

ACTIVATION and PERFORMANCE

An analysis in terms of a two
dimensional activation theory.

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PREFACE

The main text of this thesis is divided into four parts which have been called chapters for the sake of convention and convenience. Chapters are designated by Roman numerals. Each chapter is divided into sections which are designated by lower case letters and some sections are further sub-divided into sub-sections which are designated by italicised Roman numerals.

In addition to the main text there are four appendices. Apart from appendix II, which is included as an appendix because of a requirement by the examining committee, the information in the appendices is relevant to several parts of the thesis. It seemed more convenient to place this information in appendices than to repeat it at various points in the main text.

Experimental data are included in tables and graphs. The inclusion of a complete set of raw data would have resulted in excessive over-loading of the thesis. The general principle has been to include sufficient scaled data from the main experiments to permit recalculation of the statistical results, or to permit alternative interpretations to be tested. Data from subsidiary experiments are not included. Graphs are only used to illustrate particular points which might not be clear from tables alone; there seems to be little virtue in including graphs which merely repeat tabulated data.

The author has read all of the listed references except six. These six are asterisked and were either read in abstract form or were fully reported in general reviews of the literature. With the exception of one paper, by Brodie and Shore, no major conclusions were based upon

papers not read in the original form. While the author has not read the Brodie and Shore paper in the original publication he has read two full summaries of it as well as several explicit references in the general literature.

The author is indebted to many people who helped, either directly or indirectly, in the completion of the experimental programme which forms the main bulk of this thesis. Particular gratitude is due to Professor James Drever, who tolerated what may have appeared at times as excessive tardiness in carrying out actual experimental work, and who permitted the expenditure of a considerable amount of time and money on the completion of the conductance measuring apparatus upon which the whole experimental programme depended; to Professor Ian Hunter, who gave consistent encouragement in the early days when experiments were not turning out as expected; to Mr. Tim Regan, who worked with the author for nearly two years in devising really adequate apparatus for the measurement of skin conductance phenomena; and to Messrs. Roger Blackman, Michael Cowles, and Robert Ross who, as undergraduate students, performed more than adequate experiments at the suggestion of the author - experiments which pointed the way to much of the most fruitful work in this thesis. Thanks are also due to the large number of students and schoolboys who acted as subjects in what were very often extremely tedious experiments. In the latter context particular mention should be made of Mr. W.M. Dewar, Headmaster of George Heriot's School, who patiently tolerated considerable demands upon the time of a fairly large number of his boys.

Ideas which form the basis for the author's theoretical speculations are acknowledged in the text. As with the experimental work the author takes full responsibility for everything which appears in the thesis. Colleagues, students, or other research workers can in no way be blamed for any misconceptions or mistakes which appear.

Finally gratitude is due to my wife who put up with long hours of monologue on experimental procedure and theory and who often, with a short but relevant sentence, would bring the discussion back to the level of reality. The value of an intelligent and objective outside observer cannot be over-estimated. She also performed the lengthy task of typing the first draft of this thesis during which she took the opportunity to correct innumerable mistakes in grammar and spelling. Some mistakes must have escaped her careful eye and for these I apologise in advance.

CONTENTS

Page

CHAPTER I: Introduction

1. a) The Concept of Activation
3. b) The Measurement of Activation.
6. c) Outline of the Present Study.
9. d) Summary and Conclusions to Introduction.

CHAPTER II: The Measures Used

13. a) Psychological and Physiological Interpretations of Skin Conductance Phenomena.
14. i) The Psychological Correlates of Skin Conductance Change.
17. ii) Individual Differences in Skin Conductance Change.
20. iii) Psychological Interpretations of Basal Skin Conductance.
24. iv) The Physiological Nature of Skin Conductance Change.
31. v) The Physiological Basis of Basal Skin Conductance.
35. b) The Scales of Anxiety and Neuroticism.
35. i) The Manifest Anxiety Scale.
38. ii) The Maudsley Personality Inventory
41. c) Relationships between Measures.
41. i) Relationship between Basal Conductance and Conductance Change (G.S.R.).
42. ii) Relationship between M.P.I. 'N' Scale and M.A.S.
42. iii) Relationship between Basal Conductance and Anxiety and Neuroticism Scales.
43. iv) Relationship between Conductance Change (G.S.R.) and Anxiety and Neuroticism Scales.
44. v) Summary.

Page

46.	CHAPTER III: The Experiments
47.	a) G.S.R. and Performance.
47.	i) G.S.R. and Word Recall.
58.	ii) G.S.R. and Sensory Sensitivity.
58.	iii) Conclusions from G.S.R. Experiments.
62.	b) Basal Skin Conductance and Performance.
62.	i) Basal Skin Conductance and Word Recall.
72.	ii) Basal Skin Conductance and the Pursuit Rotor.
85.	iii) Basal Skin Conductance and Rapid Dotting.
94.	iv) Basal Skin Conductance and Reaction Time.
109.	v) The Significance of Basal Conductance.
118.	c) Experiments using Anxiety and Neuroticism Scales.
118.	i) The 'N' Scale of the M.P.I. and Examination Performance.
126.	ii) The 'N' Scale of the M.P.I. and Rote Learning at different Presentation Rates.
130.	iii) The Significance of the M.A.S. and the 'N' Scale of the M.P.I.
134.	CHAPTER IV: Discussion and Summary
134.	a) Conclusions from Experiments.
135.	b) The Overvaluation of the Concept of Activation.
136.	c) Activation and a Theory of Attention.
147.	d) Drive and Activation.
153.	e) The Dimension of 'Adequacy - Inadequacy'.
154.	f) The Activation Measures as Indices of Individual Difference.
157.	g) Summary

Page

160. REFERENCES.
169. APPENDIX I: Apparatus for Measuring Skin Conductance and Skin Conductance Change.
174. APPENDIX II: 'Skin Conductance Change and Sensory Sensitivity'. (Paper by Corteen and Blackman - to be published).
190. APPENDIX III: The Manifest Anxiety Scale and the Maudsley Personality Inventory.
195. APPENDIX IV: Some Notes on Methodological Problems.

I. INTRODUCTION

a) The Concept of Activation

The concept of arousal or activation level has become well established since the pioneering work of Moruzzi & Magoun (1949) on the physiological centres mediating wakefulness, and the statement of activation theory by Lindsley (1951) in Stevens' Handbook of Experimental Psychology. The concept had been presaged by the work and theories of Duffy (1941) and Freeman (1948) but it was not until the establishment of an apparently sound physiological basis for the arousal dimension that it became widely accepted.

Activation theory is clearly and adequately stated by Hebb (1955) Malmö (1959) and Duffy (1962), among many others. In its conventional form it should require only a brief restatement here. The hypothesis states that there is a single dimension of 'energy mobilisation'¹ which stretches from coma to uncontrolled diffuse excitement. An individual's position on this dimension, at a given instant of time, defines the intensity, but not the direction, of his behaviour at that instant. It is assumed that the level of activation is related to activity in the non-specific reticular system of the mid-brain. In most of the psychological literature, with the notable exception of a paper by Samuels (1959), little attempt has been made to specify the nature of the reticular activity.

An inevitable extension of the hypothesis relates it to skilled performance. The general statement here is that performance is a non-linear function of activation level, task complexity, and amount of

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The term is Duffy's

practice. This is more or less a statement of the common-sense position which tends to assume that there is an optimum level of 'tension' but that this will be less critical if the task is relatively simple, or the performer is highly practised, or both. This is a particularly common framework used by sports commentators and others to explain unexpectedly good or bad athletic performances.

Up to this point the activation hypothesis is unexceptionable if rather vague. Such a dimension, in certain circumstances, is a useful explanatory concept and, as will be seen in section a) of chapter II, it has served to bring some unity to the complex field of emotional theory. Unfortunately it has gone rather far in the hands of some psychologists, particularly in Duffy's (1962) latest statement on the topic, who have tended to ignore evidence in their attempts to establish a single all-embracing activation dimension. The rock upon which all such attempts must founder is the problem of the lack of inter-correlation between what are generally accepted as possible measures of activation. This certainly casts as much doubt upon the validity of a unitary dimension of activation as was cast upon the concept of a unitary dimension of intelligence by the inter-correlational studies of the early factor analysts.

Obviously the concept must be debased if it becomes the vaguely defined theoretical repository for all observations of differences in performance which cannot be explained by prior experience. Under these circumstances the term activation becomes precisely as useful as terms like 'personality', 'psychosis', or 'stimulus'. They are terms which we use when we wish to indicate a general field but when we have to specify with rigour we avoid them or go beyond them.

b) The Measurement of Activation

There has been no really systematic attempt made to measure activation. This is possibly because the concept arose out of physiological observations, at least in its final state, and therefore psychologists may have thought it presumptuous to define ways in which the dimension could be measured. The term 'define' is used deliberately, for while no attempt has been made to 'define' adequate measures of this dimension there has obviously been a tacit understanding that any phenomenon which was previously conceived of as a measure of emotion or drive could safely be regarded as a measure of activation. This has led to some remarkably untidy thinking.

The tendency among psychologists has been for individual investigators to assume that a given 'emotional' measure, the choice usually being quite arbitrary, is an adequate measure of activation and to come to their conclusions on the basis of this assumption. The normal procedure has been to select two groups on the basis of the favoured activation measure and to subject them to some readily measurable performance. Differences between the two groups would be noted and these differences would be attributed to the effect of different levels of activation. Very seldom would any effort be made to check whether the findings were confirmed by relating the performance to other supposed measures of activation.

The physiologists have tended to ignore the problem of measurement because their favoured method of studying variations in activation has been to study the performance of either specific neurological structures or total organisms before and after direct electrical stimulation of the non-specific reticular system, or associated centres.

There are many subtle variations on these two basic themes and several will be reported in the ensuing chapters, but basically the methods of investigation have been of the univariate type described above. It is true that many investigators, particularly those concerned with the study of stress and anxiety, have not specifically claimed to be investigating activation effects, but the tendency has been for later summarisers of this work to attribute the effects observed to variations in activation level (Duffy 1962, Feldman 1964a).

The general procedure of the activation theorists, as distinct from the experimentalists, has been to lump together, as measures of activation level, anything which purports to measure drive, emotion, anxiety, stress, motivation and even vigilance. These measures range from direct electrical manipulation of the reticular system to personality questionnaires. An exhaustive list would be excessively lengthy but the following measures have all been suggested, either directly or indirectly, as indices of activation level.

Basal metabolic rate (Duffy, 1962)

Muscle Tension (Malmo, 1957)

Electroencephalogram (Bindra, 1959)

Basal skin conductance (Malmo, 1959)

Skin temperature (Plutchick, 1956)

Galvanic skin response (Duffy, 1962)

Stress (induced or measured in various ways) (Duffy, 1962)

Manifest Anxiety Scale (Feldman, 1964a)

'N' scale of the Maudsley Personality Inventory (Furneaux, 1962)

Vigilance (Feldman, 1964a)

Situationally induced drive (Feldman, 1964a)

If all these measures represent a unitary dimension, even if they are not fully loaded by that factor or dimension, then one would expect a reasonable degree of positive intercorrelation between them. One would also expect to find a fairly consistent relationship between these measures and various behavioural patterns. As is obvious from a reading of any comprehensive review of the field (e.g. Duffy, *op.cit.*) neither of these two necessary criteria appear to be fulfilled and therefore one is forced to assume that these measures, if they are measuring anything at all, are not measuring the same thing. This is not to imply that the concept of activation is invalid. Such a dimension, adequately defined and adequately measured, might prove to exist. The objection is to the unwarranted extension of this concept to cover all the intensive aspects of behaviour, even when the evidence is strongly against such an extension.

The object of this thesis is, by studying the relationship between four of the measures listed above and certain behavioural tasks, to determine the degree to which a true activation dimension might be said to exist and to determine the relationship of this dimension, direct or indirect, with some other intensive aspects of behaviour.

The studies reported here can only be considered fumbling and tentative. The origins of the work can be traced back to a much narrower aim which included activation theory as an integral and apparently adequately formulated part. It was only when experimentation began and unexpected results were observed that it became obvious that a naive unidimensional theory, while it might appear as an attractive unifying concept, had little reality. It was clear that if the original work was to be adequately evaluated then a much more rigorous theory of behavioural intensity would

have to be formulated. This thesis can be regarded only as a very short step in the direction of such a formulation.

c) Outline of the Present Study

Basically this thesis consists of a report of nine experiments, carried out over a period of four years, and the conclusions drawn from these experiments. Each experiment investigates the relationship between a measure which has been claimed as an arousal index, and some behavioural performance. The arousal measures used were:

- i) Basal skin conductance.
- ii) Skin conductance change in response to specific stimuli (G.S.R.).
- iii) The Taylor Manifest Anxiety Scale
- iv) The 'N' scale of the Maudsley Personality Inventory.

The choice of these particular measures was conditioned partly by relevance - they represent a fair cross-section of reported activation indices - and partly by availability.

When this work was initiated the problem of measuring activation level was seen as secondary and relatively simple. Initially it was intended that a straightforward DC skin conductance recorder would be used. It soon became apparent that this apparatus was inadequate and a considerable amount of time and effort was spent in developing the apparatus outlined in Appendix I, based upon an idea outlined by Tolles and Carbery (1960), but going far beyond their limited device. When it became apparent, from initial results with this new apparatus, that the concept of a single activation dimension was probably invalid and that it would be more realistic to reconsider the question of activation in some detail, attempts were made to develop further electro-physiological measures, in particular

measurement of resting muscle potential. It soon became obvious, however, that the time which would have been involved in developing, from scratch, sophisticated devices for measuring resting muscle potential¹ would have left little time for the main part of the investigation. The use of E.E.G was considered, but as this does not differentiate among levels of alertness beyond the resting state characterised by alpha wave activity, it was considered, perhaps wrongly, that this measure would contribute comparatively little to the investigation.

Other physiological measures such as pulse rate and respiration, while easy to measure, seemed to be totally unrelated to any observable behaviour. This confirms Duffy's conclusion 'that measures of the E.E.G., of muscular tension, and of palmar skin conductance have, on the whole, shown more dependable relationships to stimulus situations and to overt behaviour'. (Duffy 1962, P.29). It was therefore decided, in view of limitations in time and resources, to use only basal skin conductance and skin conductance change (which are, as it turns out, two apparently independent measures) as physiological indices of what, for the present, we shall continue to call 'activation'. It was later decided to supplement these measures by questionnaires which have been widely reported as being measures of generalised drive or activation. The main reason for choosing questionnaires was their ready availability and relative ease of application. In the event the results obtained by using them were more interesting than the rather casual reasons for selecting them might have deserved.

Because of its relevance to the theoretical conclusions which are advanced later, a full description of these measures is included as chapter Two of this thesis.

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Some idea of the difficulties can be obtained in articles by Jacobson (1951) and Ryan et al.(1951).

The 'activation' measures were related to a wide range of performance measures. The following is a summary of the significant relationships which were observed. Many other relationships were observed as insignificant and these will be referred to in the text of Chapter Four.

- i) Skin conductance change and word recall.
- ii) Skin conductance change and sensory sensitivity.
- iii) Basal skin conductance and word recall.
- iv) Basal skin conductance and pursuit rotor performance.
- v) Basal skin conductance and rapid dotting.
- vi) Basal skin conductance and mean reaction time.
- vii) M.P.I. 'N' scale and term examination performance
- viii) M.P.I. 'N' scale and rote learning at rapid presentation rate.

The performance measures are quite diverse but this was a deliberate policy; the collection is not completely haphazard. The basic requirements for any task was that it should be easily and fairly rapidly administered, and that scoring should be purely objective. The only task in the above list which does not conform to these criteria is the term examination. While the marks were readily available, it is doubtful if the scoring was any more objective than in other essay type examinations.

Another less rigidly adhered to criterion was that the performance tasks should have been used by other investigators in similar contexts. This was considered desirable for the apparently obvious reason that the more that is known about a particular type of performance, the more sense can be made of any observations pertaining to it. In the event the need to account for relationships observed by other investigators made the interpretation of much of the data more, rather than less, difficult. In

some instances the evidence appeared to be contradictory and, to some extent, it was the observation of these apparent contradictions which led to the realisation that a more rigorous study of activation theory was required.

In two instances the performance tasks had no obvious antecedents in the context of this type of study. The rapid dotting task, which is derived from a dotting test attributed to McDougall and is described in some detail in chapter Three, has not, so far as is known, been used previously in this type of investigation. Observation of the relationship between skin conductance change and the later retention of words has been carried out by Lanier (1941), but this study did not attempt to relate the response of each individual word to the later retention of that word as was done in the study to be described in this thesis.

A full description of the behavioural tasks will be included in the relevant section of chapter Three.

d) Summary and Conclusion to Introduction

The intention of this Introduction has been to set the context for the experiments and theoretical conclusions which form the main body of this thesis.

By now it should be obvious that the work to be reported here was not carried out in support of a theoretical position which was clearly conceived from the beginning. Instead the theoretical position of the author has shifted as the evidence accumulated, until the conceptions which led to the initiation of this work are now seen to be over-simplifications which are totally inadequate to explain the reported results.

In terms of the intentions implied by its title this work is far from adequate. Unidimensional activation theory is obviously unsatisfactory and an attempt to cope with the blatant anomalies is long overdue, but the scope of the thesis falls far short of the ideal required for a realistic attack on the problem. A comprehensive study, however, would have required time and resources well beyond those which were available to the author. In view of this it is legitimate to question the value of the study as it stands. Does it, in view of its limitations, contribute anything to the understanding of this complex field? Inevitably such a question, from the author's point of view, is rhetorical, in that an unqualified negative would discourage the reader from going any further and would make pointless all that follows. Equally an over-extravagant affirmative would be rightly regarded with suspicion. The fairest answer is probably that the study contributes something to our understanding of this field in two limited respects. The first is that it emphasises the discrepancies, implicit in many other studies, by presenting these discrepancies in the context of a relatively unified set of experiments. In other words it shows up directly the lack of correspondence between various presumed measures of activation. The second is that by emphasising the two (or possibly multi-) dimensional nature of activation it enables more sophisticated questions to be asked about the relationship between the 'intensive' and 'directional' (Duffy, 1962, P.5) aspects of behaviour. More explicitly, it frees the experimenter in this field from the intellectual straight jacket of having to interpret his results on the basis of the naive combination of unidimensional activation theory and the Yerkes - Dodson Law (Yerkes and Dodson, 1908), which has tended to dominate explanations in this field for so long.

It could be said, therefore, that this study allows for more intelligent questions even if it does not answer them. This limitation does not

prevent the author from suggesting some tentative answers, particularly in the paper presented in Appendix II and in the final chapter, but the value of the study should not be judged by these alone.

Lest the general tone and content of this introduction appear purely critical and destructive, one fact must be re-stated and emphasised. The pioneers of activation theory, Freeman, Duffy, Lindsley and others, are not regarded by the author as misguided incompetents. Far from it. Their contribution to the development of an adequate theory to explain the intensive aspects of human behaviour has been of immeasurable value. They have provided a sane theoretical framework which has served to bring together many diverse aspects of human behaviour. They have made a comprehensive theory of human performance at least a possibility. The fact that this framework must now be regarded as inadequate does not mean that their achievement should be belittled. The present study, and many others which will undoubtedly be much more comprehensive and competent, are being and will be developed upon the basis of their very considerable insights. Without the starting point of general activation theory this study, ^{what} for it is worth, would never have come into being.

One final point must be made. Whatever value this work has depends upon two factors. The adequacy of the experimental work and the validity of the contention that the measures used are indices of independent dimensions. Given these two factors the basic conclusion of the theory will be established; namely that activation theory as it is conventionally presented is in need of drastic revision, and that the revision should tend towards presenting a multi-dimensional structure. This thesis in no way represents an attempt to accomplish that revision.

From time to time certain theoretical speculations are advanced and the final chapter is mainly theoretical, but these speculations are no more than vague gropings towards possibilities of revision or reinterpretation. They may have some value if they can lead to the questioning of conventional lines of thought, but this is almost certainly an over-evaluation and they can probably best be tolerated if they are thought of as symptoms of the besetting occupational hazard of academic psychologists; the desire to build elaborate theories upon the most meagre evidence.

The author does not share the Skinnerian distaste for speculative theorising. If activation theory is to continue to have any value it will certainly be necessary to present a revised version of it in the future, and this will involve considerable speculation. But at present more evidence, probably collected with a multi-dimensional orientation, is necessary before a truly adequate revised or elaborated theory can be evolved. It may be that some of the speculations advanced in the succeeding pages will be established within the evolved theory; it is far more likely that they will, in the long run, prove valueless.

II. THE MEASURES USED

a) Psychological and Physiological Interpretations of Skin Conductance Phenomena.

The attempts made by psychologists to understand and explain the phenomena associated with skin conductance are, more than anything else, a painful example of the semantic confusion which characterised discussion of emotion and motivation before the general acceptance of the concept of activation. The activation concept may be something less than a theory but, in so far as it unifies thought and terminology, even to the point of over-simplification, it is an improvement on what went before. But even among all the verbal confusion of the earlier attempts at explanation there are interpretations which are relevant to contemporary understanding. This is particularly true where we can discern through the semantic tangle an attempt to comprehend the phenomena in terms which transcended the then conventional psychological categories.

More attention is paid in this section to the physiological basis of skin conductance phenomena than is normal in psychological work involving these measures. This was necessary as they are being presented as more or less adequate measures of activation. To have simply presented these phenomena as activation measures without specifying good reason would leave the author in little better state than the investigators who are criticised in the introductory chapter for just such arbitrary decisions.

i) The Psychological Correlates of Skin Conductance Change

Two articles which appeared in the British Journal of Psychology in 1924 and 1925 pinpoint the difficulties which arose through verbal obscurity and adherence to conventional categories. Wells (1924) argued, from a study of introspective reports, that changes in skin conductance accompany 'dynamic direction of attention'. He emphasised the conative at the expense of the emotional significance of the changes. Wechsler (1925) replying to Wells' paper, pointed out that many emotional reactions are subconscious and denied that an introspective assessment of feeling concurrent with skin conductance change could be valid. And so we had a direct confrontation and the implication that conative and emotional correlates are mutually exclusive. Brown (1925) was not quite so specific in that he was willing to assume both conative and emotional correlates of conductance change, but he was still labouring under the terminological disadvantage of having to distinguish between 'will' and emotion. Both might correlate with conductance change but both are, he argued, essentially distinct. Bartlett (1927) continued the argument initiated by Wells and Wechsler, coming down on the side of a conative interpretation. He maintained that skin conductance change indicates expectancy and is also found after satisfactory task completion.

