this group with its low cortical excitation and its low drive which would be expected to show the poorest conditionability.

There is, however, no other support evident in these data which might be construed as confirming Spence's position. Indeed the only significant difference found between LN and HN groups, or between high and low Es groups, was found in the number of times the response key was used. As there were no differences between high and low neuroticism or Es, groups in respect of the "Total" score this significant result must indicate that the HN Ss used the response key more often than the LN Ss after the 500 msec. time limit had expired.

One might regard this as an example of maladaptive responding, at least to the degree that until one knows the reinforcement contingencies, by using the response key one loses information. One knows before hand that neither counter will increase after the response key has been used. On the other hand, as already remarked, if avoiding punishment is uppermost in ones mind then pressing the response key may not be at all maladaptive. One might be willing to interpret this result as being in line with Roessler's hypothesis that low Es individuals behave in a generally more defensive manner, if this group had used the response key more often than the high Es group both during and after the 500 msec. time limit, rather than only after the 500 msec. time limit had expired. the suggestion that neurotic Ss have slower RTs might be more tenable if they had used the response key less often than the LN Ss within the time limit, but they did not.

One solution to the problem might be that the HN, and LEs, Ss are generally more responsive but have slower RTs, on average. This solution is not likely to be true, however, for although 500 msec.s cannot be called a long time it is much longer than most RTs. The problem might be, of course, that the S not only had to make a simple response, but prior to that had to make a decision about whether or not to make the response at all. Perhaps the HN and LEs groups were slower in making this decision, but are also inclined to be more responsive.

This explanation is not really satisfactory. One would expect slow decision makers to be less successful, and there is no evidence here, or in the previous experiments, that this is the case. An alternative is that the HN and LEs individuals simply have difficulty in inhibiting responses. In the present experimental situation this "difficulty" may be

obviated by simply delaying the response, rather than inhibiting it totally. Though the data are not available here to chose between these alternative suggestions this last one does at least have the advantage that it corresponds with the commonly held view of the "nervous" person as being "jumpy" and "full of nervous energy." It also fits the pattern of results reported in the introduction from Elias (1965). In Elias' experiment while "irrelevant" responses were found to extinguish from low anxiety Ss, they were found to increase over trials for the high anxiety Ss.

Turning now to the general pattern of results, it was suggested at the end of the previous experiment that theories may not have the generalized predictive ability they are often assummed to have, but are limited, in their predictive ability, to particular types of situation. This type of arguement in turn led to the question of whether or not there was a single generalized factor of conditionability.

Theories such as Eysenck's suggest that there is such a factor. At least to the degree that the results of the previous experiment were explicable in terms of his theory and the predictions drawn from his theory were confirmed in the present experiment, it seems that his contention is supported. However, if there is such a factor, and, as Eysenck maintains, the introversion - extraversion factor is related to it, why have predictions drawn from the theory fared so poorly in the present series of experiments when the experiments were designed specifically to demonstrate the superior conditioning of the introverted group?

Even in the present experiment the two conditioning indices, the "reward" and "punishment" scores were not found to correlate significantly with the introversion-extraversion

dimension. Indeed, the correlation between these two indices, though significant (-.251, $p < .05$, 1 tailed), is hardly of the magnitude to persuade one they reflected the same general factor.

Several authors have commented on this problem. Morgenson and Martin (1969), for example, reported a factor analytic study by Prescott (1964) in which it was found that (a) there was no relationship between the conditioning of electrodermal activity(EDA) and introversion-extraversion, and (b) that the various methods of measuring EDA conditioning were independent.

Davidson et al (1964) reported that conditioned EDA and conditioned finger withdrawal did not correlate in normal Ss.

In a study by Mangan (1974) which looked at EDA conditioning to slides of nude females, and also at the orienting response (OR), the initial amplitude of the response, sensitivity and habituation rate in the tactile, visual and auditory modalities, spiral after effect, sex drive and imagery in a factor analytic study, he concluded: "Under the present experimental conditions, it is clear that the psychophysiological variables are the most important sources of individual differences in the acquisition and extinction of sexual CRs, and that cognitive and personality variables are largely irrelevant". (p.134)

Eysenck, himself, has considered the problem and observes that "the results (of correlational studies) between different types of conditioning are relatively low and may even be near zero." (1965, p.267). He even quotes Moore and Marcuse (1945) as arguing that "the concept of good and bad conditioners must always be with reference to a particular response".

However, Eysenck argues that peripheral factors must also be taken into account and their influence partialled out. Parameters of theoretical relevance must also be adequately controlled, of course.

Following the same line of reasoning Lovibond (1964) has listed four factors which may attenuate the observed correlations:

(i) variability in peripheral response mechanism sensitivity;

(ii) non-associative contamination of conditioning measures. "To the extent that non-associative factors not highly correlated with conditionability enter differentially into various types of conditioning measures, the observed relationships between the measures will be spuriously low". (p.142)

(iii) Central set influences in conditioning measures.

(iv) Procedural variations in conditioning; this is particularly important where the parameters, suggested by the theory to be of some relevance, are not adhered to.

Davidson et al (1964), using university students as Ss found no relationship between EDA conditioning and finger withdrawal, as has already been reported. However, in a later study (1966) which used hospitalized female neurotics as Ss the same two measures were found to be significantly related. Following up these findings (1968) they concluded that the rate of conditioning was specific to the response studied for "normal" Ss. However, in the neurotic group conditionability is found to be rather more generalized. These results are strangely reminiscent of those reported by Raush and Moos when looking at personal consistency. There it was found that the person was often a more important source of variance than the situation for the clinical groups.

Davidson et al concluded: "In view of the disparate

nature of the eyeblink conditionability in normals, the positive results relating eyeblink conditioning to personality measures become rather trivial and isolated findings and the theories predicting these relationships lose much of their heuristic importance."

Mangan (1974) makes a similar point. The Eysenck and Levey (1972) paper confirms the predictions of the theory, he concedes. Does it, he questions, necessarily support Eysenck's position on the relationship between the assumed neurological differences and observed behaviour, "since there are no grounds for assumming that 'under-arousing' conditions favouring introverts are more likely to be encountered during the course of development than 'over-arousing' conditions." (p.126) He goes on to point out that personality accounts for only a small percentage of the reliable variance observed in his experiment, despite the fact that extremes of the personality dimension were used. "On this evidence," he says, "the infered neurological processes hardly qualify as critical determinants of personality organization." (p.126)

Franks (1964), seems no more hopeful that a general factor will be demonstrated, despite the fact that data from his earlier experiments are often quoted in support of Eysenck's position. Commenting on a paper by Livingston (1964) he said "In the absense of a general factor of conditionability over and above specific factors pertaining to variables such as those listed above, the need for precision in replication becomes paramount. Even if a general factor (or factors) could be demonstrated, the need for precise replication would assume importance inversely to the amount of variance accounted for by the general factor in the specific conditioning situation under examination. Unfortunately, there is no evidence of a general factor, precise replication of

circumstances known to be pertinent is difficult, many factors are little understood and some unknown." (1964. p.550)

The present experiment employed a paradigm quite different from that normally employed when testing hypotheses drawn from Eysenck's theory, yet the theory still received substantial support. It would be wrong, however, to attempt to argue from this that, contrary to the views of the various authors cited above, these results indicate that a general factor of conditionability exists. Taking the results of this experiment together with those of experiments one and two, Eysenck's theory has received little convincing support. The difficulty in reliably eliciting superior conditioning from the introverted group suggests rather that, as Managan pointed out, there may be some truth in the theory, one might even claim that it has some generality, but the proportion of variance in conditioning contributed by the introversion-extraversion dimension is usually small, even trivial, and as a consequence of this the theory loses its heuristic value. If this conclusion is correct then Eysenck's theory loses its value as a framework which might be used to unite the idiographic and nomothetic approaches. One might even go further and suggest that as the indices of appetitive and aversive conditioning showed such a poor correlation no theory which assumes a single general factor of conditionability will, ultimately, be found to be useful. Gray's theory, of course, does not assume such a single factor, but no evidence has been found, as yet, which may be regarded as supporting this theory.

In summary then the prediction drawn from Eysenck's theory that, given appropriate conditions, introverts will display better conditioning than extraverts has been confirmed, though rather more strongly in aversive than in appetitive conditioning. Despite this confirmation, however, the

correlations between the measure of introversion-extraversion and the conditioning indices were non-significant suggesting that this dimension contributes but a small amount of the variance observed in conditioning. Indeed, considering the size of the correlation between the "reward" and "punishment" scores it was questioned whether one could assume there was any general factor of conditionability.

If the support for Eysenck's theory was equivocal then the other theories considered, those of Gray, Spence and Roessler, fared even worse for they received no support at all. Though as in the previous experiment the stable extravert group (LNHE) were found not to display any evidence of conditioning.

Ss high in neuroticism, or low in Ego-strength were found to use the response key more often than stable, or high Es, Ss but only after the 500 msec. time limit had expired. It was suggested that this might be because these Ss have difficulty in inhibiting a response. It was also suggested that this result might be seen as consonant with that reported by Elias (1965).

The finding reported in the previous experiment that rated consistency was related to rated neuroticism was not replicated in the present experiment. In the last experiment it was suggested that the consistency ratings indicated that, at least for rated consistency, the trait specific rather than the generalized factor approach might be correct. In the present experiment these results were reversed, the three consistency ratings correlating moderately well with each other.

GENERAL DISCUSSION

The general bias of the speculation put forward so far is that individuals do not display an absolute consistency in their behaviour, and in consequence an adequate appreciation of

the individual must wait upon an idiographic description, particularly one which gives pride of place to the individual's learning history.

At the same time traits are seen as useful general descriptive terms. It was suggested that some traits might be substantially more important than others. If a trait could be identified which was more, or less, closely identifiable with a general factor of conditionability, then, it was argued, such a trait might be used as the basis of a theory which integrated both the idiographic and nomothetic approaches to personality study. The final form, and ultimate utility, of the theory would depend on the characteristics, particularly physiological characteristics, associated with the conditionability factor.

Two theories were seen as particularly promising because, besides relating a personality trait to conditionability, they also provided a physiological underpinning to explain the differential conditionability. These two theories were expounded by Eysenck and Gray. The first experiment reported here asked, simple mindedly, as these two theories predicted that different groups of individuals will condition more efficiently, which is correct? The answer was not simple. The data were not clear cut, but if any group was to be singled out as "the good conditioners" it appeared to be the low neuroticism group. Not the group either theory predicted.

This first experiment had employed an operant paradigm and a social reinforcer. The same question was asked again in experiment 2, but this time using a classical conditioning paradigm and physical reinforcement. This time the effect of conditioning was clear, but the low neuroticism group was no longer found to be identified with efficient conditioning. Eysenck's theory received only the most tentative support and Gray's none at all. The data suggested that rated consistency might, on one hand, be trait specific and, on the other hand be

related to neuroticism.

The original simple question was fading. Perhaps, it was speculated, as individuals condition efficiently enough, the trait dimensions are limited in their importance to specific situations. Experiment 3 used the operant paradigm again, but this time with physical reinforcement. Again there was a clear conditioning effect. Eysenck's theory received considerable support and Gray's none at all. The question posed in experiment 1 was, then, eventually answered in experiment 3, but the two intervening "failures" pre-empted any enthusiastic theory-building.

Behaviour seemed more complex, the results relating neuroticism and rated consistency, and the suggestion that consistency was trait specific were not replicated. When it was asked how important the introversion-extraversion dimension was in determining conditioning, the answer was, "not very".

The results of these experiments seemed to confirm those summarized by Lovibond in 1964. He remarked: "Willet (1960) used the Fisher Z transformation to obtain an estimate of the true correlation between eye-blink acquisition score and the E scale. The estimate, which was based on the studies of Franks (1957), Das (1957), O'Connor (1959) and Willet (1960), was -.188". (p.119).

In experiment 3 a surprisingly similar correlation was found between E and conditioning ("difference" score) of -.1807 (p = .23). The other correlations were lower; positive conditioning and E -.1663 (N.S.); negative conditioning and E .081(N.S.).

The introversion-extraversion dimension, then, seems unlikely to be explaining much more than about 4% of the variance in conditinning. Even Eysenck does not appear to

wish to claim that it contributes substantially more. Commenting on a paper by Franks (1963) he says, "neither do they (the results of the study) disprove the hypothesis under investigation, that is, that E and conditioning correlate -.3 approximately". (1965, p.262).

If extraversion explains such a small percentage of the variance in conditioning, and its effect is so easily swamped by other variables, then it seems hardly likely that it will be found useful as a basis for integrating the idiographic and nomothetic approaches. Indeed one might agree with the opinions expressed by Davidson et al (1964) and Mangan (1974) when they observe that if one needs to establish such precise conditions before predictions from Eysenck's theory can be confirmed, and if even then the extraversion dimension accounts for only a small proportion of the observed variance, then the theory loses much of its heuristic value, and hardly qualifies as a critical determinant of personality organization.

CHAPTER X: EXPERIMENT 4

10:1 GENERAL INTRODUCTION

The present study is concerned with the question of whether or not it is possible to predict that certain groups of people will commit certain mistakes. The study is divided into three quite distinct parts.

The first part, experiment 4a, follows up the suggestion made in discussing the results of the last experiment, that perhaps high N scoring individuals have difficulty in inhibiting responses.

The second section, experiment 4b, looks at the possibility of identifying Ss who have a tendency to make errors of commission. It is speculated that the nervous system property of mobility and/or perseveration may be related to such a tendency.

The third section of the study used data drawn from the two early sections to question an assumption inherent in them both, and which was challenged in earlier chapters: that individuals are relatively consistent in their behaviour.

10:2: EXPERIMENT 4A

Introduction

It was found in the previous experiment, experiment 3, that the HN group tended to respond after the time limit for effective responding had expired. As this group was no more, or less, successful than the LN group at the task in hand it was suggested that one possible explanation for this phenomenon might be that the HN individual experiences difficulty in inhibiting a response. The present experiment was designed to put this hypothesis to the test.

On the basis of the results reported by Elias (1965) one might suggest that in the normal course of events the HN individual is especially prone to develop more than one response to any given stimulus, with the result that he never finds himself in the position of having to inhibit, completely, all tendency to respond; one response - though in an experimental situation, not necessarily the one recorded by the experimenter - will always be available and permissible. This being the case the HN S, in experiment 3, may well have found it "easier" to delay a response for around 200 msec.s than to inhibit it altogether.

One might extend this hypothesis and suggest that the HN individual represents one end of a dimension. Individuals at this, HN, end of the dimension while being ill suited for situations which require the ability to readily inhibit responses, are ideally suited for certain other situations. Individuals at the opposite end of the dimension are similarly well suited to some, but poorly suited to other situations. Perhaps rather than talking about situations one ought to refer to strategies of coping in, or dealing with, situations. Thus the two extreme groups would be ideally suited to employ certain strategies of coping, but rather poorly suited to

others, while individuals situated at the mid-point of such a dimension might be able to utilize all coping strategies with moderate success.

One might argue that such a dimension made more "biological sense" than the more traditional view which places the neurotic or anxious individual at one end of the dimension, with his "typical" behaviour having a distinctly maladaptive flavour, with the stable individual at the other end of the dimension being eminently well adjusted - a state of affairs which leads one to question why natural selection does not work against such a maladapted group.

If these suggestions are to any degree correct, in the present experiment where the S is required to respond to a simple visual stimuli either the more quickly he is required to respond, or the more alike the "respond" and "do not respond" stimuli are, the more errors of commission the HN individuals will make.

HYPOTHESES

It is predicted then, that as the task becomes more difficult errors will be found to increase

Hypothesis 4a:1 There will be a significant location of stimulus effect, with a greater number of

(i) commission errors

(ii) omission errors

being associated with the central position.

Hypothesis 4a:2 There will be a significant rate of presentation effect with the greater number of

(i) Commission errors

(ii) omission errors

(iii) anticipations

being associated with the faster rate of presentation.

It is also predicted that errors of commission will be associated with the neuroticism dimension.

Hypothesis 4a:3(i)There will be a significant neuroticism effect, the LN group making fewer commission errors than the HN group.

(ii) N may be found to interact with location of the stimulus, little difference between the LN and HN groups at the more extreme positions, significant differences at the central position

(iii) N may be found to interact with rate of presentation, the more pronounced differences between the LN and HN groups being evident at the faster rate of presentation.

(iv) N may be found to interact with both rate of presentation (s) and location of stimulus (p) i.e. N x S x P. The most pronounced differences between the LN and HN groups occuring at the faster rate of presentation in the central position.

10:3 METHOD

Design

The stimulus used in this experiment was a single point of light. This could appear, on each trial, in any one of 6 positions along the horizontal mid-line of an osciloscope screen. These positions were as follows: 1.637 cms to the left (central left) or right (central right), 4.91 cms to the left (middle left)or to the right (middle right), or 8.185 cms to the left (extreme left) or to the right (extreme right) of the vertical mid-line of the osciloscope screen.

Neither horizontal nor vertical mid-line was marked on the screen. Dots appeared one at a time and randomly with respect to position, with the restriction that dots appeared 20 times in each of the 6 available locations. Thus there were 120 trials per run.

Each S completed two runs, each one of 120 trials.

In one of these stimuli were presented at a rate of 1 per 500 msec.s, this was the "Fast" condition. In the "slow" condition stimuli were presented at a rate of 1 per second. These two conditions were presented in random order across Ss.

The S's task was to respond whenever the dot was to the left, (to the right for 50% of Ss), of centre, and to refrain from responding when ever it was to the right (left for 50% of Ss).

Ss responded to the same side in both conditions, "Fast" and "Slow".

The following data were collected for each location on each trial:

(i) The response latency

(ii) If a response was made, whether or not it was correct or an error of commission.

(iii) if no response had been made, whether or not this was correct, or an error of omission

(iv) responses of less than 100 msec.s were classified as anticipatory responses.

Ss were allocated to personality groups on the basis of their EPI scores. The same criterion being used as in earlier experiments. This gave a 4 way factorial design for both commission and omission errors: two levels of E, two levels of N, two rates of presentation and three locations.

Procedure

60 Ss took part in this experiment, but two of these were excluded from the analyses. The data of one of these Ss was lost due to a computer failure, the other S failed to complete the personality questionnaires.

All Ss were either students or academic staff of the university, all were volunteers. Ss of both sexes were

included in the experiment.

The presentation of stimuli was controlled by, and data were recorded by, computer. Ss were seated in front of a teletype approximately 4ft from an osciloscope screen. This screen was 4ft 8 ins from the floor and measured 21.6 x 17 cms.

The subject was instructed as follows:

"Points of light, dots, will appear one at a time along this (the horizontal) midline. What I'd like you to do is to press this bar, (the space bar), as quickly as you can whenever the dot is to the left (right) of centre. The centre will not be marked you'll have to judge where it is yourself".

The two conditions, "fast" and"slow", were presented in random order across Ss. On the second run the S was warned, as appropriate, that the rate of presentation would be faster, or slower than in the previous run.

A tone sounded 7 sec.s before the first stimulus appeared on the screen.

All Ss also completed the EPI (Form A), the Es scale and the self-rating scale which had been used in experiments 2 and 3.

10:4 RESULTS

As can be seen from Table 10:1 no differences were found between the HN and LN groups, nor was there an interaction between neuroticism and the speed of presentation, nor neuroticism and the location of the stimulus; nor was the three way interaction found to be significant. It seems, therefore, that the proposed hypothesis has received no support.

It was suspected at one stage that no significant difference would be found simply because few commission errors

TABLE 10:1 SUMMARY OF ANALYSIS OF VARIANCE EMPLOYING COMMISSION ERROR DATA

Source	SS	D.F.	M.S.	F	P
Between					
Extraversion (E)	0.196	1	0.196	< 1	
Neuroticism (N)	0.48	1	0.48	< 1	
E x N	4.294	1	4.294	2.836	$< .1$
Subject within cells	83.265	55	1.514		
Within					
Location of Stimulus (P)	42.706	2	21.353	20.996	$< .001$
E x P	0.828	2	0.414	< 1	
N x P	0.423	2	0.211	< 1	
E x N x P	6.549	2	3.275	3.22	$< .05$
P x Subject within cells	111.85	110	1.017		
Rate of Presentation (S)	13.29	1	13.29	7.831	$< .01$
E x S	0.196	1	0.196	< 1	
N x S	1.833	1	1.833	< 1	
E x N x S	0	1	0	< 1	
S x Subject within cells	93.33	55	1.697		
S x P	1.775	2	0.888	< 1	
E x S x P	2.02	2	1.01	< 1	
N x S x P	2.98	2	1.49	< 1	
E x N x S x P	7.608	2	3.804		
S x P x Subject within cells	249.25	110	2.266		
TOTAL		353			

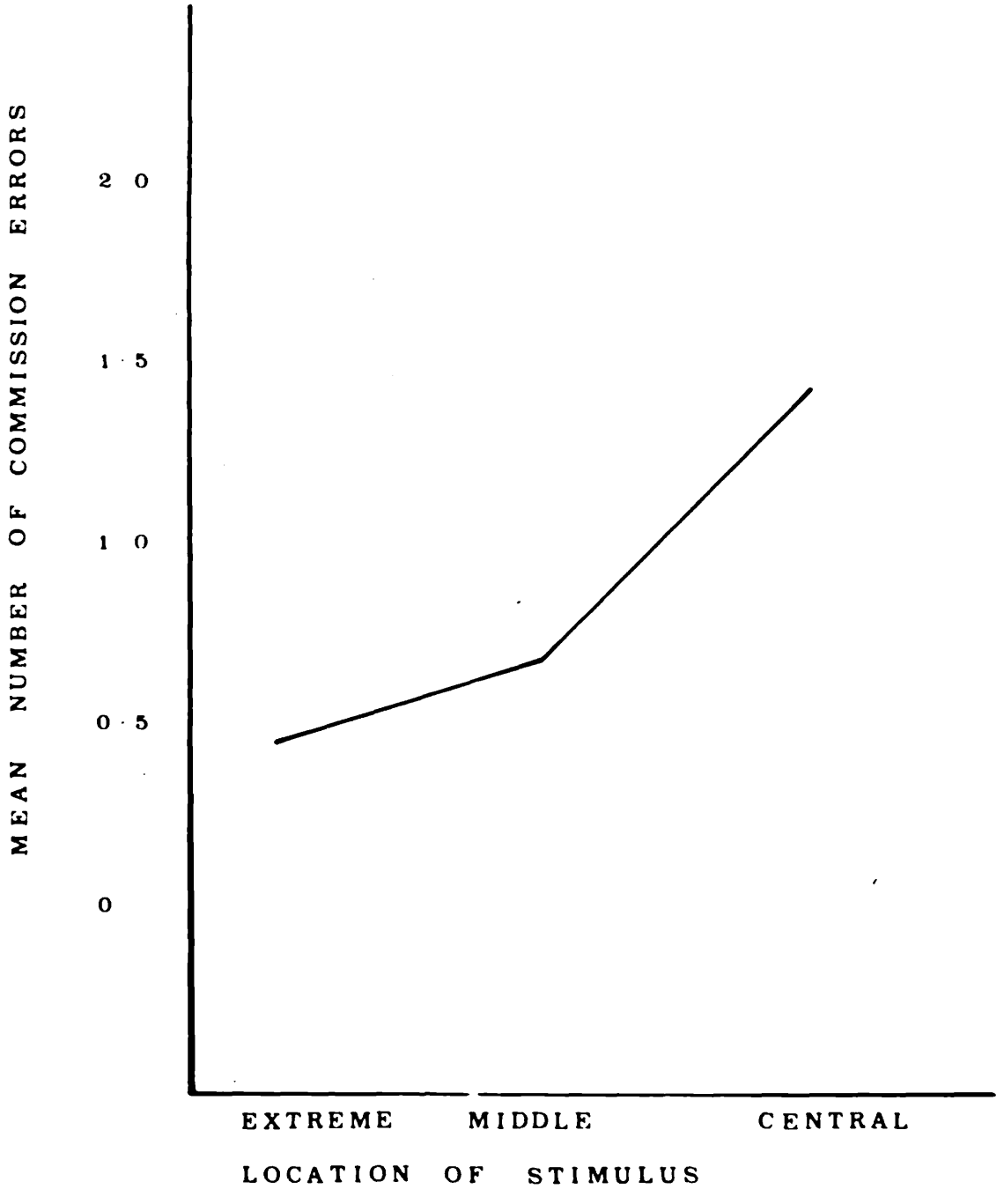


FIG. 10.1 MEAN NUMBER OF COMMISSION ERRORS IN EACH OF THREE PRESENTATION LOCATIONS

were being made, but as can be seen from Table 10:1 this can be or cannot be the explanation for this lack of significance, as both the speed of presentation ($p < .01$) and location of the stimulus ($p < .001$) were found to have a significant effect on the number of errors of commission made.

Both these results were found to be in the predicted direction. As can be seen from Fig. 10:1 the nearer the stimulus was to the centre of the screen, i.e. the more like the "respond" signal the "non-respond" signal was, the more likely it was that an error of commission would be made.

These two variables were not found to interact, however, the increase in speed affected performance at all locations to about the same extent.

Despite the fact that neither the introversion-extraversion nor the neuroticism dimensions were found to be significant, the E x N interaction did approach significance. ($p < .1$, 2 tailed)

TABLE 10:2 COMMISSION ERRORS AS A FUNCTION OF EXTRAVERSION AND NEUROTICISM

	I	E
LN	0.73	0.95
HN	1.1	0.9

As can be seen from the means presented in Table 10:2 the most substantial difference appears to be between the LN and HN groups, but only for introverted Ss. This result is, for the introverted group, in the direction predicted.

The E x N x P interaction was found to reach significance.

TABLE 10:3 COMMISSION ERRORS AS A FUNCTION OF EXTRAVERSION, NEUROTICISM AND LOCATION OF THE STIMULUS

	POSITION OF STIMULUS		
	EXTREME	MIDDLE	CENTRAL
HEHN	0.5	1	1.58
HELN	0.44	0.78	1.71
LEHN	0.5	0.93	2.0
LELN	0.45	0.57	1.13

It is clear from Table 10:3 that there is no difference between groups when the stimulus is at the extreme of the screen, i.e. when the "non-respond" and "respond" stimuli are maximally dissimilar. However, when the stimulus is presented close to the centre of the screen i.e. most like the "respond" stimulus, we find the LEHN Ss making the largest number of commission errors, and the LELN Ss making the fewest. It seems, then, that the original hypothesis that the HN group will be found to make more commission errors has found some confirmation, but it also appears to require some limitation, for it appears to be true only for Ss classified as in troverted.

TABLE 10:4 COMMISSION ERRORS AS A FUNCTION OF EXTRAVERSION, NEUROTICISM, LOCATION OF STIMULI AND SPEED OF PRESENTATION

	"FAST"			"SLOW"		
	Extreme	Middle	Central	Extreme	Middle	Central
HEHN	0.33	1.17	1.33	0.667	0.83	1.83
HELN	0.87	1.032	1.97	0.01	0.39	1.45
LEHN	1	1.143	1.57	0	0.429	2.429
LELN	0.8	.73	1.6	0.2	0.4	0.66

TABLE 10:5 SUMMARY OF ANALYSIS OF VARIANCE EMPLOYING OMISSION ERROR DATA

SOURCE	Ss	D.F.	M.S.	F	P
Between					
Extraversion (E)	13.353	1	13.353	<1	
Neuroticism (N)	0.078	1	0.078	<1	
E x N	0.002	1	0.002	<1	
Subject within cells	1455.756	55	26.468		
Within					
Location of Stimulus (P)	964.321	2	482.161	54.04	<.001
E x P	36.196	2	18.098	2.075	
N x P	17.843	2	8.922	<1	
E x N x P	0.387	2	0.194	<1	
P x Subject within cells	959.229	110	8.72		
Rate of Presentation (S)	672.358	1	672.358	69.6	<.001
E x S	11.745	1	11.745	1.216	
N x S	5.863	1	5.863	<1	
E x N x S	13.167	1	13.167	1.363	
S x Subject within cells	531.308	55	9.66		
S x P	50.422	2	25.211	1.467	
E x S x P	0.118	2	0.059	<1	
N x S x P	3.196	2	1.598	<1	
E x N x S x P	1.02	2	0.51	<1	
S x P x Subject within cells	1890.212	110	17.184		
TOTAL		353			

TABLE 10:6 SUMMARY OF ANALYSIS OF VARIANCE EMPLOYING ANTICIPATING RESPONSE DATA

Source	SS	D.F.	M.F.	F
Between				
Extraversion (E)	4.598	1	4.598	1.27
Neuroticism (N)	2.186	1	2.186	< 1
E x N	0.941	1	0.941	< 1
Subject within cells	119.076	55	3.63	
Within				
Rate of Presentation (S)	280.876	1	280.876	77.1 $< .001$
E x S	4.735	1	4.735	1.359
N x S	3.824	1	3.824	1.097
E x N x S	10.588	1	10.588	3.039
S x Subject within Cells	200.37	55	3.643	
TOTAL		117		

Table 10:4 summarizes the group means for the 4 way E x N x S x P interaction. Although this was not found to be significant it is interesting to note that both the neurotic groups, LEHN and HEHN, make more errors of commission, when the stimulus is in the central position, at the slower rate of presentation than at the faster, whereas the converse is true for the stable groups LELN and HELN.