The first break in this potentially endless semantic confrontation is found in a paper by Cattell (1929). In a long article characterised by much of the terminological confusion referred to above he concludes, among other things, that the size of skin conductance change might be proportional to 'instinctive energy arousal'. The phrase is confusing and the term 'instinctive' is used in a way which would now be inadmissible. Nevertheless it is indicative that the conventional categories of emotion,

conation, and will must be superseded if sense is to be made of skin conductance phenomena.

In a series of articles Landis (1930, 1932) and Landis and Hunt (1935) criticise the apparently futile attempts to equate skin conductance change with the conventional psychological categories. The 1932 article is an excellent review of work done to investigate the skin conductance phenomena up to 1931 and it laid the foundation for the 1935 article in which no outstanding correlation was found between skin conductance change and any specific psychological state. What was observed was a tendency for the response to occur under any conditions which we might now class as activating. This paper must be classed as one of the earliest expressions of an arousal theory, even although the now familiar terms 'arousal' and 'activation' were not used.

Darrow (1936) emphasises the point of view advanced by Landis and Hunt (op.cit.). He maintained that skin conductance change will occur in response to a wide variety of situations demanding action. He considers the secretion of sweat on the palms of the hands, which is to some extent associated with conductance change (see section on physiological interpretations of skin conductance change), to have a survival function and that skin conductance change is one aspect of a total response to activating conditions. In a later article Darrow (1937) maintains that the speed and size of the conductance response is a function of available energy. This is reminiscent of the 'instinctive energy arousal' of Cattell (op.cit.) and precedes the work of Duffy (op.cit.) which led to her conception of activation as being 'the degree of energy release within the organism' (Duffy 'Activation and Behavior' 1962). The term 'energy' is generally

used in activation theory, particularly by psychologists, without adequate specification and to some extent we can see that with Darrow's article we have extricated ourselves from one terminological muddle only to begin blundering right into another. This problem will be returned to when we come to discuss the general theory of arousal in the final chapter.

Despite the criticisms of attempts to associate skin conductance change with mentalistic categories, such studies still continued to be published. Shock and Coombes (1937) found that conductance changes were longer in response to unpleasant than to pleasant odours while Mason (1941), maintaining a more conative orientation, claimed that conductance change is associated with dilemma, surprise, and loss of meaning situations. These studies hardly contradict the activationist approach of Landis and Hunt and Darrow but they are indicative of the lag in acceptance of the idea that somehow the traditional categories are no longer very useful. But the activationist position continued to gain ground. Freeman and Katzoff (1942) carried out a factor analysis of skin conductance change and self ratings and found that one of the most important variables associated with conductance change is 'arousal to stimulation'. Later studies which emphasised the same factor are those by Clausen et al. (1955) who found a very marked relationship between skin conductance change and reported pain thresholds, by Novak et al. (1956) who found that responses to any stimulus after a period described by the subjects as boring will be large, no matter what the subjective interpretation of the stimulus might be, and by Martin (1960a) who found more rapid adaptation of the skin conductance response to repeated stimuli under the influence of depressant drugs.

The studies reported here are representative rather than comprehensive. They demonstrate a change from interpretation in terms of pre-determined categories to interpretation in terms of a new category determined by experimental observation of the response. The status and usefulness of this new category of activation or arousal will be considered in more general terms in the final section devoted to a reappraisal of general activation theory. Arousal theory grew out of a wider range of studies than those devoted only to skin conductance change, but obviously such studies formed a major component in the development of the arousal concept and this interdependence implies that any study of skin conductance change must be conducted within the general context of arousal theory and, conversely, that any new understanding of skin conductance change might have some effect upon our understanding of that theory.

ii) Individual Differences in Skin Conductance Change

Much of the original work reported in this thesis is concerned with individual differences in susceptibility to skin conductance change and in basal conductance level. In this brief section will be reviewed some of the comparatively small number of studies which have sought to explore the relationship between mean size of skin conductance change and individual characteristics. These studies appear to be less fraught with semantic confusions than do those which were concerned with the psychological nature of skin conductance change, but this may only be because our descriptive categories relevant to personality have not yet been superseded by new ways of thinking.

Syz (1926) found an apparent relationship between frequency of skin conductance change and 'stability of character' as assessed by self rating.

This finding seems to make little theoretical sense. It is included mainly as an illustration of the tendency prevalent in many such studies to observe direct relationships between the physiological phenomenon and specific psychological categories, the implication being that the categories must be as 'real' as the directly observable phenomenon; or even that the categories are more 'real' in that there is tacit in many of these studies the idea that an observed relationship would in some way explain the physiological phenomenon. In a paper already cited Cattell (1929) found a high rank correlation between ratings of 'force of character' and mean size of conductance change in a standard situation. The same remarks can be applied to this finding as were applied to that by Syz. Again no theoretical meaning can be derived from this study because the psychological category has little or no meaning in itself. The best that one can say for this sort of study is that the skin conductance changes relate to some vaguely observed or felt behavioural manifestations which were, in the cases cited, labelled 'stability' and 'force'. Put in this way it is obvious that such studies are valueless; they tell us nothing about the rating or about the phenomenon of skin conductance change.

Jones (1935) carried out a more useful study of the relationship between overt emotional response and skin conductance change. Obviously he was bedevilled by the idea of a necessary relationship between 'emotion' and conductance change, but allowing for this probably inevitably narrow view the study is of some interest. Starting from the observation, one of the most intriguing and challenging in this field, that there is a wide range of individual variability in the size of skin conductance response under standard conditions, he reviewed studies which seemed to show that overt emotional response is associated with small conductance changes and

vice versa. The conclusion which might be drawn from this is that individuals who consciously or subconsciously inhibit overt emotional expression will show more autonomic activity. In other words emotion must be expressed in some way, and if the expression is not overt it will be covert. Jones did not entirely confirm this. He found subjects conforming to the predicted pattern but others who showed a high degree of overt and autonomic response. The conclusions to be drawn from this study are scarcely startling but the approach is one which betrays a greater interest in the response of conductance change as an expression of dynamic, on-going activity, and not merely as something which can be explained in terms of some verbal category.

Jurko et al. (1952) in an unfortunately titled paper 'The pathology of the energy system' show the persistence of the approach exemplified seventeen years earlier by Jones. In a study of normal, neurotic and schizophrenic subjects they found notable variations in the form of 'energy mobilisation'. (Again the term 'energy' is used and one must view it with the same reservations expressed earlier (see p.16)). They found that normal subjects tend to express energy mobilisation mainly through internal or autonomic functions as measured by skin conductance change, while, at the opposite extreme, schizophrenic subjects express mobilisation mainly overtly. The neurotic subjects show a marked degree of both overt and internal mobilisation. This seems to confirm Jones's findings if we assume that he had some relatively neurotic subjects in his sample. In this paper by Jurko and his collaborators we find a self-conscious attempt to avoid any conventional categories, such as emotion, combined once more with an interest in the conductance response as part of the individual's total activity. A similar tendency can be observed in a paper by Welch

and Kubis (1947) where it is reported that anxious patients learn a conditioned skin conductance change more rapidly than normal subjects and tend to retain it longer. Admittedly this is basically intended to be a study of anxiety and learning, but one could conclude that what this finding indicates is a greater tendency for anxious subjects to display skin conductance changes in response to any stimuli. In other words skin conductance change plays a more prominent part in the activity of anxious individuals.

This brief summary throws little light upon the real nature of the individual differences in the size and frequency of skin conductance changes beyond illustrating a tendency to move towards interpreting such studies within an activationist viewpoint. This parallels the tendency found in studies designed to explore the nature of the response. Despite this tendency, however, one can note a reluctance to place the major emphasis on explanation in terms of conductance change. Changes in conductance are observed but true explanation is attempted in terms of the psychological or psychiatric categories. The categories remain and the conductance phenomenon must be made to conform to them. To some extent this is reasonable but it does indicate a reluctance to place full trust in objective observation and a desire to maintain categories which are somewhat vaguely derived.

iii) Psychological Interpretations of Basal Skin Conductance

So far we have considered interpretations of the phenomenon of skin conductance change in response to fairly specific, short term, stimulus conditions. These changes do take place, obviously, from some basal level, and this basal level will vary considerably from one individual to

another and will show definite fluctuations in the same individual during the course of an experimental session or during the course of a normal day.

Somewhat surprisingly the attempted interpretations of the significance of basal skin conductance have involved a much readier acceptance of what can only be called an activation hypothesis. Some terminological confusion does exist in early studies but it is of the same nature as that being currently perpetrated.

Thouless (1925) in an electronic and psychological study of the phenomenon maintained that change in basal conductance is related to the mental alertness of the individual. High conductance indicates a preparedness to react, while low conductance indicates the opposite. This is the sort of conclusion that is still being stated and it is central to the contemporary views on activation which assume that the basal conductance of the skin is an index of activation. Cattell (1928) stated that high conductance levels indicated a greater conscious flow of energy. This is almost identical with Duffy's (1962) conception of the activation dimension being related to energy release (see p.15) and it goes beyond the Thouless formulation in that it widens the scope of the concept. But yet again the term 'energy' is introduced without specification and once more we are faced with possible semantic confusion.

Farmer and Chambers (1925) observed a marked fall in conductance during sleep, while Estabrooks (1930) found that most subjects show a marked fall in basal conductance level as they undergo hypnosis. If we take a rather naive view of hypnotic state as being a form of sleep, this relates quite well with the Farmer and Chambers' study and with the work of Riesen (1942) who observed a fall in the basal conductance level of

infant chimpanzees during sleep, though at the onset of sleep there is a lag in the conductance change prior to stabilisation. These studies relate conductance levels to readily observable psychological states and confirm the idea advanced by Thouless (op.cit.) that basal conductance level relates to alertness. This is further confirmed by White (1930) who found that conductance levels rose under conditions of muscular tension, which might be considered as a correlated measure of alertness, and mental effort produced by doing problems in mental arithmetic.

Darrow (1936) carried the interpretation rather farther. He maintained that basal conductance levels vary with the alertness of the individual, that high conductance will tend to be associated with improved performance, but that there might be impairment of performance with too high a level of conductance. This reference relates to a wide range of studies which imply an inverted U-shaped relationship between activation level and arousal. Much of this work was carried out using muscle tension as an index of activation but in view of Freeman and Simpson's (1938) reported finding of a close relationship between induced muscular tension and basal skin conductance, it is probably legitimate to introduce it here.¹

In one of the earliest studies relating muscle tension to performance Freeman (1933) found that increased muscle tension appeared to facilitate speed but to have a decremental effect upon accuracy and that high levels of induced tension seem to have a disruptive effect upon fine sensory-motor tasks. Stauffacher (1937) expressed this more clearly by stating that there is an inverted U-shaped relationship between activation and performance. He based this conclusion upon a study relating nonsense syllable learning to muscle tension. Freeman (1938) extended this concept by

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One does so with reservations which should become clear in the final chapter of the thesis.

finding that the optimal level of muscle tension would vary for different tasks. He found that the more habitual or repetitive a task the higher would be the optimal level of muscular tension. This hypothesis relating muscular tension to performance was extended to include specifically skin conductance levels by Freeman (1940) who found that there would be improvement in performance up to an optimum level of conductance followed by deterioration at higher levels. This relationship of muscular tension and skin conductance to performance level was confirmed in a paper covering both measures by Sternmett (1957).

Thus the 'flow of energy' of Cattell (op.cit.) is seen to relate to behaviour in a fairly specific way. But whether we call the overall phenomenon, of which basal skin conductance is a manifestation, 'flow of energy', activation, or arousal, does not greatly add to our understanding. The observed relationship between arousal level and performance is interesting but it has never been expressed in more than vague verbal terms and is, in fact, no more than one would conclude from a casual observation of people in stressful situations.

An even more serious limitation which results from considering both conductance change and basal conductance level as essentially activation phenomena is that the development of arousal theory is too crude to allow it to cope with some obvious anomalies which arise when we accept this point of view. These will be returned to in the later stages of this thesis, when certain revisions of the currently acceptable arousal theory will be suggested. All that might be said at this stage, by way of summary, is that, while it seems reasonable, and a definite advance over earlier semantic tangles, to consider skin conductance phenomena in terms of arousal theory, this theory, as it currently stands, is too simple-minded to encompass effectively the range of phenomena which it purports to explain.

iv) The Physiological Nature of Skin Conductance Change

It would be useful to have some knowledge of the central mediation of skin conductance changes. Most psychologists have evolved an idea of the nature of the phenomena purely from observation of changes in readily identifiable psychological states. They have been remarkably uncurious about the central mechanisms involved, and while this may have been initially due to the fact that physiological evidence was not readily available, that excuse can hardly be used since the publication of Wang's (1957, 1958) definitive review of physiological work on the 'Galvanic Skin Reflex'.

In the context of arousal theory the need for understanding the underlying mediation of the phenomena should be particularly obvious. These phenomena are used as two¹ of the main indices of arousal level. Arousal theory is built upon the observation of a general activating system in the mid-brain and it would seem inevitable that the more we know about the central mediation of the basic indices of arousal the more we will understand the basic nature of human activation. And yet remarkably little has been done to further this study. Duffy's (1962) review of activation, which is one of the most comprehensive accounts of the subject written by a psychologist, contains one sentence on Wang's papers in which she writes: 'Wang concludes that the indispensable central neural structures for the galvanic skin response are to be found in the brain stem'. (Duffy, op.cit. page 26). This merely misuses Wang's findings to maintain ~~his~~ general theory of a unitary activation dimension mediated by the brain stem reticular formation.

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I refer, of course, to basal skin conductance and to skin conductance changes which many writers mistakenly identify as being the same.

Wang's paper merits much greater attention from psychologists than it has hitherto been accorded. It refers specifically to what he calls the 'galvanic skin reflex' and what has so far been referred to in this paper as a short term change in skin conductance. This latter term is appropriate only if we consider such short term changes to be simply transient variations in a constantly fluctuating level of skin conductance. The results and theoretical interpretations contained in this paper tend to indicate that this interpretation of short term changes is inappropriate and so Wang's term, abbreviated to the convenient shorthand G.S.R., will hereafter be used, although the word 'reflex' might be more meaningfully replaced by the word 'response'. Unfortunately, in the context of a review and summary of his paper, Wang uses the term 'response' to refer to changes in skin resistance or potential produced by direct stimulation of sympathetic neurons, and defines the term 'reflex' as something which is 'called forth by excitation of either a sense organ or a sensory nerve'. This is a rather more general definition than psychologists are commonly accustomed to. The present author would prefer the term 'galvanic skin response', but as the abbreviation is the same, this will be used, and specification of whether the R stands for reflex or response will be made only where Wang's use of 'response' is intended.

It might seem strange to base one's understanding of the physiological nature of the G.S.R. on a single, two-part paper, but this is dictated by the fact that no other published work, based upon recent neurophysiological evidence, exists. The paper, published in two parts, is 48 pages long, highly condensed, and lists 138 references. In its attempts to determine a relationship between central processes and the G.S.R., it is unique.

The paper may be considered in three parts. i) the dermatological basis of the G.S.R. ii) its peripheral innervation. iii) control by central neural structures. The first two aspects are fairly simple and can be briefly summarised. The question of central control is much more complex and, as it is fundamental to the thesis being presented here, it must be considered in more detail.

Wang's own summary can be quoted as an indication of his conclusions regarding the dermatological basis of the G.S.R. He states that the G.S.R. is 'due to the temporary breakdown of the semi-permeability of cell membranes - most probably of the cells in the sweat glands - in response to physiologic stimulation'. As for the final common path in the spinal cord, Wang states that these are 'the pre-ganglionic sympathetic sudomotor neurons', while the galvanic skin response, using Wang's terminology, is elicited by direct electrical stimulation of the post-ganglionic sympathetic sudomotor fibres. He rejects the hypothesis, put forward by Darrow (1936), among others, that the G.S.R. is in any way affected by parasympathetic activity.

The G.S.R.¹ increases step by step proportionately to the strength of stimulation up to a certain maximum. If stimulation is increased beyond this point there is no further increase in the size of response. This indicates that sweat glands are operating on the all-or-nothing principle and that the size of response is a function of the number of units operating in response to a particular stimulus.

This is a fairly clear and unequivocal account of the peripheral activity involved in the G.S.R. When, however, we turn to the problem of central control, the situation is very much more confusing. One point

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Response.

should be borne in mind throughout the ensuing discussion. Wang's references to the stimuli used to evoke the response are sparse. He states in his summary that 'any painful or startling stimulus evokes the galvanic skin reflex' and in section VB of his paper he asserts that direct electrical stimulation of a severed cutaneous nerve is a 'highly artificial way of evoking the galvanic skin reflex', but beyond reference to this method, he does not make it clear how the G.S.R. was evoked in his own or other studies. Judging by the rather crude recording apparatus which he describes in an appendix to his paper, to have recorded responses at all, he must have used very strong cutaneous stimulation in his own studies, and one must assume that it was noxious enough to avoid complications due to spinal habituation. (Hernandez-Peon & Brust - Carmona, 1961). This sort of factor,¹ which will be considered later in a more general way, adds further complications to our attempt to understand the G.S.R. in normal situations, but the assumption, which is almost certainly justified, that it played no part in the studies reported by Wang, will permit us to proceed to consider the role of facilitatory and inhibitory systems acting upon G.S.R.'s which occur in response to afferent stimuli reaching the supra-spinal areas of the central nervous system. This is, in itself, quite complex and should be clarified as far as possible before considering the problem of stimulus reception.

Wang lists three facilitatory areas, the sensorimotor area of the cerebral cortex, the hypothalamus of the inter-brain, and the facilitatory reticular system in the inter-brain and rostral mid-brain. He lists four areas associated with inhibition of the G.S.R.; the frontal lobe of the cerebral cortex, the caudate nucleus, the anterior lobe of the cerebellum,

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i.e. peripheral or spinal inhibition of sensory input.

and the ventromedial reticular formation of the hindbrain. The functional inter-relationship between these areas must be considered before we can attempt to understand the means of central control.

In the intact organism we might regard the cortex as being the primary centre for inhibition of the G.S.R., at least to potentially meaningful stimuli. This conclusion is based upon Wang's finding that in neo-decorticate cats the G.S.R. increases markedly and that this increased G.S.R. is very resistant to habituation. This indicates that the G.S.R. can be initiated through the facilitatory reticular system, alone or in combination with the hypothalamus, and that inhibition of the response is only achieved through the ventromedial reticular formation when this is activated by the frontal lobes acting through the caudate nucleus. The specific function of the caudate nucleus is unclear, but it does appear to act as an integrating centre for inhibitory fibres from the frontal and sensori-motor cortex. In this sense its role is little more than that of a relay station. The part played by the cerebellum in the inhibitory process is possibly even less clear, but Wang advances the tenable hypothesis that it may act as a reverberating circuit store and that this function, acting through two-way connection to the frontal lobe, may have the effect of prolonging cortical inhibition beyond the period of immediate stimulation. Wang's insistence that the ventromedial reticular formation is more than 'merely a handmaiden of the frontal cerebral cortex, the caudate nucleus, and the cerebellar anterior lobe' is rather strange, as he bases this particular conclusion upon observations of rhombencephalic cats where only the ventromedial reticular formation of the hindbrain and the anterior lobe of the cerebellum remain as areas with a demonstrably inhibitory effect upon the G.S.R. As we have seen

above, Wang argues later in his paper that the cerebellum may well act in conjunction with, and possibly through, the frontal lobe, and it is therefore not surprising that ablation of the cerebellum should have no effect upon the complete inhibition of the G.S.R. which is found in this preparation. As the only suprasegmental structure, either excitatory or inhibitory, remaining, it is not necessarily surprising that, freed from cortical control, the ventromedial reticular formation should exert a continuing inhibitory control on the G.S.R. It might be that, freed from external control, it 'locks-on' to an inhibitory mode.

The above discussion of inhibitory processes leads to the obvious implication that the use of the term 'facilitatory areas' in relation to the G.S.R. is false. This term suggests that the G.S.R. can in some way be centrally enhanced as well as inhibited, and that there is therefore some general mean level for the response (or reflex) if it were to be freed from both facilitatory or inhibitory control. That this is not so is illustrated by the fact that in acute spinal preparations the galvanic skin response (in Wang's terminology) is exhibited in an exaggerated form. This indicates that the so-called 'central facilitatory areas' are in fact areas where the G.S.R. can be initiated by direct stimulation but not areas where the naturally arising response can be 'facilitated'. What part, then, do these areas play in the G.S.R.? What is their true significance? The answer to these questions must necessarily be indefinite and somewhat speculative, but it can be attempted, at least as far as the role of the facilitatory reticular system is concerned.

The details of the answer are contained in the discussion section of a paper by Corteen and Blackman (1965) (see Appendix II). This discussion is entirely the work of the author of this thesis and therefore it seems legitimate to include a summary of it at this point.

The G.S.R. 'is assumed to be a measure of the activity in a sensitising (or selection) complex partly situated in the upper reticular system. Stimuli will tend always to activate this complex, but there will normally be varying degrees of corticifugal inhibition of this activity which will be dependent upon some as yet obscure cortical evaluation of the activating stimulus. Activity in this sensitising area (which may be thought of as a selective filter) will result in certain functional peripheral changes and also facilitation (either selective or general; this is at present not clear) of activity in the non-specific thalamic system which will result in improved sensory discrimination. There is probably some relationship between this mechanism and general activation level, but the relationship is certainly complex and non-linear.

This summary implies that the G.S.R. is inevitably initiated by incoming stimuli unless there is positive inhibition. It makes no mention of the possibility of initiation sometimes being cortical, and yet Wang's reference to the facilitatory influence of the sensori-motor area of the cortex indicates that cortical initiation can occur. In certain situations, where the subject is orientated towards external stimuli, it is quite rare. In a series of five experiments involving some 200 subjects the author observed only seven G.S.R.'s which were not obviously attributable to specific external stimuli. But, if one may be permitted some speculative introspection, it is possible to conceive of situations where a G.S.R. might occur without any obvious stimulation; such a situation as suddenly realising that one has forgotten an important appointment, etc. This, however, is to speculate beyond the evidence to an extent which is hardly justifiable.