Turning to the analysis of the omission errors (Table 10:5) and the anticipation data (Table 10:6), once again the physical manipulations effect substantial changes in behaviour, while the personality variables fail to reach significance.

TABLE 10:7 OMISSION ERRORS AS A FUNCTION OF EXTRAVERSION, NEUROTICISM, LOCATION OF THE STIMULUS AND SPEED OF PRESENTATION

	"FAST"			"Slow"		
	Extreme	Middle	Central	Extreme	Middle	Central
HEHN	3.33	3.17	7.17	1.17	1.17	3
HELN	2.97	3.13	8	0.58	0.55	3.71
LEHN	4.14	3.71	9.86	0.29	0.14	3.43
LELN	2.93	3.27	9.4	0.47	0.47	5.53

TABLE 10:8 ANTICIPATION AS A FUNCTION OF EXTRAVERSION AND NEUROTICISM

	Fast		Slow	
	I	E	I	E
LN	3.867	3.677	0.6	0.258
HN	5.429	3.667	0.143	0.5

One feature of both Tables 10:7 and 10:8 which immediately strikes one is the fact that the groups means for both omissions and anticipations are substantially larger than those for the commission errors, indicating that as compared with either an anticipaory or an omission error, commission errors were relatively rare events.

Apart from this the results are as predicted with more errors being associated with the faster rates of presentation and, for omission errors, with stimuli nearer the centre of the screen. (cf Tables 10:5 and 10:6)

10:5 DISCUSSION

Despite the fact that the specific hypotheses presented in the introduction failed to reach significance, it does appear that some support can be found in this experiment for the hypothesis that neurotic individuals are more inclined than non-neurotic individuals to make errors of commission. With the confirmation, however, comes a limitation, this difference between high and low neuroticism groups appears to hold only for individuals who are classified as introverted (cf. Tables 10:1, 10:2, 10:3).

One might be willing to interpret the pattern of results described in Tables 10:2 and 10:3 as predictable on the basis of Gray's theory. To do this one might argue that according to this theory it is the neurotic introverted (LEHN) group who are most sensitive to cues of punishment and so individuals belonging to this group might be most highly motivated to avoid failure, resulting in a large number of errors of commission. Both stable and neurotic extraverted might be motivated by the desire to succeed, to be seen to do well, resulting in a tendency to over respond, and a relatively large number of commission errors. In contrast the stable introverted

group, affected by neither an unusually high sensitivity to cues of punishment nor reward, would be predicted to be the group least likely to make erroneous commission errors.

Table 10:4 does not introduce any information which appears to contradict this interpretation, it does, however, suggest that the relationship between the physical variables and the trait variables is more complex than may have been suspected. For example the neurotic introverted group are found to have both the highest and lowest mean number of responses, and both of these occur in the slow condition. This may well suggest that this group reaches its "optimum" level for this task at some point in this condition and the extra stress produced by the more rapid rate of presentation simply "pushed them over the edge", on to the descending limb of the ubiquitous inverted U shaped curve. If this were, in fact, what was happening one would expect a generalized disruption of performance in the "fast" condition, with an increased number of both omission errors and anticipatory responses. This is, of course, the situation described in Tables 10:7 and 10:8.

These suggestions are, however, post hoc, both in terms of interpreting the results as supporting Gray's theory, and of the patterns of behaviour being describable by an inverted U shaped curve. Neither suggestion fully explains the results obtained, and replication is needed before more speculation can be justified.

Two other features of the present data are noteworthy. The first of these is that while manipulations of the physical aspects of the situation, the location of the stimulus and rate of presentation, produce substantial changes in behaviour the differences between different personality groups is small and often fails to reach

significance. This is a repetition of the pattern found in experiments 2 and 3. Any simplistic "situationalist" interpretation of these data, however, seems to be incorrect for reasons offered in earlier chapters. To state that trait dimensions do not reliably distinguish the behaviour of different individuals is not equivalent to stating that there are no reliable differences in the behaviour of individuals.

The second feature of the data may not be entirely independent of this first one, and that is that there were relatively few commission errors made as compared to the number of omission errors made. This may, on one hand be the reason why the differences between the LN and HN groups were not more pronounced. It seems almost certainly to reflect a strategy employed in this situation indicating that "person variables" may be of substantial importance in determining behaviour. It may be recalled that it was with situations such as this in mind that Mischel (1973) entitled his description of the learning of behaviour a Cognitive Social Learning Theory. In summary, then, both the location of the stimulus and the rate of presentation of the stimulus, significantly effected the number of errors made, both omission and commission, and the speed of presentation also had a significant influence on the number of anticipated responses made; neuroticism was found to affect the number of commission errors made for the introverted Ss when the location of the stimulus was taken into account.

10:6 EXPERIMENT 4B

Introduction

Three conditioning experiments have been reported thus

far and as yet no evidence has been found which convincingly supports the model of personality functioning put forward by Gray. Given the conclusion arrived at in experiment 3, that there seemed little likelihood that Eysenck's theory would provide a useful basis for intergrating different approaches to the study of personality, one might expect Gray's theory to be cast aside. Eysenck's theory, at least found some support. Gray's has found none at all. Nevertheless it was felt that it might be premature to abandon the theory at this stage.

Two considerations militated against the abandonment of Gray's theory. Gray proposed firstly that individuals would be found to be differentially sensitive to cues of reward and punishment, and only secondly that this differential sensitivity would be related to the Eysenckian dimensions of introversion-extraversion and neuroticism. It was remarked when outlining the theory that this proposed relationship was highly speculative, nevertheless it is this latter speculation which has largely been put to the test. The suggestion that differential sensitivity to cues of reinforcement may be related to the dimensions of extraversion and neuroticism may well be wrong, as indeed it appears to be, without seriously damaging the more fundamental aspects of the theory.

The second consideration was the niggling suspicion which kept appearing in the data that appetitive and aversive conditioning might, indeed, be independent of each other.

On the basis of Gray's theory it might be predicted that individuals relatively more sensitive to cues of punishment might err on the side of over inclusiveness and be found to respond not only to cues of punishment but also to cues of substantial similarity to punishment cues. That is, they might be expected to make relatively few omission errors but a

substantial number of commission errors. A complementary pattern of responding might be expected from those individuals relatively more sensitive to cues of reward - few failures to respond in the face of cues of reward but also a tendency to try to obtain rewards when they were not, in fact, available.

Viewed in this light it seems probable that the differences between individuals differentially more sensitive to cues of reward and those differentially more sensitive to cues of punishment might be more readily evident in a sample of Ss pre-selected for their tendency to make errors of commission. The previous experiment reported that individuals classified as neurotic might well display such a tendency. However, this tendency was not found to be strong, nor was there any evidence found in Experiment 3, where the pattern of commission errors was examined, that even in the neurotic group Gray's theory was strongly supported.

For this reason it was decided to turn to two other dimensions which might provide a basis for selecting Ss for the next experiment in which it is proposed to put hypotheses drawn from Gray's theory to the test once more. The two dimensions which looked most promising were the dimension Cattell refers to as dispositional rigidity and the property of the nervous system known as mobility.

Pavlov (1951 - 1952, vol. 3, book 2, p.268) has defined mobility as: "the speed with which the organism, on external demand, yields, gives preference to one stimulation over another, substitutes stimulation for inhibition and vice versa."

Thus the indices of mobility are:

(i) substituting one excitatory process for another,

(ii) replacement of an excitatory by an inhibitory process,

(iii) replacement of an inhibitory by an excitatory process.

Mobility brings with it its own set of problems, however, as Nebylitsyn (1966) has noted, "The mobility of the nervous processes, is even to-day the least defined parameter in terms of its physiological basis." (p.241)

The result of this is that there is no one, unequivocal, generally accepted index of mobility. Reviewing the most commonly employed indices Nebylitsyn concludes that none of these are, beyond doubt, unidimensional, reflecting one, and only one, property of the nervous system.

With this difficulty in mind Cattell's dispositional rigidity may be a better candidate. This is a difficulty or slowness in turning from an old to a new response in situations where the new response is clear to the individual and he has the intelligence and will to make it. Cattell and Winder (1952) define dispositional rigidity as: "a failure to 'adapt', by the use of a shorter behavioural route than the usual one to a given goal, when circumstances make it possible." (p.23).

Dispositional rigidity at first sight appears to share a remarkable similarity with mobility of the nervous system, but, it may be argued, some features included in the concept of dispositional rigidity would, when translated into the Russian system, be more happily housed under the concepts of dynamism and strength of the nervous system, dimensions which are theoretically independent of mobility.

Perhaps the strongest argument against proposing that mobility and dispositional rigidity are identical is the fact that Teplov notes that "by mobility, in the widest sense of the term, one means all the temporal characteristics of the nervous system functioning of which the description speed may be applied" (1956, p.61-62).

While Cattell states that the process he terms "structural rigidity", which is distinct from dispositional rigidity, "would be expected to reveal itself by the relative slowness of an operation requiring rapid oscillation between processes A and B, when compared with the steady repetition of A or B separately" (1952, p.28).

Thus the "inertia of the mental processes", as Cattell calls it, is best shown by alternation tests. Speed and alternation, two of the characteristics of mobility of the nervous system, are, then, considered to be related to process rigidity. Of this latter Cattell said: "on the evidence now available it is possible to say that perseveration as "mental inertia" shown by after effects and immediately successive mental processes simply does not exist" (1946, 237).

Given the very different starting places for their analyses it would be foolhardy to assume a simple corespondence between the dimensions of mobility and structural rigidity. The work on critical flicker fusion (CFF), for example, provides yet another example that the too eager mapping of these two analyses on to one another will produce errors and over simplifications. CFF has been used as an index of mobility of the nervous system by Russian workers, while Cattell has noted "the interesting finding in regard to the classical factor of p (perseveration) is that it does include rigidity of perception (and slow speed of flicker fusion) as well as the longer attested motor rigidity." (1952, p.29). Nebylitsyn, however, has suggested that CFF might be an index not of mobility, but a newer property of the nervous system, lability. "Clearly lability must be viewed as an independent property of the nervous system distinct from the traditional property of 'mobility'." (1966, p.259). This confusion helps illustrate the complexity of the situation.

The reason mobility of the nervous system and

dispositional rigidity have been discussed here is that the property we wish to tap is, or seems to be, akin to both of them. If one could be sure either (a) that these two dimensions were identical, or (b) that one of them would identify individuals prone to make errors of commission, then it would be possible, indeed preferable, to employ indices previously used and found to be satisfactory, as a basis for the pre-selection of Ss.

As there is no ready made index available it was decided to follow up some suggestions made by Nebylitsyn (1966) in attempting to devise one. Nebylitsyn, discussing the results of some studies employing RTs to stimuli presented at various points in the visual field, and the possible importance of information processing and choice RTs, although unable to offer a definite conclusion as to whether these are measuring mobility states: "we assume, however, that this index has greater intrinsic worth than others in advancing our understanding of the physiological basis of the mobility of the nervous processes" (p.261).

Given that the method of measuring mobility used here was bound to be crude in this its first outing, and given the possibility that one is looking for Cattell's dispositionally rigid individuals, it was decided that the perseveration (p) - tests should also be employed here.

Chown (1962) has reported that Cattell's p - factor fractures into two independent rigidity factors. This being the case it was felt that a new index of mobility should be devised, which was as similar as possible to the situation to be used in the next experiment, thus permiting any results to be more convincingly generalized to that situation, and which might also measure mobility of the nervous system. At the same time the four measures of rigidity were also to be used.

The justification for proceeding in this way was that if the new test was found to correlate substantially with one or more of the p-tests, Ss might be classified, economically, on the basis of these, the p-tests in future experiments. On the other hand if the p-test are not found to correlate with the mobility measure, the behavioural data will still be available on the basis of which S can be classified for the next experiment in which it is intended to put Gray's theory to the test once more.

HYPOTHESES

As more than one index was derived from the data provided by the "mobility task" it was predicted that:

Hypothesis 4b:1 The indices of mobility would be found to correlate significantly with each other.

As the value of the signals to respond, and not to respond, were to change more than once during the mobility test, it was predicted that

Hypothesis 4b:2 The mobility indices would correlate significantly and negatively on the first and second reversals of signal value.

Hypothesis 4b:3 The four tests of perseveration would be found to correlate significantly with each other.

Hypothesis 4b:4 The indices of mobility and of perseveration would be found to correlate significantly, and follow from this when these correlations are factor analysed a single factor would emerge loaded on by both mobility and perseveration indices.

10:7 METHOD

DESIGN

Partly in an attempt to follow up Nebylitsyn's suggestions, but substantially in order to (a) keep the test situation as simple as possible, while (b) make the task here as similar as possible to the tasks in experiment 4a and in the next experiment, it was decided that the stimulus should be a simple visual one, a single point of light. The S was required to respond to this.

Points of light were presented, one at a time, on an oscilloscope screen. These stimuli were presented at a rate of 1 per 500 msec.s. They were brief, but clearly visible, flashes of light.

These dots were presented approximately 1.5 cms either side of the centre of the screen, along the horizontal mid-line. The S's task was simply to make an RT response whenever the dot was to the right, (for 50% of S to the left), of centre. There were no markings on the screen, and so the decision demanded of the S was a rather difficult one.

After 40 trials, 20 presentations of the stimulus in each position in a random order, the S was required to reverse the pattern of responding, i.e. if the dot appeared on the left (right for 50% of Ss) a response was required but not when it was on the right (left for 50% of Ss).

As speed is hypothesized as being an important characteristic of mobility there was only 1 second between the signal to change the direction of responding and the next presentation of a stimulus.

There were four blocks of trials, 160 trials in all, and consequently S was required to change the stimulus to which he was responding on three occasions.

The following data were recorded on each trial:

(i) whether or not the S had correctly responded or refrained from responding, . . .
(ii) whether a commission error had been made,
(iii) whether an omission error had been made,
(iv) the latency of any responses made.

From these the following scores were derived

(i) the ratio of the mean RT on the last 10 response trials before reversal to the first 10 after it. If this task were really tapping the nervous system property of mobility then the less mobile the individual the smaller would be this ratio.
(ii) The difference in the number of commission errors between the same two groups of 10 trials. Difference was prefered to a ratio index here because of the relatively large number of error free blocks of trials. Again, if we are tapping mobility we can expect the less mobile individual to have the larger difference score.
(iii) The difference in omission scores to the same two groups of 10 trials.
(iv) RTs of less than 100 msec.s were regarded as anticipatory responses, and recorded separately.

The four perseveration tests (p-tests) used were those used by Cattell et al (1954) and Chown (1962).

(i) The ratio of the number of letters written when the word "Ready" was written forwards and reversed, each for one minute.
(ii) The same ratio, but this time for the number 237
(iii) The ratio of the number of letters written when the sentence "The sky is a deep blue" was written normally and with each letter doubled, (written twice), each task for one minute.
(iv) The ratio of the number of letters written when the sentence: "The sky is a deep blue" was written in upper

and in lower cases to the number of letters written when the case was alternated letter by letter. Again each task for one minute.

Procedure

The same 60 Ss were employed in this emperiment as in experiment 4a. This experiment and experiment 4a were conducted in a single session lasting approximately 35 minutes.

The four perseveration tasks were performed in random order, but always as a single block. The order of the "mobility" task and the block of perseveration tasks was randomized across Ss.

The "mobility" task was controlled by computer. S was seated in front of a teletype approximately 4ft from an osciloscope screen. This screen was 4ft 8ins from the floor, and measures 21.6 x 17 cms.

The S was instructed as follows:
"Spots of light will appear on the screen, one at a time, either just to the left or just to the right of the centre of the screen. What I would like you to do is to press this bar, (the space bar), as quickly as you can anytime the dot is to the left (or right) of centre. Don't press if the dot is to the right (or left). The centre will not be marked, you will have to judge where it is yourself.

After a number of trials, 40 in fact, a tone will sound and the computer will print out 'Press for right', (or left), that is just what I want you to do - press when the dot is on the right, (or left), but not when it is on the left, (or right).

The tone will sound on three occasions, and so you'll have to make this change three times. Each time the computer will print out which side you should be pressing for,

in case you get confused.

Do you understand what you have to do?

A tone sounded to start the run and one second later the first dot appeared, subsequent dots appeared at the rate of one per 500 msec.s. At the end of 40 trials the tone sounded again and there was a "pause" of one second before the next dot appeared, this was the case for all three reversals. Data were stored on paper tape.

For the perseveration tests the S was seated at a table and provided with a sheet of lined foolscap paper and a freshly sharpened pencil. Each section of the tasks lasted one minute and the S was told to write the word, number or sentence, as applicable, "as many times as possible in a minute".

The writing backwards task i.e. forming each letter in reverse, was demonstrated to the Ss to avoid any possibility of confusion.

10:8 RESULTS

Mobility

The first question to be answered is whether or not the three types of score derived from the "mobility" test show any substantial relationship with one another.

TABLE 10:9 CORRELATIONS OF "MOBILITY" SCORES

(1)	RTs	1.00		
(2)	Commission errors	-.78**	1.00	
(3)	Omission errors	-.017	.412**	1.00

(**$p < .005$)

Table 10:9 reports the correlations between the RT ratio score, the omission and commission difference scores for

the first reversal. It was felt that, at this stage at least, it would be better to limit the correlations to this first reversal, because this was least likely to yield spurious results e.g. an S with very poor mobility might appear to display a marked improvement after the second reversal, but in fact still be responding in the manner he had for the first 40 trials, having failed during the second block of trials to establish a new pattern of responding.

As can be seen from this table while the "commission index" correlates significantly with the other two indices, these two indices do not correlate significantly with each other. The two explanations of this which spring to mind most quickly are that (i) the commission index reflects a quality which both the other indices also reflect, but these indices reflect that quality less adequately, or (ii) the negative correlation between the RT ratio and the commission difference index is due to the fact that individuals who have slow RTs during the first block of trials do not show any great decrease in their RTs after the reversal, because they are already responding so slowly, and slow responders are simply less likely to make error of commission. These Ss then will have relatively large ratio scores and small commission difference scores, whereas the correlation between the omission and commission differences reflects a common underlying cause, perhaps mobility.

When we turn to the correlations between the same index on the three reversals (cf. Tables 10:10 (a) (b) and (c)).

TABLE 10:10a CORRELATIONS OF RT RATIO SCORE ON THREE OCCASIONS

First	1.00		
Second	-.116	1.00	
Third	-0.031	-0.103	1.00

TABLE 10:10b CORRELATIONS BETWEEN THE COMMISSION INDEX SCORES ON THREE OCCASIONS

First	1.00		
Second	-.676**	1.00	
Third	.224*	.199	1.00
	1	2	3

(**p < .005; *p < .05)

TABLE 10:10c CORRELATIONS BETWEEN THE OMISSION INDEX SCORES ON THREE OCCASIONS

First	1.00		
Second	-.04	1.00	
Third	.012	.023	1.00
	1	2	3

it is found that only the commission index shows any significant correlations (Table 10:10b). The correlation between the first and the second reversal is both substantial and negative (-.676), suggesting that perhaps this index is tapping a genuine difficulty in the ability to reverse ones responses, and in consequence that this measure might be more promising than the other two.

Perseveration

Table 10:11 presents the correlations between the four p-tests.

TABLE 10:11 CORRELATIONS BETWEEN PERSEVERATION TESTS

(1) Reversed Number	1.00			
(2) Reversed Word	.275*	1.00		
(3) Alternated Cases	.489**	.367*	1.00	
(4) Doubling letters	.408**	.192	.55**	1.00
	1	2	3	4

(*p < .025; **p < .005)

There seems little to say about this set of correlations. As one might expect from a set of tests which have been used for some time they all seem to show moderate and significant correlations with each other. The only test about which there might be some question on this pattern of correlations is the reversal of the word "Ready", this produced the poorest set of correlations, and the only correlation which was not significatn, with the doubling of the letters task.

It seems then that on one hand the commission index may turn out to be the best index of mobility, while all the p-test show good intercorrelations suggesting that they are all tapping the same general factor. The question of interest now is: do the p-test and suggested indicies of mobility, particularly the commission index correlate significantly with one another?

TABLE 10:12 INTER CORRELATIONS OF P-TESTS AND MOBILITY INDICES

	RT Ratio	Commission Difference	Omission Difference
Reversed Number	-.002	-.029	-.285*
Reversed Word	-.021	-.68+	-.316**
Alternated Cases	-.083	-.118	-.277*
Doubled letters	.001	.023	-.318*

(*$p < .025$; **$p < .01$ +$p < .005$)

This Table(10:12) suggests that while the omission index shows a substantial relationship with the p-test, both the commission and RT indices show a considerable independence of perseveration. However, before claiming that either there is one factor of which mobility and perseveration are different constructions, and of which the omission index is a

good measure and the commission index a bad one; or that there are two factors, mobility and perseveration, the p-tests and the omission index reflecting the latter, and the commission index the former, some of the results of a factor analysis should be reported. This factor analysis cannot, of course, be regarded as "strong" evidence, computed as it is on only 58 sets of observations. It was felt, however, that the factor analysis might provide a useful check on any interpretation of the pattern of correlations. Orthogonal factors were extracted using the varimax procedure.

TABLE 10:13 LOADINGS OF "MOBILITY" INDICES AND P-TESTS ON TWO FACTORS

	F_5	F_6
RT Ratio		
Commission Difference	.678	
Omission Difference	.722	
Reversal Number		.656
Reversal Word		.419
Alternated Cases		.666
Doubled letters		.811

* (A fuller version of the factor loadings provided in Table 10:19)

This analysis (cf Table 10:13) then seems to suggest quite clearly that there are two factors, independent of one another, one on which the perseveration tests load and another on which both the commission and omission indices load.

10:9 DISCUSSION

There appears to be little to say about the results of this present experiment, it would seem to be fairly clear

that mobility, if that is the quality being tapped by the indices derived from the behavioural data of the present experiment, is a dimension substantially independent, if not totally independent of the quality tapped by the p-tests. In as far as one would wish indices of these two dimensions to be "pure", i.e. not reflecting to any other quality, the commission index appears to be more promising. Not only is it found not to correlate significantly with the p-indices but the index at the first reversal correlates significantly, and negatively, with the index at the second reversal as it was predicted an index of mobility should.

The p-tests do correlate significantly with each other, and they are all found to load on the same factor, but as they do not predict who will, and who will not, make a commission error, e.g. the total number of commission errors was found to correlate .0699(N.S.) with the reversed number index, -.0817 (N.S.) with the reversed word index, .105(N.S.) with the doubling of the letter index, and -.0181 (N.S.) with the alternating of case index, these indices appear to be of little further interest here.

The commission index, however, whatever its promise might appear to be needs to be treated with the greatest of caution. Very few errors of commission were made, infact, in the block of trials before the first reversal which provided the opportunity to make up to 20 commission errors, 80% of Ss made only one or none at all.

Of course, even if the findings relating to the commission index had been based on a much larger range of data one would not be justified in using this index as a basis for selecting Ss, one needs to know if this index is indeed reliable. This would be true in any situation, it must be especially true in the present context where the

absolute consistency of behaviour has been questioned, and the importance of the inter-relationship between person variables and situation variables in determining behaviour has been stressed. These comments lead us to the third section of the present study.

10:10 SECTION 4C

INTRODUCTION

If, as was suggested in the previous section, one wishes to select people on the basis of some particular behaviour then one must make the assumption that if people do a thing in one situation, then they will do the same thing in another situation, that is, one must assume that people are consistent in their behaviour. On the basis of the literature reviewed in Chapter One this appears to be a rather dangerous assumption to make.

What one must do, therefore, is demonstrate some consistency in behaviour. In the previous sections the predictibility of behaviour, and hence its consistency, was related to traits, neuroticism, mobility and perseveration, but as Wallach and Leggett (1972) have argued this need not be the case. To quote them at length:

"With constructs thus turning out to be as difficult to validate within moderator-defined subgroups as they were in samples as a whole, it seems time to emphasize that the search for consistency does not stand or fall with the finding of evidence for traits or dispositions. The demonstrated elusiveness of such evidence tells us not that constructs of the kind considered don't seem to be useful conceptual entities. Whether people manifest consistency remains to be seen - by focusing not on the test responses

which are of interest only if they function as signs of some hypothetical trait or other, but rather on behaviours and effects of behaviours that are of interest in their own right. In the case of such behaviours and behavioural effects we can ask, quite straight forwardly, whether persons exhibit relative consistency in producing them across varying occasions. This is not the mere study of test-retest reliability, because test responses do not carry inherent meaning-their meaning depends on whether they serve as a clue to something else. We are talking, by contrast, about performances and products that do not call for a justification beyond themselves in order to qualify as objects of study". (p.602).

As the same Ss took part in experiments 4a and 4b we are able to ask "quite straight forwardly, whether persons exhibit relative consistency" with respect to the behaviours recorded on these two occasions.

It might also be recalled that it was remarked, when discussing Mischel (1968), that: "the distinction between the concepts of reliability and validity is, at least in part, a reference to the degree of generality of responding to which one wishes to refer", Experiments 4a and 4b were contrived partly with this statement in mind, and so it is possible to compare the consistency in behaviour to more and less similar stimuli within the same situation and between situations.

HYPOTHESES

If Wallach and Leggett are correct it may be predicted that:

Hypothesis 4c:1 The eight commission error scores available from experiments 4a and 4b will be found to correlate with each other to a significant degree.

Assumming that the similarity between stimuli is important in determining consistent behaviour, it is predicted that

Hypothesis 4c:2(1) the correlation between the commission errors made to the stimuli in the central position and that in the middle position will be greater than the correlation between the errors made to this central stimuli and the stimuli farthest from the centre of the screen. This will be true in both the fast and slow conditions.

(ii) The commission error scores from experiment 4b (the "mobility" task) will correlate most highly with those made in response to the stimuli in the central position in the "fast" condition in experiment 4a.

These hypotheses may also be repeated with respect to omission errors.

Hypothesis 4c:3 All omission error scores will be expected to intercorrelate

Hypothesis 4c:4(i) The correlation between "central" and "middle" omissions will be greater than that between "central" and "extreme" omissions.

(ii) "Mobility" omissions will correlate most highly with "central" omissions in the "fast" condition.

Hypothesis 4c:5 Anticipation scores will be expected to inter-correlate to a significant degree.

Owing to the conflicting results obtained in experiments 2 and 3 no prediction is made with respect to the relationship between the three self-rated consistency scores.

Hypothesis 4c:6 It is predicted, however, that self-rated neuroticism will be found to correlate to a significant degree with self rated consistency for neuroticism.

10:11 METHOD

DESIGN

The intention in this study was to use the data already collected in experiments 4a and 4b in order to look at the consistency question once more. Three sets of behavioural data were used: (1) commission errors, there were eight commission scores available: for the task in experiment 4a, in the "fast" condition (i) the number of commission errors made when the stimulus was in the central, (ii) middle, and (iii) extreme positions, in the slow conditions commission errors associated with the same three locations, (vii) the number of commission errors made during the first block of trials in the "mobility" task, (viii) the total number of commission errors made during the mobility task.

(2) The same eight scores were available for omission errors.

(3) Three anticipation scores were available (i) the number of anticipations made in the "fast" condition of task 4a, (ii) the number of anticipations made in the "slow" condition of task 4a, (iii) the total number of anticipations made during the "mobility" task. An anticipatory "response" was defined as any response with a latency of 100 msec.s or faster.

It was intended to inter correlate the scores within each set of data.

Ss also completed the same self-rating scale as in experiments 2 and 3, and so it was proposed to look once more at the relationship between self-rated consistency and neuroticism, and at the inter-relationships between the ratings of consistency for various traits.

PROCEDURE

The tasks and how they were performed, the equipment used and the Ss have all been described in experiments 4a and 4b. It only remains to point out here that experiments 4a and 4b were conducted during a single testing session lasting approximately 35 minutes. The S performed either the "mobility" task or task 4a first, with the order randomized across Ss. The perseveration task always came next. Finally came which ever of the two tasks, "mobility" or 4a, which had not been performed already. This procedure was adopted to allow S some respite from having to respond quickly to brief visual stimuli rapidly presented.

10:12 RESULTS

The first hypothesis (4c:1) predicted that all the commission error scores would show substantial and significant inter-correlations. As can be seen from Table 10:14 this prediction has not been confirmed.

TABLE 10:14 INTER-CORRELATION OF COMMISSION ERROR SCORES

1 Total, Mobility task								
2 1st block Mobility task								
3 Fast condition, central	.019	.151						
4 Fast condition, middle	.086	.017	.203					
5 Fast condition, extreme	.167	.114	-.024	.092				
6 Slow condition central	.104	.041	.112	-.112	-.034			
7 Slow condition, middle	.032	.38**	.016	-.018	.186	.052		
8 Slow condition, extreme	.0999	-.018	.168	-.022	.274*	.67	.383**	
	1	2	3	4	5	6	7	8

(*$p < .025$; **$p < .005$)

Only three of the 27 correlations reached a significant level, a result which does not indicate that people are consistent with respect to their tendency to make commission errors, not even when, as here, one looks at relative consistency. Nor does there appear to be any evidence that the more similar stimuli are, either within or between situation, the more consistently people will behave. The situation appears to be somewhat different, however, when one turns to omission errors, (cf. Table 10:15)

TABLE 10:15 INTER CORRELATION OF OMISSION ERROR SCORES

	1	2	3	4	5	6	7	8
1. Total Mobility task								
2. 1st block mobility task								
3. Fast condition central	.357**	.138						
4. Fast condition middle	.471**	.259*	.482**					
5. Fast condition extreme	.329**	.217*	.462**	.646**				
6. Slow condition central	.285*	-.015	.038	.029	-.039			
7. Slow condition middle	.422**	-.07	.238*	.193	.168	.673**		
8. Slow condition extreme	.537**	.058	.259*	.529**	.549**	.419**	.519**	

(*p < .05; **p < .005)

18 of the 27 correlations in Table 10:15 were found to be significant, suggesting that with respect to omission errors there is considerable consistency in behaviour. There also appears to be a considerable consistency across situations as the total number of omission errors in the "mobility" task was found to correlate with all six omission error scores from experiment 4a. Though the number of errors before the

first reversal in the "mobility" task was found to correlate with only two of these six scores, perhaps reflecting the unreliability of this smaller sample. The results with respect to the hypothesis that the more similar the stimuli are the more similar behaviour will be, is equivocal. It seems to find some support within a situation, the number of errors made in central position correlating more highly with the number in the middle position, (.482, "fast" condition; .673, "slow" condition), than in the extreme position, (.462, "fast" condition; .419 "slow" condition). The hypothesis does not appear to find any support across situations.

Turning to the anticipation data (cf. Table 10:16) there it was found that only one of the three correlations computed was significant.

TABLE 10:16 INTER CORRELATION OF ANTICIPATORY "RESPONSE" ERRORS

		1	2
1.	Total number, "mobility" task		
2.	Total number, "Fast" condition	.447*	
3.	Total number, "Slow" condition	.089	-.014

(*p < .005)

Turning now from whether people behave consistently to whether they rate themselves as behaving consistently, as can be seen from Table 10:17 the results of the present sample fall midway between those of experiments 2 and 3.

TABLE 10:17 CORRELATIONS BETWEEN RATINGS OF CONSISTENCY

		1	2
1.	Rated consistency for extraversion		
2.	Rated consistency for impulsivity	-.083	
3.	Rated consistency for neuroticism	.175	.367*

(*p < .005)

Rated consistency for impulsivity and for neuroticism were found to correlate significantly, but the other two correlations were not found to be significant.

The relationship between the consistency ratings and neuroticism is reported in Table 10:18

TABLE 10:18 CORRELATIONS BETWEEN EPI AND SELF-RATED N AND SELF RATED CONSISTENCY

	EPI N	Self-rated N
1. Rated consistency of extraversion	-.161	-.222*
2. Rated consistency of impulsivity	.069	-.133
3. Rated consistency of neuroticism	-.33**	-.436**

(*$p < .05$; **$p < .005$)

The relationship between self-rated consistency for neuroticism and self rated neuroticism was found to be significant, once more.

Table 10:19 reports the loadings of the variables on factors extracted from the correlation matrix (only loadings greater than .4 have been recorded). It seems pointless indulging in a factor naming exercise here, especially as the correlations are based on such a relatively small sample, but there are some features of this factor analysis that are noteworthy. None of the trait measures are found to load on the same factors as any of the behavioural measures, and with the exception of the anticipation scores the behavioural data derived from experiments 4a and 4b are described by quite independent factors, suggesting that one ought not to concentrate simply on behaviour, but on a variety of situational variables, most important of which might be the individuals construction of the situation.

TABLE 10:19 SUMMARY OF FACTOR LOADINGS

"Mobility" Task	F_1	F_2	F_3	F_4	F_5	F_6	F_7
RT Ratio							
Commission index					.678		
Omission index					.722		
Total Anticipations							.779
p-Tests							
Reversed Number						.656	
Reversed Word						.419	
Alternated Cases						.666	
Doubling letters						.811	
"Fast" condition							
RT - central							
RT - middle							
RT - extreme	.46	.461					
Commissions - central							
Commissions - middle							
Commissions - extreme							
Omissions - central	.535						
Omissions - middle	.777						
Omissions - extreme	.867						
Anticipations							.67
"Slow" condition							
RT - central							
RT - middle	.47						
RT - extreme	.724						
Commissions - central		.582					
Commissions - middle							
Commissions - extreme							
Omissions - central		.778					
Omissions - middle		.77					
Omissions - extreme	.669	.44					
Anticipations		.752					
Extraversion				.68			
Neuroticism			-.813				
Ego-Strength			.714				
% of variance	25.8	13.3	8.2	7.5	5.6	11.2	6.7

Only loadings greater than .4 have been recorded

10:13 DISCUSSION

Wallach and Leggett (1972) advised asking"straight forwardly" whether or not people were consistent. Of course, they meant to limit this question to behaviours which are of interest in their own right. Such behaviours were chosen in the present study and Wallach and Leggett's question was asked, but it appears that nature has a wry sense of humour. The answer was, "sometimes". Obviously nature does not feel compelled to conform to our simple schemas, with the result that while there appears to be some, moderate, degree of consistency with respect to the making of omission errors, there is less evidence of consistency with respect to anticipatory errors and no real evidence at all of any consistency with respect to commission errors. One might speculate that some behaviours simply show more consistency than others. It seems more probable however, that to some extent the results obtained here reflect the fact that there were substantially more errors of omission than of commission and in consequence the correlations between the various omissions scores are more reliable. For example in the "fast" condition the mean number of commission errors was 1.741, while the mean for omission errors was 8.517. The same two means in the slow condition were .19 and .5 respectively.

Part of the explanation for this state of affairs is, no doubt, that especially in the "fast" condition it is more likely that one will miss making a response than that one will make an extra response, suggesting that situational variables are the paramount determinants of behaviour. This can not be the whole story, however. It has already been reported that more commission errors were made if the task was made more difficult, either by increasing the rate of presentation, or by making the discrimination more difficult by placing the stimuli near the centre of the screen.

Besides this, one could point out that there was as much time available in which to make a commission error as to make a correct response, and these predominated.

Talking to Ss informally after the experiment, most of them were surprised to find how well they had done, for, it emerged, almost all S had adapted a strategy of caution - an "if in doubt don't respond" strategy. Thus the relatively large number of omissions, and in consequence the more reliable correlations, were the result not only of the physical parameters of the situation, but also of a conscious decision on the part of the individuals. It seems that we have here a good example of the complex relationship between the individual and the situation.

It is possible that it is because of the way in which this strategy was employed that there appears to be more support for the hypothesis: the more similar the stimuli are the more similar the response behaviour will be, within the experimental task (4b) than between experimental tasks (4a and 4b).

It seems, then, that things may be considerably more complex than Wallach and Leggett have argued, and more likely to be in line with the proposals of Mischel (1973) and Bandura (1977). It seems that the degree of consistency observed will depend on the strategy adopted by the individual observed, the behaviours recorded, and other critical variables such as the instructions given to the S, the range of situations sampled and, perhaps most importantly, how S views the situations i.e. whether he sees them as sufficiently similar to justify using the same strategy.

These solutions are, of course, in line with those proposed in earlier chapters. One of a different persuasion might argue that given the problematic nature of the data,

the moderate correlations obtained, especially with relation to the omission data, may well indicate a substantial degree of consistency. Each point of view might find some support in the present data. What is beyond doubt is that any simplistic model of the determinants of human behaviour is wrong.

If the behavioural data relevant to the consistency issue is somewhat confused, the self rating data is even more so. It appears that even non-significant results are not reliably replicable. In experiment 2 the three self ratings of consistency were found to be independent of one another, it was, therefore, suggested that consistency might be trait specific. In experiment 3 these same three ratings were found to correlate. In the present experiment one of the three correlations, that between rated consistency for neuroticism and for impulsivity, was found to be significant the other two were not.

On the other hand, however, one finding does appear to be reliable and that is the correlation between rated consistency for neuroticism and rated neuroticism, in experiment 2 a correlation of -.379 ($p = .006$) was reported, in experiment 3 a correlation of -.251 ($p < .05$), and in the present study a correlation of -.436 ($p < .005$).

It seems then that the common conception of neuroticism includes the concept of inconsistency.

10:14 SUMMARY

On the basis of the first experiment it was concluded that there was some limited support for the hypothesis that individuals classified as neurotic tend to fail to inhibit responses, but this statement is true only for introverted subjects. The second experiment indicated that mobility

and rigidity were likely to be independent factors, and that the tendency to make commission errors, was, in this situation, the most promising index as a measure of mobility of the nervous system. It was felt that caution was necessary, however, as few errors of commission had been made, and in consequence the correlations on which this evaluation was made, lacked reliability. Any possibility of selecting Ss on the basis of their commission error score was quashed when it was found in the third section of the study that there was little evidence of consistency with respect to this type of error. This apparent inconsistency may have been a function of the fact that there were relatively few commission errors, for there appeared to be a moderate degree of consistency with respect to omission errors, and there were considerably more omission errors made.

It was also found that, as in previous experiments, while the physical - situational variables had a substantial effect on behaviour the trait variables were rarely found to have a significant effect. However, it was argued that this should not be interpreted as indicating that situational variables are all important. In the third section of the study it was argued, indeed, that a personal variable in the shape of the strategy adapted by the S, was having a critical influence on behaviour, and it was for this reason that the hypothesis suggesting that similar stimuli will elicit similar behaviour, was only partly confirmed.

It was suggested that the data might best be interpreted as reflecting the complex reciprocal relationship between physical, personal and behavioural variables.

One finding which was replicated was that individuals who rate themselves as inconsistent with respect to the dimension of neuroticism tend to rate themselves as neurotic.

CHAPTER XI EXPERIMENT 5

11.1 ABSTRACT

Subjects were required to respond to appetitive stimuli, stimuli for active avoidance and neutral stimuli. At the behavioural level it appeared that individuals were differentially sensitive to these cues of reinforcement. Only the weakest evidence was available to link this dimension of differential sensitivity to cues of reinforcement to extraversion. A self rating of differential sensitivity was found to distinguish between individuals displaying differential sensitivity to cues of reinforcements more successfully. This dimension was also found to be substantially independent of need achievement and locus of control.

Locus of control was found to contain two sub factors related to positive and negative outcomes, though these were not independent of each other.

No real evidence was found for behavioural consistency, nor even for self-rated consistency being consistent. Self rated consistency appeared to be trait specific. Self ratings on the dimensions of extraversion, neuroticism and impulsivity were, however, found to be consistent.

11:2 INTRODUCTION

As already pointed out experiments 1 - 3 assumed that there was some substance to Gray's claim that the dimension of differential sensitivity to cues of reinforcement is closely related to the Eysenckian dimension of introversion-extraversion, with the result that emperimental situations were so contrived as to derive different predictions from the two theories, Gray's and Eysenck's. While Eysenck's theory has received some support Gray's has received none at all. This assumption is laid aside in the present experiment, and whether or not people are found to be differentially sensitive to cues of reinforcement serves, instead, as the focus of attention.

The present experiment is an elaboration of an earlier experiment. In experiment 3 Ss were able to use a response key to avoid reinforcement, it was expected that they would learn to use the key to avoid negative reinforcement, but to avoid using it when positive reinforcement was available. This they did. It was suggested in that experiment that the difference between the Eysenckian and Grayian models might be evident at the stage of learning an operant response. The results favoured Eysenck's contention that introverted Ss display superior conditionability. The present experiment dispenses even with the learning stage and questions whether or not individuals are able to respond to cues of appetitive and aversive reinforcement with differential efficiency, when they are informed of the value of these cues.

In this experiment, then, Ss were instructed that responding with one key in the face of one stimulus (CS - Rew) would gain them a point; responding with a second key to another stimulus (CS - Pun) would allow them to avoid losing a point. There was also a third stimulus available but this

was associated with neither positive or negative reinforcement, though the S was instructed to press a third key in response to it.

This situation provided information about the manner in which Ss responded in face of various cues of reinforcement. It also necessitated some thought about what might constitute the data for analysis.

Scores

As one ultimately wishes to draw some conclusions from the data obtained, it seems worth while to give some thought to what the various scores, which might be derived from this data, might "mean".

(1) Ss, during the course of the experiment, both gained and lost points according to the accuracy of their responding. It might seem appropriate, therefore, to use the Ss final total net score as the raw data for the analyses. However, an S responding correctly on every avoidance trial but incorrectly on every gain trial would achieve a net score of zero; an S responding correctly on every gain but incorrectly on every avoid trial would also achieve a total net score of zero. Thus two Ss responding in very different fashions would achieve identical scores, and obviously this measure is not sensitive enough to differential sensitivity for our needs. It also appears to a greater or lesser degree to reflect speed of responding, motivation, fatigue and other such variables. This measure will be refered to as the "net score" and may be defined as the number of points gained minus the number of points lost.

(2) One might improve on the net score by looking instead at the total number of times the two keys "gain" and "avoid", were pressed, whether or not the responses were correct. Though this measure preserves some information lost in the net score, it too has its short comings. This measure clearly

reflects two, quite distinct, components;

(i) Ss sensitivity to the cues of reinforcement

(ii) Ss tendency to respond to, or the strength of his need, or his desire to gain or avoid reinforcement.

It is only the first of these which is of interest with respect to Gray's theory. So this measure does not appear to be entirely satisfactory. This measure will be referred to as the "Total" press score, and is derived as follows: The total number of times the gain key (TG) was used minus the total number times the avoid key (TA) was used, divided by the total number of responses (TR) made.

$$\frac{(TG - TA)}{TR}$$

(3) The magnitude of the net score will be limited by two types of error which are confounded: the number of failures to respond correctly with the gain key, and the number of failures to respond correctly with the avoid key. It would be desirable to attempt to differentiate between correct and erroneous responses. To this end one might derive a measure from the correct responses only, arguing that with respect to Gray's theory what one is interested in is, on one hand, the number of correct responses to cues of reward and punishment, reflecting sensitivity, while on the other the difference between the number of correct responses to each of these cues reflecting the degree of differential sensitivity. Thus if Gray is correct one would predict that the individual relatively more sensitive to cues of punishment will make more correct avoidance than gain responses, while the situation will be reversed for the individual relatively more sensitive to cues of reward. This measure though an improvement on the Net score will not be absolutely pure, still reflecting to some degree Ss desire for reinforcement.

This measure will be called the "correct" score and may be defined as follows: The number of correct gain responses (CG) minus the number of correct avoid (CA) responses divided by the total number of correct responses (TC).

$$\frac{(CG - CA)}{TC}$$

(4) One might look at the general desire for or need for one or the other type of reinforcement by looking at the difference in the number of erroneous "gain" and "avoid" responses made. This measure, though, will suffer from the same limitations as the "Total" press score, reflecting both sensitivity and need.

This measure will be called the "error" score and may be defined as follows: The number of erroneous "gain" responses (EG) minus the number of erroneous "avoid" responses (EA) divided by the total number of error responses (TE)

$$\frac{(EG - EA)}{TE}$$

(5) Three keys were available to the S one related to cues of reward, one to cues of punishment and the third to neutral stimuli. One might obtain a better measure of the differential need for, or tendency to respond with the key of reward by looking at the difference in the number of times the gain key and avoid key were used when the neutral key ought to have been used. This measure will be free of sensitivity to cues of reward and punishment. As the term "error score" has already been used it is proposed to call this measure the "need" score, as it is assumed to reflect the differential need for one type of reinforcement over the other and the tendency to respond with the key associated with that reinforcement, even when the reinforcement is not available.

The "need" score then may be defined as follows:

The number of times the "gain" responses are made to the neutral stimulus (NG) minus the number of times the "avoid" response is made to the neutral stimulus (NA) divided by their sum

$$\frac{(NG - NA)}{(NG + NA)}$$

Having indicated the main indices of performance and what they are thought to reflect this seems to be an appropriate place to state the first set of hypotheses:

Hypothesis 5:1 (i) The "net" score will not be found to correlate with any of the other measures of performance.

(ii) There will be little overlap between the "correct" and "need" measure of performance as they are reflecting essentially different things. Thus the correlation between them although positive, will be small.

With relation to the relationship of differential sensitivity to cues of reinforcement and the Eysenckian dimensions one might offer the following hypotheses:

Hypothesis 5:2 (i) On the "correct" measure extraverts will obtain a significantly larger score than the introverts; there will also be an E x N interaction.

(ii) These differences will not be evident on the "net" score measure, nor on the "need" score.

Hypothesis 5:3 (i) There will be a positive correlation between the EPI E score and the "correct" score.

(ii) EPI N will be found to correlate significantly with the total number of correct responses made, regardless of whether they are responses to cues of positive or negative reinforcement.

As no evidence has been forth coming to support the hypothesis that Eysenck's introversion-extraversion is related to the dimension of differential sensitivity to cues of

reinforcement it was decided to obtain from Ss a rating indicating to which they felt more sensitive, cues of reward or punishment. The self-rating, of course, presents a special problem, for although Ss were asked about sensitivity to the likelihood of pleasant or unpleasant outcomes - "Are you more motivated by, or sensitive to the likelihood of a pleasant outcome or the likelihood of an unpleasant outcome?" - they may well have been rating their sensitivity to rewards and punishments rather than to the cues of reinforcement. This results in two possible hypotheses:

Hypothesis 5:4 (i) If Ss are rating sensitivity to cues then those who rate themselves more towards the reward end of the scale should obtain a higher "correct" score

(ii) If Ss are rating sensitivity to reward and punishment itself the difference between Ss rating themselves as more sensitive to rewards and punishment should be evident on the "need" score.

Need - Achievement

There are certain, at least superficial, similarities between Gray's concept of differential sensitivity and the older ones of need-achievement (n-ach) and fear of failure, those individuals relatively more sensitive to cues of reward resembling the high need-achievers. It can be maintained, however, that the two concepts are quite distinct, one being a sensitivity, the other a need and willingness to engage in activity to obtain a goal. It is possible, indeed likely, that, at times, the two qualities will be evident in the same individual, but it is not necessary that they are.

Given the superficial similarities it was felt worthwhile including a measure of n-ach in the present study. It is hypothesized that:

Hypothesis 5:5 (i) High n-ach scorers will obtain higher "need" scores than low n-ach scorers.

(ii) The high n-ach scores may also obtain higher "net" scores

(iii) No significant difference will be found between high and low n-ach scorers on the "correct" score measure

(iv) n-ach will not be found to correlate with any supposed measure of sensitivity i.e. "correct" score, extraversion, rated sensitivity to cues of reward, to cues of punishment, or differential sensitivity to cues of reinforcement.

LOCUS OF CONTROL

AWARENESS

In experiment 2 a significantly greater proportion of introverted, than of extraverted, Ss became aware of the prevailing reinforcement contingencies. The pattern was the same, though the differences were not significant in experiment 3. On the basis of this finding it might be speculated that introverts, as a result of their superior awareness, might feel that they have some degree of control over the environment - they can relate their behaviour directly to changes in the environment. Extraverts, in contrast, not noticing the relationship between responses and reinforcement might well not experience this sense of control and instead feel they were simply responding to the situation. This sense of controling or being controlled is, of course, similar to Rotter's (1966) concept of Locus of Control, (LoC). Having remarked on this similarity one might predict that individuals characterized by internal beliefs might well be cue sensitive. Indeed if LoC were divided into internal-external beliefs for negative and for positive beliefs, then

one might suggest that those individuals classified as internals for positive beliefs (In+) are the same individuals Gray classifies as relatively more sensitive to cues of reward. While internals for negative events (In-) may be regarded as identical with those people Gray classifies as relatively more sensitive to cues of punishment.

Before continuing with this line of speculation, however, it seems appropriate to say something of Rotters concept of LoC and refer to some recent research relevant to it.

LoC belongs to the same tradition as the social learning theories of Mischel (1973) and Bandura (1977) outlined earlier and so it should be regarded as what Mischel calls a "person variable" rather than a personality trait. Rotter (1966) describes it as follows: "The effect of reinforcement following some behaviour on the part of a human subject..... is not a stamping in process but depends on whether or not the person perceives a causal relationship between his own behaviour and the reward..... when reinforcement is perceived by the subject as following some action of his own but not being entirely contingent upon his action, then, in our culture, it is typically perceived as the result of luck, chance, fate, as under the control of powerful others, or as unpredictable because of the great complexity of the forces surrounding him. When the event is interpreted in this way by an individual, we have labelled this a belief in external control. If the person perceives that the event is contingent upon his own behaviour or his own relatively permanent characteristics, we have termed this a belief in internal control." (p.1)

Thus internals (Ins) are aware of, or perceived, the relationship between their behaviour and its consequences

whereas externals (Exs) do not. This is much as we found introverted and extraverted Ss, respectively, to behave. It seems, then, that it may be reasonable to look at the relationship between these two - LoC and introversion-extraversion.

Eysenck, of course, contends that the introversion-extraversion dimension is closely related to a factor of conditionability, though this has received only limited support in experiments 1 - 3. As for LoC there appears to be little evidence that Ins condition more efficiently than Ex.s. The situation is somewhat complex, however, as there is some suggestion that In.s strenuously resist any attempts at subtly influencing their behaviour.

Getter (1966) found that though Ex.s conditioned more efficiently than In.s in a verbal conditioning situation, the In.s showed an increase in the conditioned response once reinforcement was withdrawn. This he interpreted as latent conditioning. Similarly Strickland (1970) found that though In.s quickly became aware of the prevailing reinforcement contingencies,they did not display the CR. Ude and Vogler (1969) also reported that In.s were more likely to become aware of reinforcement contingencies. Lichtenstein and Craine (1969) failed to support the results of Getter and Strickland.

Jolly and Spielberger (1973) reported that in their study only Ss aware of the reinforcement contingencies were found to condition. LoC and anxiety were found to be unrelated to performance among Ss classified as unaware. Amongst the "aware" Ss high anxiety - Ex.s were more responsive to social reinforcement than high anxiety - In.s, but low anxiety - In.s were more responsive than low anxiety - Ex.s.

Doctor (1971) found that only aware Ex.s conditioned,

and Gore (1962) found that In.s were resistant to subtle influences. Biondo and MacDonald (1971), have called these findings into question, however.

Studies by Ritchie and Phares (1969), Hjelle and Clauser (1970), Rychman, Martens, Rodda and Sherman (1972) and Snyder and Larson (1972) suggest the Ex.s are more interested in the source than the content of a message. While Hamsher, Galler and Rotter (1968) found that it was Ex.s who tended to disbelieve the Warren Commission Report.

Approaching the problem from a different angle Lefcourt (1972) found that Ex.s performed a task according to the instructions they were given, while In.s did not. Lefcourt, Lewis and Silverman (1968) found that In.s were more reluctant to accept instructions which emphasized the role of chance, i.e. instructions which were at variance with their own beliefs.

Similarly Crowne and Liverant (1963) using an Asch - type conformity situation found that in some conditions In.s displayed less conformity than Ex.s, and Tolor (1971) reported that Ex.s were more susceptible than In.s to the auto-kinetic effect.

Phares (1976) has concluded "one might assert that externals appear readily persuadable, conforming to what they believe is expected of them, and accepting of information or other sources of influence. This is not to say that internals never conform or never move their attitudes in the direction of the applied persuasion. But when they do, it appears to be on the basis of a considered analysis of the merits of the message. Majorities, peer influence, prestige of communicators, or the social reinforcements available in the situation all affect internals to a much lesser extent than they do externals. Indeed, the evidence suggests that there may be active resistance to influence, particularly subtle

influence, on the part of internals." (p.92)

Whether or not this conclusion is entirely justified on the basis of the available data is debatable, but what does seem to emerge with some consistency is that In.s become aware of what is "going on" more quickly than do Ex.s and respond in a manner they feel is most appropriate. On the basis of this, then, it seems reasonable to postulate that there will be at least a moderate relationship between LoC and the introversion-extraversion dimension. This, then, may be taken as another hypothesis.

Hypothesis 5:6 There will be a positive correlation between Rotter's (1966) LoC scale (scored in the direction of externality) and the EPI E scale.

Anxiety and Ego-Strength

Another aspect of LoC which promotes interest in the present situation is that In.s, at least those who are moderately internal, might be viewed as looking at the world in a realistic manner and responding appropriately to it. This is,of course, how Roessler views Es.

Rotter (1966), himself, has commented on the relationship. "Perhaps less clear is the relationship of internal versus external control of reinforcement to the notion of 'ego-control'. Although the concept of ego-control is not always defined similarly, it seems to contain the idea of confidence and ability to deal with reality. While it seems likely that individuals at both extremes of the internal versus external control of reinforcement dimension are essentially unrealistic, it is not unlikely that the people towards the middle of the distribution are less confident. We do have indications, however, that people at either extreme of the reinforcement dimension are likely to be maladjusted by most definitions, and,

to the extent that ego-control is another type of definition of maladjustment, it will bear some curvilinear relationship to the variable we are concerned with here." (p.4).

Es is, of course, closely related to neuroticism, and a number of studies have reported significant relationships between LoC and anxiety, if not neuroticism (cf Phares; 1976, p.121). It is, therefore, hypothesized

Hypothesis 5:7 (i) In view of Rotter's suggestion that the relationship between LoC and Es is likely to be curvilinear it is predicted that any relationship between these two variables will be moderate in size and negative

(ii) Similarly EPI N is expected to show a moderate and positive relationship with LoC.

Competence

One would expect competence to be allied to an accurate perception of reality, and perhaps the greatest distinction that has been made between In.s and Ex.s has been in terms of their differential efficiency and success.

Much of the early experimental work in this area was directed towards demonstrating that people behave differently when they believe they have or have not control of reward contingencies (eg. Phares, 1957; James, 1957; James and Rotter, 1958; Bennion, 1961, Rotter, Liverant and Crowne 1961; Blackman, 1962; Fazio and Hendricks, 1970; Walls and Cox, 1971). Differences were found in the success with which tasks were performed, learning and extinction rates, in behaviour related to changes in the porbability of an outcome. "Whether (people) are confronted with chance instructions, a task they have learned in the past is chance - controlled, or a highly variable or unpatterned performance, the results

are the same: They learn a great deal less, and this decrement in learning seems directly attributable to the effects on expectancy of a belief that, in a given situation, they do not control the relationship between behaviour and reinforcement". (Phares, 1976, p.30).

Given that predictable differences in behaviour could be expected from people who held different beliefs about the control of reinforcement, on the basis of experimental manipulations, Rotter, Phares and others, from the early 1960's, have set out to demonstrate that similar differences might be elicited from people who would be characterized as internals or externals. The differences in behaviour being attributed to the different beliefs held by internals and externals.

It has been claimed that In.s seek out and use information more efficiently than Ex.s (e.g. Seeman and Evans, 1962; Seeman, 1963; Davis and Phares, 1967; Willis and Stock, 1972). Lefcourt and Wine (1967) reported that In.s attempted to engage in more eye contact, and made more observations of the situation when it was ambiguous than Ex.s did. Lefcourt (1967) suggested that Ex.s did not categorize the situation in a way that facilitated awareness of the contingencies of reinforcement, although they were able to use reinforcement cues if they were made explicit.

DuCette and Wolk (1973) found that internals remembered successes better than externals did, and used experience both to improve their own performance and to decipher rules. In.s have also been found to display more incidental learning (Wolk and DuCette, 1974).