The problem of peripheral inhibition of input has been mentioned. This is very well dealt with by Livingston (1958) among others and is of undoubted importance to an understanding of any hypothetical sensitising system. It presents certain quite specific problems for the theory advanced here, particularly as it requires consideration of mechanisms of selective attention. In other words relevant stimuli will both avoid being inhibited peripherally and will tend to receive larger 'weightings' from the central control system. Non-relevant stimuli will have to be of considerable import before they will avoid either peripheral or central inhibition.

Obviously such a mechanism of selective peripheral inhibition will be of considerable value if only in regard to the considerable economies it will confer upon the inhibitory operation of any central sensitising system. The interaction between central and peripheral inhibition and the hypothesised sensitising centre must inevitably be very complex and it would be premature at this stage to attempt any full exposition. The knowledge is simply not available.

v) The Physiological Basis of Basal Skin Conductance

So far we have considered only the problem of the central mediation of the G.S.R.; that is short term changes in skin conductance. The question of the mediation of long term conductance levels must now be considered. Obviously we cannot appeal to the same or similar central mechanisms. The processes just described clearly refer to what Wang calls a reflex. They would be inappropriate as mechanisms underlying fairly long term phenomena.

Unfortunately no direct study of basal conductance equivalent to Wang's study of the G.S.R. has been carried out, and therefore we must appeal to more indirect evidence. Many workers have observed a fall in basal conductance with the onset of sleep (Richter, 1926; Kleitman, 1939; Riesen, 1942) and fluctuation during both sleeping and waking which seem to reflect the alertness of the individual (Darrow & Freeman, 1934; Levy et al. 1958; Waller, 1919; Wechsler, 1925). To some extent the psychological implications of these observations have already been discussed (p. 21). What we are here concerned with is the physiological basis of the fluctuation, and this might best be approached through a consideration of the physiological basis of sleep, which has been extensively studied. The justification for this approach could be questioned on the basis that sleep, or at least its onset, is a unique event in the activation continuum, and that the physiological mechanisms underlying long term fluctuations in conductance cannot be understood fully through studying the changes which occur during this unique event or period. There may be some justification in such a criticism but it cannot be denied that sleep, in the normal sense of the word, must be assumed to represent the lowest activation level which most of us normally experience. It tends to recur at regular intervals and could usefully and fairly be regarded as the low point of a diurnal cycle of activation.

Physiologists have advanced two basically contradictory hypotheses to explain sleep. Hess (1954) and others, e.g. Akert (1961), have advanced theories which imply a sleep centre with a positive action causing sleep, while Bremer (1954) and Oswald (1962), among others, favour a hypothesis which explains sleep in terms of blocking of the arousal system

in the reticular formation. While no final conclusion can be drawn regarding the validity of either theory, it seems likely that some rather complex form of hormonal balance is involved (Brodie & Shore, 1957; Lewis, 1965) leading to a waxing and waning of sympathetic activity, among other effects, which would result in concomitant variations in basal skin conductance. It may be that the central areas involved are located in the hypothalamus with connections to the limbic system and reticular activation system, but there is evidence which makes any categorical statement on this point dangerous. For example Rossi (1963) implies that the reticular formation is the important structure when he says "Reticular neurones having sleep inducing functions and reticular neurones of the activating system seem to be intermingled at the same level of the brain stem. A continuous tonic activity is probably going on in both types of neurones. Therefore, sleep and wakefulness would result from the competition between them".

This does not contradict the Brodie and Shore hypothesis but it does shift the main emphasis from the hypothalamus to the reticular system.

Brodie & Shore (op.cit.) use the terms 'sympathetic' and 'parasympathetic' to define the two opposing systems or centres which, they hypothesise, are operative in fluctuations of arousal. Hess (1964) is careful to avoid those terms when defining his functionally similar systems and uses instead 'ergotropic' and 'trophotropic' to define the centres controlling action and rest respectively. While allowing some considerable degree of identification between this division and the generally accepted division of the autonomic nervous system, he is careful to point out that the identification is not complete. In fact he goes

so far as to say that, in anxious states '.... an increase in perspiration ... obviously ... belong(s) to a trophotropic function that is directed towards a decrease in tension' (Hess, 1964, p.72). Thus he describes a sympathetically innervated function as being part of the trophotropic system. This seems a doubtful conclusion and one might more reasonably conclude with Darrow (1936), that palmar sweating is a response which occurs preparatory to action and is therefore concomitant with dominance of the ergotropic system.

This account of the physiological basis of long term fluctuations in skin conductance level is admittedly very indirect and imprecise. It is permissible to advance only a tentative summarising hypothesis which is that basal conductance level is a reflection, at any given time, of the balance between, to use Hess's terminology, the ergotropic and trophotropic systems. It seems that these systems are under fairly complex hormonal control, the details of which are at present imperfectly understood.

b) The Scales of Anxiety and Neuroticism

The amount of research work relevant to these scales, particularly to the Manifest Anxiety Scale, is immense and all that is intended in this section is the presentation of a summary of this evidence, with particular reference to the functional significance of the scales. What is particularly in question in this thesis is their use as indices of activation level and it is this aspect which must receive specific emphasis.

i) The M.A.S. (Manifest Anxiety Scale - Taylor, (1953))

Work on the M.A.S. up to 1960 is fully reviewed by Dahlstrom and Welsh (1960). This scale consists of fifty items selected from about 200 M.M.P.I. items which might be thought indicative of anxiety. Selection was by five judges and an 80% criterion of agreement was used as a basis for selection. The actual scale used, together with the M.P.I. (see below) forms Appendix III of this thesis.

The relationship of the scale to clinically diagnosed neuroticism and anxiety has been extensively investigated. Dahlstrom and Welsh (op.cit.) cite 14 studies¹ and the general conclusion which can be drawn from them is that the M.A.S. will differentiate groups of neurotics from groups of normals in terms of significant mean differences between the groups, but that there is always considerable overlap. Whether this indicates a deficiency in the scale, in the clinical diagnosis, or both, is not clear. The scale is less successful in differentiating between clinically diagnosed anxiety neurotics and neurotics with other diagnoses.

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The original articles were referred to by the author but they add nothing of significance to the understanding of the M.A.S. as a measure of arousal and so are not considered in any detail.

It might therefore be concluded that the scale is more a measure of general neuroticism than of anxiety alone. This is partially confirmed by the high correlation with the neuroticism scale of the Maudsley Personality Inventory (see section d) below).

The main reason for the development of the M.A.S. was the need for a measure of anxiety for use in learning theory experiments. It has been extensively used by Spence and his co-workers as a measure of potential emotionality which they assume contributes to drive level. (see Spence, 1959, for an adequate summary). In some studies they simply equate the M.A.S. score with drive level (Spence et al., 1956). The results of the studies based upon this theoretical position have been far from unequivocal. It appears that under certain conditions high scoring subjects will show faster rates of eyeblink conditioning than low scoring subjects, and will also show greater resistance to extinction. (Spence and Taylor, 1953; Baron and Connor, 1960). This might indicate a relationship between M.A.S. scores and drive level, but any such interpretation must be considered carefully as it would only hold good if we accept Spence's theoretical definition of drive and its relationship to conditioning. Also there has been a large enough number of negative findings in studies relating M.A.S. scores to conditioning rate (King et al., 1961; Klinger and Prokasy 1962) to make any such interpretation very tentative.

Another general finding is that high M.A.S. scores will tend to be associated with poorer performance when the task involves a large number of competing responses, particularly when competing, incorrect responses tend to be stronger than the correct response (Spence, 1958). The assumption therefore, that M.A.S. scores are an index of drive, again

depends upon the acceptance of Spence's definition of drive. But even accepting this definition,¹ it is by no means obvious from the reported findings, namely that high anxious subjects perform better than low anxious subjects on simple tasks but tend to perform worse on complex, difficult tasks, that M.A.S. scores are an index of activation. Duffy (1962), however, reporting some of these findings, is prepared to state, 'it may be tentatively held that the anxious group was a group functioning at a very high level of activation'. This again exemplifies her tendency to jam everything into the rag-bag of an imperfectly formulated activation theory.

In view of the large number of contradictory studies which have been reported concerning the relationship between M.A.S. and various learning and performance tasks one is perhaps justified in accepting the conclusion of Bendig and Vaughan (1957) that the relationship is at best an ephemeral one. In view of the results to be reported in the next chapter (see particularly experiments ic and iic) it can probably be said that the measurement of chronic anxiety and/or neuroticism is useful in that it seems to define an intensive dimension which is related to performance in its own right, but that the M.A.S. is a relatively poor measure of this dimension. This seems to be confirmed by the factor analytic study of O'Connor et al. (1956) who found that the M.A.S. consists of five factors, namely chronic anxiety, physiological reactivity, sleep disturbance, sense of personal inadequacy, and motor tension. Several of these factors may have the same or similar sorts of relationships with performance, but it is unlikely that all of them do.

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Such an acceptance commits one only to a position where one might say that Spence's D is different to, say, Hull's D or Estes' D. It does not commit one to a general definition of 'drive'. This is not satisfactory but the point regarding the relationship between M.A.S. and 'drive' will be returned to and, one hopes, clarified in the final chapter.

Before concluding this section, and despite the rather critical appraisal afforded to the M.A.S. above, it might be of some interest to list some of the situations in which M.A.S. scores were found to be negatively related to various aspects of performance. This is done because it is this negative relationship between anxiety/neuroticism and performance which receives particular emphasis in the later parts of this thesis.¹

These relationships were:

- 1) Between M.A.S. and serial learning tasks (Farber and Spence, 1953)
- 2) Between M.A.S. and mechanical aptitude (Kamin, 1955)
- 3) Between M.A.S. and discrimination reaction time (Grice, 1955)
- 4) Between M.A.S. and intelligence (Spielberger, 1958)
- 5) Between M.A.S. and incidental learning in a complex situation (Spielberger, Goodstein and Dahlstrom, 1958)
- 6) Between M.A.S. and discrimination learning (Stevenson and Iscoe, 1956)

It can be seen from this brief list above that high M.A.S. scores tend to be related to poor performance in a wide variety of situations and despite the invocation of the Yerkes - Dodson law, it seems unlikely that all of these can be explained by assuming that high anxiety is synonymous with high activation.

ii) The Maudsley Personality Inventory

This Inventory contains scales for the measurement of neuroticism (N) and extraversion (E). It is described in Eysenck's Manual of the

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The significant reported relationships in this thesis were between the 'N' scale of the M.P.I. and performance but in view of the small amount of published work in this scale, and in view of an observed correlation of .+78 between the 'N' scale and the M.A.S. it is of some value to give the above synoptic review of M.A.S. and performance studies.

Maudsley Personality Inventory (Eysenck, 1959) and a copy is included in Appendix III.

Considering its origins there has been little reported work involving these scales. Eysenck (1962) reports highly variable results, particularly from the use of the 'N' scale in studies of reminiscence phenomena. The most definite report comes from Furneaux (1962)¹ who found superior university examination performance with neurotic intraverbs, using this inventory as his measure. He implied that the N-scale might be an adequate measure of generalised drive or activation and it was this suggestion which, in a spirit of scepticism rather than naive optimism, motivated the use of the scale in this study.

The basic assumption relating neuroticism scores to drive level is much the same as that stated by Spence (1958) in relating anxiety scores to drive, namely that individuals with high neuroticism or anxiety will more readily enter a high drive state when confronted by a stressful situation. This implies that the scales do not measure actual drive level at the time the questionnaire is being completed (unless this is regarded as a stressful situation) but rather the potential drive level of the subject in a task situation which produces some stress or, in the case of eyeblink conditioning, discomfort through noxious stimuli. Thus the drive level which these scales measure is chronic only in so far as it will constantly recur in certain situations which are, in the widest sense, noxious or frustrating.

Both in terms of its origins and in terms of the results to be reported later in this thesis it would appear that the 'N' scale of the

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In the actual report Furneaux does not mention the M.P.I. but in a personal communication he confirmed that it was the scale used.

M.P.I. is factorially purer than the M.A.S. In other words, while both scales are probably, in view of their high intercorrelation, measuring the same intensive dimension, the 'N' scale seems to do it better. What is almost certain is that the dimension these scales do measure, and they are measuring something, is not the same as the dimension or dimensions of activation as measured by physiological indices such as skin conductance. This point is illustrated both in the next section and in the results reported in the following chapter and its implication will be fully discussed in the final summary.

c) Relationships between Measures

This section consists of a brief summary of the relationships which have been reported in the literature and which have been observed in the experiments conducted by the author.

i) Relationship between Basal Conductance and Conductance Change (G.S.R.)

The basic assumption in most of the literature on the subject has been that response size and basal conductance level should be unrelated. This is emphasised in the work of Lacey and Siegal (1949) and Haggard (1949) who support the use of the log. conductance unit as an index of both G.S.R. size and basal conductance level. Expression of the measures in these units gives both normality of distribution and independence. The emphasis upon the unrelatedness of G.S.R. to basal conductance level might appear surprising in view of the fact that both have been suggested as measures of the same (activation) dimension, but the explanation of this emphasis is obvious when we realise that the use of straightforward resistance units results in a high positive correlation between G.S.R. and basal resistance. This would mean that, in terms of these units, high activation as measured by a large mean G.S.R., would be related to low activation as measured by high basal resistance.

In the six studies carried out in connection with the work which is reported in this thesis no significant relationships were observed between mean size and/or number of G.S.R.'s and basal conductance level when both were measured in log. conductance (micromhos) units. In view of the fact that these units were chosen because they guarantee independence, this finding is hardly surprising, but it does serve to emphasise the

fact, which is often overlooked, that a measure such as mean G.S.R. cannot measure activation level if basal skin conductance is taken to be measuring activation level also.

ii) Relationship between M.P.I. 'N' Scale and M.A.S.

This has already been discussed (see section b; above). Franks (1956) reports a highly significant positive correlation between the two scales, and with a group of 82 student subjects the present author noted a correlation of + .782. As it has already been indicated there is some reason to feel that the 'N' scale is a more satisfactory measure, but it is quite obvious that both scales are, to some extent, measuring the same dimension. This is almost inevitable in that several of the questions are almost identical in each scale and many of the others are quite similar.

iii) Relationship between Basal Conductance and Anxiety and Neuroticism Scales.

There are very few even indirect positive reports in the literature of any relationships between anxiety and neuroticism scale scores and basal skin conductance level. Altschule (1953), in a review of studies relating clinically diagnosed neuroticism to skin resistance level, reports little consistency in the results. Cattell and Scheier (1961) include decrease in electrical skin resistance as a physiological indicator, with a fairly high degree of association, of Anxiety state but there is no guarantee that their 'anxiety' is the same as that measured by the M.A.S., although they do cite Taylor's 1953 article (Cattell and Scheier, op.cit. p.272) as if accepting some identity between M.A.S. 'anxiety' and their 'anxiety'. This, however, is slim evidence indeed.

In an experiment in which M.A.S., M.P.I. 'N' and basal conductance were all measured within the space of thirty minutes no significant correlations were observed between the M.A.S. scores and basal conductance nor between the M.P.I. 'N' scores and basal conductance (N = 82 in each case).

There is, of course, one difficulty here. As has been already indicated the relationship which has been hypothesised between the M.A.S. or 'N' scale scores and drive or activation level is indirect. It is assumed (see section b¹¹ above) that these scales measure potential drive level in a stressful situation. As the conditions under which the basal conductance level was measured¹ in the above experiment could hardly be referred to as stressful - although the term is vague - it may be that the potentially higher activations levels of the high anxiety subjects were not being adequately reflected in the basal conductance levels which were measured. In order to clarify this M.A.S. and M.P.I. 'N' scores were obtained for subjects who had had their basal conductance levels measured during the course of a variable interval reaction time experiment. Again the correlations were totally insignificant. (N = 24).

This evidence seems to be sufficient to permit rejection of any hypothesis that basal conductance level is related to scores on scales of neuroticism and/or anxiety.

(iv) Relationship between Conductance Change (G.S.R.) and Neuroticism and Anxiety Scales

A vast number of studies have sought to relate G.S.R. to various personality descriptions. Some of them have been reviewed in ⁰¹¹ _{^ ^}

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Measurement was made during the reading of a list of words at the rate of one word every 20 seconds.

above. It would be futile to attempt a review of them all because it would have little relevance to the current problem. A small sample from the recent literature will, however, serve to illustrate the range of speculation. Jurko et al. (op.cit.) as has been stated before found excessive G.S.R.'s in neurotic subjects and very little responsiveness in schizophrenics. Greenfield et al. (1963) found a relationship between low physiological (G.S.R.) responsivity and scores on the depression scale of the M.M.P.I., using psychiatric patients. There was no relationship between G.S.R. and clinical diagnosis. Chan Chang-Keng (1964) found greater G.S.R. activity in 'restless, anxious, and over-sensitive neurasthenics' than in normal subjects. In a study of the relationship between the emotional involvement of delusional psychotics in their delusion and G.S.R., Howard (1964) found that G.S.R. might serve as an indicator of delusional intensity. Cattell and Scheier (1961) report a negative relationship between superego activity and G.S.R. size.

Despite the wide ranging nature of the studies carried out there has been, as far as can be ascertained, no study reporting any relationship between G.S.R. and the personality scales which have been used in this thesis. In the present series of experiments no significant relationships were observed between mean size or mean number of G.S.R.'s and either the 'N' scale of the M.P.I. or the M.A.S.

(v) Summary.

From the above it appears fairly certain that the two skin conductance measures are independent of each other, in that they both cannot be used as indicators of a general activation level; and that neither of the conductance measures is measuring the same dimension as the two

related anxiety/neuroticism scales. This is not to say that, in certain situations, highly neurotic subjects will not exhibit larger G.S.R.'s than stable subjects, or even the reverse; but this does not imply an identity between G.S.R. and neuroticism. Analogously we might find that, in certain situations, highly intelligent subjects will display more overt anxiety than low intelligence subjects. This does not mean that intelligence and anxiety are related except in so far as a highly intelligent person might see a potential threat in some situations where a person of low intelligence will not see it. One could legitimately claim an identity between intelligence and anxiety only if they were consistently correlated in a wide variety of situations where no linking hypotheses could be advanced to explain the relationship.

Another more subtle problem has been mentioned above. It may be that a relatively transient measure, like skin conductance level, will reflect an effective relationship with performance only when it is measured in a situation identical or similar to the performing situation. The same might be said of skin conductance change. If measures taken during a comparatively innocuous recording session are then related to the chronic measures of anxiety or neuroticism which are said to reflect potential drive level in a frustrating situation, the relationship may be insignificant because the measurement is being made at an inappropriate time. This problem was tackled in one experiment (see iii above) which seemed to confirm the earlier findings of an insignificant relationship, but it is a problem which must influence the methods of further experiments in the field of activation. Whenever a transient measure is being compared with a chronic measure the transient measure must be obtained in conditions which are similar to the conditions under which the potential indicated by the chronic measure will be exhibited.

III THE EXPERIMENTS

This section is comprised of a straightforward description of each experiment carried out in connection with this thesis. A general interpretation of the experimental findings is not included in this section but is to be found in the General Conclusions. However, some limited conclusions are drawn at the end of each experiment and at the end of each section.

Experiment II is only briefly described here as a full description is to be found in Appendix II which is the text of a joint paper submitted for publication and accepted before the completion of this thesis. The conception of that experiment and the conclusions drawn from it (which will be discussed further in the next section) are entirely the work of the present author and it seemed justifiable to include this work as part of the thesis.

The order in which the experiments are described is explained by the section headings. Grouping is according to the 'intensive' measure used. Table I gives the experiments in their chronological order. A knowledge of this order will clarify some of the points made in the following sections.

Table I

Chronological order of experiments described in ensuing sections.

1. G.S.R. and word recall (2 experiments) Spring and Autumn 1961.
2. Basal skin conductance and word recall (and repeat) Spring 1962.
3. G.S.R. and sensory sensitivity (pilot experiment) Summer 1962.
4. Basal skin conductance and pursuit rotor Autumn 1962.

5. M.P.I. 'N' scale and examination performance (together with certain other studies described in the text). Spring 1963
6. G.S.R. and sensory sensitivity (main experiment). Winter 1963-64.
7. Basal skin conductance and rapid dotting Spring 1964
8. Basal skin conductance and reaction time Winter and Spring 1964-65.
9. M.P.I. 'N' scale and rote learning Spring 1965.

a) G.S.R.¹ and Performance

(i) G.S.R. and Word Recall

Introduction: This experiment was the first one carried out in the series to be described here. Basically the aim was to determine whether the size of the G.S.R. produced in response to a word would be in any way related to the later recall of that word. The reason for conducting this experiment leads back to the origin of this thesis when the author was interested in trying to explain why uncommon words are more readily recalled than common words in a list composed of equal numbers of both, while the reverse is true when the lists contain either all common or all uncommon words. The idea was conceived that this is due to the 'arousing' effect of uncommon words which is effective in mixed lists but is more than nullified by the factor of difficulty when the lists are homogeneous. The experiment completely failed to prove or disprove this rather weak hypothesis and an explanation of this readily reproducible phenomenon is still being sought. The results, however, were interesting and the experiment is included in this thesis because of its relevance for an explanation of the nature of G.S.R.

1

See Section 2 for the justification for using this rather misleading term.

Apparatus: The apparatus for measuring the G.S.R. was as described in Appendix I. In addition a microphone, loudspeaker and lists of words were used. The word lists used, in the order presented, are given in Table 2.

Table 2

List I (Immediate Recall)

Garden, Obtrude, Wound, Meander, Example, Ugly, Emporium,
Mother, Black, Medicate, Floor, Death, Object, Loquacity,
Orgasm, Stream, Lavatory, Futurist, Gentle, Taxonomy, Dance.

List II (20 minute delay)

Support, Optimum, Kiss, Plaudit, Journal, Sexual, Contain,
Cygnet, Direct, Lone, Verger, Divorce, Record, Breast,
Modulate, Branch, Jetsam, Woman, Present, Marry, Transfuse.

List III (2 week delay)

Testify, Return, Lamine, Person, Tumult, Dinner, Emulsify,
Matrix, Jungle, Bird, Kindle, Optimum, Rusty, Ingress, Middle.

The odd nature of the words is due to the inclusion of words of low frequency of usage (as determined by the Thorndike-Lorge word count, 1944) but as this factor is irrelevant to the reported results the specific frequencies of usage are not included. The fact that list three is comprised of only fifteen words, while the other two lists are twenty-one words long, should not influence the final result, although it is regarded as an unnecessarily confusing feature of the experiment. The reason for reducing the length of list three was that it seemed at the time that the administration of the experiment was excessively prolonged. At this time the author was unfamiliar with the apparatus

and each of the longer lists was taking about 45 minutes to administer. Subjects were complaining, quite justifiably, of the discomfort involved in sitting still with their right hands constrained for such a long period and it was therefore decided to use a shorter list in the third part of the experiment.