As might be expected given the superiority of the In.s in the acquisition and use of information LoC has been associated with differential success. Indeed LoC has been

associated with all kinds of behaviours, from birth control practices (Lundy, 1972; MacDonald, 1970; Phares and Wilson, 1972) to giving up smoking (Straits and Sechrest, 1963; James, Woodruff and Werner, 1965) with In.s generally exhibiting the greater degree of control. Although Lichtenstein and Keutzer (1967) failed to replicate the findings of earlier studies relating smoking to LoC.

Several studies have looked at the relationship between LoC and risk taking. (e.g. Liverant and Scodel 1960; Lefcourt, 1965; Strickland, Lewicki and Katz, 1966; Baron, 1968; Julian, Lichtman and Ryckman, 1968). It would be misleading to say that all these studies yield consonant results, for, at least at first sight, they appear to contradict each other. However, it does appear that there is a general trend with the In.s behaving in a manner which might be described as "appropriate".

In.s have been found to be the more successful in an auctioning situation (Elkins and Cochran, 1978); they are more often office holders, (Brown and Strickland, 1972); they are more willing to take steps to correct personal short comings (Phares, Ritchie and Davis, 1968). In.s have been found to be more "efficient" in eliciting the desired experimental results (Felton, 1971); and they have even been found to induce a greater degree of attitude change than externals, when reading from the same prepared script. (Phares, 1965).

Several studies (e.g. Bailer, 1961; Strickland, 1972; 1973; Walls and Smith, 1970) have reported a relationship between LoC and the ability to delay gratification. Other studies, however, (e.g. Walls and Miller, 1970; Zytkoskee, Strickland and Watson, 1971) have failed to confirm that such a relationship exists.

Many studies (e.g. Cellura, 1963; Crandall, Katkovsky and Crandall, 1965; Chance, 1965; McGhee and Crandall, 1968; Gurin, Gurin, Lao and Beattie, 1969; Buck and Austrin 1970; Lao, 1970; Solomon, Houlihan, Busse and Pardius 1971; Brown and Strickland, 1972; Clifford and Cleary, 1972; Messer, 1972) have reported a relationship between internality and school achievement. Not all studies have managed to find such a relationship, however, (e.g. Eisenman and Platt, 1968; Katz, 1967; Warehime, 1972).

"The most basic characteristic of internal individuals appears to be their greater efforts at coping with or attaining mastery over their environments. This is the most elemental deduction that could be made from the nature of the I - E (internal ity - externality) variable. Fortunately, this deduction has received wide spread support from experiments with many different populations in a variety of situations." (Phares, 1976, p.78).

This description of In.s as exerting greater efforts at coping and being generally more successful leads to the suggestion that LoC may well be related to n-ach. Rotter (1966) has remarked on the similarity: "Perhaps one of the major conceptions which bears some relationship to the belief in internal versus external control of reinforcement is that of need for achievement. The work of McClelland, Atkinson, Clark and Lowell (1953) and of Atkinson (1958) and their colleagues working primarily with adults, and Crandall (1963) with children, suggests that people who are high on the need for achievement, in all probability, have some belief in their own ability or skill to determine the outcome of their efforts. The relationship is probably not linear, however, since a person high on motivation for achievement might not

be equally high on a belief in internal control of reinforcement, and there may be many with a low need for achievement who still believe that their own behaviour determines the kinds of reinforcements they obtain". (p.3).

Empirically the two dimensions have not been found to be closely related. Lichtman and Julian (1964), and Gold (1968), and Wolk and DuCette (1971) found no relationship between the two variables. Odell (1959) and Pedhazur and Wheeler (1971) reported small correlations; Mehrabian (1968, 1969) reported a correlation of .64 for males and .41 for females, between LoC and n-ach. Chance, on the other hand reported a negative correlation between internality and n-ach.

Durand and Shea (1974), looking at the entrepreneurial activity of black small business men attempted to distinguish between LoC as a belief in control of reinforcement, on n-ach - a "know(ing) what to do", or a "typically thinking of instrumental activities that lead to an achievement goal" (p.59). They reported a correlation of -.19 between locus of control and n-ach.

Given, then, that In.s are generally more successful than Ex.s it might be predicted that in the present experiment:

Hypothesis 5:8 (i) Those Ss classified as internal will be found to have a larger Net score than those Ss classified as external.

(ii) The correlation between LoC (scored in the direction of externality) and n-ach will be small and negative.

Unitariness

To this point LoC has been spoken of as if it were unitary, but this, of course, is not necessarily so. There appears to be no a priori reason for maintaining that those

who believe they are responsible for their success must also believe that they are responsible for their failures, or even that those who feel they are in control of their fate to a substantial degree must feel that luck and chance are unimportant. This is not the first time the unitariness of LoC has been questioned. Phares (1976) noted that Liverant, Rotter, Seeman and Crowne (Rotter, 1966) looked for sub-scales to the general internality-externality one.

Mirels' (1970) factor analysis yielded two subscales: (1) personal mastery in life, (2) the extent to which an individual can influence society. Abrahamson, Schulderman and Schulderman (1973) found similar factors. Gurin, Gurin, Lao and Beattie (1969) constructed a questionnaire with two sub-scales: (1) personal, (2) ideological. Schneider and Parson's (1970) identified five sub-dimensions (1) general luck (2) respect, (3) politics (4) academic and leadership, (5) success. Levenson (1973a, b, c), constructed scales related to internality, powerful others, and chance.

Similar to the distinction made earlier between internality-externality for success and failure is that made by Crandall, Katkovsky and Crandall (1965) in their Intellectual Achievement Responsibility questionnaire (IAR). This has two sub-scales, one measuring internality for achievement (In+), the other internality for failure (In-). These two sub-scales have been found to relate differentially to academic success.

A recent study, reported after the present study was completed, by Gregory (1978) seems worth considering in some detail here. Gregory claimed that "No studies have been designed expressly to test whether the Rotter (1966) scale can distinguish between Internals and Externals in both positive and negative outcomes." (p.841).

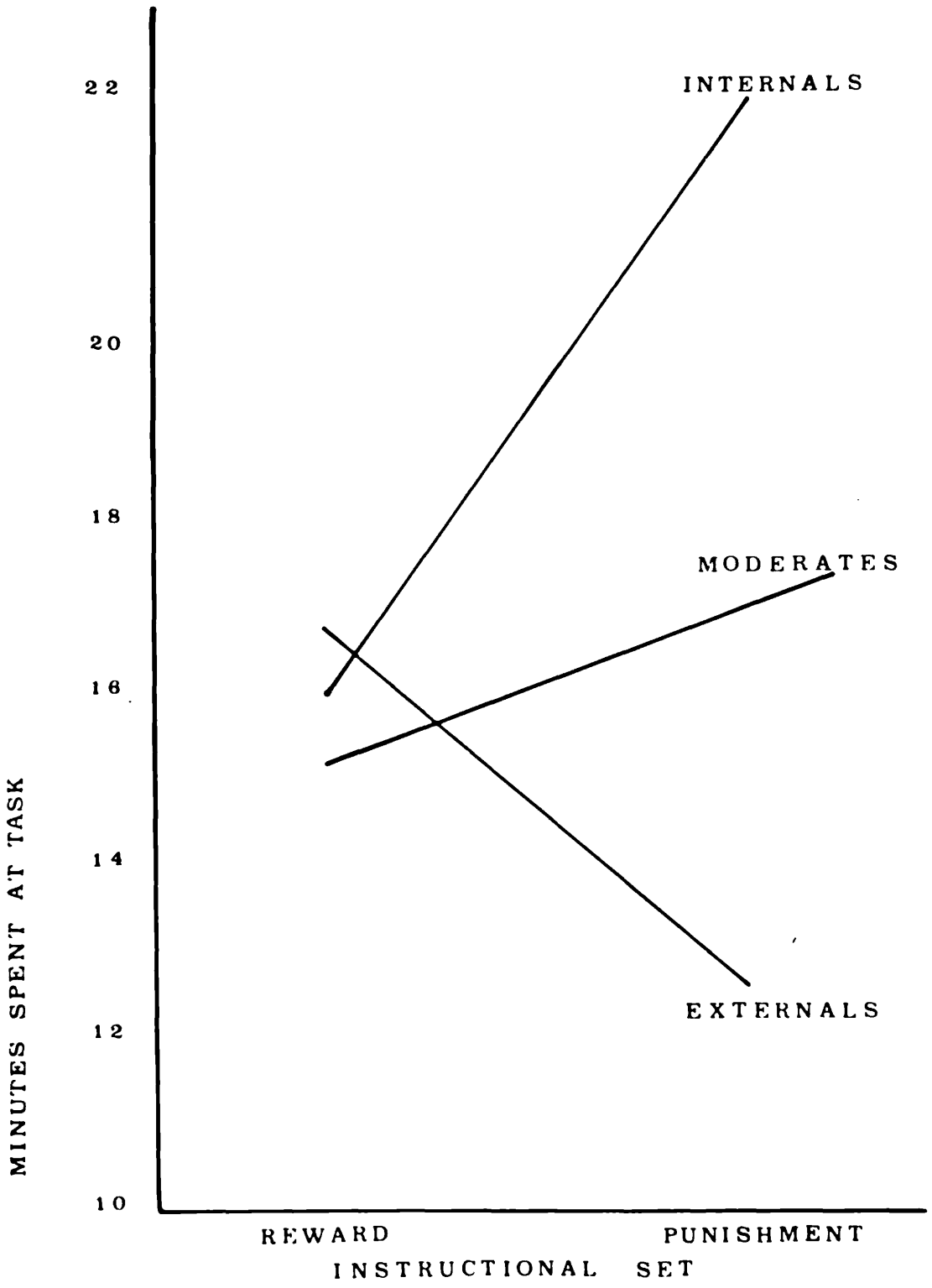


FIG 11:1 MEAN AMOUNT OF TIME SPENT BY Ss ON TASK AS A FUNCTION OF LoC AND INSTRUCTIONAL SET
(GREGORY 1978, p 845

His prediction was, however, that the LoC scale would distinguish between the more and less successful individuals only in aversive conditions i.e. In.s are only superior to Ex.s with respect to avoiding punishments.

59 Females and 48 males took part in the experiment and they were required to match various angles with a stimulus angle. The task was the same as that used by James (1957) and Rotter and Mulry (1965). All the test angles were within 2^{o} of the stimulus angle but presented in a different rotation. There were no correct choices available. Course credits were used as both positive and negative reinforcers. Ss were classified both on the basis of Rotter's (1966) LoC scale and the Malevolent- Benevolent Questionnaire (MBQ) (Gregory et al, 1978). On the basis of this Ss were allocated to categories: (1) In+ In- (2) In+ Ex- (3) Ex+ In (4) Ex+ Ex-

As can be seen from Fig. 11:1 it is only in the punishment condition that a substantial difference is found between groups, as Gregory had predicted. It would seem then that there is some basis for Gregory's claim that Rotter's (1966) scale is really measuring locus of control for avoiding unpleasant outcomes.

One would predict that in those situations where the S feels he has more control he will take more time in order to be sure of making the correct decision. Table 11:1 indicates that when individuals feel they have more control in positive than in negative situations (In+Ex-) or vice versa then they do, indeed, take more time over the task in the appropriate condition. While if they see no difference in their control whether the situation is positive or negative (In+In-, and Ex+Ex- groups), then as might be expected they do not display any difference in decision

time in the two situations.

TABLE 11:1 TIME SPENT ON A MATCHING TASK AS A FUNCTION OF INSTRUCTIONAL SET AND MBQ CATEGORIZATION

LoC				
Positive Outcome	Negative Outcome	Reward	Punishment	P
In+	In-	18	17.5	N.S.
	Ex-	27.6	14.5	$p < .01$
Ex+	In-	11.7	19.0	$p < .08$
	Ex-	15.5	18.3	N.S.

These results, however, raise some problems of their own. Only one of the four comparisons reached the $p = .05$ level of significance, though admittedly one other was substantially in the predicted direction. If the hypothesis put forward to explain these results was correct then one would expect the In+ I_n- group to take longer in making their decision in both reward and punishment conditions than the Ex+ Ex- group, but as can be seen this does not appear to be the case. Similarly though the In+ groups take longer in the reward condition than the Ex+ groups, the situation is not equally reassuring with respect to the In- groups, in the punishment condition.

Ss were allocated to groups on the basis of their MBQ scores for this analysis, and so it appears we have confounded the problem. (1) Are there two LoC dimensions one referring to positive - rewarding events, the other to negative - punishing events? (2) Does the MBQ measure them both adequately?

From the results reported in Fig. 11:1, where Ss were allocated to groups on the basis of their scores on the Rotter LoC scale, it does appear that there are two substantially independent variables, with the Rotter scale relevant only to the punishment one. One might wish to argue that the results are as they are because reward has some over-riding effect which obscures any effect due to belief in LoC. This, though possible, seems unlikely given the fact that internals and externals diverge from this moderate level obtained in the reward condition in different and predictable directions in the punishment condition.

One might argue that externals are simply more motivated in the face of reward, but this does not explain why the internal group takes less time in this condition than in the punishment condition.

It seems likely that the problems with the MBQ results lie, at least in part, with the instrument itself. Gregory, himself, has remarked that the MBQ was "not designed to serve as a locus of control measure". (p.842).

It is also worth noting in the present context that Mischel (Mischel, Zeiss and Zeiss, 1974) has also devised a locus of control scale, for use with pre-school children. This scale also employs the In+, In- distinction.

The locus of control dimension, of course, grew out of Rotter's (1954) social learning theory and though at times it has been used as if it were a personality trait Rotter has stressed that it is not. Mischel (1974) in his turn has said: "The present study has been guided not by the search for the global correlates of pervasive generalized dispositions but by an interest in the particular relations between differences in expectancies and theoretically relevant behaviour in specific situations. The overall

results suggest that individual differences about their ability to control outcomes are partial determinants of their goal directed behaviour, but the relationships hinge on extremely specific moderating conditions with regard to both types of behaviour and type of belief" (p.278).

On the basis of the distinction between belief in control of outcomes for positive and negative events, and particularly with Gregory's (1978) experiment in mind the following hypotheses may be put forward.

Hypothesis 5:9 (i) If Ss are asked to rate their belief in their control of positive and negative outcomes separately then: Rotter's scale (scored in the direction of externality) should (i) correlate negatively with rated internality for negative outcomes.
(ii) but not with positive outcomes.

Hypothesis 5:10 (i) We might also expect rated In+ to correlate with rated sensitivity to cues of reward
(ii) Similarly rated In- should be found to correlate with rated sensitivity to cues of punishment.

Hypothesis 5:11 On the basis of the results of Gregory's experiment it might be predicted that those Ss classified as internal on the Rotter scale will have a low "Correct" score (reflecting the fact that they make proportionally more correct avoidance than gain responses).

Hypothesis 5:12 There ought to be a significant In- x In+ interaction, and In+Ex- Ss obtaining the highest "Correct" score and the Ex+In- group the lowest "Correct" score.

Hypothesis 5:13 A similar interaction might also be predicted between LoC and differential sensitivity to cues of reward and punishment, with the Internal-punishment

sensitive Ss obtaining the lowest correct score

Consistency

Several subjects took part in both the present experiment and the previous one, and this gave the opportunity to look at the consistency of both (i) self report data and (ii) behavioural data.

With respect to the self report data, as the same questionnaires were used and there seemed to be little in the two situations which might persuade people to respond in a markedly different way, and the time interval between the two testings was 3 - 4 months, it might be expected that substantial correlations would be obtained.

For the behavioural data no significant correlations are predicted. In experiment 4 some consistency was found for errors of omission. In the present situation, however, to employ a strategy which resulted in a large number of omissions would prove costly. Thus, because, of the different strategies suggested by the two experimental situations, different patterns of behaviour are expected, and no evidence of rank order consistency in behaviour.

With regard to the commission errors, while there may well be many in the present experiment, as there were so few in the previous experiment, and the various commission error scores were not found to correlate, it seems highly unlikely that significant correlations will be found in the present instance.

Hypothesis 5:14 There will be significant correlations between the self ratings obtained in this present experiment and the ratings obtained in experiment 4 for the same dimension.

Hypothesis 5:15 No significant correlations will be found between the commission errors or the omission errors made in the two experiments

As Ss were being asked to complete several questionnaires it was decided to administer these same questionnaires to a larger group of individuals. This was to make a factor analysis of the results possible. The main point of interest here was whether or not sensitivity to cues of reinforcement would emerge as a meaningful self report factor, or whether it would be subsummed under some other factor. A second point of interest was whether or not locus of control would be found to split into two factors.

Hypothesis 5:16 Several factors ought to emerge from the factor analysis:

(1) Extraversion loaded on by EPI -E, rated Extraversion (sociability), and rated Impulsivity
(2) Neuroticism loaded on by EPI -N, Es and rated neuroticism
(3) Differential sensitivity to cues of Reinforcement
(4) Sensitivity to cues of reward
(5) Sensitivity to cues of punishment
(6) Locus of control for positive events
(7) Locus of control for negative events, loaded on by the Rotter scale, and the self rating for In-.

No one factor of consistency is predicted.

With both questionnaire and behavioural data available it is possible to turn a question round and ask not only if people who rate themselves differently behave differently, but also if people who behave differently rate themselves differently.

Hypothesis 5:17 If S are divided into high, medium and low "Correct" scorers, they ought to be found to rate themselves differently on the EPI -E scale, on sensitivity to cues of

reward and punishment, on the Rotter scale, and on the In+ and In- questions.

Hypothesis 5:18 Ss divided into groups on the basis of their "Need" scores ought to be found to rate themselves differently with respect to n-ach.

11:2 SUMMARY OF HYPOTHESES

5:1 Measures derived in different ways will be found to reflect different qualities:

(i) The "Net" score will not correlate with any of the other measures of performance.

(ii) The correlation between the "Correct" and "Need" scores will be small but positive.

5:2 (i) When the "Correct" score is used as the dependent variable the extraversion main effect and the E x N interaction will be found to be significant.

(ii) No significant differences will be found between extraversion groups when the "Net" score or "Need" score is used as the dependent variable.

5:3 (i) There will be a positive correlation between EPI E and the "Correct" score.

(ii) There will be a positive correlation between EPI N and the number of correct responses made.

5:4 (i) There will be a significant difference between those rating themselves as relatively more sensitive to reward and those rating themselves as relatively more sensitive to punishment on the "Correct" measure.

(ii) There will be a significant difference between Ss rating themselves as more sensitive to cues of reward and those rating themselves as more sensitive to punishment on the "Need" score.

5:5 (i) High n-ach Ss will obtain higher "Need" scores than low n-ach Ss

(ii) High n-ach scorers will obtain higher "Net" scores than low n-ach scorers

(iii) No significant difference will be found between high and low n-ach scorers on the "Correct" score measure.

(iv) N-ach will not be found to correlate with EPI E, rated extraversion, rated sensitivity to cues of reward or punishment, or rated differential sensitivity to cues of reward.

5:6 There will be a correlation between externality and EPI E

5:7 (i) There will be a small, negative correlation between Es and externality

(ii) There will be a positive correlation between EPI N and externality.

5:8 (i) Internals will obtain larger "Net" scores than externals.

(ii) There will be a small, negative correlation between externality and n-ach.

5:9 (i) Rotter's scale will correlate negatively with self rated internality for negative out-comes.

(ii) Rotter's scale will not correlate with rated internality for positive out-comes.

5:10 (i) Rated internality for positive out-comes will correlate with rated sensitivity for cues of reward.

(ii) Rated internality for negative out-comes will be found to correlate with rated sensitivity to cues of punishment.

5:11 Internals will be found to have a lower "Correct" score than externals

5:12 There will be an In+ x In- interaction when the "Correct" score is used as the independent variable.

5:13 There will be a locus of control x differential sensitivity to cues of reinforcement interaction when the "Correct" score is used as the independent variable.

5:14 There will be a significant correlation between self ratings obtained in the present experiment and ratings obtained for the same dimensions in the previous experiment.

5:15 No significant correlations will be found between commission errors or omission errors made in the present experiment and those made in the previous experiment.

5:16 A factor analysis will yield factors of: extraversion, neuroticism, sensitivity to cues of reward and punishment, differential sensitivity to cues of reinforcement locus of control, locus of control for positive and negative out-comes, and possibly several factors of consistency.

5:17 Ss differentiated on the basis of their "Correct" scores will be found to rate themselves differently on the EPI E scale, on sensitivity to cues of reward and punishment, on Rotter's Locus of Control scale and on rated internality to positive and negative out-comes.

5:18 Ss differentiated on the basis of their "Need" scores will be found to rate themselves differently on the n-ach scale.

11:3 METHOD

DESIGN

Subjects were required to make differential responses to each of three classes of stimuli.

1. When a syllable of the form vowel - consonant - vowel (VCV), (for 50% of Ss CVC), appeared on the screen Ss were

required to press the right hand most key (left for 50% of Ss) in order to gain a point. This response had to be made within 700 msec.s of the on-set of the stimulus. If the S failed to respond with the correct key, or responded after the time limit had expired he did not lose a point - this was purely a reward condition.

2. When a CVC appeared on the screen (for 50% of Ss a VCV) S was required to respond with the left-hand most (right for 50% of Ss) of the three keys within 700 msec.s of the on-set of the stimulus in order to avoid losing a point. This was the punishment condition, correct responding did not gain a point for the S.

The relationship between key, stimulus and reinforcement was randomized across Ss.

3. When a syllable made up of either three vowels (VVV) or three consonants (CCC) was presented the S was required to press the second (middle one) of the three keys. This was the neutral condition and S received neither reward nor punishment in connection with these stimuli.

Ss were given feed back on their performance after each trial in the form of a running score (Net score). On each trial the type of stimulus presented, the response made and the latency of the response were recorded. This information was used to compute a series of behavioural scores which served as the dependent variables in the analyses of the present experiment.

Ss also completed a number of questionnaires: EPI (Form A), Internality-Externality scale (Rotter, 1966), Es scale, the Quike Measure of Achievement Motivation (Smith, 1973), and a series of 9 point rating scales (cf Appendix).

Self-rating scores, commission error and omission error scores were available from the previous experiment (4a and 4b)

Procedure

65 Subjects took part in this experiment, 28 of whom had taken part in the previous experiment. 162 Sscompleted the questionnaires. Of the 65 experimental Ss 55 provided questionnaire data (37 Female, 18 Male). Subjects were volunteer university students, with a mean age of approximately 22 years

Each S was tested alone. The S was seated 3ft from an osciloscope screen which was 4ft 8ins from the floor. On a table in front of the S were three keys, attached by means of a magnet to a large metal plate. The S was told that he would be allowed to use only his preferred hand, and was asked to place the keys in a position which was most comfortable for him, and which allowed him to respond as quickly as possible.

The S was given the following instructions. They were typed, and the S was allowed to read them at his own pace

Instructions

This experiment takes the form of a game; your task is to gain as many points as possible and lose as few as possible.

Three types of syllables will appear on the screen:

(1) consonant-vowel-consonant (CVC)

(2) vowel-consonant-vowel (VCV)

(3) Three vowels (VVV) or three consonants (CCC).

If a CVC* appears and you press Key 1*, the right hand key* within half a second you gain a point. If you fail to press this key you do not lose a point

PRESS TO GAIN

If a VCV* appears on the screen you must press Key 3*, the left hand key* to avoid losing a point. Again you have half a second, you do not gain any points for pressing this key.

PRESS OR LOSE

If a CCC or VVV appears on the screen then you are to press Key 2, the centre key. You will not gain or lose anything when these syllables are presented.

Your score will be displayed on the screen after each response.

You will have twenty practice trials, at a slow speed to get used to the task."

* These were randomized across Ss yielding four combinations.

(1) CVC - 1 = gain, VCV - 3 = Avoid

(2) CVC - 3 = gain, VCV - 1 = Avoid

(3) CVC - 1 = Avoid, VCV - 3 = gain

(4) CVC - 3 = Avoid, VCV - 1 = gain

The S was given 20 practice trials, and more if he still did not feel confident about what was required of him. Syllables were presented for 1500 msec.s during these practice trials, and S could respond at anytime in this period. There was 500 msec.s between trials during which the running score was displayed.

For the test trials S was alone in a room, the computer controling the presentation of stimuli and recording response data. Stimuli were presented for 700 msec.s with a 500 msec inter-trial interval during which the running score was displayed. Syllables were presented in random order.

There were 180 trials, 60 per condition.

The oscilloscope screen on which the syllables were presented measured 21.6 x 17 cms. Syllables were displayed in the middle of the screen, slightly above centre; scores were presented in the middle of the screen slightly below centre.

The main behavioural measures were described in the introduction, but for the sake of completeness they will be described here once more.

"Net" score: The number of points gained minus the number of points lost.

"Total" score: The total number of times the gain key (TG) was pressed minus the total number of times the avoid key (TA) was pressed, divided by the total number of responses (TR) made.

$$\frac{(TG - TA)}{TR}$$

"Correct" score: The number of correct gain responses (CG) made minus the number of correct avoidance responses (CA) made divided by the total number of correct responses made (TC)

$$\frac{(CG - CA)}{TC}$$

"Error" score: The number of erroneous "gain" responses (EG) minus the number of erroneous "Avoidance" responses (EA) made divided by the total number of erroneous responses (TE)

$$\frac{(EG - EA)}{TE}$$

"Need" score: The number of "gain" responses made in response to neutral stimuli (NG) minus the number of "avoid" responses made to neutral stimuli (NA) divided by their sum

$$\frac{(NG - NA)}{(NG + NA)}$$

Ratio scores were preferred as these take into account the number of responses made, and so eliminate any effects due simply to the number of responses made. Other scores were computed however

Ratio scores:

(1) TC/TA

(2) CG/CA

(3) EG/EA

(4) NG/NA

Difference scores:

(1) TC-TA

(2) CG-CA

(3) EG-EA

(4) NG-NA

Mean RT scores

(1) (RT of TC)/(RT of TA)

(2) (RT of CG)/(RT of CA)

(3) (RT of EG)/(RT of EA)

(4) (RT of NG)/(RT of NA)

11:4 RESULTS

In such a long series of trials, 180 without pauses, it seems likely that practice and fatigue effects may well influence the S's behaviour. It also seems likely that an S will employ different strategies at different points in the task. In order to minimize the risk of arriving at spurious conclusions, therefore, where appropriate four separate scores will be reported: those relevant to the first, second and third thirds of the experiment, and results derived from all 180 trials.

The Behavioural Measures

The first hypothesis dealt with the behavioural measures, the argument being quite simply that if there was any substance to Gray's theory then some measures ought to be tapping differential sensitivity while others should not

TABLE 11:2 CORRELATIONS BETWEEN THE "NET" SCORE AND THE FOUR OTHER MEASURES OF BEHAVIOUR

Behavioural Measures	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
"Total" Score	-.0168	.0235	.0134	-.0118
"Correct" Score	.0585	.0434	.0277	.09
"Error" Score	.0009	.0597	-.0043	.06
"Need" Score	.0317	-.1058	.0034	-.143

As can be seen none of these correlations is significant.

A more important prediction, because the interpretation of later results depends on it, is that the "Correct" measure reflects differential sensitivity, while the "Need" measure reflects, substantially, the desire to gain or avoid certain types of reinforcement. These two measures, therefore, ought to be substantially independent.

TABLE 11:3 CORRELATIONS BETWEEN THE "CORRECT" AND "NEED" SCORES

Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
.227*	.11	.259*	.287*

(*$p < .05$)

The correlations between these two measures, as can be seen from Table 11:3, are significant, probably reflecting the motivational element in the "Correct" measure, but they also are small suggesting, as was predicted, a substantial independence of the two measures.

Gray's Model and the Eysenckian Dimensions

No support has been found so far for Gray's suggestion that differential sensitivity is related to the introversion-extraversion dimension, or general sensitivity to cues of reinforcement to the neuroticism dimension. As the "Correct" measure is assummed to best reflect differential sensitivity it should be on this measure that the predicted differences are most evident.

TABLE 11:4 SUMMARY OF ANALYSES OF VARIANCE EMPLOYING E AND N AS THE INDEPENDENT VARIABLES

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41- 60 per condition	Trials 1 - 60 per condition
"Net" score	N.S.	N.S.	N.S.	N.S.
"Total" score	N.S.	N.S.	N.S.	N.S.
"Correct" score	E p = .038	N.S.	N.S.	N.S.
"Error" score	N.S.	N.S.	N.S.	N.S.
"Need" score	N.S.	E p = .051	N.S.	N.S.