Procedure: Subject and experimenter were situated in separate, but adjacent rooms. This was found to be necessary because, in some preliminary studies it was discovered that the recording pens, although comparatively silent, produced enough noise when large responses occurred, to set up a positive feed-back loop between pen and subject. In other words the subject heard the increase in pen noise, became aware that he had produced a large response to the last word, and proceeded to produce a further response to his original response. Unless pen sensitivity was drastically lowered to reduce pen noise this would go on ad infinitum and it made measurement of a true response to the stimulus word quite impossible.

Electrode leads and one from the microphone in front of the experimenter were fed through an aperture in the wall into the small room where the subject sat facing the loudspeaker. Before the experiment the sensitivity of the microphone amplifier was adjusted so that the only sound which it picked up in the recording room was that of the experimenter's voice speaking distinctly at a distance of approximately six inches. The experiments were carried out in the evening so that extraneous noises from within the building were at a minimum.

When the subject arrived he was met by an assistant who, having asked him to wash his hands, took him to the subjects' room, fixed on the electrodes, and read the following instructions.

'In a short time you will hear words coming from the loudspeaker. I want you to listen to the words and think about their meaning to you. The first five words are test words and these will be followed by the experimenter saying, 'Are you ready for the experiment?'

'The things on your hand are electrodes. The cables from them lead to a recording apparatus in the next room. They are quite harmless and you will receive no electric shock or anything nasty like that.

'Sit as comfortably as you can and try to move as little as possible during the experiment.

'Are you ready?'

During the reading of these instructions the experimenter balanced the recording apparatus in the way described in Appendix I, and in receipt of the assistant's signal (a short rap on the intervening wall) that all was ready, the experimenter commenced with the reading of the test words. They were necessary to allow the subject to adapt to the experimental situation and to indicate to the experimenter the level of amplifier sensitivity which would be appropriate to the subject.

It will be noted that the instructions gave no hint of the fact that the subjects would later be asked to recall the words. This was a deliberate omission. Had subjects known of the recall test they could, at the slow rate of presentation, have memorised and recalled all the words later without any difficulty. The fact that none of them could recall all the words indicates the success of the deception.

As indicated above the time taken for each recording session was approximately 45 minutes for the 21 word lists. For the 15 word list the time was reduced to about 30 minutes. In both cases this included the time to read the test words. The words were read in a different order for each subject.

Immediately after the last word had been read and the response recorded, the experimenter said, after reducing amplifier sensitivity to zero, 'That was the last word. You may remove the electrodes'. The procedure then varied according to the group the subject was in. With group I (immediate recall) the assistant gave the subject pencil and paper and told him to write down as many of the experimental words as he could remember. After three minutes the paper was removed and the subject was told that the experiment was over and that he could go. He was asked, before he left, not to tell any friend who also might be a subject, of the procedure. With group II (short delayed recall) the assistant gave the subject a simple card sorting task to perform and he spent twenty minutes doing this. At the end of this period the procedure was exactly the same as for group I subjects.

With group III (long delayed recall) the subject was reminded that there was a second part to the experiment in two weeks' time and allowed to go. When he returned in two weeks he underwent the same recall procedure and was given the same caution, as subjects in groups I and II.

Subjects: Subjects were all first year Psychology students in the second or third term of their course. Their ages were between 18 and 23. None of them had undergone a G.S.R. experiment before. The numbers in each group were:



Group I	18	(8 male	10 female)
Group II	23	(10 male	13 female)
Group III	19	(8 Male	11 female)

The differences in group size, which is not an important factor in this experiment, were due to the failure of certain subjects to appear at the appointed time.

Treatment of results: All responses were converted to log. change in conductance, a measure which is generally regarded as the most satisfactory expression of skin response (Lacey and Siegal, 1949). In order to make inter-subject comparisons valid all sets of responses were then converted to a T-scale (Guilford, 1956). This was done separately for each subject's set of responses. The scaled responses to each word are included as Tables 3, 4 and 5. The rationale behind this procedure should probably be explained at this point.

TABLE 3

T-Scores (no delay)

Bracketed numbers indicate recall of word

Sub.	Word order																				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	(49)	54	48	62	(41)	41	(57)	41	57	62	(51)	54	(54)	62	41	41	41	(49)	(70)	(41)	41
2	(49)	51	36	(45)	45	30	(45)	45	49	(36)		(56)	(36)	(56)	(56)	(56)	(67)	56	56	(67)	(56)
3	56	(70)	(47)	39	(56)	(47)	56	56	(39)	47	39	(65)	(47)	(47)	56	(56)	56	(30)	39	(47)	(56)
4	(46)	(60)	(37)	55	(51)	37	65	60	(37)	46	(37)	70	46	(46)	(55)	51	46	(51)	60	(46)	(55)
5	37	54	44	49	49	37	(62)	49	44	54	37	54	37	44	62	(70)	62	54	49	(58)	(49)
6	(46)	61	(46)	61	61	46	(61)	46	(61)	46	61	46	46	(46)	46	46	(46)	46	46	46	46
7	(70)	(55)	(61)	46	(55)	(46)	(61)	38	51	38	(55)	(61)	38	(61)	(46)	(38)	51	(46)	38	46	(51)
8	(56)	43	45	43	(61)	(43)	43	(67)	56	(43)	(43)	(45)	(43)	(61)	(45)	(67)	56	43	(56)	(43)	43
9	51	63	51	40	51	58	(40)	58	(51)	40	(40)	51	40	58	51	(63)	51	40	51	(70)	40
10	48	(67)	48	48	48	48	48	(48)	48	48	(48)	48	(48)	(62)	48	48	67	48	48	48	48
11	(44)	(54)	62	(44)	(54)	54	(70)	44	44	(44)	(44)	44	(54)	(44)	62	(44)	54	(44)	62	54	44
12	(48)	(30)	39	48	(48)	48	(48)	(39)	(39)	48	(39)	56	48	(56)	56	(56)	(48)	65	60	(65)	(65)
13	44	(44)	44	60	(44)	44		44	52	44	44	67	67	(54)	57	44	60	44	(54)	57	(44)
14	(52)	(46)	30	40	49	(46)	(52)	40	40	(56)	40	62	52	(58)	(62)	55	62	(40)	46	(52)	(70)
15	(54)	54	44	44	(70)	(44)	(44)	44	44	(44)	(54)	(60)	(44)	(44)	(63)	44	(54)	(44)	54	54	(63)
16	35	(45)	35	(52)	(52)	52	58	70	63	(55)	(35)	45	45	(45)	45	58	63	45	(52)	(45)	(58)
17	(43)	61	43	(43)	(52)	56	43	(52)	43	(52)	(52)	58	43	(43)	43	(65)	(43)	(56)	70	(61)	43
18	(35)	44	44	51	51	56	(51)	56	(56)	51	51	51	35	(44)	(61)	61	(65)	35	44	(70)	44

TABLE 4

T-Scores (20 min. delay)

Bracketed numbers indicate recall of word

Sub.	Word order																				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	41	(65)	(35)	65	54	41	51	56	30	60	47	(58)	51	51	47	(65)	47	41	(47)	41	54
2	43	61	48	(55)	48	48	37	70	(43)	48	55	(55)	(37)	48	(37)	(61)	37	(48)	61	61	(52)
3	(64)	(48)	53	(53)	53	43	43	53	43			(53)	(34)	(43)	(34)	(48)	64	43	64	58	58
4	50	42	33	42	(63)	(70)	50	(42)	50	(63)	(57)	57	57	50	(42)	(50)	57	42	(50)	33	50
5	(61)	(61)	51	(56)	56	(51)	58	(67)	38	(51)	(46)	46	51	38	38	46	46	(38)	(67)	38	51
6	56	67	48	48	(56)	67	48	(62)	48	56	(60)	56	48	48	44	44	44	37	37	37	(37)
7	(70)	(70)	55	(70)	55		55	55	55	(55)	(47)	49	40	40	(40)	55	40	(47)	(47)	40	(40)
8	(40)	(55)	40	(40)	40	64	(40)	55	40	(49)	(69)	(49)	49	49				60	55	60	49
9	61	(53)	53	(70)	(53)	53	61	(61)	53	47	61	47	53	30	47	40	40	47	(40)	40	(40)
10	48	(57)	35	63	(48)	48	48	48	48	(48)	48	(57)	48	(48)	63	(57)	(35)	35	(70)	48	(57)
11	48	60	38	(38)	54	60	54	65	60	(54)	48	(54)	70	30	38	48	(48)	44	(44)	54	44
12	44	65	(54)	(50)	35	44	(44)	(44)	35	(35)	50	(57)	54	44	50	(70)	61	(44)	57	61	(52)
13	42	42	(49)	(42)	42	42	53	65	42	53	53	53	65	42	42	(59)	65	(59)	(42)	53	(53)
14	(48)	(56)	56	38	38	48	(56)	(56)	38	(48)	(63)	(56)	38	48	38	(70)	48	48	48	(48)	63
15	59	67	59	(51)	51	51	55	59	42	(46)	46	(54)	42	42	30	(59)	37	46	(51)	37	(67)
16	47	47	47	(47)	38	47	30	(70)	38	47	62	54	47	54	54	57	38	47	62	57	(62)
17	70	58	51	(51)	58	58	42	(58)	65	(51)	(51)	58	42	42	42	51	42	42	42	42	(42)
18	(61)	54	44	(61)	(44)	54	44	(61)	44	44	(44)	(70)	44	(44)	44	54	54	54	(61)	44	44
19	38	(63)	(57)	(55)	63	(52)	44	(63)	44	63	30	(52)	44	(44)	38	(44)	38	(52)	(57)	(52)	(44)
20	(44)	44	44	(44)	57	(51)	44	53	44	(33)	33	(44)	(57)	51	57	(65)	62	44	70	57	53
21	(37)	(43)	30	(43)	61	54	48	70	48	(48)	(61)	61	54	(43)	(54)	(61)	43	54	(54)	48	37
22	57	(49)	49	57	49	49	(43)	57	(57)	(43)	70	(57)	39	57	49	(65)	49	49	(35)	30	39
23	44	(49)	35	(49)	51	44	(65)	44	54	(54)	(58)	(35)	44	(44)	35	(54)	(44)	60	(54)	65	65

TABLE 5

T-Scores (2 week delay)

Bracketed numbers indicate recall of word.

Sub.	Word order														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	53	48	57	(61)	61	48	35	53	48	(69)	43	43	35	43	53
2	43	43	53	58	69	58	53	43	(50)	(43)	58	58	43	43	43
3	55	49	42	49	61	(42)	(69)	52	61	42	55	(42)	42	55	42
4	55	42	(63)	(42)	50	55	(55)	(69)	50	(50)	42	42	(42)	(60)	42
5	45	54	(45)	(65)	45	(65)	45	54	57	45	45	45	45	60	45
6	51	(43)	43	53	65	(53)	53	43	(43)	(43)	51	(60)	(65)	(43)	43
7	57	46	46	46	46	57	46	46	(63)	46	57	46	69	46	46
8	53	(37)	53	(58)	46	46	58	37	46	(65)	37	(46)	50	(55)	(65)
9	55	63	63	55	43	37	46	52	(49)	(55)	37	46	49	(63)	37
10	(69)	60	53	53	(60)	60	53	53	(40)	(40)	40	47	40	48	40
11	56	46	46	46	60	(65)	46	46	46	65	46	46	46	46	56
12	69	61	45	(55)	45	(61)	45	(55)	45	45	55	45	(45)	45	45
13	53	(69)	(61)	40	47	47	(53)	(57)	(53)	40	40	61	40	40	(53)
14	(69)	46	(56)	46	46	46	61	46	46	(61)	46	46	56	46	46
15	55	61	55	(61)	45	45	55	45	45	45	69	45	45	45	45
16	52	57	(69)	52	43	(57)	(57)	43	43	52	43	(69)	43	43	43
17	52	53	50	55	58	40	65	40	(40)	58	47	47	40	65	40
18	(69)	55	43	(55)	(35)	(55)	47	55	(55)	(43)	55	55	(43)	47	35
19	(57)	45	(65)	(45)	45	45	45	45	45	(57)	45	53	(45)	(57)	65

Subjects differed widely in their average response size. The smallest response of a large responder might be larger than the biggest response of a small responder. Obviously any relationship between response size and word recall, considered as an intra- rather than inter-subject variable, would be obscured if unscaled response measures were used. Assuming some relationship to exist, it was felt that the largest response of a large responder would exert relatively no more influence on the likelihood of a word being recalled than the largest response of a small responder, even if the two responses were very different in their unscaled state. This view might be regarded as arbitrary if it were not to some extent supported by the observation that the mean size of response was in no way related to the number of words recalled. If an absolute, rather than a relative, relationship exists between response size and word recall, then one will expect large responders to recall more words. The interpretative problems which arise out of this finding will be considered in more detail in the discussion to this Section.

The T-scaled responses were then related to recall by means of a biserial coefficient correlation. The choice of biserial rather than point-biserial was made because it was felt that recall - non-recall is not a genuine dichotomy in the sense required by Guilford (op.cit.) for the use of point-biserial correlation. It was felt that the recall of a word is not an all-or-nothing thing. Some words which are recalled on one occasion may not be recalled on another and vice versa. Also, by the use of different methods (e.g. recognition or relearning) it might be found that some words are more readily available than others.

The obtained correlations were:

Group I	(Immediate recall)	$r_b = +.13$	$\sigma_{+b} = .064$
Group II	(20 minute delay)	$r_b = +.23$	$\sigma_{+b} = .056$
Group III	(2 week delay)	$r_b = +.40$	$\sigma_{+b} = .071$

All these are significant, the degree of significance increasing with the length of delay between initial presentation and recall.

As expected the percentage of words recalled decreased with the length of delay. The actual figures, which are amazingly even, are:

Group I	45%
Group II	35%
Group III	25%

Discussion: The main interest in these results lies in the increasing correlation between relative response size and likelihood of recall as the delay between initial presentation and recall increases. It is probably reasonable to assume, on the basis of the fact that fewer words are recalled the larger the delay, that this increasing correlation reveals that words which produce a large response are more thoroughly learned at the time of presentation. The immediate recall situation is probably one in which a large number of almost chance factors determine the words recalled. Potentially all words could be above the threshold of recall. As time passes the chance factors, which may be highly related to the immediate context of presentation, will tend to become less influential. Obviously, despite the fact that the correlation of response and recall in the long delay situation was highly significant, factors other than size of skin conductance response influence the mechanism responsible for the effective, incidental memorisation and recall of words. Without further knowledge of what these factors are it would be idle to speculate upon their nature or their method of functioning.

It remains, however, to attempt some explanation of why a large G.S.R. should be significantly related to the effective incidental memorisation of words. Before attempting this explanation, however, it will be appropriate to consider another experiment in which G.S.R. was related to a very different measure of behavioural efficiency.

(ii) G.S.R. and Sensory Sensitivity

This experiment is fully described in Appendix II. In fact two experiments were carried out, the first pilot experiment, which is briefly described in the Introduction to the Appendix, being conducted by the author approximately a year before the main experiment. In order to prevent constant reference to the Appendix the introductory summary, is included here:

Investigation was made of the relationship between mean log. change in conductance and sensory ~~discrimination~~. Highly significant relationships were found with c.f.f. and two point tactile thresholds. No significant relationship was found with discrimination of a pure tone from background white noise. An attempt at explanation is offered in terms of a **critically** controlled centre mediating a sensitising or orienting response of which conductance change is a peripheral manifestation.

The findings reported in this Appendix should now be considered in conjunction with the findings reported in Experiment I above.

(iii) Conclusions from G.S.R. Experiments

The primary question to be considered here is to what extent can the results from Experiment I be explained in terms of the theory advanced to explain the results included in Appendix II. In other words, is it

possible to maintain a coherent theory which explains G.S.R. and related phenomena in terms of a short term orienting or sensitising response not necessarily related to long term activation?

It might be advantageous at this point to be more explicit about the basic premises underlying the terse statement of the theory in the Appendix.¹

Any stimulus² is potentially capable of initiating activity in the reticulothalamic centre which basically mediates the sensitising response, but the stimulus will reach the cortex by the specific sensory pathways before it completes its journey through the densely synapsed reticular formation. It is assumed that any evaluation of the stimulus will be made, not in the reticular formation, but in the cortex, probably in the primary association areas, though the pre-frontal lobes may also be involved. The reason for doubting the role of the reticular formation as an evaluation centre is simply that it is almost certainly not large or complex enough to contain the resources to make an adequate evaluation of all the potentially meaningful stimuli which may impinge upon a human organism.

Once the evaluation has been made it seems likely, on the basis of evidence presented in the Appendix and in Chapter 2 of this thesis, that the cortex will differentially inhibit the response which, without inhibition, would inevitably arise in the reticulothalamic mediating centre. It is assumed that inhibition can be non-existent (though rarely), partial, or complete. Thus we respond to significant, alerting stimuli but the

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This brevity was a consequence of editorial pressure and is not altogether the fault of the author.

2

The word is used in its loose sense to mean either stimulus or stimulus complex.

assessment of significance is a cortical function. Any other explanation must needs place a hom^vunculus in the mid-brain.

The purpose of the sensitising response is, as Cannon (1932) implied, to prepare the organism for action. So we have among other phenomena, the G.S.R., which is, as we have already seen, mediated by the sympathetic branch of the autonomic nervous system. A further consequence of the response is an increase in cortical reactivity (Jung cited in Appendix II).

Now let us consider the results of Experiment I in terms of this theory. Each word will be evaluated¹ in terms of its significance. This significance is almost certainly a function of both general, and individual long-term meaning, and meaning in the particular context. A response will follow, its size being dependent upon the evaluation. A subject who attaches little significance to the situation will normally produce small responses, though these will vary according to the long-term meaning associated with the word. A subject who attaches considerable significance to the situation will normally produce large responses. These, likewise, will vary according to long-term meaning. Now a word which is followed by a large response, at least relative to the other words in the list, will benefit (if that is the word) from the increased cortical reactivity mentioned above. It could be assumed, though it is a large assumption, that this increased activity will facilitate the formation of a relatively permanent memory trace. Whether this trace be a cell assembly as postulated by Hebb (1949) or a change in the structure of cellular R.N.A. as postulated by more recent workers in the field is largely irrelevant to the present rather hypothetical discussion.

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The evaluation process is probably pre-conscious, but it is by no means demonstrably so.

This tentative explanation will cope with one feature of the results of Experiment I rather well. The suggestion that the cortical activity related to a sensitising response aids the formation of a relatively permanent memory trace will explain why the correlations increase in significance with elapsed time. The earlier recalls will probably be dependent upon relatively dynamic short-term memory to a greater degree, but short-term memory, if we are to assume that dynamic activity continues for only about fifteen minutes after the event which initiated it (Hunter, 1957), will have no part to play in the recall after a period of two weeks.

Unfortunately another aspect of Experiment I(a) is not at all adequately explained by the above hypothesis. Taken as it stands the hypothesis would lead to a fairly confident prediction that large responders will recall more words in a given list than small responders, but as has been stated above, there is no evidence from this, or from any other experiment conducted by the author, to confirm this prediction. In an experiment specifically carried out to test the prediction 34 boys aged between 15 and 18 were put through an experimental procedure very similar to that described for Experiment I. Word lists were twenty words long. The correlation between mean G.S.R. and number of words later recalled, at a period of between one and seven days after initial presentation, was totally insignificant.¹

It could be tempting to indulge in some speculative theorising in an attempt to explain away this puzzling anomaly. It is, however, probably better at this stage simply to suggest that the factors controlling

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This experiment did confirm the earlier positive findings. A biserial correlation coefficient of $+0.37$ ($r_0 = .053$) was found between size of response to a word and delayed recall of that word.

the number of words which might be learned incidentally in a given time are not the same as those which control which word or words will be learned in that time.¹

The findings reported in this section, as much as any others to be reported in this thesis, emphasise the contradictions which arise when one attempts to maintain a simple activation hypothesis, even when that hypothesis implies an activation dimension which is independent of the generally accepted dimension of long-term activation.

These problems will be considered in a wider context in the final chapter of this thesis. In the meantime we shall consider the next set of experiments.

b) Basal Skin Conductance and Performance

i) Basal Skin Conductance and Word Recall

Introduction: This experiment was the first to be carried out with the specific purpose of studying the relationship, if any, between basal skin conductance level and the memorisation of words. In the earlier studies, described in Section a) above, no relationship was found between basal skin conductance and the nature or number of words recalled. But in these experiments the measurement of conductance level had not been made with any great degree of care, and fluctuations in basal conductance level over the period of the experiment made it difficult to assign a meaningful value which would represent the level at which all the words were received. This experiment still reflects an interest in the problem of activation and word recall. Having discovered that mean size of G.S.R. is not related to number of words recalled, it was felt that possibly basal skin conductance level, taken to be a measure of

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See footnote on p.14⁰ for a possible explanation.

long-term activation, would be so related. A very simple-minded hypothesis lay behind the experiment, namely that high conductance levels, reflecting high activation, would be associated with greater alertness and efficiency and that this would, in turn, enable the subject to utilise some perhaps imperfectly formulated learning strategy in an effective way, resulting in a greater number of words being learned and recalled. In other words it was felt that as the number of words learned and recalled in a given time is obviously not a function of the mean size of G.S.R. produced, that is the short term activation of the subject; it might be a function of relatively long term activation as measured by basal conductance. As it turned out the experiment completely failed to confirm the hypothesis. The number of words learned and recalled may be a function of the effective use of learning strategies but this effectiveness does not seem to be related to activation as measured by basal skin conductance.

The experiment did, however, produce one interesting, if highly confusing, result, and so it is included here even although the result is difficult to understand, even in terms of a complex multi-dimensional activation theory.

Apparatus: Basal skin conductance was measured on the apparatus described in Appendix I. The only additional material was two word lists; one a list of 20 common words, occurring more than 100 times per million words of written English, according to the Thorndike-Lorge Word Count (1944), and one a list of 20 uncommon words occurring less than once in a million words of written English. The actual words used are given in Table 6.

Table 6

List 1. (common words - 100+)

Union, Require, Practice, Morning, Leave, Interest, Example,
Demand, Battle, Different, Order, Light, Further, Garden,
Steam, Train, Continue, Animal, School, Least.

List 2. (Uncommon words - -1)

Undercut, Transfuse, Snooze, Kettledrum, Intermediary, Cygnet,
Agnostic, Emulsify, Doeskin, Jetsam, Laminate, Ingress,
Optimum, Pleurisy, Paternalism, Cordon, Bullfinch, Matrix,
Asymmetrical, Canvaser.

The use of common and uncommon words was probably not necessary in terms of the intention of the experiment but as it happened it was the use of the uncommon word list which led to the one interesting finding.

Procedure: Subjects attended for one session only. When they arrived subjects were asked to wash their hands. When they had done this they were fitted with palmar electrodes on their non-writing hand and the electrode leads were affixed. The measuring apparatus, which had been switched on for warm-up twenty minutes before the subject arrived, was then adjusted for reasonable sensitivity. As it was not intended to measure G.S.R.'s, maximum sensitivity was not used. Accurate recordings of basal conductance level can be made at one-fifth of maximum amplification. As there was minimum pen noise it was not necessary for subjects to be in a separate room.

Once initial balancing of the bridge had been completed subjects were told that they were going to hear some words. Five words were then read to the subject at the rate of about one every minute. This was done purely to give the subject time to settle down and to allow his basal

conductance level to stabilise. Typically, for the first five or six minutes of an experimental session, conductance will tend to decrease rapidly. Thereafter the drift will be more gradual, and while most records do show a gradual decline in conductance throughout an experimental session, this is not inevitable, and a very few records do show a rise in conductance after the initial unstable period.

After the five 'dummy' words had been read subjects were told:

'I am going to read a list of words to you at the rate of about one word every second. I want you to listen to these words very carefully. When I have finished I will give you pencil and paper and I want you to write down as many of the words as you can remember. Is that clear?'

When these instructions had been given the apparatus was carefully balanced to give a null reading, the resistance noted, and one or other of the two lists was read at the speed indicated. As soon as the reading was completed the subject was given pencil and paper, the apparatus was rebalanced, and the resistance reading again noted. The subject was given three minutes to write his recalls. After the pencil and paper had been removed the same procedure was repeated with the second list except that the instructions to the subject were simply:

'I am going to read another list of words to you now. I want you to do the same as before. Is that clear?'

Half of the subjects were read List 1 followed by List 2; for the other half the order was reversed.

The reason for reading the list rapidly rather than imitating the slow presentation rate in earlier experiments, is implied in the introduction

to this experiment. Slow presentation combined with a fluctuating basal conductance level would have made it impossible to assign a realistic level to a subject. This is emphasised in Experiment iv of this section where the assigning of a mean conductance level to subjects over a forty minute experimental session is shown more or less to obscure the relationship which seems to hold between basal conductance level and reaction time.

Another procedural point which might be questioned is the substitution of intentional for incidental learning. This was done out of deference to the subtle minds of students. While it is fairly certain from questioning that no subjects in the earlier experiments suspected that they were going to be asked for later recall of the words presented to them, it is rather unlikely that this would have followed with a list read at the fairly rapid rate that was necessary in this experiment. Some subjects might well have guessed the intention of the experimenter and rather than have this factor confusing the issue it was decided to standardise the situation by informing all subjects of the requirement of later recall.

For these reasons it was difficult, if not impossible, to make the situation in this experiment more directly comparable with the situation in the earlier experiments. Had the results of this experiment been more encouraging, and not almost directly opposed to expectations, an attempt might have been made to carry out a more sophisticated study. As it was, the experiment, probably more than any other, convinced the author of the need to have a closer look at the theory of activation as he then understood it, and persuaded him to defer the detailed study of the relationship between activation and word recall until such time as

he had a fuller understanding of the more fundamental concept of activation itself.¹

Subjects: Twenty first year Psychology students were used. They were all volunteers and their ages ranged from 18-27.

Results: Basal conductance levels were obtained for the beginning and end of each list presentation. As the list presentation time was only twenty seconds these were normally nearly identical. A mean basal conductance level, in micro-mhos, was obtained and its log. taken to obtain log. of the basal conductance level which is the basal measure which most nearly conforms to a normal distribution.

The mean log. basal conductance values were then correlated, using Pearson product moment method, with the number of words recalled and one highly significant correlation was obtained. It was found that there was a negative correlation of $-.57$ ($P < .01$) between log. basal conductance and number of words recalled from the uncommon word list. Individual results are given in table 7.

Table 7 - Recall of Uncommon Words

Subject	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
Log Base Conduc.	201	216	235	222	190	157	163	226	233	235	175	181	149	153	152	154	168	179	210	188	$M=189 \sigma = 0.18$
No. Words	7	6	7	6	9	12	10	12	5	9	7	7	11	9	12	13	10	7	9	9	$M=8.9 \sigma = 2.4$

$r = -.57$ (significant at beyond .01 level).

Because the result was totally unexpected the experiment was immediately repeated with sixteen new subjects. A correlation of $-.43$ ($P < .05$) was

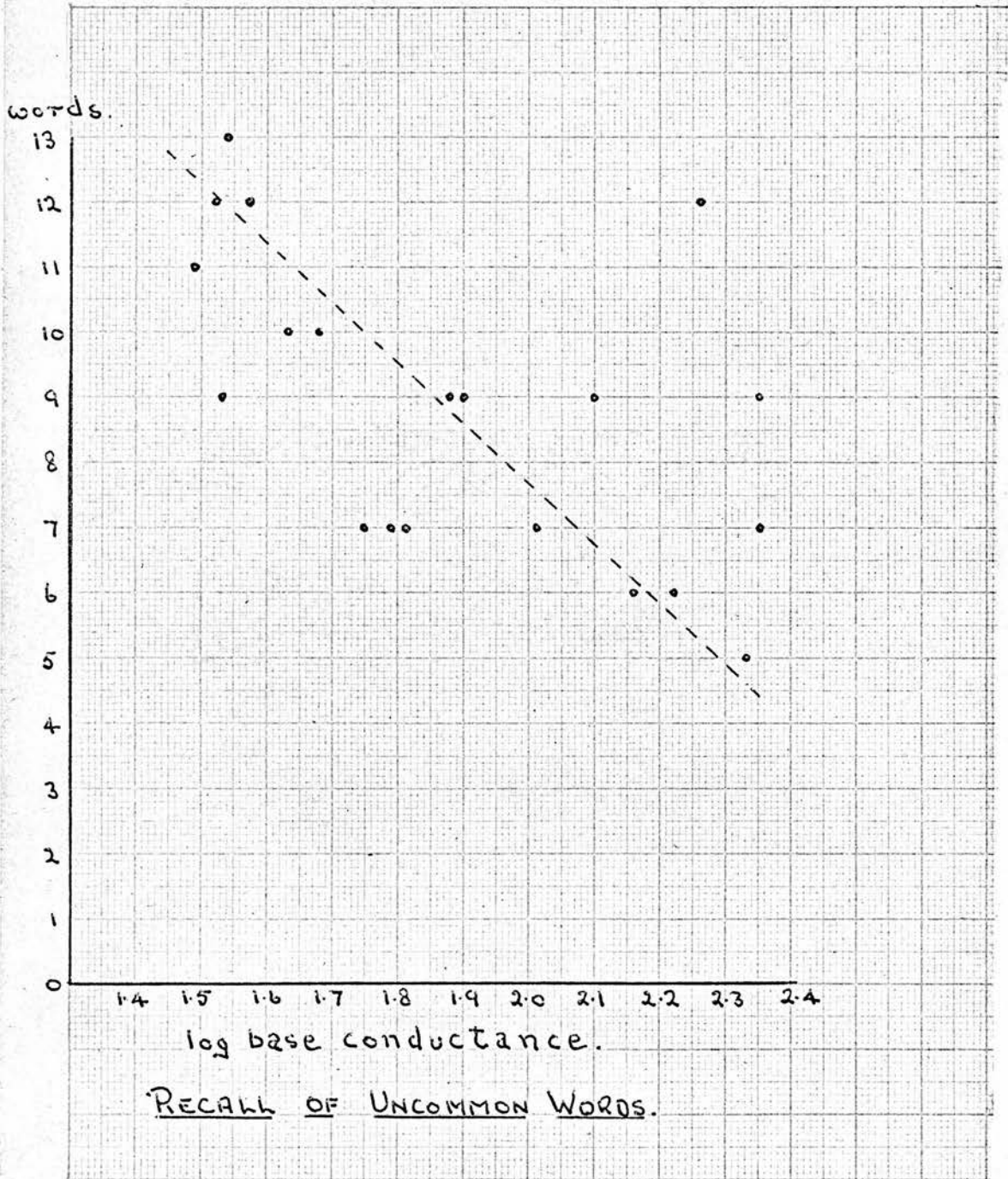
¹

A glance at Table I will place this experiment in the context of the complete study.

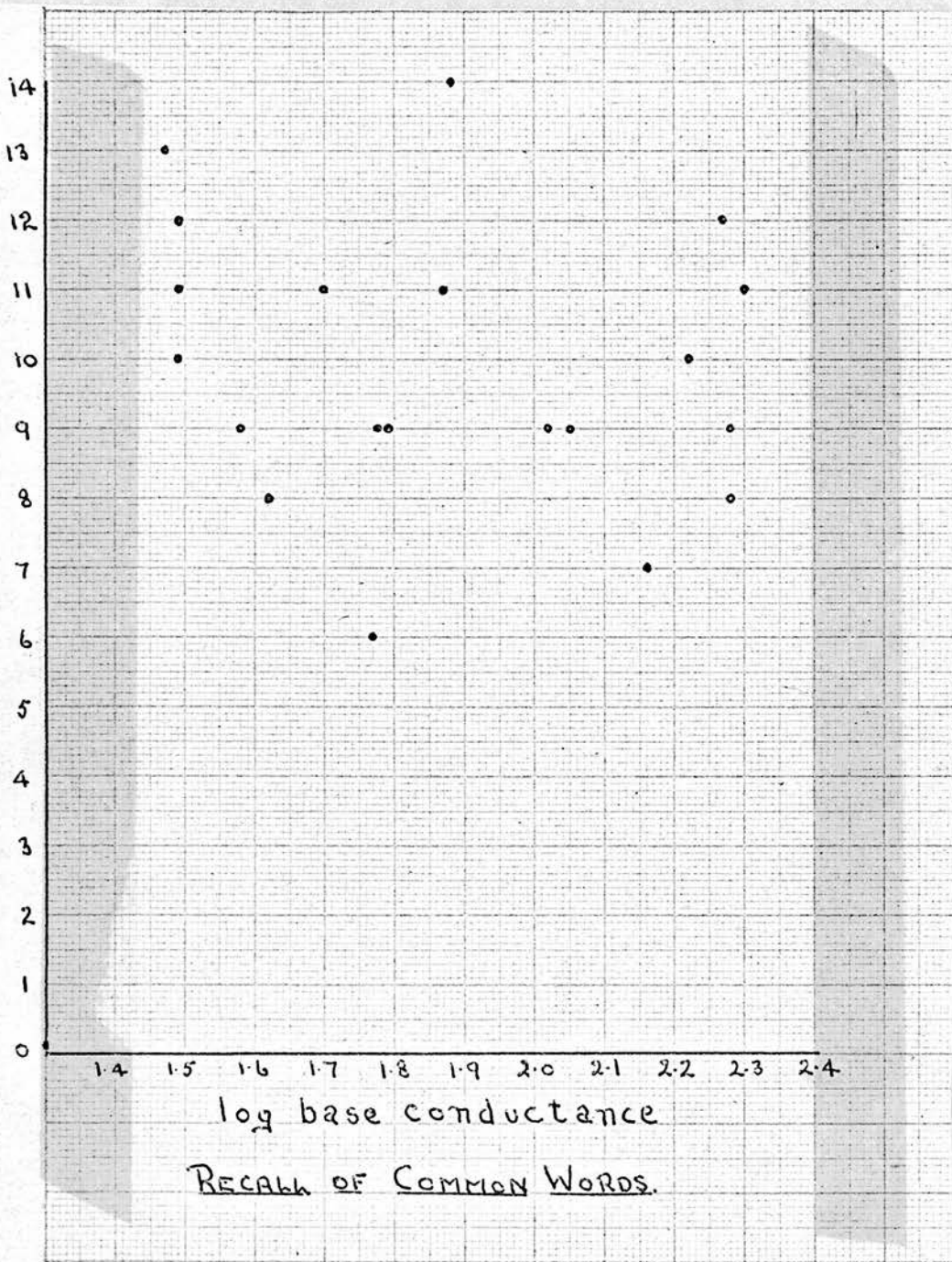
obtained between log. basal conductance and number of uncommon words recalled. Thus the original result was confirmed.

A graph of the original results is included (graph i). This graph in no way suggests any significant departure from a negative linear relationship between log. basal conductance and number of words recalled.

graph i.



GRAPH II



log base conductance
RECALL OF COMMON WORDS.

No other significant relationships were observed.

Discussion: This result is very difficult to explain. On the basis of the original hypothesis one would have expected a positive correlation between basal conductance and recall. At worst one might have expected an insignificant result. The result that was obtained, at least at first sight, seems to turn activation theory on its head.

Assuming no prior theoretical bias it could be said that the result indicates that high conductance (i.e. high activation) subjects find more difficulty in coping with a list of very unusual words but that this relative inability is not apparent when the words are common. If we assume that learning the uncommon words is a more difficult task than learning the common¹ then we might generalise this conclusion and say that high conductance subjects perform relatively worse than low conductance subjects with increasing task difficulty.

The actual processes involved are, of course, obscure but some light is shed upon the problem by the observation that the ten highest conductance subjects tended to recall more words from the second half of the uncommon word list (average serial position of recalled words 12.3, as compared with 10.2 for the ten lowest conductance subjects), thus indicating that the sheer novelty of the words used, or the sheer unexpectedness of the situation, adversely affected their performance at the beginning of the list. Once they adjusted to the situation their performance was no worse than that of the low conductance subjects. It could, on this basis, be suggested that the high conductance subject is slower to adjust to a novel situation. But this sort of generalisation, on the basis of one experiment, is precisely what has led activation theory into its present confused state.

¹ This is partly confirmed by the average number of words recalled by all subjects from each list:

List 1 (common)	9.9 words
List 2 (uncommon)	8.8 words

One other possible explanation must, however, be considered as it is based upon one of the central pillars of the activation edifice. This is an explanation in terms of the inverted U-shaped relationship, first advanced by Yerkes and Dodson (1908), between activation and performance, which is suggested in all general studies of the subject (Malmo 1959, Duffy, 1962; also see Introduction). The argument starts from the premises (i) that performance will improve up to a certain optimum activation level and at higher levels it will decline, (ii) that the more difficult the task the lower is the optimum activation level.¹ On the basis of these two statements, it could be said that the optimum activation level for the difficult task of learning uncommon words is low, in fact somewhere around the lowest level of the group tested, and that the individuals above this level will tend to perform worse. This is a temptingly simple explanation but it will not stand close scrutiny. In the first place it is extremely unlikely that the lowest activation levels in the group would happen to coincide with the optimum activation level for the task. It is theoretically possible but it seems to be too conveniently arranged to conform to this rather glib explanation. A more firmly based criticism is that no explanation is offered for the totally insignificant correlation between basal conductance and recall of uncommon words. If the inverted U-shaped relationship is invoked to explain the results of one part of the experiment then it should be capable of explaining the other results. As we presume the learning of common words to be an easier task than the learning of uncommon words we would expect to find either that there is a positive correlation between conductance and recall or that there is a marked curvilinear relationship with the optimum conductance level being higher than for the

¹ There is a third premise, (iii) that the more practised the individual the greater the range of activation level at which he will be optimally effective, but this is not important in relation to the argument above.

more difficult task. Certainly the latter alternative might explain the lack of significant correlation, but a study of graph ii which is the graph relating conductance level to the recall of common words, will indicate no sign of any such curvilinear relationship.

There has been a marked tendency, particularly in studies of the relationship between manifest anxiety and learning (see section c below), to use the concept of the inverted -U relationship to explain away any results in activation studies. Just about any observed relationship between activation and performance can be explained by using the appropriate part of the inverted -U. While, in certain areas of activation studies, such a relationship might hold, it must be very much more explicitly defined before recourse to it as an explanatory concept can be regarded with anything but suspicion.

All this leaves the result of this experiment very much unexplained. In the meantime it must be left in this unsatisfactory state but some attempt will be made to cope with it later in this section and in the wider context of the final chapter of this thesis.

(ii) Basal Skin Conductance and the Pursuit Rotor

Introduction: The findings reported in Experiment i(b) above posed some questions about what it is that basal skin conductance represents. It was decided to carry out a series of experiments to explore the relationship between basal skin conductance and some apparently straightforward performance tasks in the hope that a clearer picture would emerge.

The first task chosen was the pursuit rotor. In his 'Dynamics of Anxiety and Hysteria' (1957) Eysenck describes the use of a pursuit rotor to study the effects of massed and distributed practice, relating the superiority of the performance in distributed practice to the

dissipation of negative drive (D-), which accumulates during practice, in the rest periods. It was felt that if activation is synonymous with generalised drive then high activation subjects, starting at a higher drive level, would continue to improve for a longer period during practice sessions. It was thought high drive subjects would show greater reminiscence after a rest period, as they had more negative drive to dissipate during the rest. This latter point was made by Eysenck and Maxwell (1961) and demonstrated by Eysenck (1963) in a study reported after the experiment to be described here had been completed.

The basic aim of the experiment was therefore to study the extent to which the level of generalised drive can be related to activation level as measured by basal skin conductance. If the two are virtually synonymous then we should expect that:

- a) In a series of relatively massed trials subjects with high basal conductance will continue to improve from trial to trial to a greater extent than low conductance subjects (Greater initial D+).
- b) High conductance subjects should have larger reminiscence scores after a rest period (more D- to dissipate, or faster dissipation).
- c) As a consequence of a) and b) above high conductance subjects should end a series of trial sets with greatly superior performance to that of low conductance subjects.

An alternative hypothesis about the dissipation of D- might lead to a modified prediction. If D- dissipates evenly throughout the rest period and the rest period is not long enough to allow for total dissipation, then the amount of dissipation might be equal in high and low conductance subjects and no differences in reminiscence scores would be observed. In

fact Eysenck (1963) implies that dissipation is slower in low drive subjects¹ and we would therefore expect our initial hypothesis to hold. On the basis of the Eysenckian definition of high generalised drive we would, under no circumstances expect higher reminiscence scores from low conductance subjects if, in fact, basal conductance level is an index of generalised drive.

Once again the predictions were to be confounded.

Apparatus: The apparatus used to measure basal skin conductance was as described in Appendix I.

The pursuit rotor consisted of a gramophone motor and turntable, mounted in a wooden box and geared to run at 60 revolutions per minute. A black plastic disc, twelve inches in diameter, was mounted on the turntable and a brass disc, half an inch in diameter, was set into the plastic disc, the centre of the brass disc being four and a half inches from the centre of the motor spindle. The wand used by the subject consisted of a ten inch metal rod with a four inch plastic handle covering one end. From the other end hung a three inch long tightly wound spring, covered in insulating material, with a brass stud, $\frac{11}{8}$ an inch in diameter, attached to its lower end. The rotor and wand were so wired that when the end of the wand and the brass disc came into contact a circuit was completed which started a clock. As soon as contact was broken the clock stopped. The clock could readily be read to an accuracy of one hundredth of a second. An accurate stop watch was also used.

Procedure: Skin conductance was measured before and after each full block of trials on the pursuit rotor. This was done while the subjects were read a short list of words. These conductance measures were

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He uses the extreme case of schizophrenics and so generalisation is dangerous.

averaged to give a mean conductance level during the trials. For reasons already outlined in Experiment i(b) above this was not an ideal method, as the time between making first and last measures was approximately twenty-seven minutes. Unfortunately, attempts to measure conductance during the trials proved abortive. The electrodes constantly became displaced in the middle of trials resulting in recording pen breakages and cessation of the trials. All attempts to make the palmar electrodes more secure ended in failure.¹

Pursuit rotor performance was measured during three sets of five trials each, giving fifteen trials in all. The procedure for each subject was as follows. Before the first set of trials subjects were shown what they had to try to do, namely hold the end of the wand on the brass disc while the disc was rotating. They were told that each trial would be followed by a short break, during which the disc would continue to rotate, and that at the end of five trials they would be given a five minute rest before the next series of trials. They were then given the wand and the motor was switched on. One second before they were due to start they were given a 'ready' signal and then told to begin. The stopwatch was started at the moment the word 'begin' was uttered. A trial lasted for thirty seconds, the words 'stop now' being uttered when the thirty second period was completed. There then followed a thirty second break during which the subject remained standing in front of the still rotating disc. During this period the experimenter noted the time on target and reset the recording clock. This continued for five trials. At the end of the

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The problem of electrode fixing for measurement of basal conductance and conductance changes during active tasks proved insoluble during the series of experiments described in this thesis. This was a severe limitation. The problem could probably be solved by using electrodes attached to the sole of the foot and held in place by pressure of body weight. Attempts in this direction have proved promising, but the practical difficulties with normal subjects are considerable. See Appendix IV for a full analysis of procedural difficulties.

fifth trial the rotor was switched off and the subject was offered a chair. The rest period lasted for five minutes during which experimenter and subject indulged in general conversation. Questions about the nature and aims of the experiment, which were almost inevitable, were evaded at this stage. Subjects were given an account of the experiment only when the session was completed. At the end of the five minute break the procedure outlined above was repeated for a further five trials and again, after another five minute break, for a further five trials.

Apart from actually seeing the end of the tracking wand in contact with the target disc the subject could also affirm that contact had been successfully made by listening for the sound of the recording clock which made a readily audible sound when running. There is evidence to show that auditory confirmation of successful target tracking produces superior performance more readily than when no such confirmation is present (Reynolds and Adams, 1953).

Subjects: Twenty-one male subjects were used. All were volunteers from the first year Psychology class. The pursuit rotor was completely new to all of them and none of them had had his skin conductance measured previously. Their ages ranged from 19 to 27.

Results: Basal conductance was measured in the way indicated above. The mean of the two readings was calculated and converted to log. of basal conductance.

Three measures of pursuit rotor performance were taken.