As can be seen in Table 11:4 only one result is found to be significant on the "Correct" measure, where, if Gray were correct, one would expect a significant extraversion effect and extraversion x neuroticism interaction in each of the four analyses. However, in this one significant result the groups do arrange themselves as predicted (cf Table 11:5)

TABLE 11:5 GROUP MEANS ON THE "CORRECT" MEASURE, FOR THE FIRST EPOCH OF TRIALS (1 - 20)

	Introverts	Extraverts
LN	-.045	.104
HN	-.168	.227

The only other result to reach significance in this set of analyses was again a significant difference between the introverted and extraverted Ss this time when the "Need" score was used as the dependent variable (cf Table 11:4)

TABLE 11:6 GROUP MEANS FOR THE "NEED" SCORE, FOR TRIALS 21-40

	Introverts	Extraverts
LN	.201	-.241
HN	.036	-.11

The means reported in Table 11:6 indicate that it is the introverted subjects who respond with the "gain" key more often in face of the neutral stimuli.

It was hypothesized (hypothesis 5:3(i)) that the EPI - E score would correlate with the "correct" score, but as can be seen from Table 11:7, except for the first third of the experiment this is not the case.

TABLE 11:7 CORRELATIONS BETWEEN EPI - E AND THE FOUR MEASURES OF PERFORMANCE

Behavioural Measures	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
"Total" score	.166	.029	-.003	.09
"Correct" score	.254*	.076	.048	.17
"Error" score	.058	-.114	.041	-.027
"Need" score	.005	-.293*	.034	-.037

($p < .05$)

The hypothesized relationship between neuroticism and sensitivity fared even less well (cf. Table 11:8)

TABLE 11:8 CORRELATIONS BETWEEN EPI N SCORES AND THE NUMBER OF CORRECT RESPONSES TO DIFFERENT STIMULI

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
1. Correct "Gain" responses	.1	.113	.024	.086

TABLE 11:8 (Continued)

2. Correct "Avoid" responses	.16	.111	.09	.131
3. Correct "Neutral" responses	-.003	.056	-.001	.021
"1"/ Total No responses	-.113	-.05	-.129	-.157
"2"/Total No responses	.071	.108	.019	.0895

None of the correlations between neuroticism and correct responding were found to be significant (Table 11:8), and if Gray's hypothesis were correct one would have expected them to be significant.

It seems then that once more the hypothesized relationship between sensitivity to cues of reinforcement and the Eysenckian dimensions of introversion - extraversion and neuroticism has failed to find confirmation. The first hypothesis suggesting that the introversion - extraversion dimension is related to differential sensitivity received only weak support, the second hypothesis relating neuroticism to a dimension of general sensitivity to cues of reward received no support at all.

Self-Rated and Behavioural Differential Sensitivity to cues of Reinforcement

Ss were asked to rate themselves on a nine point scale to the following question:
"Are you more motivated by, or sensitive to the likelihood of a pleasant outcome or the likelihood of an unpleasant outcome". (Question 6 of the self-rating questionnaire). This question was intended to function as a self-rating on differential sensitivity to cues of reinforcement, and in consequence it was

hypothesized that significant differences would be found between high and low self-raters on the "Correct" score. (Hypothesis 5:4(i)). On the other hand if Ss were using this question to rate themselves as differentially sensitive to reinforcement then the significant difference ought to appear on the "Need" measure. (Hypothesis 5:4(ii)).

As can be seen from Table 11:9 the significant differences are found on the "Correct" measure. For the purpose of these analyses Ss were allocated to one of 3 groups:

(i) those classified as differentially more sensitive to cues of punishment, scoring half a standard deviation or more below the mean

(Ratings 1 - 4)

(ii) the medium group, sensitive to cues of reward and punishment in roughly equal degrees (rating 5 - 7), scoring within half a standard deviation of the mean.

(iii) those classified as differentially more sensitive to cues of reward (ratings 8 - 9), and scoring half a standard deviation or more above the mean.

TABLE 11:9 SUMMARY OF ANALYSES OF VARIANCE EMPLOYING SELF RATINGS OF DIFFERENTIAL SENSITIVITY TO CUES OF REINFORCEMENT AS THE INDEPENDENT VARIABLE

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
"Net" Score	N.S.	N.S.	N.S.	N.S.
"Total" score	N.S.	Linear Trend $p = .076$	N.S.	N.S.
"Correct" score	N.S.	Linear Trend $p < .025$	Linear Trend $p < .05$	Linear Trend $p < .05$
"Error" Score	N.S.	N.S.	N.S.	N.S.
"Need" score	N.S.	N.S.	N.S.	N.S.

(All probabilities two tailed)

Not only are these results significant on the measure predicted in hypothesis 5:4(i) but they are also in the direction predicted by the Grayian theory (cf. Table 11:10).

TABLE 11:10 GROUP MEANS FOR"CORRECT" MEASURE

	Sensitive to cues of Punishment	Medium	Sensitive to cues of Reward
Trials 1 -20 per condition	-.1006	+.1058	+.1586
Trials 21-40 per condition	-.0633	-.02	+.4166
Trials 41-60 per condition	-.0618	+.0565	+.2778

The Relationship between differential sensitivity and other dimensions of personality

The data summarized in Table 11:10 and the analyses summarized in Table 11:9 clearly suggest that there is some basis to Gray's contention that people are differentially sensitive to cues of different types of reinforcement, it now seems pertinent to ask whether the properties of differential sensitivity are subsumed under some other well know dimension.

N-Ach.

The first such dimension considered in the introduction was need-achievement (n-ach). It was suggested that this dimension was quite independent of differential sensitivity, and in consequence would not be found to correlate with any of the hypothesized measures of differential sensitivity. As can be seen from Table 11:11 there is no evidence of a relationship between n-ach and extraversion, rated sensitivity to cues of reward, to cues of punishment, or differential sensitivity to cues of reward.

TABLE 11:11 CORRELATIONS BETWEEN N-ACH AND VARIOUS OTHER SELF REPORT MEASURES

EPI - E	Sensitivity to cues of Reward	Sensitivity to cues of Punishment	Differential Sensitivity
.15	.07	-.087	.195

TABLE 11:12 SUMMARY OF ANALYSES EMPLOYING N-ACH AS THE INDEPENDENT VARIABLE

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
"Net" Score	N.S.	N.S.	N.S.	N.S.
"Total" Score	N.S.	N.S.	N.S.	N.S.
"Correct" Score	N.S.	N.S.	N.S.	N.S.
"Error" Score	N.S.	N.S.	N.S.	N.S.
"Need" Score	N.S.	N.S.	N.S.	N.S.

As can be seen from Table 11:12 no significant differences were found between high and low n-ach scorers on the "Correct" measure, strengthening the contention that n-ach and differential sensitivity are independent qualities. However, no difference was found between high and low n-ach scorers on the "Need" measure either, where it was predicted there would be a significant difference (Hypothesis 5:5(i)).

Locus of Control

The predicted correlation between introversion-extraversion and LoC did not emerge, (-.047, N.S.), but internality-externality as measured by the Rotter scale was found to

correlate with neuroticism (.579, $p < .001$), and Es (-.549, $p < .001$). A moderate correlation between internality and n-ach was also observed (-.302, $p < .001$)

TABLE 11:13 CORRELATIONS BETWEEN ROTTER'S (1966) LOCUS OF CONTROL SCALE AND VARIOUS SELF RATING OF LOCUS OF CONTROL

Rated Internality	Rated Externality	Rated Relative In - Ex	In. (-ve)	In. (+ve)
-.395	.505	.249	.308	.285

(These correlations employ the self-rating on Q's 8, 7, 9, 10, 11 respectively, of the self rating scale)

Given the large number of observations on which the correlations reported in Table 11:13 are based, despite their moderate size they are all highly significant. Though internality for negative outcomes (In-) did correlate with the Rotter scale to a greater degree than did internality for positive outcomes, as was predicted, both correlations are of a moderate size and the difference between them is minimal.

As for the relationship between locus of control and sensitivity to cues of reward neither the correlation between In- and sensitivity to cues of punishment (.11) nor that between In+ and sensitivity to reward (-.003) were found to be significant. Though interestingly the Rotter scale was found to correlate with sensitivity to cues of punishment, but in the direction opposite to that which would have been predicted (.367). The Rotter scale was not found to correlate with sensitivity to cues of reward (.073).

It was predicted (Hypothesis 5:8(i)) that internals would have larger "Net" scores than externals, and smaller

"correct" scores (Hypothesis 5:11). For the purpose of the analyses summarized in Table 11:14 Ss scoring 9 or less on the Rotter scale were classified as internals, those scoring 10-14 as average, and those over 15 as externals. These divisions correspond approximately to half a standard deviation above and below the mean.

TABLE 11:14 SUMMARY OF ANALYSES OF VARIANCE EXPLOYING LOCUS OF CONTROL AS THE INDEPENDENT VARIABLE

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 - 60 per condition
"Net" Score	N.S.	N.S.	N.S.	N.S.
"Total"Score	N.S.	N.S.	N.S.	N.S.
"Correct"Score	N.S.	N.S.	N.S.	N.S.
"Error"Score	N.S.	Linear Trend p = .017	N.S.	Linear Trend p = .022
"Need" Score	N.S.	N.S.	N.S.	Linear Trend p = .032

TABLE11:15 SUMMARY OF ANALYSES OF VARIANCE EMPLOYING LOCUS OF CONTROL AND DIFFERENTIAL SENSITIVITY TO CUES OF REINFORCEMENT AS INDEPENDENT VARIABLES

	Trials 1 - 20 per condition	Trials 21 - 40 per condition	Trials 41 - 60 per condition	Trials 1 -60 per condition
"Net"Score	LoC p=.015 Loc x Sensitivity p = .06	N.S.	N.S.	LoC p=.038 LoC x Sensitivity p =.073
"Total"Score	N.S.	N.S.	N.S.	N.S.
"Correct Score	Sensitivity p < .05	Sensitivity p = .058	N.S.	N.S.
"Error"Score	N.S.	N.S.	N.S.	N.S.
"Need" Score	N.S.	N.S.	N.S.	N.S.

For the purpose of the analyses summarized in Table 11:15 subjects were divided into two groups, those scoring above and those scoring below the mean, on the two dimensions. There appears to be no support for either hypothesis from the analyses summarized in Table 11:14. Internals and externals are, however, found to obtain significantly different "Net" scores in the analyses summarized in Table 11:15, however, the results are in the direction opposite to those predicted as can be seen from the group means reported in Table 11:16.

TABLE 11:16 GROUP "NET" SCORES MEANS, FOR TRIALS 1-20 PER CONDITION

	Internals	Externals
Reward Sensitive	-9.615	-8.143
Punishment Sensitive	-13	-2.867

TABLE 11:17 GROUP "CORRECT" SCORE MEANS FOR TRIALS 1-20 PER CONDITION

	Internals	Externals
Reward Sensitive	.244	.122
Punishment Sensitivie	-.2	.084

There is no evidence from Tables 11:14, 11:15 or 11:17 that internals have gained the lower "Correct" scores, but although hypothesis 5:13 is not confirmed in its prediction of an interaction the internal-punishment sensitive Ss did obtain the lowest mean score. The internal-reward sensitive individuals, however, obtained the largest mean score indicating that these Ss pressed the "gain"key correctly more often than the "avoid" key.

Hypothesis 5:12 predicted an interaction between internals

for positive and negative outcomes on the "Correct" measure. As can be seen from Table 11:18 this interaction was found to be significant

TABLE 11:18 SUMMARY OF ANALYSES OF VARIANCE EMPLOYING INTERNALITY FOR POSITIVE AND NEGATIVE OUTCOMES AS THE INDEPENDENT VARIABLES

	Trials 1 - 20 per Condition	Trials 21 - 40 per Condition	Trials 41 - 60 per Condition	Trials 1 - 60 per Condition
"Net" Score	In(-) $p = .012$	N.S.	N.S.	N.S.
"Total" Score	In(+) x In(-) $p = .048$	N.S.	N.S.	N.S.
"Correct" Score	In(+) x In(-) $p = .028$	In(+) x In(-) $p = .062$	N.S.	In(+) x In(-) $p < .025$
"Error" Score	N.S.	N.S.	N.S.	N.S.
"Need" Score	N.S.	N.S.	N.S.	N.S.

(Ss scoring 3 or less on the rating scale were classified as internals)

TABLE 11:19 GROUP "CORRECT" SCORE MEANS FOR In(+) AND In(-) GROUPS

	Trials 1 - 20 per condition		Trials 21-40 per condition		Trials 41-60 per condition		Trials 1-60 per condition	
	In(+)	Ex(+)	In(+)	Ex(+)	In(+)	Ex(+)	In(+)	Ex(+)
In(-)	-.057	.27	-.04	.201	-.003	-.004	-.033	.156
Ex(-)	.34	0	.29	-.099	.226	-.063	.285	-.054

It is evident from Table 11:19 that hypothesis 5:12 is only partly confirmed. As expected those who rate themselves as either internal or external for both types of outcomes do not

demonstrate a greater sensitivity for either cue, their mean scores not differing greatly from zero. Also as predicted the In(+) Ex(-) group displays the highest mean score, but the Ex(+) In(-) group do not display the lowest mean score. Indeed the mean score for this group is larger than the mean scores for either the In(+) In(-) or Ex(+) Ex(-) groups.

Classification according to Behaviour

It was suggested in the introduction that we might be primarily concerned with what individuals do, and in consequence it was decided to reverse the usual question and instead ask if people who behave differently also tend to rate themselves differently (Hypotheses 5:17 and 5:18)

The "correct" score, it will be recalled, is a measure of the tendency to make more correct gain or avoidance responses, and in consequence is assummed to reflect differential sensitivity to cues of reinforcement. A positive score on this measure reflects relatively more correct gain responses, a negative score relatively more correct avoidance responses. When individuals were divided into three groups on the basis of their scores on this measure, they were not found to rate themselves as significantly different with regard to introversion-extraversion as measured by the EPI

Individuals were found to rate themselves differently with regard to differential sensitivity, however, as can be seen from Table 11:20

TABLE 11:20 GROUP "DIFFERENTIAL SENSITIVITY TO CUES OF REINFORCEMENT" MEANS FOR THE HIGH, LOW AND MEDIUM "CORRECT" SCORING GROUPS

"Correct" Score	Low (half S.D. below Mean)	Medium (within half S.D. of Mean)	High (half S.D. above Mean)
Self rating	4.8	6.0	6.21

(This trend is significant at the .05 level (1 tailed))

The trend discribed in Table 11.20 is significant only for the first epoch (trials 1-20 per condition).

On this same measure during the first epoch a significant quadratic trend was found for both EPI N scores and scores on the Rotter Locus of Control scale (cf. Tables 11:21 and 11:22)

TABLE 11:21 GROUP EPI N MEANS FOR LOW, MEDIUM AND HIGH "CORRECT" SCORING GROUPS

"Correct" Score	Low	Medium	High
EPI N Mean Score	7.9	12.115	7.68

(Quadratic trend, p = .005, 2 tailed)

TABLE 11:22 GROUP LoC MEANS FOR LOW, MEDIUM AND HIGH "CORRECT" SCORING GROUPS

"Correct" Score	Low	Medium	High
LoC Mean Scores	8.1	14.261	10.63

(Scale score in externality direction;
Quadratic trend, p = .001, 2 tailed)

As for n-ach, a quadratic trend was found for this measure also, but this time in the final epoch of the experiment (cf. Table 11:23), and in the direction opposite to the two already reported.

TABLE 11:23 GROUP n-ach MEANS FOR LOW, MEDIUM AND HIGH "CORRECT" SCORING GROUPS

"Correct" Score	Low	Medium	High
n-ach Mean Score	11.73	10.857	11.833

(Quadratic trend, $p < .05$, 2 tailed)

As for the groups differentiated on the basis of their "Need" scores, these rated themselves differently with respect to n-ach, only in the second epoch of the experiment (Table 11:24)

TABLE 11:24 GROUP n-ach MEANS FOR LOW, MEDIUM AND HIGH "NEED" SCORING GROUPS

"Need" Score	Low	Medium	High
n-ach Mean score	12.188	10.73	11.385

(Quadratic trend, p = .011, 2 tailed)

It seems then that those individuals who rated themselves as having higher need-achievement might show a preference for either the "gain" or the "avoid" key, perhaps suggesting that fear of failure, as well as n-ach is being tapped.

Consistency

It was predicted (hypothesis 5:14) that there would be a significant degree of consistency in self-ratings, Table 11:25 reports the relevant correlations.

TABLE 11:25 CORRELATIONS BETWEEN SELF RATING SCORES ON TWO OCCASIONS

Dimension	Correlation Co-efficients
Extraversion (EPI)	.589
Neuroticism (EPI)	.95
Ego-Strength	.804
Rated Extraversion	.642
Rated Consistency of Extraversion	.244 (N.S.)
Rated Impulsivity	.894
Rated Consistency of Impulsivity	.093 (N.S.)
Rated Neuroticism	.813
Rated Consistency of Neuroticism	.571

(N = 28)

All the correlations reported in Table 11:25 are significant, with the exception of those for rated consistency of extraversion and rated consistency of impulsivity. The magnitude of the EPI - E correlation is, however, surprisingly low. (.589)

Turning to those results reflecting behavioural consistency, here fewer significant results were expected

TABLE 11:26 CORRELATIONS BETWEEN OMISSIONS MADE IN EXPERIMENTS 4 AND 5

EXPERIMENT 4 — EXPERIMENT 5	Trials 1-20 per Condition	Trials 21-40 per Condition	Trials 41-60 per Condition	Trials 1 - 60 per Condition
Expt. 4A				
Fast condition				
Extreme	.338*	.346*	.368*	.368*
Middle	.236	.212	.231	.236
Centre	.071	.072	-.074	.076
Slow Condition				
Extreme	-.143	-.09	-.088	-.112
Middle	-.023	.043	.028	.017
Centre	-.278	-.11	-.044	-.147
Expt. 4B				
Before first reversal	-.045	-.04	-.129	-.075

(* $p < .05$, 1 tailed)

Only one of the omission scores from experiment 4 is found to correlate with the number of omissions made in experiment 5.

TABLE 11:27 CORRELATIONS BETWEEN COMMISSION ERRORS MADE IN EXPERIMENTS 4 AND 5

EXPERIMENT 5

Stimulus	Positive		Negative		Neutral	
Response	Avoid	Neutral	Gain	Neutral	Gain	Avoid
EXPERIMENT 4						
Expt 4A						
Fast Condition						
Extreme	-.268	.076	-.14	.036	-.294	-.175
Middle	.0325	.111	-.075	.058	-.172	-.24
Centre	-.03	.068	.133	.25	-.163	-.277
Slow Condition						
Extreme	-.199	.007	.133	.286	-.031	-.037
Middle	-.186	-.006	.261	-.04	.017	-.074
Centre	-.024	-.033	.194	.5*	-.076	-.042
Expt. 4B						
Before first reversal	-.032	.32*	-.07	.0846	-.132	-.261

(*$p < .05$, 1 tailed)

There appears to be little evidence of behavioural consistency in this data (Table 11:27). It was, of course, remarked in experiment 4 that the correlations based on commission errors from that experiment might not be too reliable as so few commission errors were made, the total number of commission errors in the fast condition (experiment 4A) was, therefore, correlated with the total number of commission errors in the present experiment, but this correlation was not found to be significant (.086), nor was the correlation between the total number of commission errors made in the slow condition (Experiment 4A) and the total number of commission errors in the present experiment (.083)

TABLE 11:28 reports the loadings of the personality variables on the 10 factors to emerge from the principle components analysis. Orthogonal factors were extracted using the varimax procedure.

TABLE 11:28 FACTOR LOADINGS

	Neurotic ism	Extraver- sion	Consis- tency with Respect to LoC	Locus of Control	Consis- tency of Sensit- ivity	N-Ach	Differ- ential Sensi- tivity	Consis- tency- of Neurot- icism	Consis- tency of Extraver- sion	General Sensitiv- ity to Cues
Extraversion (EPI)		.906								
Neuroticism (EPI)	.865									
Ego-Strength	-.783									
Locus of Control (Rotter, 1966)	.587			.434						
Need Achievement (Smith, 1973)						-.616				
Variables from the Self Rating Scale										
Extraversion		.799								
Consistency of Extraversion									.787	
Impulsivity		.772								
Consistency of Impulsivity									.536	
Neuroticism	.73									
Consistency of Neuroticism								.766		
Sensitivity to cues of Punishment	.403									.638
Consistency of Sensitivity to cues of Punishment					.828					
Sensitivity to cues of Reward										.609
Consistency of Sensitivity to cues of Reward					.634					
Differential Sensitivity to cues of Reinforcement							.669			
Consistency of Differential Sensitivity							.773			
Externality				.758						
Consistency of Externality			.555							
Internality				-.804						
Consistency of Internality			.467							
Differential Strength of Internal-External Beliefs				.65						
Consistency of Differential Strength			.708							
Internality-Externality for Success				.431		.427				
Consistency of In(+)			.814							
Internality-Externality for failure						.652				
Consistency of In(-)			.792							
Awareness of Consequences of Behaviour						-.44				
Consistency of Awareness										

(Only loadings greater than .4 have been included in this table.)

TABLE 11:29 INTER-CORRELATIONS OF RATED CONSISTENCY SCORES

PEARSON CORRELATION COEFFICIENTS

	CONSOC	CONIMP	CONEURO	CONPUN	CONREW	CONRVP	CONEXTR	CONINTR	CONINVEX	CONFAL	CONSUC
CONSOC*	1.0000	.4586	.2758	-.0177	.2093	.3664	.0248	.1278	.1741	.1524	
CONIMP	.4586	1.0000	.2961	-.0264	.0831	.3485	.2506	.1594	.1277	.2804	
CONEURO	.2768	.2961	1.0000	.0527	.0863	.3916	.2728	.2085	-.0267	-.0211	
CONPUN	-.0177	-.0264	.0527	1.0000	.2521	.1110	.3508	.3124	.2824	.1685	
CONREW	.2093	.0831	.0863	.2521	1.0000	.2178	.3657	.2885	.2425	-.0019	
CONRVP	.3664	.3485	.3916	.1110	.2178	1.0000	.2790	.3784	.3597	.4193	
CONEXTR	.0248	.2506	.2728	.3508	.3657	.2790	1.0000	.4294	.3481	.2980	
CONINTR	.1278	.1594	.2085	.3124	.2885	.3784	.4294	1.0000	.5234	.4290	
CONINVEX	.1741	.1277	-.0267	.2824	.2425	.3597	.3481	.5234	1.0000	.5693	
CONFAL	.1529	.2804	-.0211	.1685	-.0019	.4193	.2980	.4290	.5693	1.0000	
CONSUC	.1545	.1035	-.0499	.0624	.3494	.3283	.3379	.5158	.6233	.6038	
CONAWA	.04331	.07886	.3271	.18559	.0727	.03135	.15174	.11573	.359	.406	.2291

(Correlations underlined are significant at $p < .05$)

*CONSOC - Rated consistency of extraversion
CONIMP - Rated consistency of impulsivity
CONEURO - Rated consistency of neuroticism
CONPUN - Rated consistency of sensitivity to cues of punishment
CONREW - Rated consistency of sensitivity to cues of reward
CONRVP - Rated consistency of differential sensitivity to cues of reinforcement
CONEXTR - Rated consistency of externality
CONINTR - Rated consistency of internality,
CONINVEX - Rated consistency of differential strength of internal - external beliefs
CONFAL - Rated consistency of internality-externality for failures
CONSUC - Rated consistency of internality-externality for success
CONAWA - Rated consistency of awareness of consequences of behaviour

Table 11:29 reports the inter-correlations between the 11 ratings of consistency. As can be seen almost two-thirds of these correlations are found to be significant, suggesting that they do share something in common. However, very few of the correlations are greater than .4 suggesting, as does the principle components analysis that, at least for self rating, there is no one factor of consistency.

11:5 DISCUSSION

Although a great many results have been reported the situation is, nevertheless, a relatively straight forward one. Several related questions were being asked:

(i) Are the various performance scores all reflecting the same quality, or are they independent of each other? In particular are the "Correct" measure, assumed to reflect differential sensitivity to cues of reinforcement, and the "Need" measure, assummed to reflect preference for one type of reinforcement independent of each other?

(ii) Is there a discrete relationship between the "Correct" measure and certain theoretically important self report variables?

(iii) What relationship does the Grayian dimension of differential sensitivity to cues of reinforcement bear to other well used constructs which are employed to describe individuals, or predict behaviour?

(iv) Is there any evidence that people behave in a consistent manner?

GRAY'S MODEL

Differential Sensitivity

The score which seems to be the most obvious measure of performance, and, in consequence, the one that it is most tempting to employ is the S's "Net" score. However, as is often

the case the most obvious measure is also the grossest and least precise. Instead it was argued that the two most sensitive measures were the "Correct" measure and the "Need" measure. If the interpretation of these measures is correct then they ought to be substantially independent of one another. It was, in fact, reported, Table 11:3, that they were found to correlate to a significant degree (.287). But the overlap is small, and probably reflects the fact that the "Correct" measure contains an element of preference for one or other type of reinforcement. This, of course, may indicate that these two qualities are not, in fact, entirely independent but, for example, individuals sensitive to cues of reward also have a greater than average desire for reward. However, the two measures, and therefore, presumably the two qualities, do appear to be substantially independent of each other, a point emphasized by the discrete patterns of results obtained.

It seems fair to conclude that for the first time empirical support has been found for Gray's theory, support in the form of an independent behavioural dimension predicted on the basis of Gray's theory.

The next step in the investigation would seem to be to determine whether or not this behavioural dimension is related in a predictable way to any of the self-report variables of theoretical interest: introversion-extraversion, neuroticism, and self-rated differential sensitivity to cues of reinforcement.

As predicted by the theory the HEHN group did obtain the highest "Correct" score, and the LEHN group the lowest score (cf Table 11:5), reflecting the fact that the neurotic extraverted group made more correct "gain" responses than they did avoidance responses, whereas the neurotic introverted group made more correct avoidance than "gain" responses. However, enthusiasm must be tempered with realism and it must be noted

that the E x N interaction never reached significance, while the difference between introverts and extraverts was significant only in the first epoch of the experiment (Table 11:4). This hypothesis must, therefore, be regarded as receiving the most tentative of support.

A possible explanation for this relative weakness of support presents itself in Tables 11:4 and 11:6. It appears that the introverted group responds by pressing the "gain" key more often than the "avoid" key when the neutral stimulus is presented. It has already been mentioned that such a tendency will obscure the sensitivity of the "Correct" measure. It seems likely that here this tendency in the introverted group will have the effect of artificially inflating the number of times they press the "gain" key correctly, and so obscure the difference between the introverted and extraverted groups on the "Correct" measures. Too much must not be made of this however, for, on the one hand, with such a large number of analysis being performed it is to be expected that some differences will be found to be significant by chance; on the other hand, even if this result is not due to chance it serves to illustrate that the introversion-extraversion dimension contributes relatively little variance to the behaviour measured by the "Correct" measure.

One might have suggested that this tendency of the introverted group to respond with the gain key is complementary to their tendency to become aware of contingencies of reinforcement, which in turn might well result in more internal beliefs as to the source of reinforcement and also lead to greater achievement orientation. Such speculation is scotched, however, by the fact that introversion-extraversion is found to correlate neither with internality (-.047), nor In(+) (.08), nor with n-ach (.15).

It has already been remarked several times that the link between the Eysenckian dimensions and differential sensitivity is in no way essential to the structure of Gray's theory. As no support had been found for the hypothesized link in experiments 1 - 3 it was decided in the present experiment to ask people to rate themselves with respect to differential sensitivity. It was found that those Ss who rated themselves as relatively more sensitive to cues of punishment, than to cues of reward behaved in such a way as to support their self ratings. Similarly those rating themselves as relatively more sensitive to cues of reward were found to show a greater number of correct responses to cues of reward (cf. Tables 11:9; 11:10, 11:15, 11:17).

It is noteworthy that, as can be seen from Tables 11:9 and 11:15, the significant differences occur only on the "Correct" measure, strengthening the claim of this to be an adequate measure of Gray's differential sensitivity dimension.

When the question is asked: "Is there any relationship between behavioural and self report data?" rather than the one that has been asked: "Do groups defined by self report data behave differently?" We find that there is again only the weakest of support for the hypothesized relationship between introversion and extraversion (cf. Table 11:7). More surprisingly, however, the "Correct" measure is found to correlate no more highly with self rated differential sensitivity, as can be seen from Table 11:30.

TABLE 11:30 CORRELATIONS BETWEEN SELF RATED DIFFERENTIAL SENSITIVITY TO CUES OF REINFORCEMENT AND "CORRECT" SCORES

Trials 1 - 20 per Condition	Trials 21 - 40 per Condition	Trials 4 - 60 per Condition	Trials 1 - 60 per Condition
.234	.161	-.008	.057

This result is somewhat surprising, and must be regarded as indicating that the possibility of predicting behaviour from such global ratings is severely limited. Although this in itself is not necessarily a bad thing, and this result might serve as a reminder, as might the fact that the magnitude of the correlation declines over the three epochs, that we are not dealing with an hypothesized trait, that is, not a trait regarded as a generalized disposition to behave in a particular way. The importance of the Grayian dimension, like the locus of control dimension, is in its value in predicting what is learned, how it is learned and the elements in the situation that are more or less likely to be noticed, and consequently determine behaviour. The consequence of this is that this variable is unlikely to account, consistently, for a large proportion of the variance in all situations. Even if the salience of the cues of reinforcement did not vary over time the need for various reinforcements would change, and in consequence behaviour would be expected to change. In the present experiment, for example, after having attempted to respond correctly to both "gain" and "avoid" cues for sometime, in order to gain the maximum possible score, the subject could take a "rest" by responding with the "avoid" key on every trial. This strategy would allow him to maintain the status quo until he was ready to attempt to gain more points, but it would also destroy any simple relationship between the self report measure and the behavioural measure. Changes in strategy, preferences for gaining or avoiding reinforcement, and factors such as fatigue will all effect the observed magnitude of the relationship between the Grayian dimension of differential sensitivity and behaviour. Because of this we can also expect the magnitude of the observed relationship to vary across time and across situations.

If this is the case one might be tempted to ask whether there is any use in proposing such a dimension. The answer would seem to be that it could prove to be extremely fruitful as part of the explanation why people learn particular behaviours, one of the "person variables" discussed in Chapter 2. It can be argued that each person will have a unique repetoire of behaviours at his disposal, and also that being differentially sensitive to cues of reinforcement we can expect different behaviours from people in the same situation. Thus we may have uniqueness without chaos.

It might still be argued, however, that this is all fine at the theoretical level; that all behaviour is, in principle, explicable but so much information would be needed that it is, in fact, no more than a pretty argument. The present experiment is, however, an empirical refutation of such a point. The theory deals with the way in which the individual perceives things, or to be more strictly accurate the things certain individuals are likely to perceive. In simple situations where there is an important determinant of behaviour accurate predictions may be made of group behaviour. On the other hand in more complex situations more information will be needed before realistic predictions can be made about behaviour, factors such as which elements of the situation the individual uses as cues of reinforcement, whether reward or punishment is more obviously important in relation to the situation, what opportunities for different behaviours are available within the person and within the situation, and a host of other factors will assume a greater importance in making specific predictions on specific occasions.

Generalized Sensitivity

Gray has suggested not only that people differ in their differential sensitivity to cues of reward and punishment, but

that they will also differ in their overall sensitivity to cues of reinforcement, irrespective of whether the cues are related to reward or punishment. This dimension, Gray suggests, is related to the Eysenckian dimension of neuroticism. However, neuroticism, as measured by the N scale of the EPI was not found to correlate with the number of correct responses made (cf. Table 11:8) , nor was the E x N interaction found to be significant (Table 11:4) when the "Correct" score was used as the dependent variable. As already noted the means (Table 11:5) are in the predicted direction, but they do not reach an acceptible level of significance. It seems then that once more we have failed to discover any support for this aspect of Gray's model. However, when it is considered that the extraversion results were weak, but the self-rating data much stronger in support of a dimension of differential sensitivity to cues of reinforcement, and bearing in mind the fact that the neuroticism results were in the predicted direction it would be over-hasty to conclude that no dimension of general sensitivity to cues of reinforcement exists. A more appropriate conclusion at this stage would seem to be that such a dimension has not been demonstrated, and that it now seems unlikely that, if such a dimension does exist, the EPI N scale is an adequate measure of it.

Overall, then, it seems that this experiment has yielded a good deal of support for Gray's theory. However, it ought to be made clear just what does seem to have been demonstrated. Individuals who rate themselves as relatively more sensitive to cues of reward than to cues of punishment responded correctly more often to cues of reward than to cues of punishment. Those individuals who rated themselves as differentially more sensitive to cues of punishment than to cues of reward respond correctly more often to cues of punishment. It should be noted that

what has been contrasted here is an active avoidance response and an appetitive response. These, of course, are assumed, in the Grayian model, to be mediated by the same mechanisms and so the individuals sensitive to cues of one ought to be sensitive to cues of the other. However, it was suggested when outlining Gray's theory that when mildly aversive stimuli were employed those individuals sensitive to cues of punishment, having a higher arousal level, might learn the avoidance response more quickly than those individuals with a lower level of arousal. If this suggestion is not acceptable, then the present set of results must be regarded as disconfirming Gray's hypothesis. Either way the theory obviously needs to be modified.

If one maintains that active avoidance and appetitive responses are mediated by the same mechanism and consequently that the same individuals will display superiority in tasks relating to both these measures, then the analyses employing the "Correct" measure must be seen as disconfirming the theory, as must the fact that neither the extraverts (Table 11:4) nor those who rate themselves as differentially more sensitive to cues of reward (Table 11:9) obtain significantly higher "Net" scores than the introverts, or those who rate themselves as differentially more sensitive to cues of punishment.

N-Ach.

Having pointed out that it is possible to interpret the results of the present experiment as supporting Gray's theory, the next step would seem to be to discover how similar the construct of differential sensitivity is to other well used constructs. In the introduction it was suggested that differential sensitivity bore a superficial resemblance to n-ach. It was argued, however, that this resemblance was no more than superficial as n-ach is really concerned with the

need and consequent approach behaviour in relation to reinforcement, while Gray's dimension is primarily concerned with a sensitivity to cues of reinforcement. It was hypothesized that:

(i) n-ach would not correlate with either the EPI E scale nor self report differential sensitivity,

(ii) that no difference between high and low need achievers would be evident on the "Correct" measure,

(iii) that high and low need-achievers would be distinguished on the "Need" measure.

The first two of these hypotheses were confirmed (cf Tables 11:11 and 11:12) although the fact that extraversion does not appear to be an adequate measure of differential sensitivity, and that self rated differential sensitivity does not correlate with the "Correct" score reduces the importance of the first hypothesis somewhat. The third of these hypotheses was not confirmed, however. This, of course, may be because of a limitation in either the "Need" measure, or in Smith's measure of n-ach.

In this context a recent paper by Vestewig (1978) is of importance. Vestewig suggested that Smith's measure of n-ach was not entirely satisfactory. He reports that for a sample of 90 American males the test had "an effective range of 5 points (4 - 8). None of the males, for example, scored below 4, and only three scored above 8." (p.111).

Smith does not report the distribution for his British sample, but in the present sample the range and distribution of scores was found to be far more satisfactory than those Vestewig reports for his American sample (cf. Table 11:31).

TABLE 11:31: FREQUENCY DISTRIBUTION FOR SMITH'S QUICK MEASURE OF ACHIEVEMENT MOTIVATION (1973)

	SCORE ON SMITH'S SCALE								
	7	8	9	10	11	12	13	14	15
152 Ss	1	6	16	23	39	35	22	9	1
55 Experimental Ss	0	2	6	8	16	10	7	5	1

No. of S obtaining each score

As can be seen from Table 11:31 there is little overlap between the distribution obtained in the present experiment and Vestewig's. The most likely explanations for the non-significance of n-ach differences on the "Need" measure are that: (i) n-ach is a much broader concept than that being tapped by the "Need" measure, and (ii) n-ach is not mediated by a simple preference for rewards over punishments.

It seems fair to conclude on the basis of the present experiment that there is no evidence of an overlap between n-ach and the Grayian dimension of differential sensitivity to cues of reinforcement.

Locus of Control

It was suggested in the introduction that introverts might be found to hold internal beliefs, particularly with respect to unpleasant outcomes, this suggestion appears not to have been borne out, for the EPI E scale was not found to correlate significantly with either the Rotter scale (-.047), In(-) (-.066) or In(+) (-.08). The relation between self-rated differential sensitivity and LoC, though significant, was equally unimpressive (-.191, $p < .05$). The correlation between In(-) and self rated differential sensitivity was only slightly

larger (-.241), but that between self rated differential sensitivity and In(+) was not significant (-.003).

It was also predicted, on the basis of Gregory's experiment that Rotter's scale would correlate more highly with In(-) than In(+). Though this expectation was fulfilled the difference between the two correlations is minimal, they being .308 and .285 respectively. The importance of the form taken by the question turned out to be critical. When Ss were asked a forced choice question, mimicking the style of Rotter's questionnaire, this rating correlated .249 with the Rotter scale. When asked to rate their internality, this rating produced a correlation of .395, while rated externality correlated .505 with the Rotter scale. In(+) and In(-) were found to correlate .368. This suggests that even if the Rotter scale is somewhat more sensitive to In(-) it is unlikely that it measures this exclusively for this, itself, is not totally independent of In(+). It also suggests that the form of a question may prove to be the critical determinant of an observed relationship, or equally may obscure relationships which really exist, serving as a salient reminder that to use self-ratings on questionnaires without some external validation may ultimately prove to be dangerous.

In an attempt to impose some order on this mass of correlational data it seems fair to conclude that there may well be a small overlap between reported locus of control and self rated differential sensitivity. The relation is in the direction of externals rating themselves as sensitive to cues of punishment. This pattern appears to be mainly due to Ex(-) individuals being more sensitive to cues of punishment. One might characterize the situation as follows: Those who rate themselves as more sensitive to cues of punishment, presumably attempt to avoid the punishment signalled by these cues.

When failure to avoid punishment does occur the failure is attributed to factors outside themselves. It is interesting to note, in this context, that externals tend to rate themselves as more sensitive, (as opposed to differentially sensitive), to cues of punishment than do internals.

A complementary relationship between differential sensitivity, or sensitivity, to cues of reward and internality does not emerge from the present data.

As predicted externality was found to correlate with both neuroticism (.579) and Es (-.549), and as might be expected, in view of the failure of neuroticism to emerge as a factor of general sensitivity to cues of reinforcement, externality was not found to be related to the degree of correct responding to cues of either reward or punishment. On the other hand, locus of control was found to be related to the number of correct responses made to the neutral stimuli (.222). It seems that this result may well be, if not in itself an artifact, at least part of a larger effect. For externality was found to correlate with the total number of times the "neutral" key was used (.271). The experimental situation was such that by pressing the "gain" key when appropriate a point could be gained, while correctly responding with the "avoid" key enabled the S to avoid losing a point, but responding with the neutral key, whether correctly or not had no benefit. Most Ss abandoned the use of the "neutral" key early in the experiment. It appears that those Ss who persisted in using this key tended to be externals.

Two explanations, not necessarily mutually exclusive, may be offered for this behaviour, both consonant with results reported in the introduction: (i) Externals adhere, faithfully, to rules given to them for the performance of a task, (ii) Externals are less adapt, or less willing, than internals

to adopt strategies which allow them to maximize reinforcement.

These observations may also help to explain the correlation of .302 between n-ach and internality.

It was suggested that internals would prove to be more successful at gaining points than externals, but this did not prove to be the case (cf. Table 11:14 and 11:15), and, as can be seen from Table 11:16, it was the externals who proved to be the more successful.

Another prediction which failed to find any support was that there would be a locus of control by rated differential sensitivity interaction. The results were in the predicted direction (Table 11:17), with the internal-punishment sensitive group showing the largest negative score on the "Correct" measure, reflecting the fact that they correctly respond with the "avoid" key often as compared to their correct responding with the "gain" key. The internal-reward sensitive group obtained the highest mean score, with both external groups yielding intermediate scores. It is interesting that such a pattern of results should emerge, for it suggests, once more, that the Rotter scale taps both In(+) and In(-).

The predicted In(+) by In(-) was found to be significant, (Table 11:18). The significance of this interaction is important beyond the fact that it indicates that Locus of Control might profitably be divided into two subfactors. If the "Correct" measure may be regarded as a behavioural index of differential sensitivity, then this significant In(+) x In(-) interaction may serve to emphasize the fact that a number of factors may determine whether or not this differential sensitivity is observed in behaviour. To predict behaviour with any degree of accuracy it may well, at times, be necessary to know, not only about the situation, but also what the actor thinks about ths situation and himself.

It may be seen from Table 11:19, however, that despite the fact that the interaction was significant the results were not entirely as predicted. The In(+) Ex(-) had the largest mean score, the In(+) In(-) and Ex(+) Ex(-) groups yielded scores not markedly different from zero, but the Ex(+)In(-) group who, it was predicted, would yield the largest negative score, in fact obtained a relatively large positive score.

One might summarize the situation, then, by noting that there is some overlap between locus of control and differential sensitivity, with externals seemingly rating themselves as more sensitive to cues of punishment. However, this overlap seems to be small, there is no evidence that it extends to the behavioural level and it appears that the extent of the relationship might well be influenced by the manner in which questions are asked. As for LoC itself this appears to be related, to a limited extent, to neuroticism and ego-strength, and also to n-ach. However, despite this relationship with n-ach and the fact that externals appear to be more rule bound and inefficient internals were not found to be more successful than externals at the task in hand.

Behaviourally Defined Groups

To this point groups have been defined on the basis of their self ratings and then one has looked to see if these groups behaved differently in relation to some behaviour of theoretical importance. It was felt that it would be interesting to turn this approach to the problem on its head and start, instead, from the fact that people do behave differently and ask whether or not they also view themselves differently. It was, therefore, asked whether high, medium and low "Correct" scorers rate themselves differently on the various self report measures that were available.

No differences in rating were found with respect to the

introversion-extraversion dimension. However, people who behave as if they are more sensitive to cues of reward than punishment also rate themselves as differentially more sensitive to cues of reward than to cues of punishment. Similarly those who are behaviourally more sensitive to cues of punishment rate themselves as if this were the case (see Table 11:20 for group means).

Individuals who show no stronger sensitivity to cues of reward than to cues of punishment tend to regard themselves as more neurotic, their fate controlled by external factors and as having low need for achievement. Whereas those individuals who show greater sensitivity to either sort of cues see themselves as stable, controlling their own fortunes and motivated to achieve.

It seems reasonable to assume that those individuals who show no differential sensitivity for either type of reinforcement will find it relatively more difficult to decide on a course of action than those who are predominantly sensitive to one type of cue. This state of affairs might well lead them into vacillation and inconsistency with the result that they are more likely to rate themselves as neurotic, feel that their behaviour is as much determined by outside factors as anything they contribute, feeling as they do drawn in opposite directions by different cues, the net result being that they cannot see themselves as being achievement oriented.

This pattern of results is especially interesting in view of the fact that in experiments 2 - 4 it was found that neuroticism was related, to some degree, to self rated inconsistency, and the pattern of results here suggests that these people may well be inconsistent, behaviourally, as compared to the stable, internal, achievement oriented individuals.

This set of results then, as might have been anticipated, echoes, the support found earlier for the Grayian dimension of

differential sensitivity to cues of reinforcement. This dimension does not appear to be adequately tapped by the EPI E scale. It appears that differential sensitivity does, at times, overlap with other well used dimensions but the relationship is not always linear. These relationships do not appear to be substantial and may usefully be regarded as factors to be taken into account when attempting to predict behaviour relevant to Gray's theory.

Principle Component Analysis

A principle components analysis was calculated partly to see whether a distinct self report differential sensitivity factor emerged, but also to look at the factor, or factors, of locus of control which might emerge, as well as to see whether or not a single factor of self rated consistency would emerge. This analysis was limited to self report data, where scores from 162 individuals were available and some variables were obviously related to each other e.g. EPI N, Es and self rated neuroticism, or EPI E, self rated extraversion and self rated impulsivity. It was not felt that it would be reasonable to include the behavioural data in this analysis as there were potentially a large number of variables, but relatively few Ss (65).

The factor loadings are reported in Table 11:28 Factors 1 and 2 are clearly neuroticism and extraversion factors respectively, though it is interesting to note that externality (.587), rated sensitivity to cues of punishment (.403), and also n-ach (-.314) also loaded on the first factor reflecting the externality, sensitive to cues of failure, low achievement pattern discussed earlier.

Factor 2 is a relatively pure factor with the only substantial loadings on it being EPI E, rated extraversion and rated impulsivity. Though it is worth noting that self rated

differential sensitivity to cues of reinforcement does show a small loading on this factor (.398)., which suggests, at the self report level at least, that Gray's speculation relating differential sensitivity and introversion-extraversion may not have been totally awry though the relationship is nothing like as strong as he suggested.

Differential sensitivity does not emerge as a factor until the relatively small factor 7. Though it is small it is, nevertheless, interesting. As might be expected of such a small factor it is relatively pure with only two variables showing significantly sized loadings on it: differential sensitivity and consistency of differential sensitivity. The two poles of the factor are then, consistent sensitivity to cues of reward versus inconsistent sensitivity to cues of punishment. This element of consistency - inconsistency gives added interest to the factor. Gray developed his theory mainly on the basis of physiological investigations and animal studies, which may have led to the overlooking or minimizing of the interpretation of information in which humans engage. The consistency-inconsistency dichotomy may bring this into more prominance. This dichotomy may be due to the fact that the salience of cues, particularly cues of punishment, is not constant, or alternatively that "punishment" is not perceived as unitary.

These two possibilities, though not incompatible do not, necessarily, imply each other. Pleasantness may well be regarded as being "more unitary" in our society than punishment. Pleasant things are always to be enjoyed, although it may sometimes lead to unpleasant outcomes. When these unpleasant outcomes are foreseeable then the avoidable cues are no longer cues of reward, positive reinforcement, but instead cues of frustration, classed by Gray with punishment. Punishment, or unpleasantness,

do not appear to be nearly so straightforward. Sometimes unpleasant situations must be endured either in the hope of something pleasant being obtained inconsequence of the unpleasant situation, or because there is no alternative. Similarly sometimes it is necessary to delay gratification which may produce a transient frustration. It would seem to be advantageous to be less sensitive to cues of this type of experience, which only serves to highlight unpleasant situations. There are other occasions, however, when it is critical that we notice the possibility of an unpleasant outcome. The situation is no doubt far more complex than this simple characterization, but if nothing else the principle components analysis has proved to be valuable in highlighting the possibility that, at least in the way people view the world, rewards and punishments may not be unitary concepts, and this in itself may be a help in understanding behaviour.

The final factor to emerge from the analysis is loaded on by just two variables: sensitivity to cues of reward and sensitivity cues of punishment. This appears to be the second of the Grayian dimensions. It seems, then, that there is some evidence at the self report level for both Gray's dimensions, general sensitivity to cues of reinforcement, and differential sensitivity.

Factor 4 appears to be the Locus of Control factor. This factor does not appear to fracture into two factors, however, as it was suggested it might. Strangely although both the Rotter scale and In(+) load on this factor In(-) does not. In(-) instead loads on factor 6 which appears to be the need-achievement factor loaded on as it is by n-ach (-.616) and also In(+) (.427) and awareness of the consequences of of behaviour (-.44). This suggests that internality, believing one controls outcomes, is associated with wanting to control them. The two poles of this factor seem to be achievement

orientation coupled with awareness of the consequences of ones behaviour and a sense of responsibility for what results from ones behaviour, versus low achievement motivation, little awareness of the consequences of ones behaviour and a tendency to attribute outcomes to external factors.

The four remaining factors all appear to be factors of consistency, none of them appearing to be a general factor. Admittedly only the first of these consistency factors, F_5, a factor of consistency of locus of control, contributes a substantial proportion of the observed variance. Nevertheless it is of theoretical interest that at the self rating level the extent to which people say they are consistent in relation to one factor is substantially independent of the extent to which they feel they are consistent on other factors. The four factors to emerge were consistency of locus of control, of general sensitivity to cues of reinforcement, of neuroticism and of extraversion.

Only limited evidence of consistency in behaviour has been found, and this seems easily wiped out with situational changes, it now appears that even at the self report level there is little evidence of generalized consistency, It appears then that if one wishes to talk about consistency, even in the senses of relative consistency and coherence one must limit oneself to certain dimensions or groups of dimensions and situations which the individual sees as equivalent. It seems probable that in familiar situations the individual will be sensitive to more cues and respond to more subtle changes reducing the liklihood that any consistency in behaviour will be observed. It is certainly reasonable to suppose he will have a larger repertoire of responses available in these situations whereas his behaviour in less familiar situations may well have a certain stereotypicalness about it.

The correlations between the various self ratings of consistency are reported in Table 11:29. It can be seen that although many of the correlations reach significance they are generally very small.

Wallach and Leggett (1972), argued that traits and consistencies were not necessarily related, but that consistency would be observed in those behaviours which might be regarded as typical of the individuals style. It has been argued here that simple relative consistency will not be found even for behaviour regarded as "typical", for this would seem to indicate that the individual was not attempting to construe the situation and employ an appropriate strategy. This brings us to the final questions of the present study (i) do people rate themselves in a consistent manner? (ii) Do people behave in a consistent manner?.

With respect to the first of these questions (i) there is no element evident in the present situation which should lead people to give reports of themselves different from those they gave in the previous experiment. (ii) As the time between the two reports was relatively short, 3 - 4 months, there is no reason to suppose that the Ss have experienced situations which have caused them to view themselves in a different way. Some may well have experienced such a situation, but there is no reason to believe a substantial proportion of the sample has. (iii) Similarly as the two reports were made with a short interval there is no reason to believe that Ss will choose markedly different sets of behaviours on which to base their responses.

The relevant correlations are reported in Table 11:25 and it does appear that there is considerable consistency in self rating. This consistency is not uniformly high, however, neuroticism showing a much higher set of correlations than

extraversion. This result was not anticipated, especially as both the self ratings, extraversion and impulsivity, are found to correlate more highly than the EPI E scale. It could, of course, be argued that this moderate correlation was a good thing, indicating that the EPI E scale is sensitive to change, or that the correlation of the E scale is smaller than those for the self ratings simply because of the greater specificity of its questions, and so its greater accuracy in reflecting inconsistency that is really there. This does not really ring true, however, given the way the EPI was constructed and Eysenck's own view on the consistency issue.

As for rated consistency, a significant, though moderate correlation was found for consistency of neuroticism (.571). The relationship between this consistency rating and rated neuroticism was again found to be significant, but this was inevitable with such a large sample and the correlation itself is small, -.292. The correlation between self rated consistency for neuroticism and EPI N is even smaller -.138. It is always possible that this correlation reflects a social desirability element. Those individuals who answer the neuroticism questions in a socially desirable manner insisting that they really are always stable, while those "admitting" to being neurotic are also either limiting this behaviour to "only sometimes", or they are genuinely less concerned with social desirability and so they take no heed of whether it is desirable or not to be consistent or neurotic and instead reflect the true state of things, neuroticism being, a least in this non-clinical group, an inconsistent aspect of their behaviour.

The rated consistencies of extraversion and sociability were not found to be consistent. It is always possible, of course, that this lack of observed consistency is due to the questions themselves lacking in validity. It seems more likely,

however, that this reflects something about perceived consistency, particularly in relation to dimensions such as extraversion or sociability and impulsivity. As situations, or the frequency of different types of situations change so will experienced consistency of behaviour. For example, a person may spend much of his time with many friends, at which point he will rate himself as consistently sociable. Later he may form a particular relationship which results in his spending less time with other friends and acquaintances. He may still view himself as sociable but his behaviour may appear to him less consistent, sometimes with many people, often with just one. If this relationship persists this pattern of being mainly with one person, occassionally with many may become the dominant view of himself, he may now rate himself as quite sociable, but once more consistent.

Turning to behavioural consistency it was not expected that any would be found with respect either to commission or omission errors. In experiment 4 a strategy which resulted in a large number of omission errors seemed to be the most appropriate one, while in this present experiment errors could result in failing to gain points or actually losing them. The results are reported in Table 11:26. There seems to be no evidence of consistency with respect to omission errors only 4 of 28 correlations proving to be significant and even these being rather small, .338 - .368, in size. One must not dismiss these too easily, however, for they do suggest that individuals may well have an element of stylistic consistency, but this consistency is in no way straight jacketing. That is, although a person may have a more or less typical way of responding if there are elements in the situation which demand a particular response, or even a change of style individuals can change appropriately.

TABLE 11:27 summarizing the correlations with respect to commission errors is included mainly for the sake of completeness. There were so few commission errors in experiment 4 that it was virtually impossible for any strong evidence in favour of the consistency hypothesis to emerge from this data.

In conclusion then it seems that for the first time results have been obtained which might be interpreted as supporting Gray's concept of differential sensitivity, but it appears that different people are differentially sensitive to cues of reward and punishment irrespective of the fact that the cue of punishment was the stimulus for an active and not a passive avoidance response. There was only tentative support for the hypothesis that this dimension was related to the Eysenckian dimension of introversion-extraversion. Self-ratings with respect to differential sensitivity proved to be a better predictor of behaviour, at the group level, but it was concluded that perhaps the dimension ought to be regarded as similar to locus of control, reflecting the kind of things the individual will attend to, and consequently learn, rather than as a predictor of behaviour itself. There does appear to be an element of over lap between the dimension of differential sensitivity and n-ach and locus of control, but this is slight and appears to be curvilinear.

As for the second of the Grayian dimensions, the only evidence for this is that the two questions rating sensitivity to cues of reward and punishment load on an independent factor. Neuroticism does not appear to be related to this factor.

It was also suggested that the theory may have to be modified to take account of the fact that people may not regard, especially, punishment as a unitary concept. Which in turn leads back once more to the fact that much of the data obtained

in the present experiment although demonstrating the importance of situational variables emphasizes the importance of the way in which the individual interprets the situation, the beliefs he holds and the strategies he employs in consequence.

With regard to locus of control there does appear to be some evidence that there are two subfactors In(+) and In(-) but these do not appear to be closely related to differential sensitivity, and are themselves closely related to each other, not forming two orthogonal independent factors.

With regard to consistency there appears to be only the weakest of possible suggestions that people may have a stylistic consistency in behaviour, but even this might be easily cast aside when necessary. It appears, from the self ratings, that even consistency is not terribly consistent and, hardly surprisingly that no one factor of consistency exists, at least at the self rating level.

Self-ratings on specific dimensions did appear to be reasonably consistent.

CHAPTER XII: CONCLUSION

Mischel (1968, 1973) has suggested that those personality traits put forward to date lack predictive utility, and from this seems to have emerged two questions, or rather two points of debate:

1. Do traits exist? or perhaps more properly, :Are statements made by trait theorists more than dispositional statements?
2. Do people behave in a consistent manner?

These two questions have often become so intertwined as to obscure each other. At times they have been dealt with independently, and then, when dealing with consistency, people have found it necessary to distinguish between the various meanings of the term. Some authors (e.g. Wallach and Leggett, 1972) have maintained that people are consistent while rejecting the utility of the trait position; others have suggested that consistency, itself, might be regarded as a moderator variable, or else have suggested that individuals may be found to be consistent within their own reference systems.

Two major outcomes seem to have resulted from this debate: situational variables were seen as being more important in determining behaviour than had hitherto been the case, at least their importance was explicitly acknowledged; idiographic approaches to the study of personality received something of an impulse.

What has been termed "situationalism," viewing the situation as the major, if not the sole, determinant of behaviour soon gave way to "interactionism" - the explicit acknowledgement that the individual, the environment and the

manner in which these interact are all important determinants of behaviour.

Even at this stage, however, there was some lack of agreement. Some authors set about partitioning the variance observed in different situations and across various samples and argued that one or another source of variance was of special importance. Other authors, however, took a rather different and somewhat more complex view suggesting that though one might logically distinguish between the individual, the environment or situation and behaviour, such a division was misleading for all these elements were both intimately related to each other, and constantly influencing one another. The relationship is a dynamic one, and as such, the type of interaction exemplified by the analysis of variance model was clearly inadequate. Bandura (1977) suggested that this state of affairs might be better described by the term "reciprocal determinism" rather than the more usual term interaction.

The social learning theory approach to behaviour acknowledged not only the importance of situational variables but also gave some importance to the cognitive aspects of behaviour. The perceptions of the individual, the rules he abstracts, the strategies he employs and the goals he wishes to attain were all given a degree of prominance.