- a) Average improvement (in seconds) from fourth to fifth trials
in set one and two.¹

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Series three was not included as several subjects complained of physical fatigue at the end of this series and this might have introduced an irrelevant factor into the experimental results. This also accounts for the use of trials 2 and 3, rather than 4 and 5 or 3 as an index of optimum performance.

b) Average reminiscence (in seconds) between set one and two and two and three.

c) Average score in trials 2 and 3 of set three.¹ (p.76)

The results are summarised in Table 8.

Table 8

Table 8 - Pursuit Rotor

Subject	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	
Log Base Con.	202	187	216	210	235	177	216	143	190	168	149	148	163	152	227	153	233	149	228	179	175	$M = 1.86 \sigma = 0.31$
Increment (Mean)	+08	+01	+14	+04	+07	+10	+26	+28	+17	+68	+150	+34	+02	+18	+23	+25	+23	+72	-20	+34	+43	$M = 3.04 \sigma = 3.44$
End Change (Mean)	+20	+13	+30	+07	-03	+10	-20	+12	+24	-19	-10	-36	-27	-39	+17	+09	+20	-16	+30	+18	-16	$M = -0.6 \sigma = 2.7$

$r_{LBC/INC} = - .50$ (sig. at beyond .05 level)

$r_{LBC/EC} = + .54$ (sig. at .01 level)

Two significant correlation coefficients were calculated. These are:

Between log. basal conductance and average improvement
(a) + .54 ($P < .01$)

Between log. basal conductance and reminiscence
(b) - .50 ($P < .05$)

The correlation between basal conductance and final performance
(c) was insignificant.

Discussion: These results hardly conform to the predictions made in the introduction to this experiment. It appears that the superiority gained by a high conductance subject within a set of trials is offset by the greater reminiscence scores of low conductance subjects, leading to a similar overall performance level. If drive level is taken to be related to skin conductance this latter result is completely contrary to the results published by Eysenck (1963). Even if it is argued that the initial work period was rather short (four and a half minutes observing the target, of which two and a half minutes involved actual tracking) one would hardly predict, from his results, that the reminiscence scores would favour the low conductance group to a significant extent.

The prediction that the scores of high conductance subjects would continue to increase throughout a series of trials was confirmed. This is an observation which is suggested but not confirmed by Feldman (1964b), but it is one which is not difficult to assimilate or explain.

It would be tempting simply to state, on the basis of the contradiction between these and Eysenck's findings, that basal conductance is not a measure of generalised drive. Unfortunately in a recent publication Eysenck and Warwick (1964) go so far as to assert that 'the state of conductivity of the skin appears to constitute an acceptable measure of the organism's drive state'. They base this statement upon measures of skin resistance

in kilohms taken from two groups differentiated on the basis of the degree of situationally induced drive.¹ Faced by this statement it is difficult simply to ignore the problem of the relationship between basal conductance level and drive and, in fact, it seems reasonable to consider it in very much more detail.

The theory underlying Eysenck's approach to the question of drive level and reminiscence is fully expounded by Feldman (1964b). It can be expressed in the neo-Hullian formula:

$$S^E_R = f(S^H_R - S^I_R) \times f(D - I_R)$$

That is, performance, at a given time, is a function of positive habit strength less negative habit strength (in other words, a function of practice and, possibly, ability) times positive drive level minus negative drive level. Positive drive level is taken to be the drive associated with the task, which might be all drive active in the testing situation.

I_R - negative drive level - is taken to be some form of central fatigue which accumulates with repetition of the response. When $I_R = D$ performance level is theoretically zero. There is, therefore, a block in activity which allows I_R to dissipate partially, thus giving rise to further activity when I_R again builds up to equal D , leading to a further block, and so on. With a prolonged rest I_R dissipates completely producing the reminiscence effect. When D is high initially more I_R will accumulate during practice, (I_R can never exceed D but will obviously reach higher levels when D is high) and, therefore, with high D subjects one would expect greater reminiscence because more I_R had dissipated during rest. Feldman implies that

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One group thought they were undergoing selection by test (High Drive), the other simply that they were acting as experimental subjects. This was also the method used for differentiating the high and low drive groups in Eysenck's experiments on the pursuit rotor. It is probably quite an effective method and is to some extent justified, if rather tautologically, by the observed differences between the groups.

beyond pre-rest practice periods of more than ten to fifteen minutes this theory may not apply without modification but this qualification does not apply to the present study.

If we return to the original hypothesis which this experiment set out to test, namely that basal conductance level is a measure of generalised drive which may, therefore, be almost synonymous with activation, then, taking careful account of the nature of this experiment, we can state what would have happened, assuming the hypothesis to be correct, on the basis of the above theory.

During the thirty second trials I_R would build up and probably partially dissipate during the thirty second breaks.¹ By the end of the fifth trial I_R would have reached a level either less or equal to the D level in the low conductance subjects. It would certainly have reached a level less than D in the high conductance subjects. This is based upon the observation that it takes from ninety seconds to two minutes of massed practice for I_R nearly to equal D in a group of relatively low motivation. In a group of high drive, according to Eysenck and Maxwell (1961), continuous practice of at least six minutes is required for I_R to build up to the level of D. Whatever the level of I_R relative to D we should expect the residual D to be greater in high conductance than in low conductance subjects at all times, with the ratio constantly increasing in favour of the high conductance subjects up to the point where $D = I_R$ in low conductance subjects when it then declines to the level of high conductance subjects. We may illustrate this by a simple numerical example.

If D is 5 in a low drive subject and 10 in a high drive subject at the start of a practice trial and I_R accumulates at the rate of one unit per

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Nowhere in the statement of the theory is the rate of dissipation of I_R suggested. It could be slower, equal, or greater than the rate of acquisition. From a graph by Eysenck and Warwick (1964) it is assumed that it is slower but the assumption is insecurely founded.

minute for both subjects during practice then, at the start, HD will be twice LD, after three minutes HD will be three and half times LD, after four minutes HD will be six times LD, but after ten minutes HD will equal LD as both will be fluctuating around zero. From this it follows that the performance of high drive subjects should be consistently relatively better than that of low drive subjects up to the point where $I_R = D$ in both cases. This is quite explicit if the function $(D - I_R)$ is to have any meaning in the general formula quoted above.¹

Still assuming the initial hypothesis to be valid we can make two assumptions about the level of I_R at the end of the five practice trials in each set of the experiment. Either I_R for low conductance subjects is the same as for high conductance subjects, neither having reached maximum, or it will be higher in high conductance subjects, maximum for low conductance subjects having been reached at ~~some~~ time prior to the end of the set of trials. In the first case reminiscence scores, assuming uniform dissipation of I_R for all subjects, will be equal; in the second case reminiscence scores for high conductance subjects will be higher than for low conductance subjects.

Reference to the results of the experiment indicate the tendency for scores to diverge, relative to basic performance, in favour of the high conductance subjects, particularly towards the end of a series. This is entirely according to prediction. We would not necessarily expect absolute scores to be higher. These depend also upon the function $(S_R^H - S_R^I)$, but we would expect divergencies in the direction observed. Unfortunately the reminiscence scores are completely inexplicable in terms of the theory.

¹

The extraordinary feature of the studies by Eysenck (1963) and Feldman (1964) is that this quite obvious prediction is not confirmed. This feature is recognised by both but no really satisfactory answer to the anomaly between prediction and results is advanced.

Under no circumstances, except the possibility of more rapid initial acquisition of I_R by low conductance subjects, would we expect higher reminiscence scores for those subjects. This possibility is not envisaged in any of the theoretical interpretations by Eysenck and Maxwell (1961) or Feldman (1964b).

In an attempt to clarify the situation a further experiment was carried out. Twenty-six male subjects were used and the procedure was similar to that of the first experiment except that practice was continuous for four and a half minutes, scores for each thirty second period being measured. For technical reasons the speed of rotation was reduced from sixty revolutions per minute to forty-five. The slower rotation speed may have made the task too easy. Whatever the reason the results were inconclusive. The correlations between conductance level and increment in performance from 7th to 9th and 8th to 9th trials were totally insignificant while the correlation between conductance level and reminiscence effect was an insignificant $-.17$, in the same direction as in the previous experiment, but probably meaningless, although, certainly on the basis of the theory described by Feldman, one would have expected a marked positive correlation after four and a half minutes of genuinely massed practice. In this respect the experiment did have some significance.

Before concluding, further reference must be made to the reported relationship between skin conductivity and situationally induced drive (Eysenck and Warwick, 1964). If this is valid then one must accept some identity between basal conductance level and generalised drive and assume at best that the neo-Hullian theory outlined above is severely limited in its application. If this relationship is doubtful then we might assume that basal conductance is no measure of generalised drive and that activation

and drive cannot be as simply identified as Malmo (1958) has suggested. This latter conclusion, of course, assumes some general validity for the theory relating I_R and reminiscence.

The only figures quoted by Eysenck and Warwick to substantiate their claim are given below:

Table 9

	I		II		
	Mean	S.D.	Mean	S.D.	
High D	86.88	53.55	92.54	58.64	N = 50
Low D	95.96	116.54	104.81	90.19	N = 50
	t=1.5474	(P<0.10)	t=24912	(P<0.027)	

MEAN LEVEL OF SKIN RESISTANCE FOR LOW AND HIGH DRIVE GROUPS

These figures, at first glance, appear strange and so they were recalculated. t values of 0.4962 (insignificant) and 0.7968 (insignificant) were obtained for conditions I and II respectively. One forbears to comment on the standard of statistical computation at the Institute of Psychiatry.

This finding, combined with the more or less negative relationships observed between basal conductance level and reminiscence must lead to the rejection of the idea that basal conductance is a measure of generalised drive, certainly when that drive is situationally induced.

The fact that conductance is related to the relative performance of subjects towards the end of a series of trials and that it is, at least under certain circumstances, negatively related to reminiscence, does indicate that it has some significance as a measure. But before considering that significance two more experiments involving basal skin conductance, must be described.

(iii) Basal Skin Conductance and Rapid Dotting

Introduction: There was no very complex rationale behind this experiment. The results of the experiment reported above remained something of a puzzle and a general exploratory experiment was carried out to see if basal skin conductance related either to scales of manifest anxiety or neuroticism or to scores on examinations or intelligence tests (see Experiment i(c) below). When the results from these, at least as far as basal skin conductance was concerned, proved insignificant it was decided to reconsider the relationship of conductance and motor performance. Having already carried out two experiments using the pursuit rotor, one of them, admittedly, inconclusive, it was decided to select a different task. The criteria of choice were that it should require continuous concentration and persistence over a period of time and that it should be easily and objectively scored. The idea for the task eventually devised came from an old piece of apparatus discovered during a removal and called by some of the more senior members of the Department -- 'The McDougall Dotting Test'. This consisted of a spindle mounted roll of paper covered with randomly arranged red dots, a cover with a small slot in it, and the mounting for an absent motor. The aim of the task was obviously to see how many dots a subject could hit with a pencil as they passed the open slot. One could presumably count the number of dots hit in a given time at a given paper speed and assign a score for hand-eye co-ordination, or something of the sort.

Rather than use the old apparatus which, apart from being in a dilapidated state, obviously required the experimenter to count hits by inspection, it was decided to construct a more up-to-date modification. The basis of this was an old 4-channel E.E.G. paper drive. This was mounted at an angle

of 20° from the horizontal in a wooden box with the metal paper runner almost flush with the top of the box. The box was covered with a lid which had a slot $3\frac{1}{2}$ " wide and $\frac{3}{8}$ " deep cut in it to expose a part of the paper runner. The runner was wired to a 12 volt battery which was wired to an impulse counter which was in turn wired to a metal stylus. When the stylus was touched to the metal paper runner the circuit was completed and the counter registered the contact. In operation the paper drive was fed with E.E.G. paper with $\frac{1}{8}$ " diameter holes punched in it. The aim of the task was to hit as many holes with the stylus as possible. Each hit was registered on the counter; misses were not registered. On trial runs this task seemed to be one which demanded constant attention and was challenging enough to grip the interest of the subjects.

The basic aim of the experiment was to study the relationships between basal skin conductance and overall performance level, increments between sets, increment from middle to end of a set of trials, and any other performance measures which might appear relevant. Having been possibly over-involved in theoretical postulates in the previous experiment on skin conductance and motor-skill performance it was decided to indulge in a more Skinnerian approach to data acquisition. At this stage it was by no means clear what basal skin conductance did relate to and this experiment was aimed at finding out more about the phenomenon rather than at testing any formulated theory. Obviously the author had certain expectations but these were vague rather than explicit. Two experiments which had contradicted more or less precise predictions had made him wary of premature theorising.

Apparatus: The apparatus for measuring basal skin conductance was as described in Appendix I.

The rapid dotting apparatus has already been described above. The paper used consisted of a length of E.E.G. paper 4" wide and twenty 12" segments long, plus a blank lead-in segment. The first three segments were perforated with 180 $\frac{1}{8}$ " diameter holes, 60 holes per segment, randomly arranged. This was followed by a blank, unperforated segment, which was followed by three perforated segments, which were again followed by a blank segment, and so on. Each length of paper consisted, therefore, of five sets of 180 perforations spread over three segments each, with a blank segment coming between each set. Thus each length of paper, when running, presented to the subject five trials with a short interval between each trial. The length of trial and interval would depend upon the speed of the driving motor. The speed selected for this experiment was one inch per second; in fact the motor ran rather more slowly and each trial lasted forty seconds with a thirteen second interval between trials.

Procedure: Basal skin conductance was measured before and after the rapid dotting trials. The method has already been described and the same limitations apply as in Experiment ii(b) above.

For the rapid dotting task the subjects were instructed as follows:

'This task is to study the speed of your hand-eye co-ordination. A short time after I start the motor you will begin to see holes passing beneath this slot. I want you, with this stylus (hand them stylus), to try to hit as many of these holes as possible. You will not be able to hit them all but try to hit as many as possible. When you hit a hole you will hear a sharp click. That will be the counter recording a hit. I will be listening and watching for double hits, so do not try to hit one hole more than once. There will be five trials in each set with a short break between each trial. At the end of the first set of trials

there will be a break of five minutes. This will be followed by a second set of trials, another break, and a third and final set of trials. Is that clear? Now get ready to begin'.

The trials proceeded in the way indicated in the instructions. It was possible to record double hits by listening for a characteristic double click of the impulse counter and an appropriate correction was made to the score for each trial.

During the five minute rest breaks subjects were given a card sorting task while the experimenter loaded the paper for the next set of trials. Subjects: 27 subjects were used, 12 male and 15 female. All subjects were from the second year Psychology class and their ages ranged from 19 to 37, though only two subjects were older than 23. Eleven subjects had had skin conductance measures taken before but all were, of course, unfamiliar with the rapid dotting task.

Results: Correlation coefficients were calculated for the following relationships.

- (i) Basal skin conductance and mean performance on set 1 trials.
- (ii) Basal skin conductance and mean performance on set 2 trials.
- (iii) Basal skin conductance and mean performance on set 3 trials.
- (iv) Basal skin conductance and mean increment from trial 5 in sets 1 and 2 to trial 1 in sets 2 and 3.
- (v) Basal skin conductance and mean increment from trial 4 to trial 5 in all sets.

The following coefficients were found:

- (i) + .38 - ($P = .05$)
- (ii) + .59 - ($P < .01$)
- (iii) + .53 - ($P < .01$)
- (iv) + .05 - (N.S.)
- (v) + .26 - (N.S.)

Significant scores are summarised in Table 10.

Table 10

Table 10 - Rapid Dotting Task

Subject	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25		
Log Base Con.	220	220	188	212	168	163	195	163	168	182	201	168	189	168	195	191	192	156	147	81	98	194	208	161	178	198	$M = 184.6$ $\sigma = 0.20$
Mean Set 1	492	743	363	783	665	085	024	984	764	305	823	165	027	265	984	887	603	802	284	492	780	364	348	508	512	$M = 49.60$ $\sigma = 14.0$	
Mean Set 2	640	818	466	432	456	489	560	520	482	442	624	105	026	586	206	108	888	396	334	540	768	572	382	594	652	$M = 53.40$ $\sigma = 13.4$	
Mean Set 3	616	824	450	454	614	510	560	532	502	546	616	422	536	598	706	590	878	468	436	560	738	614	493	550	700	$M = 58.00$ $\sigma = 11.4$	

$r_{LBC/S.1} = + .38$ (sig. at only .1 level)

$r_{LBC/S.2} = + .59$ (sig. at beyond .01 level)

$r_{LBC/S.3} = + .53$ (sig. at beyond .01 level)

The average scores for each set of trials were as follows:

Set 1 49.6

Set 2 55.4

Set 3 58.0

There was no evidence of any reminiscence effect between sets of trials.

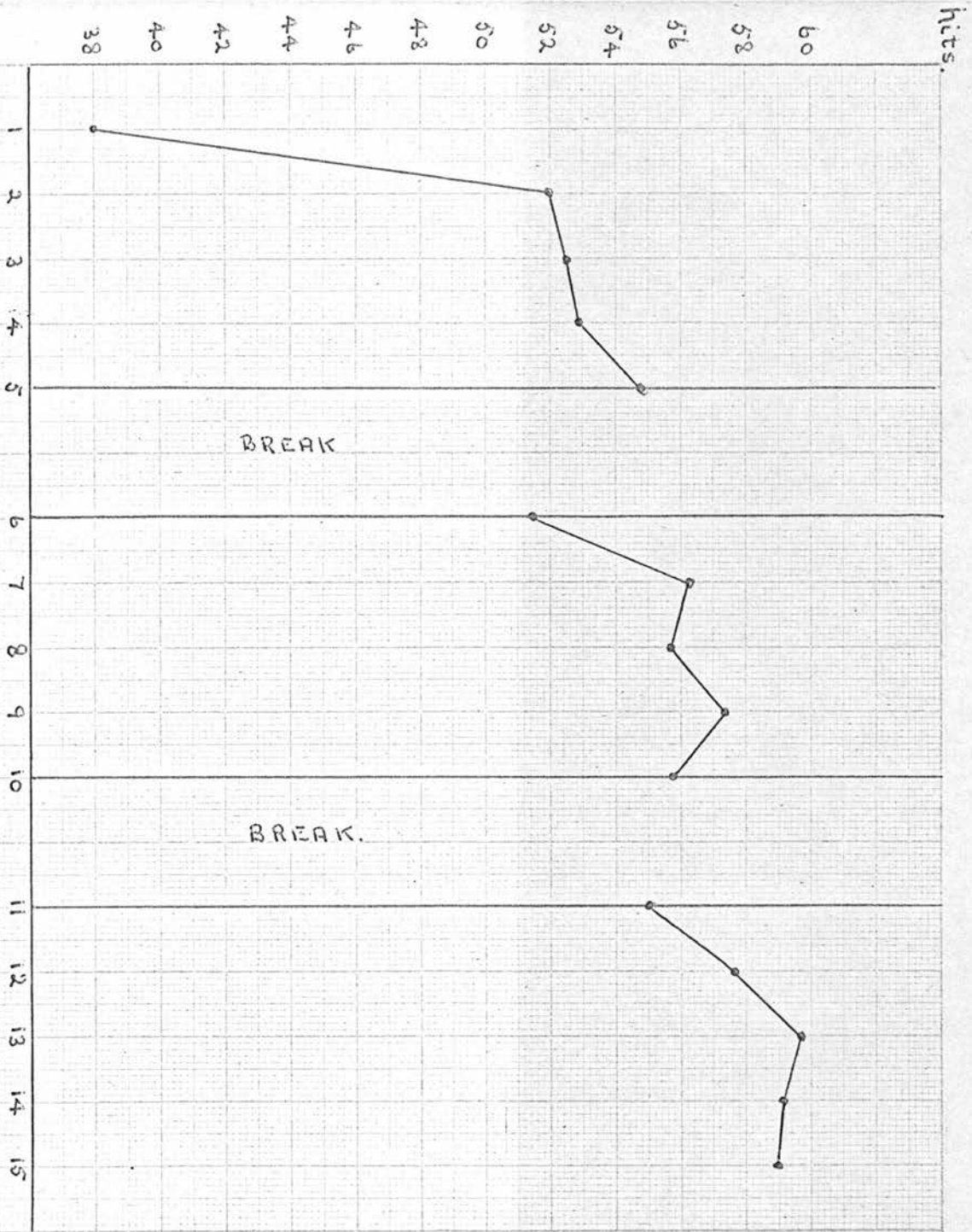
In fact there was a mean decrement of 2.2.

Discussion: The results indicate a marked relationship between basal conductance and absolute level of performance in this task. This contrasts with the results found on the pursuit rotor where only relative performance seemed to be related to basal conductance. The explanation for this difference must obviously lie in the nature of the two tasks.

The dotting task involves little learning. As can be seen from the mean scores for each set there is comparatively little improvement in performance after the first set of trials. If the first trial in the first set is omitted the mean score per trial is 53.0 for that set. This tendency for there to be only very slow improvement after the initial trial is illustrated in graph iii where the mean scores per trial are plotted. This can be compared directly with graph iv where the mean scores per trial in the first pursuit rotor experiment are plotted.

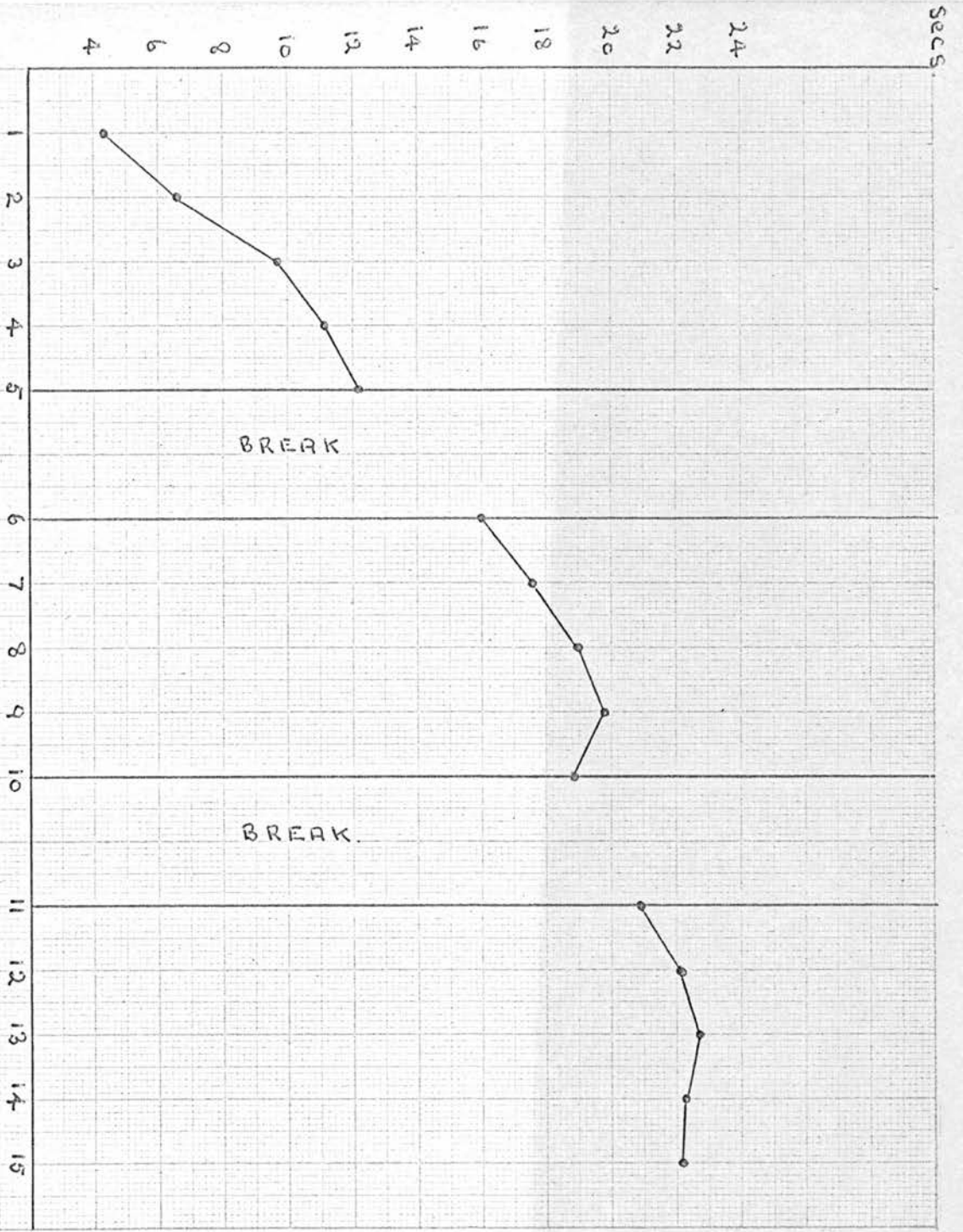
LEARNING CURVE - RAPID DOTTING.

Trials



LEARNING CURVE - PURSUIT ROTOR

Trials



These two graphs illustrate as effectively as is possible the difference between the two tasks. While the acquisition curve for the pursuit rotor illustrates the gradual achievement of effective performance over thirteen trials, with only one set-back in performance at trial 10, the curve for the rapid dotting task shows that almost complete mastery is achieved in the first trial. After that trial improvement is very gradual and erratic.

These differences are obviously a function of the novelty and complexity of the motor movements involved in the tasks. The rapid dotting task makes little demand upon the individual as far as the learning of a complex new pattern of muscular co-ordination is concerned. The movements required are completely familiar and once the speed of the paper movement has been assessed, which obviously occurs in the first trial, the subject's performance will obviously depend upon the amount of attention he is prepared or able to allow to the task and the determination with which he attempts to hit each target. The pursuit rotor is quite different. Attention and determination are effective only once a fairly novel and quite complex pattern of movements has been mastered.

This leads one to an obvious, if rather vague, conclusion: that basal skin conductance is related to attention and/or determination. In the rapid dotting task these appear to be the primary requirements and therefore, once the very basic skills required have been mastered, the absolute level of performance is significantly related to basal skin conductance. With the pursuit rotor attention to the task is only relatively effective in relation to the subject's basic mastery of the complex motor skill involved. This shows up in the significant correlation between performance improvement at the end of a series of trials and basal skin conductance (see Experiment ii above).

This is a very inadequate explanation as the underlying problems are obviously very complex. It must, however, serve as an interim statement. The next experiment to be reported is also relevant to the problem, and the question of the nature and significance of basal skin conductance will be returned to and considered in much more detail in the final Chapter of this thesis.

Before going on to the next experiment, however, the problem of reminiscence must be considered. The rapid dotting task is one in which one would expect reminiscence to occur, and yet the opposite effect, a decrement in performance, is apparent when we look at graph iii. This throws some doubt upon the interpretation of reminiscence being due to the dissipation of I_R . One would have expected the relatively massed practice condition of this rapid dotting experiment to be ideal, if that is the word, for the build-up of I_R and one would certainly have predicted a large reminiscence effect. It is, of course, conceivable that I_R , if it in fact exists, has nothing to do with reminiscence. It may well be that in a complex task, such as the pursuit rotor, a large number of inappropriate responses develop during a massed practice session and that, during the rest pause, these tend to die out. This suggestion is, of course, not original. It has been advanced as an explanation of reminiscence for almost as long as psychologists have been aware of the phenomenon (McGeoch and Irion, 1956). It does, however, serve to explain the differences which occur between sets in the pursuit rotor and rapid dotting experiments. In the rapid dotting test the response pattern is so simple that one would hardly expect inappropriate responses to arise. The rest pause then simply becomes a forgetting period, although, as one would expect, the forgetting is very slight. This interpretation of reminiscence might also

explain the rather puzzling negative correlation which was found between reminiscence and basal skin conductance (see Experiment ii above). If there is a relationship between skin conductance and what we shall call, for the present, attention, then one might expect those subjects who are attending more closely to the task to develop fewer inappropriate responses. Their relatively superior performance at the end of a set of trials seems to indicate this. Having developed fewer inappropriate responses they will benefit less from a rest pause,¹ and, therefore, their reminiscence scores will be lower.²

(iv) Basal Skin Conductance and Reaction Time

Introduction: Having arrived at a tentative conclusion about the psychological significance of basal skin conductance, namely that it is related to 'attention', it was necessary to test this in a new experimental situation. The differences observed between the pursuit rotor and rapid dotting situations had indicated that the relationship between task and conductance might be quite specifically determined by the precise nature of the task. Premature generalisation in a situation like this is inadvisable.

The choice of reaction time, although now rather obvious, was, at the time, somewhat fortuitous. The author was still interested in the nature of the G.S.R. (see section a) above) and he suggested to an undergraduate

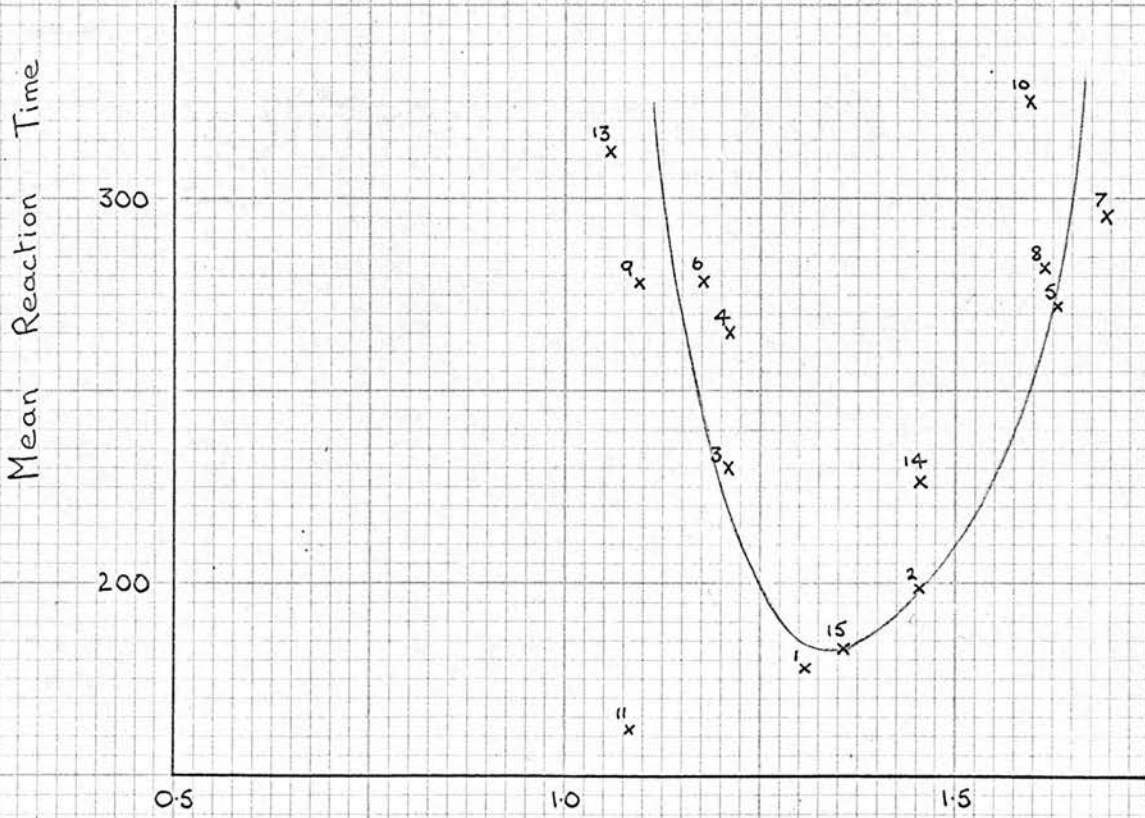
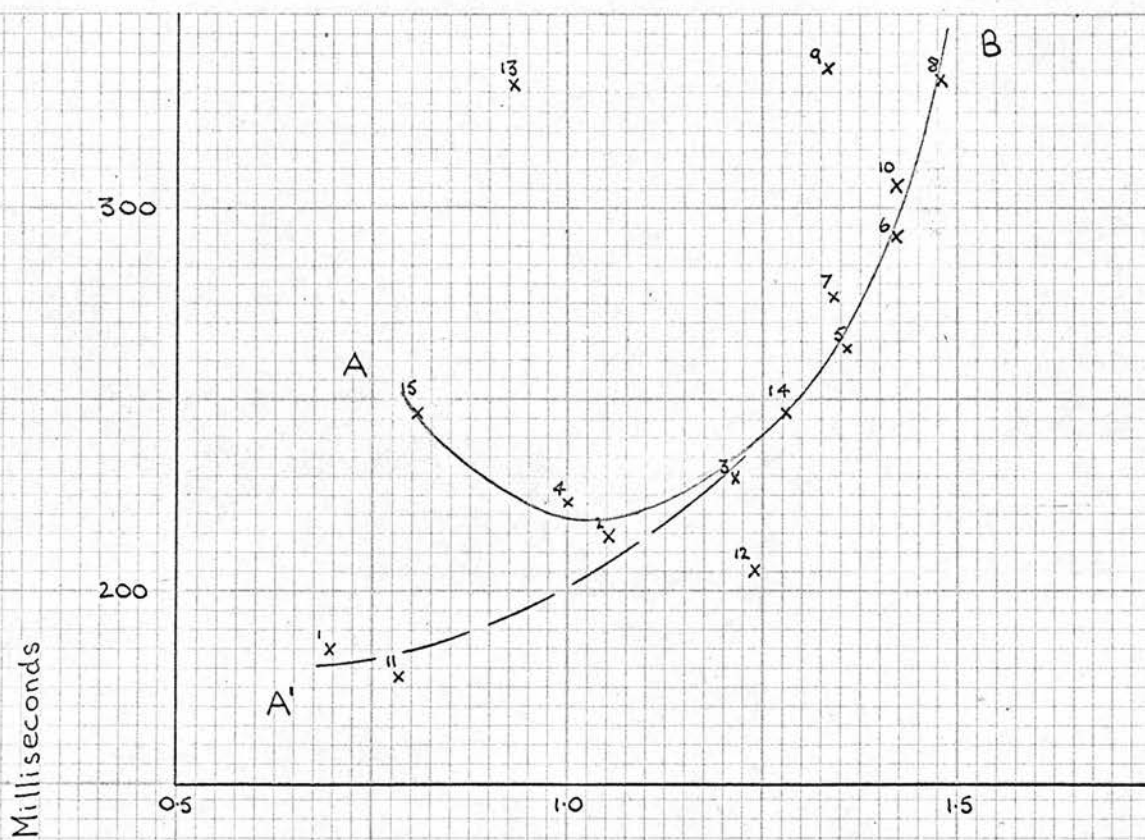
¹ They may not, even with this complex task, benefit at all. One high conductance subject had a mean decrement of 2 seconds.

² If this hypothesis is more than simply ingenious it makes nonsense of the Eysenckian equation of drive and reminiscence, at least in terms of the theory used by Feldman (op.cit.). It may be, of course, that high drive subjects develop more inappropriate responses during periods of massed practice. This would further emphasise the distinction between drive and conductance level, and therefore, almost certainly, between drive and activation.

student that he might study the relationship between the phase of the response to a warning at which the reaction stimulus occurred, and the speed of reaction. The idea being that if the G.S.R. is, in fact, a peripheral manifestation of activity in a stimulus facilitation centre, and if its rise and fall parallel, or even consistently lag behind, the activity in the centre, then fast reaction times should be consistently related to stimulation at a certain phase of the response. The results were interesting, if somewhat inconclusive, (Cowles, 1965), but, in addition to the basic aim of the experiment, some interesting relationships were observed between basal resistance and mean reaction time. The observed relationships were not exactly straightforward. Basal resistance and reaction time were measured on two separate occasions. Resistance was measured at the same time as reaction time was being tested. On the first occasion a correlation coefficient of $+0.59$ (significant at beyond $.05$ level) between log. basal resistance (the measure used in the study) and mean reaction can be calculated from the data provided. Because there is some indication that the relationship is not linear (see ~~graph W~~) this coefficient might be an underestimate. On the second occasion the relationship is an almost perfect U curve (see also graph v.) but because this is so unlikely, particularly in the second testing situation, a correlation coefficient has been worked for the obtained data. This is $+0.49$, significant at only the $.1$ level. This, of course, assumes that the curvilinearity is fortuitous.

See graph page 96a.

Whatever the precise nature of the relationship, this experiment did seem to indicate some relationship between basal skin conductance and reaction time. On reflection this was only to be expected. A prolonged series of random reaction time trials is precisely the sort of situation where 'attention' of the sort implied above would have a beneficial effect upon performance without any confusion being caused by the introduction of complex motor movements. All the subject has to do is to concentrate on the stimulus source and press down a key as soon as he is aware of the stimulus. Because the Cowles experiment used only fifteen subjects (all male) and because basal resistance had only been measured at the beginning and end of the experimental session (the experiment was basically a study of skin resistance responses) the author decided to carry out a further experiment, the basic aim of which would be



Log Base Resistance

Figures by each plot correspond to Subject No. in TABLE A

to study the relationship between basal skin conductance and reaction time, taking into account the fluctuation in conductance level which would occur during a fairly prolonged experimental session. In other words, it was intended to study the relationship between conductance and reaction time continuously throughout the experimental sessions instead of simply working out a mean conductance level for each subject and a mean reaction time and relating these. It was also the intention to consider the incidence of abnormally high reaction times. In any prolonged series of reaction time measurements there are usually some reaction times which are markedly higher than the normal. If one assumes basal conductance to be a measure of 'attention' in this sort of situation then one would expect the proportion of these abnormally high reaction times to be higher at low conductance levels.

Two related predictions were therefore made about the outcome of this experiment:

- a) Basal conductance will be negatively correlated with reaction time. The higher the conductance the lower the reaction time.
- b) The higher the level of conductance the lower will be the proportion of abnormally slow responses and vice versa.

Procedure: Subjects were asked to wash their hands. When this had been done they were seated in front of the reaction time apparatus (for description see below) and the electrodes of the skin conductance recorder were attached. The bridge of the recorder was then balanced and left at low sensitivity until the start of the experiment. Subjects were then told:

'This is an experiment to measure your speed of reaction to a light. It is similar to the experiment you have done in class.¹ After

¹ Because of the reported effect of practice (Woodworth and Schlosberg, 1955), which is admittedly, very slight, it was decided to use experienced subjects. All subjects had previously carried out sixty simple reaction time trials in a class experiment. Skin conductance was not recorded at the original session.

I have finished these instructions, I shall say nothing more to you. You will simply hear a buzzer and at a certain, variable time after the buzzer the right hand light in front of you will come on. When it comes on I want you to press down the right hand key as quickly as possible. Hold it down until you hear the second buzzer. You should sit with your hand resting lightly on the key at all times. The series of trials will be quite long; it will last, in all, about forty-five minutes. Are you ready to begin? Right, we shall begin now'.

After reading the instructions the experimenter adjusted the sensitivity of the skin conductance recorder to a level suitable for the adequate measurement of basal conductance and the experiment began.

Subjects were given sixty reaction time trials in all. The period between warning buzzer and actual stimulus could be varied between one and six seconds in one second steps and ten trials were given at each foreperiod. The foreperiods were allocated randomly; therefore subjects did not know what interval to expect. After each reaction had been made the experimenter locked the deatron timer, at the same time sounding, automatically, another buzzer which indicated to subjects that they could release pressure on the reaction key. The experimenter then read the reaction time and the basal conductance level prior to the reaction. The recorder bridge was then rebalanced, after time had been allowed for any G.S.R., which might have occurred, to fall. The next reaction time trial then began.

As was indicated in the subjects' instructions, the experimental session lasted about forty-five minutes.

Apparatus: The skin conductance recorder was as described in Appendix I.

The reaction time unit was mounted on a 3ft. x 2ft. table divided by a 2ft. high partition which ran the width of the table. On the experimenter's side was decatron timer which could read from 0 - .9999 seconds and a control panel containing a foreperiod time selection switch with positions marked from 1 through to 6, and an activating switch. On the subject's side there was a red neon bulb mounted on the partition at eye level, and a reaction switch.

The apparatus worked in the following way. The experimenter selected the desired foreperiod (the period between warning buzzer and reaction stimulus) with the foreperiod time selection switch. This operated silently so that the subject could not count the clicks and estimate the coming foreperiod. Once the foreperiod had been selected the experimenter turned the activating switch to the 'on' position. This, simultaneously, activated a buzzer and started the timing operation. When the foreperiod had run its course, controlled by a C-R time constant circuit of the appropriate value to produce the desired foreperiod, the decatron timer started simultaneously with the stimulus light coming on. The subject then pressed his reaction switch which stopped the timer and turned off the stimulus light. As soon as he had done this the experimenter switched the activating key to the 'off' position. This, again simultaneously, sounded a buzzer and locked the timer. If the subject released his switch before the second buzzer (i.e. with the circuit still active) the timer would continue to record and the genuine reaction time would be lost.¹

1

In the actual experiment, when this happened, the trial was repeated but only after two trials had intervened with different foreperiods.

Reaction times and conductance levels were recorded on a printed form. This was prepared before each experimental session by entering a list of sixty numbers between 1 and 6 which were as random as possible allowing for the fact that ten entries of each value had to be made.

Subjects: Twenty-six subjects took part in the experiment, nineteen female and seven male. All subjects were from the second year psychology class and all, as has been mentioned, had taken part in an ordinary class reaction time experiment using similar apparatus.

Results: The following results are presented:

a) Three groups of correlations (for reasons for deciding upon these particular groups see text that follows). Each group consists of six correlations, each correlation being between the mean reaction time¹ within a ten trial set and the basal conductance level during that set. As there were sixty trials for each subject there are obviously six sets. The N in all cases is 26.

This procedure was adopted in order to study any trends which might occur during the course of the experiment. An overall correlation would fail to do this and would seem to be a much cruder measure of relationship.

The data and correlations are summarised in Table 11.

1

In all calculations the reaction times of female subjects were reduced by a constant of 28 milliseconds in order to make them comparable with the reaction times of male subjects. The overall mean for males was 200ms. and for females 228ms.

TABLE II - REACTION TIME

SET 1	BASE LOG. CON.	1.99	1.91	1.92	2.15	1.88	2.01	2.03	2.30	1.88	1.90	2.06	1.84	1.81	1.77	1.88	2.13	1.88	2.16	2.27	1.77	1.68	2.03	1.79	1.97	1.86	1.62
TRIALS	MEAN R/T	195	163	160	152	192	196	172	148	210	203	155	224	195	228	180	176	211	177	183	232	162	179	171	175	207	180
1-10	R/T-ABNML.	195	163	160	152	183	186	172	148	199	194	155	215	160	211	180	176	200	177	183	223	162	179	171	175	207	180
	BEST 5 R/T	173	135	147	141	165	180	152	132	176	189	139	201	153	194	156	158	182	152	172	208	150	167	150	147	182	152
SET 2	BASE LOG. CON.	1.93	1.81	1.81	2.10	1.76	1.97	1.91	2.33	1.77	1.82	2.03	1.78	1.66	1.64	1.84	2.10	1.71	2.10	2.25	1.72	1.62	1.96	1.62	1.91	1.79	1.53
TRIALS	MEAN R/T	191	196	177	152	247	196	196	162	182	197	162	213	183	225	164	235	210	167	205	239	194	212	171	179	207	175
11-20	R/T-ABNML.	191	182	177	152	212	196	183	148	188	197	162	213	183	206	164	186	201	167	205	210	178	178	171	179	190	175
	BEST 5 R/T	173	159	160	137	203	177	161	139	173	175	140	187	165	195	148	164	191	153	179	189	156	163	147	158	177	161
SET 3	BASE LOG. CON.	1.85	1.74	1.70	2.08	1.61	1.96	1.70	2.33	1.69	1.73	2.00	1.62	1.53	1.57	1.81	2.08	1.60	2.03	2.22	1.70	1.51	1.86	1.48	1.83	1.73	1.40
TRIALS	MEAN R/T	193	219	187	166	247	204	225	148	218	229	142	194	178	226	153	216	224	163	212	212	210	180	197	199	213	194
21-30	R/T-ABNML.	193	199	187	166	225	204	218	148	184	222	142	194	178	226	153	205	202	163	212	189	193	180	183	176	196	194
	BEST 5 R/T	180	175	152	152	207	187	198	139	173	198	128	179	160	209	139	191	193	152	186	177	177	167	162	162	176	171
SET 4	BASE LOG. CON.	1.75	1.71	1.59	2.08	1.47	1.95	1.50	2.30	1.63	1.63	1.93	1.49	1.47	1.51	1.79	2.06	1.53	1.92	2.20	1.66	1.41	1.75	1.41	1.77	1.69	1.32
TRIALS	MEAN R/T	199	232	200	164	225	214	207	160	242	218	166	182	210	275	208	217	213	183	211	226	222	184	176	177	198	228
31-40	R/T-ABNML.	199	217	200	164	200	209	199	160	213	212	166	182	200	230	175	217	205	183	211	226	195	184	176	177	198	186
	BEST 5 R/T	182	198	180	148	199	187	186	138	203	193	138	160	179	232	159	197	188	162	195	207	174	171	151	149	188	174
SET 5	BASE LOG. CON.	1.62	1.73	1.50	2.08	1.38	1.90	1.39	2.27	1.57	1.54	1.82	1.37	1.36	1.46	1.78	2.05	1.45	1.80	2.20	1.63	1.35	1.65	1.37	1.67	1.62	1.27
TRIALS	MEAN R/T	190	219	204	165	251	227	191	148	265	228	176	189	186	216	185	197	201	203	222	191	239	214	176	194	184	237
41-50	R/T-ABNML.	190	170	204	165	215	222	178	148	228	223	176	189	186	207	185	197	191	193	222	182	217	207	176	184	184	226
	BEST 5 R/T	176	153	192	150	198	212	162	128	225	203	152	172	170	193	169	187	181	163	217	160	201	193	152	166	178	206
SET 6	BASE LOG. CON.	1.51	1.77	1.44	2.06	1.30	1.85	1.32	2.27	1.53	1.49	1.81	1.27	1.27	1.41	1.79	2.05	1.41	1.60	2.18	1.57	1.32	1.53	1.32	1.58	1.56	1.20
TRIALS	MEAN R/T	213	215	219	181	266	263	256	162	267	217	194	172	213	225	226	180	230	184	214	215	243	198	207	183	182	250
51-60	R/T-ABNML.	200	206	219	181	236	210	234	162	234	217	179	172	213	213	193	180	192	184	214	215	207	190	188	183	182	240
	BEST 5 R/T	183	185	197	162	232	207	223	139	226	191	153	156	184	188	175	152	180	163	199	203	192	175	174	152	164	218
	SUBJECT	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26

$T_{BNC} - M R/T$

SET 1	-.37 (P < .10)
SET 2	-.23 (N.S.)
SET 3	-.44 (P < .05)
SET 4	-.45 (P < .05)
SET 5	-.37 (P < .10)
SET 6	-.43 (P < .05)

$T_{BNC} - R/T-AB$

SET 1	-.38 (P < .10)
SET 2	-.19 (N.S.)
SET 3	-.38 (P < .10)
SET 4	-.26 (N.S.)
SET 5	-.27 (N.S.)
SET 6	-.48 (P < .02)

$T_{BNC} - B.5 R/T$

SET 1	-.47 (P < .05)
SET 2	-.37 (P < .10)
SET 3	-.35 (P < .10)
SET 4	-.30 (N.S.)
SET 5	-.24 (N.S.)
SET 6	-.44 (P < .05)

Group 1 (r_{BLC} & MR/T). Between basal log. conductance and overall mean reaction time.