It was observed earlier, however, that to regard one approach as enshrining all truth and the other only false promises might, to say the least, prove to be short sighted. The whole situation seemed much more complex than this with different authors, or groups of theorists, concentrating on different aspects of behaviour and wishing to emphasize different facets of behaviour or its organization. It was also pointed out that different theorists might well be presenting theories and observations properly belonging to

different levels of explanation and generality, with the result that any attempt to translate from one to another might not prove to be particularly useful.

Little attempt seems to have been made, so far, to integrate the nomothetic and idiographic approaches to personality. As these two approaches are not, necessarily, mutually exclusive, it was argued, this might prove to be a fruitful step. It was speculated that any theory which did manage to integrate these two approaches might also yield the further advantage of indicating what types of nomothetic statements there were to be made. If, for example, all behaviour was learned then any detailed description and prediction of behaviour would have to rely on an equally detailed learning history of the individual being available. If learning ability itself were predetermined then one could make a series of statements of increasing generality, statements uniquely true of the individual, statements true of those of a similar learning ability living in the same sub-culture, or culture, statements universally true of individuals of a similar learning ability.

Because of the importance placed on learning in the social learning theory approach to individual differences it was speculated that a theory which offered a biological basis for learning might also provide a suitable basis for a more general theory integrating the idiographic and nomothetic approaches to individual differences. It was at this point that the theories of Gray and Eysenck were introduced, as they do explicitly relate personality, biological differences and conditioning.

In a series of experiments predictions drawn from these two theories were contrasted to see which, if either, might prove to be more useful as a touch stone for further speculation.

The first experiment employed a verbal conditioning situation, but because of the differential levels of responding of the stable and neurotic groups prior to the introduction of reinforcement it was regarded as incautious to attempt to draw any firm conclusions from this study, though there was no obvious support for either theory. The second experiment, however, did produce a clear conditioning effect, though once more it proved difficult to find any convincing evidence in support of either theory.

Whereas the second experiment had employed a classical conditioning situation the third experiment used an operant conditioning situation. Again there was a clear conditioning effect, and this time the results were interpretable as supporting Eysenck's theory.

Despite these significant results, it was felt that Eysenck's theory lacked the robust generality to be taken usefully as the foundation of the type of theory outlined. Support for Eysenck's theory had been found only once in three experiments, the conditions needed to demonstrate the superior conditioning of the introvert were so difficult to contrive that it is difficult to see how one could regard them as prevelent in everyday situations, with the result that the theory loses its predictive utility as it is impossible to say who will, in fact, condition more efficiently in any but the most contrived situations. Even when introverts were found to condition more efficiently than extraverts the introversion-extraversion dimension was found to contribute only a trivial proportion of the total observed variance, and even this might be easily swamped by other factors. It was felt, therefore, that though Eysenck's theory might be "true" to some degree it had little heuristic value.

What was evident from these experiments was the

frightening efficiency with which people did condition and the extent to which physical aspects of the situation influenced people's behaviour. The efficiency with which people conditioned, responded to situational cues, displayed adaptability, call it what you will, was far more pronounced than a mere inspection of the data would ever indicate and it would have been easy to become converted to the situationalist point of view were it not for an equally strong effect which emerged in experiments four and five.

The focus of experiments four and five was partly consistency, both behavioural and self report, and partly Gray's suggestion that people are differentially sensitive to cues of reward and punishment. The most striking effect to emerge, however, was the importance of the strategies employed. These determined behaviour and, in consequence, the degree of consistency observed. Of course, the physical features of the situation and the reinforcement contingencies in part determine the strategy which, having been adapted and behaviour-initiated, changes the situation at least to the degree that the perception of the situation is changed, different cues becoming differentially important for the successful execution of the strategy. The behaviour, determined by the strategy may well change the situation and the reinforcement contingencies obtaining.

Before starting these experiments it was suggested that theories of personality did not adequately reflect the true complexity of human behaviour, and it was suggested that a more complex model was needed. What emerged from the experimental data was that those suggestions which were made, vague as they were,did little more justice to the true extent of that complexity.

With respect to the consistency of behaviour there was only the vaguest suggestion that people did behave in a

consistent manner, and this only a relative consistency in making omission errors. The strategy, determined by the situation in which the individual found himself, proved to be a far more important determinant of behaviour.

There seemed to be little evidence of a general factor of consistency from self ratings, indeed there seemed to be little evidence that individuals were consistent with respect to the degree of consistency they assumed themselves to display either across traits or across situations. This, of course, makes it difficult to follow the suggestion that consistency, itself, might be used as a moderator variable.

Returning to Gray's theory, despite the failure to find any support for it in experiments 1 - 3, it was decided that it ought to be looked at further as it had been investigated largely assuming that the identification of the Grayian with the Eysenckian dimensions was correct. It also seemed possible that Gray's theory might provide some insight into behaviour in the guise of indicating which type of strategy an individual was most likely to adopt. Experiment 5 did, indeed, provide some support for Gray's contention that people are differentially sensitive to cues of reward and punishment; though this dimension of differential sensitivity was not related to the Eysenckian dimension of introversion-extraversion.

It seems possible that Gray's dimension functions to a greater extent as a performance, rather than a learning variable, determining the strategy adopted because it directs attention to one type of cue in preferance to another.

It was decided to pursue this possibility in something more like a "real life" situation. One might regard phobics as adopting a strategy of passive avoidance initiated by cues of negative reinforcement. Many of the S who had completed the questionnaires used in experiment 5 had also completed the

Wolpe-LANG(1964) Fear Schedule for J. Mervyn-Smith (1979, unpublished). It was decided to combine these data to discover whether individuals who consistently rated themselves as fearful also tended to rate themselves as differentially sensitive to cues of reinforcement, or as highly sensitive to cues of punishment.

TABLE 12:1 CORRELATIONS BETWEEN SELF-RATED SENSITIVITY TO CUES OF REINFORCEMENT AND SELF-RATED FEARFULNESS

	Differential Sensitivity to cues of Reinforcement	Sensitivity to cues of Reward	Sensitivity to cues of Punishment
Fear of Noise (4)	-.185	.0884	.248*
Fear of Noise (5)	-.116	-.069	.061
Classical Phobias (4)	-.0506	.113	.225*
Classical Phobias (5)	-.132	.135	.175
Fear of Animals (4)	-.072	-.0445	.163
Fear of Animals (5)	-.222*	.135	.138
Social Phobias (4)	-.117	.117	.343*
Social Phobias (5)	-.27*	.0414	.153
Miscellaneous Phobias (4)	-.15	.091	.229*
Miscellaneous Phobias (5)	-.2998*	.186	.299*
Fear of Tissue damage (4)	.008	.096	.079
Fear of Tissue damage (5)	-.27*	.0414	.153

(*$p < .05$, 2 tailed)

(Items belonging to 6 classes of phobia were rated on a five point scale. The correlations were computed between the number of items rated at points 4 and 5 on the fear scale and the ratings for sensitivity)

Although some of the correlations reported in Table 12:1 do reach significance they are generally small suggesting that though differential sensitivity to cues of reinforcement and sensitivity to cues of punishment may well play some part in determining which individuals will become phobic they are neither the sole nor the major determinants. It should be remarked, of course, that the individuals who took part in this study did not find their life disrupted by their fears, and it is always possible that a stronger relationship might have been found had a more extreme group been employed. It seems, then, that one cannot disregard the possibility that differential sensitivity to cues of reinforcement may in part determine which strategy an individual adopts, though this has not been clearly demonstrated here, but it is likely to be only one of a number of such factors.

It might be concluded from this set of experiments that individuals do alter their behaviour, so as to take advantage of the existing reinforcement contingencies. They do this with remarkable efficiency, and even in some situations when they are unable to report the prevailing contingencies. This facility is not always remarked on, especially when one is primarily concerned with personality trait variables, and is focusing one's attention on the differences between groups of individuals or the differential predictions drawn from alternative theories.

This, of course, leads one to conclude that the physical aspects of a situation are important in determining behaviour, and also that learning may be important in determining future behaviour. Cognitive variables, however, are also of considerable importance and so it seems that what is learned in any situation is more than a mere physical response, one learns a whole approach to the situation, a strategy and a

method of evaluating out comes.

The net effect of this is that unless one establishes remarkably similar situations consistency - whether absolute, relative or in the form of coherence - is unlikely to be observed. On the evidence presented here more than the immediate physical aspects of stimuli need to be similar if consistency is to be observed.

As for the two theories, Eysenck's and Gray's, from which predictions were drawn neither of them seem to be adequate for the task of providing the basis for a general theory of personality, one which would integrate both the nomothetic and idiographic approaches to personality. Hypotheses drawn from both theories did receive some measure of support, however, suggesting that on one hand neither theory can be disregarded at present, and any future theory must be capable of either incorporating the relevant elements of these two theories or else re-explaining these findings.

Some support did emerge for Eysenck's contention that introverts condition more efficiently than extraverts, but one must adhere closely to the parameters delineated by the theory, and so the differnnce does not appear to be robust; and the percentage of variance contributed by the introversion-extraversion dimension is very small. Overall Eysenck's theory seems to lack heuristic value.

As for Gray's theory it appears that there may well be some truth to his contention that individuals are differentially sensitive to cues of reinforcement. It is possible to view this differential sensitivity as a "person variable" (Mischel, 1973), and as such incorporate it into the social learning theory frame work. It seems unlikely, however, that this variable is related to the Eysenckian trait dimension of introversion-extraversion.

No evidence emerged to suggest that differential sensitivity is related to learning or conditioning - though it cannot be claimed that this is to any degree established and such a relationship might well still emerge if this variable were more adequately measured. It also seems probable that even in relation to behaviour most obviously related to differential sensitivity the differential sensitivity to cues of reinforcement of the individual is only one of a number of factors which determines the observed behaviour.

It is also worth remarking once more that though it was never clearly established there was often a suspicion in the data that conditioning itself is not unidimensional and may well fracture into at least two components. This, of course, has implications not only for the theories of Gray and Eysenck, but also for any general theory of personality which sees learning as playing an important part in determining behaviour.

Directions for Future Research.

At the more specific level it does appear that more research is needed in relation to Gray's theory. Several shortcomings of the model were noted in Chapter Five, but what proved to be the most important inadequacy of this theory was its vagueness when it came to making predictions. This proved to be especially true with reference to the concepts of positive and negative reinforcement. Gray has equated active avoidance and appetitive behaviour with the implication that those individuals who are relatively more sensitive to cues for active avoidance are also relatively more sensitive to cues of appetitive reinforcement. Yet in experiment five this proved not to be the case, significant differences were found between those who rated themselves more sensitive to cues of reward and cues of punishment in their relative sensitivity, or accuracy

of responding, to cues of appetitive reinforcement and cues of active avoidance.

Given the central position of this assumption in the building of the theory it seems essential that this aspect of the theory must be put to right if the theory is to develop in to a model useful to us in the understanding of human behaviour.

It seems, with respect to Gray's theory, that at each stage of experimentation one had to abandon more and more of the assumptions of the theory and retreat to the more fundamental and central ones. Initially it was assumed that the Grayian and Eysenckian dimensions were related, and these dimensions were related in turn to conditionability. This appeared not to be the case. It was then suggested that perhaps different theories, or different personality variables would prove to be important in different types of conditioning situations, but no evidence was found to support this suggestion. Later it was suggested that Gray's theory, or the dimension of differential sensitivity might prove to be important as a performance rather than as a learning variable, an element of support was found for this. Now it is being suggested, on the basis of the data from experiment five, that Gray's formulation of the relationships between active avoidance, passive avoidance and appetitive reinforcement may not be correct.

These problems may, of course, arise because Gray is attempting to generalize from one set of empirical findings, not all of which were conclusive in themselves, obtain from non-human subjects to human behaviour, and even human typologies. It may be that in this case it is simply not possible. It seems possible that the elaboration of the higher central nervous system in man may well have produced some fundamental changes of organization, particularly with respect to learning, in man as compared to other animals like the laboratory rat or even monkeys.

It seems probable that despite the fact that differential sensitivity does survive in man the effect of the growing importance of the abstract organization of information, and the influence of other cognitive variables may well have been to reduce the importance of this variable in the determination of behaviour. It seems that it might be necessary to rebuild Gray's model, repeating and re-evaluating his findings but this time exploying, where ever possible, human subjects. This would demand a series of experiments on learning and conditioning in humans, with particular attention paid to the relative effectiveness and the relationships between cues of active and passive avoidance and appetitive reinforcement within an individual and between groups of individuals.

One would also like to investigate the specific effects of drugs e.g. alcohol, on learning to see if findings are in line with those reported by Gray for animal subjects.

In line with the results reported earlier in this chapter, having developed a reliable measure of differential sensitivity one would like to know to what extent this variable influences behaviour in general e.g. is it related to depression, or phobic reactions or to so called psychopathic behaviour?

In a similar direction it would be interesting to look at the effects of various pharmacological agents within the framework provided by Gray's theory. For example to what degree might it be possible to reduce the dosage of a drug, or change from one substance to another to facilitate the eradication of one behavioural response and its replacement by another?

It seems, then, that Gray's theory itself may suggest several areas for research, but before it can provide a useful frame work in which these can be integrated some major modifications of the theory need to be carried out, most

importantly so that it yields unequivocal predictions.

Leaving aside Gray's theory and turning to theories of personality in general it has been an underlying assumption here that learning and conditioning play an important role in determining behaviour. It was suggested that if one could find a dimension, preferably one with a neurological basis, which influenced the conditionability of the individual then this might be a fruitful place from which to start developing a theory of personality. In making this suggestion it was implicitly assumed, though such an assumption is not necessary, that there was some consistency in conditioning or that it was unidimensional, and also that we have a sound understanding of the process of conditioning. It seems that both these assumptions might be seriously questioned. It might, therefore, prove useful, if not to abandon the usual western approach of specifying groups or dimensions and investigating differences as these determine, at least to broaden the approach employed. One approach which might prove to be useful is that exemplified by Pavlov, Teplov, Nebylitsyn and their colleagues. These researchers have investigated the various properties of conditioning and attempted to relate these to properties of the nervous system and then gone on to attempt to determine the extent these nervous system properties co-exist within the same individual. For example to what extent are conditioning to positive and negative reinforcement related, or conditioning of passive and active avoidance? Are classical and operant conditioning related, or does one depend to a greater degree on "cognitive abilities"? Does the fast development of a response indicate whether or not it will quickly extinguish? To what extent is the strength of the stimuli important in relation to these other factors?

Having done this kind of research one might attempt to

develop some sort of typology of individuals based on the clusters of conditioning or nervous system properties one had discovered. Alternatively after some of the preliminary work one might put forward a theoretical model which would predict which nervous system properties or aspects of conditioning would co-exist, or be manifestations of common processes and in consequence which type of "personalities" could exist. As with all such models these might prove in the long run to be incorrect or inadequate, but until replaced they would help to integrate findings and direct future research.

Adopting such a line of research would also bring research on personality more closely into line with the rest of experimental psychology.

Some would, no doubt, find such suggestions for the future of research in personality particularly unpleasant assumming as they do that conditioning and learning are of central importance in determining individual differences, and the results of the present series of experiments do suggest that this research on its own might not prove to be adequate. Strategies, planning or the abstracting of rules, it has been observed several times, appeared to play a major part in determining behaviour, with the result that any description of, or theory of, the determinants of behaviour which ignored these would be inadequate.

It seems evident even at this stage that one needs to refine these concepts and possibly even to sub-divide them, never-the-less several questions do spring to mind which seem worth asking. To what extend do strategies influence or determine behaviour? What determines the selection of a particular strategy, as opposed to any other, on a particular occassion? How are strategies developed? Do individuals have sets of prefered strategies and is it this that produces

the aura of consistency? To what extent are strategies idiosyncratically or situationally or culturally or universally determined?

Some of these questions are dealt with in a rather vague "common sense" way in social learning theory, but there appears to be a lack of precise experimental investigation.

The most satisfactory direction for future research would be one that united these diverse suggestions within a single theoretical framework, one that explicitly acknowledged that each individual is unique, but that behaviour is not chaotic and that generalized statements can be made on one hand about the determinants of behaviour and on the other dispositional statements, describing behaviour typical of the individual or group. The ultimate aim would be a theory which integrated the nomothetic and idiographic approaches to, and traditions of, the study of individual differences; one that described the relationships between "person variables," learning and conditioning, and cognitive factors such as the development and employment of strategies both in general and with reference to specific behaviour; a theory that explained not only how and why people were different, but also how and why, to what extent and in which ways people could be expected to be similar.

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APPENDIX I

ROESSLER

Roessler has reported a number of studies in which he has attempted to relate the dimension of Ego-Strength (Es) as measured by the Barron's Scale drawn from the MMPI (Barron, 1956,), to a number of physiological variables.

Roessler, Alexander and Greenfield (1963) presented Ss with a series of six tones of 1000 Hz varying in intensity from threshold values to 120 dbs. Four physiological measures were recorded: skin resistance (SR), heart rate (HR), muscle potential and finger blood volume (FBV). Es was viewed as a measure of coping ability and Ss were divided into high, medium and low Es groups. The authors summarized their results as follows: "The principle hypothesis tested in this experiment was that high, middle and low Es groups would rank themselves in 1, 2, and 3 order respectively, in their responses on any physiological measure. Although the relevant co-variance terms were not significant for heart rate and did not quite attain significance for muscle potential, the trend in the predicted direction was evident for these physiological measures as well as for skin resistance and finger blood volume where the differences were significant." (pp. 150-151).

The results of this experiment are not quite as clear cut as Roessler seems to suggest. The Es main effect reaches significance only for FBV, although some of the interactions were significant e.g. Es x intensity, Sex of S x Es x intensity. There was, therefore, some support for Roessler's hypothesis that the high Es group would be more responsive physiologically, especially as the largest differences between groups were at the lowest levels of intensity. Roessler views this responsiveness as adaptive and the data from this experiment to

some degree justify his assertion that Es "is the psychological concept most directly related to the physiological concepts of adaptation and homeostasis. The emphasis in the concept of Es is upon the capacity to intergrate environmental adaptive demands with psychological economy." (p.143)

In a further study (Pfaehler and Roessler, 1965) glucose tolerance was the measure employed. Here, as predicted, the high Es scorers showed the higher mean level of glucose on the intravenous glucose tolerance test. The high Es group were also found to reach the peaks earlier, indicating that the low Es scorers were, for some reason using up their glucose more quickly.

In a further study (Roessler, Burch and Childers, 1966) designed to look at "real life" stress, Ss were tested on four occasions at monthly intervals. All Ss were students and the third of the testing sessions was held within ten days of their examinations. This time no significant differences were found, the amplitude of the skin conductance response was the measure used, although the high Es group did have the higher mean amplitude, as predicted. The stimuli used in this experiment were five levels of sound intensity. Roessler, however, has offered two possible explanations as to why the predicted effects did not appear to a significant degree:

(i) this experimental session(Session 3) started too long before the examination. To ensure that all Ss were tested the pre-examination session had to start ten days before the actual examination date, and Roessler felt that stress may not have begun to build up until 3 or 4 days immediately prior to the examination.

(ii) Roessler also suggested that the experiment was treated as a break from examination pressure, at least by some Ss. This was the third testing session in the series, and in

line with his suggestion Roessler reports that several Ss actually fell asleep in the course of the experiment.

The suggestions received some report when the excreted catecholamine levels were examined (Roessler, Burch and Meffend, 1967),where differences were in the predicted direction. In the high Es group those tested in the 4 days immediately prior to the examination showed a significant increase in the level of epinephrine ($p = .057$) and norepinephrine ($p = .029$) over the previous test level. When the four Ss tested in the final four days were compared with those tested in the previous seven days the former group showed a greater increase in epinephrine ($p = .115$) and norepinephrine ($p = .055$)

"In the low Es group", Roessler reports, "none of these differences reached or approximated significance; even when threat was imminent these Ss did not respond with greater excretion of catecholamines." (p.182)

When the two groups were compared using data over the whole 10 day period "the levels of catecholamine excretion of the low Es group was less under stress than under basal conditions (testing 2) and the high Es group levels were greater under the stress than under the control conditions." (p.182).

In order to strengthen his claim that the responsivity of the high Es group is adaptive Roessler notes that a number of studies e.g. O'Handlan(1965), Frazier (1966), have found that a fall in catecholamine excretion during stress was associated with a fall in performance, whereas a rise was associated with stable performance.

In the fourth testing session (McCollum, Burch and Roessler, 1969) respiration amplitude (RA) and respiration rate (RR) were recorded and it was found that "high Es Ss showed significantly greater excursion and slower rate than low Es Ss."

Feeling that he had established that high Es scorers were more responsive to simple stimuli Roessler turned to the differential responding of high and low Es scorers in the face of more complex stimuli (Roessler and Collins, 1970). The Ss viewed two films, each 12 mins. 50 secs long. SC and HR were the measures recorded. The stressor film was an accident safety film depicting three accidents. The second film, the control, was a nature film. It was found that in all situations the high Es group had a higher SC level than the low Es group, but this difference was significant only during the stressor film. Both groups showed a significant SC level increase to the three accidents but the difference between the groups was significant only for accident two. It is note worthy that while the range of SC scores showed by the high Es group was large and in the "appropriate" direction, "the SC of the low Es group was lower during most of the stressor film than during the bland film, rising above the bland film levels only at the time of the second and third accidents." Roessler continues, "This parallels the lower catecholamin values found under real life stress conditions previously". (p.736)

Roessler draws the following conclusion from this experiment: "high Es persons are more responsive in SC to complex stimuli of symbolic value as well as to simple ones. In addition, for the first time, high Es persons have been shown to be more responsive to stimuli whose effect is to lower levels of physiological activation - i.e. their range of activation is greater, varying with the nature of the stimulus configurations. High Es Ss appear to be persons who are pervasively more responsive to their environment and to changes in it. Low Es persons, on the other hand, appear to be characterized by relatively less physiological responsivity to the environmental changes. They may be employing perceptual

defence more pervasively and indiscriminantly." (p.738).

The point Roessler seems to be stressing in all this is not so much that high Es scorers are more responsive, but that they are more appropriately responsive than the low Es scorers. Roessler 's enthusiasm must, I feel, be tempered by noting that the reported differences, from which all the conclusions are drawn, were for the SC measure, while no significant differences were found in the HR measure. In fact, changes in this measure were not all in the predicted direction.

The next step in Roessler's research strategy was to look at a behavioural variable to see if here too the same pattern of superior responsiveness of the high Es group emerged. The task employed was a vigilance task under sleep deprived conditions (Strausbaugh and Roessler, 1970).

Each S was tested on three occasions; on the first occasion the S was required to sleep in the laboratory over night and then perform the vigilance task. This session was used to familiarize the S with the laboratory and the testing procedure. The second run was the same as the first and the data from this run was used to establish a baseline. On the third occasion the S was required to report to the lab at the same time as on the two previous occasions, but this time he was not allowed to sleep.

Four conditions were employed:

(i) 20 signals in a 10 period. S had 4.5 secs to respond to a signal

(ii) as "i" except that S received an electric shock if he failed to respond with in the time allowed.

(iii) 24 signals in a 10 period, with a 1.88 secs time limit in which to respond.

(iv) as "iii" except that S received an electric shock if he failed to respond within the time limit

Roessler reports that again the high Es group had a higher SC level than the low Es group, the difference being significant during sleep deprivation. The high Es group also had a higher SC level in the shock condition in the sleep deprived situation. High Es Ss had faster reaction times, but the Es x runs interaction was not found to be significant. When looking at the number of correct responses a significant interaction was found between Es and conditions but again no Es x runs interaction. Thus while there is some eivdence that the high Es group were sometimes faster or more efficient in their responding there is no evidence to suggest that they were coping better.

When Roessler analysed each run separately he found no significant differences between groups in run two (base level), but in run three (sleep deprivation),he reports that there were significant Es x conditions interactions for SC and all performance measures in which the high Es Ss "were the better performers and had the higher SC values." (p.675). On the basis of these findings Roessler feels justified in concluding: "The separate analyses of Runs II and III with larger groups (16 for run II and 20 for run III as opposed to 14 in the previous analyses) of Ss, therefore, supports the original analyses and extends them towards significant support of the hypotheses 4 (i.e. that high Es Ss would show less performance decrement following sleep deprivation than low Es Ss)."

In a final experiment (Roessler 1973) Ss were required to perform the same vigilance task as in the previous experiment but this time instead of using sleep deprivation two levels of drugs and placebos were employed. Ss were tested on seven occasions and SC and HR were recorded. In line with the predictions the high Es group was found to have a higher SC level in the high drug condition both as compared with itself in the other conditions and with the low Es group. Here again as the low Es group did not

respond differentially to any test situation whereas the high Es group did. Roessler feels that once more that he has demonstrated the "discriminative" responding of the high Es group.

This then is the data reported by Roessler and he sums it up by saying: "In every experiment the differences described are difference scores. In general ego-strength groups do not differ in resting or pre-stimulus level. In those few instances where such differences exist, co-variance adjustments of the change score was carried out yielding differences free of any pre-stimulus effects. In every experiment high Es Ss were found to be more responsive." (1973, pp 320 - 321).

The picture Roessler presents is contrary to what one might expect. Es is negatively correlated to both anxiety and neuroticism, Roessler reports correlations of -.76 with the MA scale, and -.72 with the EPI N scale. In Roessler's scheme of things, therefore, it is the less anxious, more stable person who appears to be the more appropriately, physiologically responsive. Roessler did find that high and low Es groups differed on the MA scale, but when groups were redefined on the basis of this dimension the physiological differences which had previously been apparent disappeared, and so he feels justified in maintaining that it is ego-strength and not anxiety which is the dimension responsible for, or which best reflects, the physiological differences in responsivity.

Even here, I feel, a note of caution should be added. Several measures were employed in a series of experiments but they did not all show this differential responsivity, consequently "responsivity" may, in the long run, have to be interpreted in a very limited sense. HR has proved to be a very poor measure of the hypothesised difference between the groups while SC has proved to be much more successful, though even with this measure there was a notable failure in the Roessler, Childers and Burch

study. With this in mind, then, it appears that there will inevitably be "failures" whilst testing out this theory, until such time as all the relevant parameters are specified. Indeed Roessler himself has noted the limitations of the theory at the moment and has remarked: "The percent of total variance of Es interactions related to SC and performance parameters is small but of considerable theoretical interest." (Strausbaugh and Roessler, 1970, p.626).

Knowing that Es might be related to responsivity in peripheral indices and possibly to some performance variables is, of course, of only limited interest, more important is the manner in which Roessler conceptualizes Es. He points out that when a stimulus is correctly perceived then an appropriate response may be expected, however, when a stimulus is incorrectly perceived there is a high probability that the response will be maladaptive. This leads him on to the importance of what is termed "reality testing".

"Reality testing, the ability to accurately appraise the nature and intensity of a stimuli, is one attribute of successful coping. Another attribute of successful coping is the ability to respond to the need created by stimuli in such a degree and in such a pattern so as to fulfill the need. He continues: "The interpersonal coping abilities most relevant to psychiatry include the ability to accurately assess and respond to behaviour of others, while at the same time maintaining the integrity of the constellation of previously learned self-percepts called the ego. The ability to maintain ego integrity is ego-strength." (Roessler, 1973, pp. 316 - 317).

This then is how Roessler views Es, the ability to respond to situations in such a way as to preserve "ego-integrity", to leave the ego unscathed. There are, of course, two ways in which "ego-integrity" might be preserved: (a) to employ many and strong

defences frequently, the net result being the reduction of the intensity of the perceived stimuli. (b) "If a strong ego is one characterized by little or no perceptual defence all stimuli would be perceived fully, but less threatening ones less intensely than more threatening ones. (We would) therefore expect such a person to respond more to threatening stimuli and less to non-threatening ones." (1973, p.325).

The results reported by Roessler support the latter view with high Es scorers showing both heightened responsivity to stressful stimuli and a more relaxed responsiveness to non-threatening stimuli, while the differentiation of responses to various stimuli appears to be much poorer in the low Es Ss, at least as indicated by some peripheral measures.

Pfaehler and Roessler (1965) expressed the idea as follows: "Our previous research has lead us to the tentative theoretical formation that persons of low ego-strength as compared to presumedly more healthy persons are characterized by a greater apparent rigidity in response to simple physical stimuli. To state this differently, our previous results suggest that when persons of low ego-strength are subjected to a stimulus which displaces them from their physiological pre-stimulus levels, the antithetical ('homeostatic') physiological responses which are evoked are of greater force than those of high ego-strength persons and reduces the amplitude of their responses to the displacing stimulus to a greater degree. Such physiological rigidity, we speculate, parallels the psychological rigidity of such persons. In other words, we suspect a relative physiological 'blunting' that parallels the well known affective blunting of severly ill psychiatric patients." (p.432)

From this Roessler goes on to argue that given a threatening situation e.g. the first night in an unfamiliar laboratory, the high Es individual will reduce anxiety by

exploring and appraising the situation, whereas the low Es individual would employ defences and attempt to reduce anxiety responses by using a deactivating strategy. Consequently, though the two groups may have apparently reached the same level of e.g. SC, their states - the results of the methods employed to reach the observed level - may, in fact, be very different. The result of utilizing a deactivating strategy will be that low Es groups will be less responsive not only to threatening stimuli, but as they are using deactivation to return to prestimulus levels while leaving the activation constant they can be expected to be less responsive to non-threatening, deactivating, stimuli also. The high Es group, on the other hand, attempt primarily to reduce activation by appraising the activating stimuli.

As has already been noted not all physiological indices were equally successful in demonstrating the difference in responsivity between the two groups with the result that there is considerable danger in jumping from the reported data to conclusions about behaviour "in general". Roessler, himself however, cannot be criticised strongly here. He says: "I am not content, therefore, to label autonomic responses as behavioural, establish a relationship between them and the scorers on the MPI scale, and then make the indefensible leap from differences of the kind I have described to broad generalizations about behaviour generally. If ego-strength is a measure of adaptability or coping ability it should be possible to design experiments in which adaptive somatomotor behaviours are specified and quantified. In other words, the behaviours themselves should be the primary criterion of adaptation." (p.322)

As observed earlier most of the data reported by Roessler are derived from peripheral indices, and is not consistent within itself, so generalizations to more overt behaviour must be made cautiously. It seems, given that not all measures were

equally sensitive to this difference between the high and low Es groups in responsivity, that situation and task variables, as well as other person variables will all at times attenuate any observed relationship between Es and "appropriate" responsivity.

Although the results of experiment one of the present series were unclear it was suggested that they might be interpreted as being consonant with Roessler's theory, as it was the LN Ss who showed the more adaptive responding, using the rewarded pronoun more and the punished pronound less once reinforcement was made contingent on appropriate responding.

No support for Roessler's theory emerged from experiment two but on one hand Es itself was not measured while on the other it was suggested that perhaps this theory, like the other theories considered, yielded accurate predictions only in a restricted range of situations.

In experiment three once again reinforcement will be contingent on appropriate responding. If Roessler is correct and Es is related to "the ability to accurately appraise the nature of stimuli" we can expect the high Es individuals to both gain reward and avoid punishment more successfully than low Es individuals and so, as Roessler advises we will be able to evaluate the differential adaptability of high and low Es individuals by observing their behaviour.

APPENDIX II

VERBS USED IN EXPERIMENT I

Replied	Watched	Regulated	Emerged	Travelled
Restored	Applauded	Lost	Stayed	Stood
Freed	Bluffed	Came	Sold	Opened
Knew	Presented	Explained	Aspired	Fixed
Returned	Spoke	Said	Walked	Stated
Swam	Consoled	Enclosed	Indicated	Visited
Chuckled	Saw	Ran	Shrugged	Ate
Played	Snored	Began	Looked	Obtained
Showed	Reached	Found	Admired	Supported
Accepted	Approved	Entertained	Smiled	Picked
Sustained	Congratulated	Called	Joined	Inquired
Wrote	Expressed	Told	Thought	Enjoyed
Attended	Saluted	Hurried	Received	Created
Defended	Waited	Stopped	Reported	Cheered
Went	Misplaced	Relieved	Heard	Carried
Reclined	Reassured	Surpassed	Recited	Hoped
Listened	Liked	Described	Arrived	Commemorated
Sent	Sympathized	Rejoiced	Rushed	Agreed
Remembered	Answered	Read	Lived	Observed
Collected	Changed	Entered	Remained	Welcomed
Obeyed	Assisted	Preserved	Decided	
Followed	Enlarged	Completed	Heeded	
Scribbled	Relaxed	Brought	Danced	
Allowed	Complimented	Mentioned	Idolized	
Finished	Praised	Asked	Decorated	
Planned	Continued	Befriended	Helped	
Compiled	Bought	Subtracted	Honoured	
Closed	Devoted	Assigned	Turned	
Chose	Rewarded	Talked	Behaved	
Ordered	Moved	Delivered	Drove	

Questions used in Experiment One to classify subjects as to whether or not they were aware of the reinforcement contingencies obtaining:

1. Did you give the first phrase you thought of?

2. How did you go about deciding which pronouns to use?

3. Did you think you were using some pronouns more than others?

4. (If '3' was answered positively)
   Can you give any reason why you used the pronoun more frequently?

5. Did you notice I said anything?

6. Did you figure out why I said "mmm", (or "tut")?

7. Did you think my saying "mmm" (or "tut") had anything to do with the sentences you made up?

8. Did you think the "mmm" (or "tut") had anything to do with the pronoun you used?

(Questions were taken from Gidwani, 1971)

SYLLABLES USED IN EXPERIMENTS TWO AND THREE.

Syllables used in conditioning phase of Experiment Two:

XIY	YEQ	XUF
XIG	YEX	DUJ
JIH	ZEV	QUG
YIV	ZEJ	CUJ
ZIH	XEH	VUJ
XIC	XEB	XUJ
SIJ	XEY	ZUF
NIJ	XEK	XUD
YIJ	XEQ	XUQ
XIQ	VEZ	ZUJ
XIK	XEN	QUH
JIY	XEF	KUJ
XIF	XEG	TUJ
HIX	NEJ	XUW
XIW	QEP	ZUQ
QIH	QEH	VUH
CIJ	QEF	XUH
QIX	QEX	XUG
ZIQ	CEJ	QUW
ZIX	YEQ	XUK

Syllables used in the test phase of Experiment Two:

WIJ	YEJ	XUV
ZIY	ZEQ	QUJ
XIN	ZEH	XUP
XIB	ZEQ	KUQ
QIF	VEQ	WUQ
ZIJ	XEP	XUZ
YIX	XEV	VUY
XIJ	XEM	XUC
QIJ	QEX	YUF
KIJ	NEJ	QUX

The thirty syllables characterized by the central vowels I and U were used as the stimuli in Experiment Three

STIMULI USED IN EXPERIMENT FIVE

	TYPE 1 (CVC)		TYPE 2 (VCV)		TYPE 3 (CCC)	(VVV)
1.	XOJ	QUX	OXE	IQA	QXJ	IOE
2.	XEJ	ZAJ	OJE	UQA	QYX	IAO
3.	ZOJ	QIY	UXE	UZI	YZX	AEO
4.	QUJ	ZIY	IZE	OZU	JYQ	EAI
5.	XIJ	ZUQ	OQE	AQO	JXZ	EOU
6.	XUJ	XUZ	OZE	UQA	ZXJ	UOE
7.	XUY	YIX	OZA	IJE	ZXQ	UOI
8.	ZOQ	XAY	OQA	IYU	QYJ	IAU
9.	XIY	ZIX	OXI	UQI	QXZ	IUO
10.	XOQ	QEJ	AJE	EYA	ZYQ	UAI
11	YEJ	QAZ	OZI	AJI	JQX	EIO
12.	XUQ	ZIQ	OXU	UQE	QJX	IEO
13.	XOZ	YAJ	OYE	OJA	YZQ	AUI
14.	XAJ	YEQ	IXE	AJO	JZQ	EUI
15.	QIJ	ZIJ	AZE	EJI	YXJ	AOE
16.	QOJ	XEY	OQU	EQA	QJY	IEA
17.	YUJ	YEX	OXA	IXU	ZYJ	UAE
18.	XIZ	JEQ	IJO	IXO	JQY	EIA
19	XOY	JIY	OYU	AXI	ZQX	UIO
20.	QEX	QOZ	UZE	AZU	XZJ	OUE
21	XAZ	QOX	UJE	UZA	QXY	IOA
22.	ZUJ	YOQ	UJI	UZO	ZJY	UEA
23.	ZEJ	YUZ	AQI	AQU	QZX	AIO
24.	ZEQ	ZUY	OYI	AZI	JYZ	EAU
25.	YIQ	ZUX	OJU	AZO	YXQ	AOI
26.	XAQ	YIZ	OQI	IXA	XQJ	OIE
27.	XEZ	YOX	AQE	UXI	ZYX	UAO
28.	XIQ	XUQ	AXE	OYA	ZQY	UIA
29.	YIJ	YUX	IZO	AXO	XZQ	OUI
30.	YOJ	QOY	UYE	IQE	XJY	OEA

APPENDIX III

3a. Eysenck Personality Inventory

3b. Barron's Ego-Strength Scale

3c. Rotter's Locus of Control Scale

3d. Smith's Quick Measure of Achievement Motivation

3e. Wolpe-Lang Fear Survey Schedule

3f. Self-Rating Questionnaire.

Questions 1 - 3 were used in experiments 2 - 4

Questions 1 - 12 in experiment 5.

E.P.I.

FORM A

NAME..... AGE.....

OCCUPATION..... SEX.....

N= ☐ E= ☐ L= ☐

Instructions

Here are some questions regarding the way you behave, feel and act. After each question is a space for answering "YES" or "NO".

Try to decide whether "YES" or "NO" represents your usual way of acting or feeling. Then put a cross in the circle under the column headed "YES" or "NO". Work quickly, and don't spend too much time over any question; we want your first reaction, not a long-drawn out thought process. The whole questionnaire shouldn't take more than a few minutes. Be sure not to omit any questions.

Now turn the page over and go ahead. Work quickly, and remember to answer every question. There are no right or wrong answers, and this isn't a test of intelligence or ability, but simply a measure of the way you behave.

HODDER & STOUGHTON

E ○ N ○ L ○ ☐☐☐☐☐☐☐☐☐☐

FORM A

		YES	NO
1.	Do you often long for excitement?	○	○
2.	Do you often need understanding friends to cheer you up?	○	○
3.	Are you usually carefree?	○	○
4.	Do you find it very hard to take no for an answer?	○	○
5.	Do you stop and think things over before doing anything?	○	○
6.	If you say you will do something do you always keep your promise, no matter how inconvenient it might be to do so?	○	○
7.	Does your mood often go up and down?	○	○
8.	Do you generally do and say things quickly without stopping to think?	○	○
9.	Do you ever feel "just miserable" for no good reason?	○	○
10.	Would you do almost anything for a dare?	○	○
11.	Do you suddenly feel shy when you want to talk to an attractive stranger?	○	○
12.	Once in a while do you lose your temper and get angry?	○	○
13.	Do you often do things on the spur of the moment?	○	○
14.	Do you often worry about things you should not have done or said?	○	○
15.	Generally, do you prefer reading to meeting people?	○	○
16.	Are your feelings rather easily hurt?	○	○
17.	Do you like going out a lot?	○	○
18.	Do you occasionally have thoughts and ideas that you would not like other people to know about?	○	○
19.	Are you sometimes bubbling over with energy and sometimes very sluggish?	○	○
20.	Do you prefer to have few but special friends?	○	○
21.	Do you daydream a lot?	○	○
22.	When people shout at you, do you shout back?	○	○
23.	Are you often troubled about feelings of guilt?	○	○
24.	Are *all* your habits good and desirable ones?	○	○
25.	Can you usually let yourself go and enjoy yourself a lot at a lively party?	○	○
26.	Would you call yourself tense or "highly-strung"?	○	○
27.	Do other people think of you as being very lively?	○	○

		YES	NO
28.	After you have done something important, do you often come away feeling you could have done better?	○	○
29.	Are you mostly quiet when you are with other people?	○	○
30.	Do you sometimes gossip?	○	○
31.	Do ideas run through your head so that you cannot sleep?	○	○
32.	If there is something you want to know about, would you rather look it up in a book than talk to someone about it?	○	○
33.	Do you get palpitations or thumping in your heart?	○	○
34.	Do you like the kind of work that you need to pay close attention to?	○	○
35.	Do you get attacks of shaking or trembling?	○	○
36.	Would you always declare *everything* at the customs, even if you knew that you could never be found out?	○	○
37.	Do you hate being with a crowd who play jokes on one another?	○	○
38.	Are you an Irritable person?	○	○
39.	Do you like doing things in which you have to act quickly?	○	○
40.	Do you worry about awful things that might happen?	○	○
41.	Are you slow and unhurried in the way you move?	○	○
42.	Have you ever been late for an appointment or work?	○	○
43.	Do you have many nightmares?	○	○
44.	Do you like talking to people so much that you never miss a chance of talking to a stranger?	○	○
45.	Are you troubled by aches and pains?	○	○
46.	Would you be very unhappy if you could not see lots of people most of the time?	○	○
47.	Would you call yourself a nervous person?	○	○
48.	Of all the people you know, are there some whom you definitely do not like?	○	○
49.	Would you say that you were fairly self-confident?	○	○
50.	Are you easily hurt when people find fault with you or your work?	○	○
51.	Do you find it hard to really enjoy yourself at a lively party?	○	○
52.	Are you troubled with feelings of inferiority?	○	○
53.	Can you easily get some life into a rather dull party?	○	○
54.	Do you sometimes talk about things you know nothing about?	○	○
55.	Do you worry about your health?	○	○
56.	Do you like playing pranks on others?	○	○
57.	Do you suffer from sleeplessness?	○	○

PLEASE CHECK TO SEE THAT YOU HAVE ANSWERED *ALL* THE QUESTIONS

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Below you will find a list of questions. I would like you to consider whether they are more true or false for you, and answer by putting a tick in the appropriate box; T (true) if it is true for you; F (false) if it is false for you.

Try to answer all the questions. Remember there are no right or wrong answers.

	T	F
I have a good appetite		
I have diarrhoea once a month or more		
At times, I have fits of laughing and crying that I cannot control		
I find it hard to keep my mind on a task or job		
I have had very peculiar and strange experiences		
I have a cough most of the time		
I seldom worry about my health		
My sleep is fitful and disturbed		
When I am with people, I am bothered by hearing very queer things		
I am in just as good physical health as most of my friends		
Everything is turning out just like the prohets of the Bible said it would		
Parts of my body often have feelings like burning, tingling, crawling or "going to sleep"		
I am easily downed in an argument		
I do many things which I regret afterwards (I regret things more, or more often than others seem to)		
I go to a place of worship almost every week		
I have met problems so full of possibilities that I have been unable to make up my mind about them		
Some people are so bossy that I feel like doing the opposite of what they request, even though I know they are right		
I like collecting flowers or growing house plants		
I like to cook		
During the past few years I have been well most of the time		
I have never had a fainting spell		
When I get bored, I like to stir up some excitement		
My hands have not become clumsy or awkward		

	T	F
I feel weak all over much of the time		
I have had no difficulty in keeping my balance in walking		
I like to flirt		
I believe my sins are unpardonable		
I frequently find myself worrying about something		
I like science		
I like to talk about sex		
I get mad easily and then get over it soon		
I brood a great deal		
I dream frequently about things that are best kept to myself		
My way of doing things is apt to be misunderstood by others		
I have had blank spells in which my activities were interrupted and I did not know what was going on around me		
I can be friendly with people who do things which I consider wrong		
If I were an artist, I would like to draw flowers		
When I leave home I do not worry about whether the door is locked and the windows closed		
At times I hear so well it bothers me		
Often I cross the street in order not to meet someone I see		
I have strange and peculiar thoughts		
Sometimes I enjoy hurting persons I love		
Sometimes some unimportant thought will run through my mind and bother me for days		
I am not afraid of fire		
I do not like to see women smoke		
When someone says silly or ignorant things about something I know, I try to set him right		
I feel unable to tell anyone all about myself		
My plans have frequently seemed so full of difficulties that I have had to give them up		
I would certainly enjoy beating a crook at his own game		
I have had some very unusual religious experiences		
One or more members of my family is very nervous		

	T	F
I am attracted by members of the opposite sex		
The man who had most to do with me when I was a child, (such as my father, step-father etc) was very strict with me		
Christ performed miracles such as changing water into wine		
I pray several times every week		
I feel sympathetic towards people who tend to hang on to their griefs and troubles		
I am afraid of finding myself in a closet or small closed place		
Dirt frightens or disgusts me		
In my home we have always had the ordinary necessities (such as enough food, clothing etc.)		
I am made nervous by certain animals		
My skin seems to be unusually sensitive to touch		
I feel tired a good deal of the time		
I never attend a sexy show if I can avoid it		
If I were an artist I would like to draw children		
I sometimes feel that I am about to go to pieces		
I have often been frightened in the middle of the night		
I very much like horseback riding		

Name _____ Sex _____

Date _____ Age _____

This questionnaire is made up of a series of pairs of statements. Indicate, by putting a tick in the appropriate box, which of the two statements you agree with more. Be sure not to miss out any questions.

1.a. Children get into trouble because their parents punish them too much

b. The trouble with most children nowadays is that their parents are too easy with them

2.a. Many of the unhappy things in people's lives are partly due to bad luck

b. People's misfortunes result from the mistakes they make

3.a. One of the major reasons why we have wars is because people don't take enough interest in politics

b. There will always be wars, no matter how hard people try to prevent them

4.a. In the long run people get the respect they deserve in this world

b. Unfortunately, an individual's worth often passes unrecognized no matter how hard he tries

5.a. The idea that teachers are unfair to students is nonsense

b. Most students don't realize the extent to which their grades are influenced by accidental happenings

6.a. Without the right breaks one cannot be an effective leader

b. Capable people who fail to become leaders have not taken advantage of their opportunities

7.a. No matter how hard you try some people just don't like you

b. People who can't get others to like them don't understand how to get along with others

8.a. Heredity plays the major role in determining one's personality

b. It is one's experiences in life which determine what they're like

9.a. I have often found that what is going to happen will happen

b. Trusting to fate has never turned out as well for me as making a decision to take a definite course of action

10.a. In the case of the well prepared student there is rarely if ever such a thing as an unfair test

b. Many times exam questions tend to be so unrelated to course work that studying is really useless

11.a. Becoming a success is a matter of hard work, luck has little or nothing to do with it

b. Getting a good job depends mainly on being in the right place at the right time

12.a. The average citizen can have an influence in government decisions

b. This world is run by the few people in power, and there is not much the little guy can do about it

13.a. When I make plans, I am almost certain that I can make them work

b. It is not always wise to plan too far ahead because many things turn out to be a matter of good or bad fortune anyhow

14.a. There are certain people who are just no good

b. There is some good in everybody

15.a. In my case getting what I want has little or nothing to do with luck

b. Many times we might just as well decide what to do by flipping a coin

16.a. Who gets to be the boss often depends on who was lucky enough to be in the right place first

b. Getting people to do the right thing depends upon ability, luck has little or nothing to do with it

17.a. As far as world affairs are concerned, most of us are the victims of forces we can neither understand, nor control

b. By taking an active part in political and social affairs the people can control world events

18.a. Most people don't realize the extent to which their lives are controlled by accidental happenings

b. There really is no such thing as "luck"

19.a. One should always be willing to admit mistakes

b. It is usually best to cover up one's mistakes

20.a. It is hard to know whether or not a person really likes you

b. How many friends you have depends upon how nice a person you are

21.a. In the long run the bad things that happen to us are balanced by the good ones

b. Most misfortunes are the result of lack of ability, ignorance, laziness, or all three

22.a. With enough effort we can wipe out political corruption

b. It is difficult for people to have much control over the things politicians do in office

23.a. Sometimes I can't understand how teachers arrive at the grades they give

b. There is a direct connection between how hard I study and the grades I get

24.a. A good leader expects people to decide for themselves what they should do

b. A good leader makes it clear to everybody what their jobs are

25.a. Many times I feel that I have little influence over the things that happen to me

b. It is impossible for me to believe that chance or luck plays an important role in my life

26.a. People are lonely because they don't try to be friendly

b. There's not much use in trying too hard to please people, if they like you, they like you

27.a. There is too much emphasis on athletics in high school

b. Team sports are an excellent way to build character

28.a. What happens to me is my own doing

b. Sometimes I feel that I don't have enough control over the direction my life is taking

29.a. Most of the time I can't understand why politicians behave the way they do

b. In the long run the people are responsible for bad government on a national as well as on a local level

Name _____ Sex _____

Date _____ Age _____

INSTRUCTIONS

Read each of the following statements. If you think that it is true underline the TRUE. If you think that it is false underline the FALSE.

Please do not miss out any statements. Even though it may be difficult, you must decide one way or the other.

1.	I am not clear about the instructions for this test	TRUE FALSE
2.	When I was young I enjoyed the feeling of accomplishment after I had done something well	TRUE FALSE
3.	The feeling of a job well done is a great satisfaction	TRUE FALSE
4.	I don't think I'm a good trier	TRUE FALSE
5.	I would sooner admire a winner than win myself	TRUE FALSE
6.	If it's worth doing, it's worth doing well	TRUE FALSE
7.	Failure is no sin	TRUE FALSE
8.	Incentives do more harm than good	TRUE FALSE
9.	In an unknown situation it doesn't pay to be pessimistic	TRUE FALSE
10.	I dislike red tape	TRUE FALSE
11.	I work best when I have a job that I like	TRUE FALSE
12.	It's never best to set one's own challenges	TRUE FALSE
13.	I don't care what others do, I go my own way	TRUE FALSE
14.	Even a good poker player can't do much with a poor hand	TRUE FALSE
15.	Modern life isn't too competitive	TRUE FALSE
16.	You can try too hard sometimes, it's best to let the world drift by	TRUE FALSE
17.	Most people want success because it brings respect	TRUE FALSE

Please check back to see that you haven't missed any out.

The items in this questionnaire refer to things and experiences that may cause fear or other unpleasant feelings. Write the number of each item in the column that describes how much you are disturbed by it nowadays.

		Not at All	A Little	A Fair Amount	Much	Very Much
1.	Noise of vacuum cleaners					
2.	Open wounds					
3.	Being alone					
4.	Being in a strange place					
5.	Loud voices					
6.	Dead people					
7.	Speaking in public					
8.	Crossing streets					
9.	People who seem insane					
10.	Falling					
11.	Automobiles					
12.	Being teased					
13.	Dentists					
14.	Thunder					
15.	Sirens					
16.	Failure					
17.	Entering a room where other people are already seated					
18.	High places on land					
19.	Looking down from high buildings					
20.	Worms					
21.	Inaginary creatures					
22.	Strangers					
23.	Rats					
24.	Journeys by train					
25.	Journeys by bus					
26.	Journeys by car					
27.	Feeling angry					
28.	People in authority					
29.	Flying insects					
30.	Seeing other people injected					
31.	Sudden noises					
32.	Dull weather					
33.	Crowds					
34.	Large open spaces					

		Not at All	A Little	A Fair Amount	Much	Very Much
35.	Cats					
36.	One person bullying another					
37.	Tough looking people					
38.	Birds					
39.	Sight of deep water					
40.	Being watched working					
41.	Dead animals					
42.	Weapons					
43.	Dirt					
44.	Crawling insects					
45.	Sight of fighting					
46.	Ugly people					
47.	Fire					
48.	Sick people					
49.	Dogs					
50.	Being criticized					
51.	Strange shapes					
52.	Being in an elevator					
53.	Witnessing surgical operations					
54.	Angry people					
55.	Mice					
56.	Blood					
	a - Human					
	b - Animal					
57.	Parting from friends					
58.	Enclosed places					
59.	Prospect of a surgical operation					
60.	Feeling rejected by others					
61.	Airplanes					
62.	Medical odors					
63.	Feeling disapproved of					
64.	Harmless snakes					
65.	Cemeteries					
66.	Being ignored					
67.	Darkness					
68.	Premature heart beats (Missing a beat)					
69.	Nude Men (a) Nude Women (b)					
70.	Lightning					

	Not At All	A Little	A Fair Amount	Much	Very Much
1. Doctors					
2. People with deformities					
3. Making mistakes					
4. Looking foolish					
5. Losing control					
6. Fainting					
7. Becoming nauseous					
8. Spiders					
9. Being in charge or responsible for decisions					
10. Sight of knives or sharp objects					
11. Beooming mentally ill					
12. Being with a member of the opposite sex					
13. Taking written tests					
14. Being touched by others					
15. Feeling different from others					
16. A lull in conversation					

Name.....

Sex..... Age.....

Date.....

Occupation.....

Educational background.....

Below you will find a list of 12 descriptions and questions. Following each of these will be two scales.

I want you to use the first scale to rate your typical behaviour. Put an X in one of the boxes (not on the lines between the boxes). The more you feel that one of the terms is applicable to you the more towards that end of the scale you are to put the cross.

The second scale runs from 'Always' to 'Sometimes' (N.B. 'Always' is not on the right-hand side for every question). I want you to use this scale to rate what you have said in the first scale. e.g. if, in the first question, you have put a cross in the last box, at the Introvert end of the scale, I want you to use the second scale to say whether you always behave in an introverted way or only sometimes or something in between.

If you put a cross in the centre box it might be for one of two reasons (i) because you feel that your behaviour is mid way between Extrovert and Introvert. In this case in the second scale you would put a cross towards the 'always' end of the scale; or (ii) because neither of the two terms describes your behaviour better, you behave in some situations in one way, in others in the other. In this case in the second scale you would put your cross towards the 'sometimes' end of the scale.

1. The typical extravert is a sociable person, likes parties, has many friends, needs people to talk to, and does not like reading or studying alone.
   The typical introvert is a quiet, retiring sort of person; he is reserved and distant except to intimate friends.

Extravert — Average — Introvert

Always — Sometimes

2. An impulsive person takes chances, often sticks his neck out and acts on the spur of the moment.
   A cautious person tends to plan ahead, "looks before he leaps", and distrusts impulses of the moment.

Cautious — Average — Impulsive

Sometimes — Always

3. A nervous person tends to be "highly strung", becomes depressed or experiences changes in mood for no good reason; tends to worry, sleeps poorly, is easily hurt by criticism and is troubled by feelings of inferiority.

Nervous — Average — Not nervous

Always — Sometimes

4. Are you sensitive to, or motivated to avoid unpleasant things or situations?

Very — Not at all

Always — Sometimes

5. Are you sensitive to, or motivated to obtain pleasant things or situations?

Not at all — Very

Sometimes — Always

6. Are you more motivated by, or sensitive to the likelihood of a pleasant outcome or the likelihood of an unpleasant outcome?

Pleasant — Unpleasant

Always — Sometimes

7. Do you feel that your behaviour is determined by factors outside yourself (such as luck, other people or the situation you find yourself in) over which you have no control?

Totally — Not at all

Sometimes — Always

8. Do you feel you determine your own behaviour?

Not at all — Totally

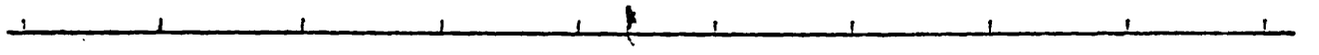
Sometimes — Always

9. Which has the greater influence on your behaviour yourself or factors outside yourself(such as luck, other people or the situation you find yourself in)

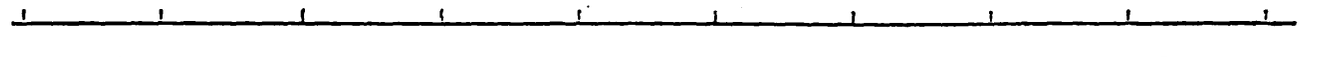
Myself — Outside factors

Always — Sometimes

10. Do you feel that failures you experience are your own fault or due to outside factors (such as luck, other people or the situation you find yourself in).

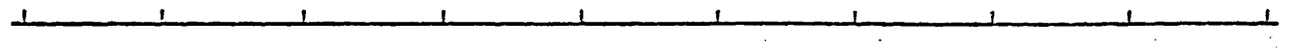


Outside factors — Myself

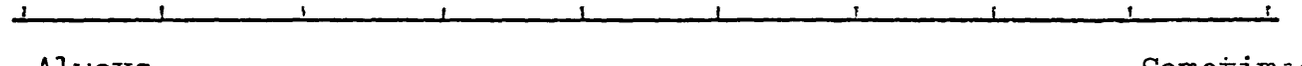


Sometimes — Always

11. Do you feel that your achievements are due to yourself or to outside factors (such as luck, other people or the situation you find yourself in).

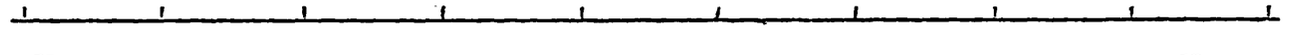


Myself — Outside factors



Always — Sometimes

12. Do you take into account the consequences of your behaviour?



Yes — No

Always — Sometimes

APPENDIX IV:

Raw data from experiments 1 - 5, including Fear Survey Schedule data.

Data from each experiment is recorded on a separate microfiche.