Group 2 (see page 107) (r_{BLC} & R/T - Ab). Between basal log. conductance and mean of all reaction times in set except abnormally slow ones.

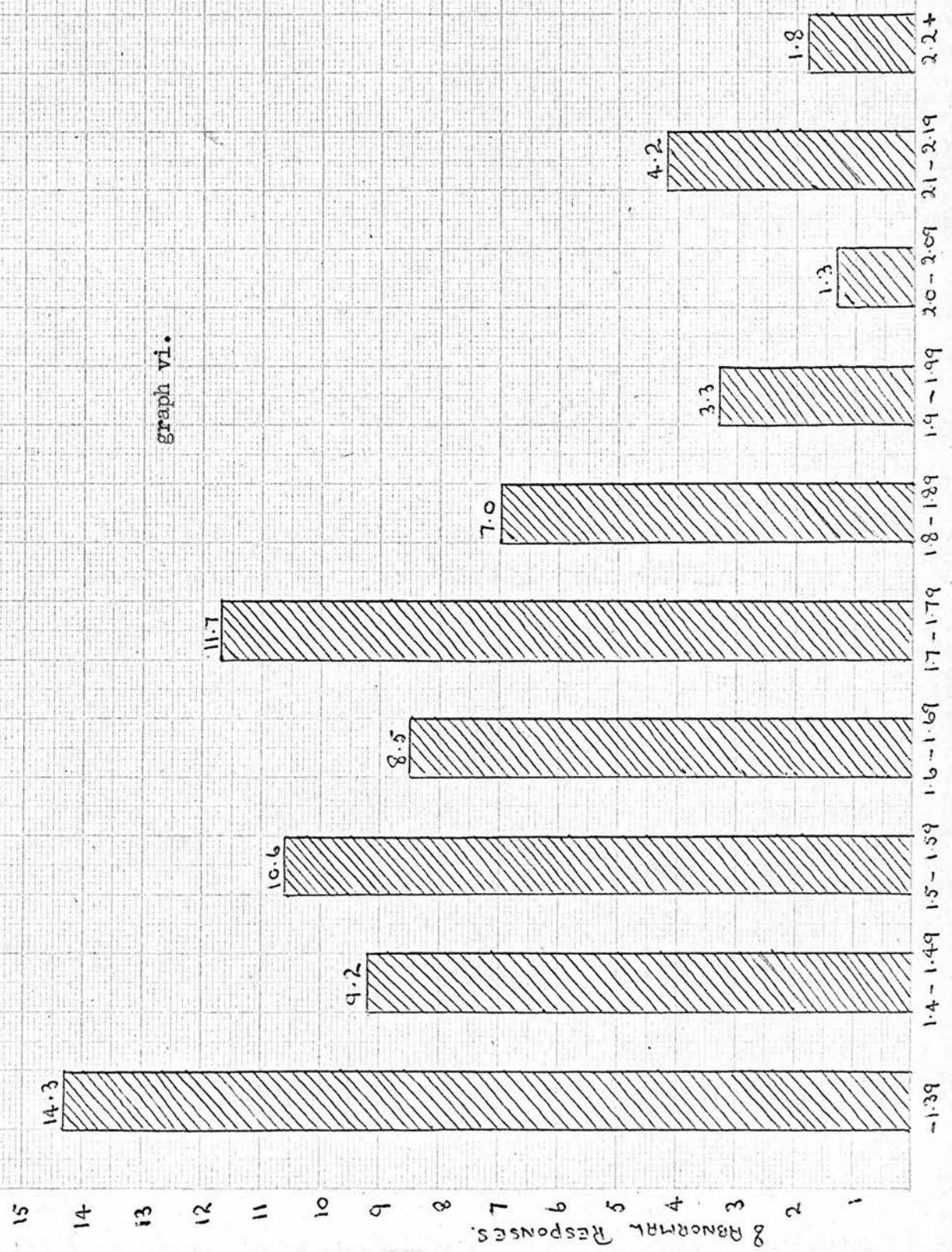
Group 3 (see page 108) (r_{BLC} & BST 5 R/T). Between basal log. conductance and mean of best 5 reaction times in a set.

b) A graph showing the percentages of abnormally high responses¹ for different log. conductance levels. (graph vi)

¹

Responses more than one and half standard deviations above the mean.

graph vi.



c) The overall mean reaction times and the overall mean basal conductance levels were calculated for each set of ten trials (see Table II) and the correlation between them calculated.

The results were:

- a) i) Set 1 $-.37 (P < .10)^1$
 - ii) Set 2 $-.23 (N.S.)$
 - iii) Set 3 $-.44 (P < .05)$
 - iv) Set 4 $-.45 (P < .05)$
 - v) Set 5 $-.37 (P < .10)$
 - vi) Set 6 $-.43 (P < .05)$
- c) $RHO = -.94 (P < .01)$

Discussion: The results, to some extent at least, accord with the predictions made on the basis of the hypothesis that basal conductance level is a measure of 'attention'. They also confirm the findings by Cowles (op.cit.) upon which the hypothesis was partially based. In some details however, the results are not an ideal confirmation of the hypothesis. This is particularly true of the most apparently satisfactory result, which is the almost perfect negative correlation between reaction time and log. of the basal conductance over the six sets of trials.

What this means, obviously, is that both reaction time and basal resistance (the reciprocal of conductance) tend to rise throughout the experimental session. This could mean that they are powerfully related but the degree of relationship observed could equally well be partly

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In view of the large number of correlations falling around or just beyond the .10 probability level it was decided to include this level as one of some significance. One such correlation in a mass of insignificant findings could and should be dismissed but a large proportion of correlations at this level cannot be ignored.

fortuitous. In other words it could mean that there is a tendency for basal resistance to rise during a session and that there is a tendency for reaction time to increase also but, that this could be due to different influences operating upon the two measures independently.¹ This point can be emphasised by some figures. If the relationship were a valid one, one would expect it to hold for a significantly large number of subjects. In other words, the degree to which conductance level falls should, to some extent, be related to the rise in reaction time. But a correlation between change in conductance and change in reaction time from first set to last set of ten trials works out at only $-.05$, totally insignificant.

The implications of this for the general hypothesis must be carefully considered. The most obvious implication is that a fall in conductance level of a certain amount does not produce a similar rise in reaction times in each individual. There are, in fact, three cases in which a fall in conductance is accompanied by a fall in reaction times, in one case by a remarkable 52 milliseconds. This could be a freak result but the general lack of relationship remains. This would seem to make nonsense of the hypothesis if it were not for two other facts. 1) With twenty-three subjects a fall is accompanied by a rise in reaction times, even although the rise is not necessarily related to the amount of conductance change. 2) There is a general relationship between individual reaction times and individual conductance levels throughout the course of the experiment, becoming generally significant in the last four sets of trials. This last point is emphasised because it confirms the findings in Experiment

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This is potentially true of any correlational study but in this instance the observed relationship is so high that one is forced to consider the possibility of an artefact. Of particular relevance is the discussion of the problem of change of conductance during a recording session in section d of the final Chapter.

(iii) above where more marked relationships were observed in the latter part of the performance test situation.

These two facts do not answer the doubts raised by the odd relationship between conductance and reaction time change over time, but they make it impossible to reject the hypothesis by stating that a genuine relationship does not exist; obviously some relationship does exist. At any given time those subjects with a high conductance level tend to produce faster reactions than those with a low conductance level, the tendency becoming more marked after an initial period during which one might expect factors other than long term 'attention'¹ to influence the speed of response.

The problem may be partly clarified by considering the influence of abnormally slow responses. As can be seen from graph vi the percentage of these responses decreases markedly as conductance level rises. Is this the total explanation of the observed relationships? It certainly does not explain the relationship over time. While the percentage of abnormally high responses does tend to increase from set to set the increase is totally insignificant and quite insufficient to account for the marked rise in reaction times. This is confirmed if we recalculate all the results eliminating those responses defined as abnormally slow. The mean reaction time for set 1 is now 181 milliseconds and this rises from set to set until a level of 202 milliseconds is reached in the 6th set. Thus

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The problem of defining this constantly referred to 'attention' factor will be faced in the final Chapter of this thesis. At this stage it can at least be operationally and perhaps tautologically defined as that factor influencing performance on monotonous tasks which require relatively prolonged concentration. A fuller definition would require an excessive digression at this point.

the rise in reaction time seems to be due to a general slowing down of response rather than an increase in the number of abnormally slow responses. This question, however, must be returned to later.

One observation made on the basis of the reaction times which were recalculated eliminating abnormal responses is of some interest. If the correlation between reaction time and conductance level is worked out for each set it is now found to be:

- Set 1 - .38 (P < .10)
- Set 2 - .19 (N.S.)
- Set 3 - .38 (P < .10)
- Set 4 - .26 (N.S.)
- Set 5 - .27 (N.S.)
- Set 6 - .48 (P < .02)

This indicates that much of the relationship between conductance level and reaction time is due to the influence of abnormal responses. The only odd result is the increased correlation in set 6, but this could be due to chance. This approach might be taken further to indicate that nearly all the observed relationship is a function of slower than optimum responses. In other words that the speed of response in ideal conditions for any subject is not a function of whatever process it is that basal conductance level measures, and that the relationship between basal conductance and mean speed of response is purely a function of the number of slow responses due to lapses in attention. In order to test this it was decided to consider only the five fastest responses of each individual in each set. Taylor (1965) has suggested that minimum reaction time values are very little affected by factors such as attention and arousal. This seemed to support the tentative hypothesis which motivated this

re-examination of the data. The decision to choose the five fastest reaction times, rather than simply the fastest, was made in order to avoid undue contamination of the results by responses which might have fractionally anticipated the onset of the stimulus. The recalculated correlations are:

- Set 1 - .47 (P < .05)
- Set 2 - .37 (P < .10)
- Set 3 - .35 (P < .10)
- Set 4 - .30 (N.S.)
- Set 5 - .24 (N.S.)
- Set 6 - .44 (P < .05)

These figures certainly do not confirm the hypothesis, nor do they support Taylor's suggestion, assuming that basal conductance is a measure of activation. It appears, in fact, that the closer we approximate to the minimum response level the greater does the relationship between basal conductance level and reaction time become, once the abnormal responses have been eliminated.

Admittedly none of the observed correlations is very high and the differences between them are not very great. It would, therefore, be inadvisable to place too much stress on the differences between these three sets of correlations. What can be said, however, with a fair degree of confidence, is that there is a relationship between basal conductance level and mean reaction time and that there is a relationship both between basal conductance level and the incidence of abnormally slow reaction times and between basal conductance level and optimum reaction time, the latter two relationships combining to produce the former relationship. This suggests that there is no significant relationship between the

intermediate reaction times and basal conductance level. To confirm this these correlations were calculated and were found to be totally insignificant.

The implications of this rather exhaustive analysis of the results of this experiment should now be considered, but to avoid undue repetition they will be considered in conjunction with the implications of the other experiments which have sought to explore the significance of basal conductance.

(v) The Significance of Basal Conductance

In view of the comparatively small number of significant correlative studies in the vast literature on skin conductance phenomena,¹ particularly on basal conductance level, it is perhaps surprising that this Chapter should include no fewer than four experiments which demonstrate a fairly conclusive relationship between basal skin conductance and performance, and that these relationships should be observed without recourse to the normal expedient of comparing the performance levels of two extreme groups.

It should be emphasised here that this is not due to any uncannily insightful selection of performance criteria by the author. The large number of significant findings must be attributed almost entirely to the method by which basal skin conductance was measured.

The apparatus used is fully described in Appendix II, but some further comment is pertinent at this stage.

Almost all reported studies of basal conductance level have used a D.C. source with its attendant problems of electrode polarisation and

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These are reviewed in Chapter 2 above. A study of this review will reveal that the amount of positive, direct evidence for a relationship between G.S.R. and performance is staggeringly small.

contamination of the results by the additive or subtractive effects of resting skin potential. Only two studies report the use of an A.C. source. James and Thouless (1926) reported polarisation effects with an A.C. source but this seems very unlikely and one is forced to the conclusion that they encountered the capacitance effect mentioned in Appendix II and confused this with polarisation. As they report fluctuations in polarity according to frequency, in the same way as one would expect fluctuations in the net capacitance effect, this is a probable explanation. Tolles and Carberry (1960) report the use of an A.C. source but they made little use of their apparatus in actual experimental studies.

It seems likely that the use of a D.C. source makes the measurement of actual conductance levels a very chancy and inaccurate matter. Even if polarisation problems can be overcome, which is by no means certain,¹ one is still left with the problem of resting skin potential. Venables (1962) reports resting skin potential levels of from + 12.5 mV to - 42.5 mV. These could produce relative inaccuracies of up to 550 ohms in readings of basal resistance level using a 4 volt D.C. supply at basal levels of about 20 kilohms. With a smaller source voltage, such as has been used by several investigators, the relative inaccuracies could be very much greater. These errors, which might be of the order of 1 - 10%, depending on source voltage level, are large enough to affect seriously any correlative studies of basal conductance level. With an A.C. source the problem disappears. Even if the resting potentials are a nuisance because they shift the oscillation centre line they can be eliminated by using a condenser coupling between amplifiers. The fact that Wilcott (1958) found no correlation between basal measures of resistance and potential in

¹ Static checks in saline solution (Martin, 1960) may not reveal the degree of polarisation which occurs in contact with the skin.

normal subjects makes the elimination of this effect particularly important.

Having advanced a likely reason for the unexpectedly fruitful results of the experimental programme described here it is now necessary to consider the overall significance of the results.

Three of the experiments show a more or less positive linear relationship between basal conductance level and performance. This could be explained by saying that basal conductance level reflects the general alertness of the subject, which is in line with what we might expect if basal conductance is a measure of long-term activation level. This is hardly a suggestion of striking originality; it was suggested by Thouless (1925), who claimed that change in resistance reflected a change in mental alertness of the subject, and by Darrow (1936) who made much the same claim. It is strongly implied throughout the section on basal conductance level in the previous chapter. The only novelty in this restatement is that it is backed by strong empirical evidence. This evidence, however, is not unequivocal. Apart from the relatively minor difficulties which have been covered in the discussion sections of the individual experiments there are at least two problems presented by the experimental evidence which require careful consideration.

(1) The negative relationship between basal conductance level and the number of uncommon words recalled (Experiment bi).

(2) The two-part relationship between basal conductance level and reaction time (Experiment b iv)

(1) The first problem could be considered in terms of the already referred to U-shaped relationship between activation and performance, but this type of explanation is difficult to reconcile with the observed

relationship between basal conductance and the recall of common words (see discussion to Experiment b i above). A more likely explanation is that fewer uncommon words were recalled by the high activation subjects because they were more attentive to the list. This may seem paradoxical but it is not as unreasonable as it at first appears. With the common words it is comparatively easy to assimilate sound and meaning, which, with such common words, might be assumed to be very closely identified, to the point of inseparability. This means that each word could be understood as it was read. With the uncommon words the sound might be assimilated without a satisfactory meaning being readily apparent, at least in a significant number of cases. Now an attentive or alert listener might 'search' for satisfactory meaning to attach to the sound while the inattentive listener might simply accept the sound with some vague and possibly unsatisfactory meaning. In the 'search' for satisfactory meaning of one word the alert listener might hear only the sound of the following word.

Recall was almost immediate and would be from a relatively short term memory store of the type envisaged by Broadbent (1958)¹. Now with the common words both high and low activation subjects would have a store of meaningful words and their recall performance would be identical. With uncommon words, high activation subjects would have a store of meaningful words and possibly a store of sounds, while low activation subjects would have a store of semi-meaningful sounds. If we assume that the short term store has a limited capacity of, say, fifteen units over a twenty second period and if we further assume that sound + meaning takes up more

¹ The idea of a limited memory store might explain the lack of relationship between mean size of G.S.R. and number of words recalled reported in experiment a(i) above. One does not wish to make too much of this because the experiment was quite different, but it is a possible explanation.

capacity than sound + semi-meaning,¹ then we would expect immediate recall from this limited store to favour low activation subjects. One does not wish to push this tenuous theorising beyond reasonable limits but one might use hypothetical figures to illustrate a possible case. Let us take sound to equal 1 unit of capacity, meaning (see footnote) to equal 1 unit of capacity and semi-meaning to equal $\frac{1}{2}$ unit of capacity. Now a high activation subject would store 7.5 words³ if all the words he stored were meaningful. He might, of course, store 15 words if all the words were represented only by sounds but it seems likely that meaningful words have precedence in the store. A low activation subject would store 10 semi-meaningful words. With common words, where effort after meaning will be so much less, we might assign $\frac{1}{2}$ unit of capacity to meaning. This will mean that both high and low activation subjects will recall around 10 words. Admittedly the above figures were chosen to support a point but the actual experimental figures were:

Uncommon words:	Low conductance: ²	10.1 words
	High conductance:	7.8 words
Common words:	Low conductance:	9.8 words
	High conductance:	10.0 words

There are, of course, a great number of other ways in which the above figures could be arrived at, and it is almost certain that the example given is an idealisation of a potentially very complex situation. It does,

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The actual capacity taken up by the meaning should probably be more accurately attributed to 'effort after meaning'. This explains the observed differences between common and uncommon words more effectively.

2

Low conductance means all subjects below mean conductance level, high conductance means all those above.

3 ASSUMING MEAN STORE OF 15 UNITS.

however, offer a possible explanation of this result without recourse to the vague concept of the U-shaped curve, while permitting the retention of the hypothesis that basal conductance is directly related to alertness. This hypothesis might also explain the fact that high conductance subjects tend to recall from later in the list. With a limited store and a series of items taking up a large capacity there will be a tendency for later items to eliminate earlier items. This will, of course, interact with the normal serial position effect in immediate recall which Corteen (1960) has shown to be similar to the curve found in rote learning. This serial effect will tend to favour earlier items as well as later ones. Welsh and Burnett (1924) produced a serial curve for immediate recall which was markedly skewed in favour of the later items, but the present author has been unable to duplicate this curve in a wide range of studies on immediate recall.

(2) The second problem requires rather less tenuous hypothesising. Bills (1931), in a classic paper, reported the occurrence of periodic blocks during continuous and homogeneous mental work. He found that the frequency and size of these blocks increased with fatigue. As we can now legitimately assume that basal skin conductance is a measure of the fatigue-alertness dimension currently referred to as activation, we would expect a greater number of blocks to occur at low conductance levels, thus explaining the relationship between conductance level and abnormally slow responses.

In view of the relationship reported by White (1930), Freeman and Simpson (1938) and Baker and Taylor (1954) between skin conductance level and muscle tension it may be thought that the lower minimal reaction times of high conductance subjects reflect greater response effectiveness brought

about by increased muscle tension. This interpretation is plausible but unfortunately there is evidence which throws some doubt upon it. All of the above studies reported a relationship between induced muscular tension and skin resistance. Jacobson (1932), Wenger (1943), and French (1944), on the other hand, all report no relationship between various measures of muscle tension and various measures of skin conductance. Even if we do accept that the evidence for a relationship between skin conductance and muscle tension does outweigh the evidence that there is no such relationship we find that Martin (1960b) reports no effect upon reaction time following the administration of the drug meprobamate which, according to her, 'is believed to reduce muscular tension'. If a drug which reduces muscular tension has no effect upon reaction time¹ then it is unlikely that any reduction of muscular tension accompanying a fall in conductance level will be the reason for the slower reaction times observed at lower conductance levels. As it is very unlikely that basal conductance level is related to receptor or central sensory sensitivity, in view of the lack of relationship observed by Corteen and Blackman (1965), it is very probable that basal conductance level reflects the effect of activation level upon a central mechanism which affects reaction time by increasing cortical or sub-cortical efficiency. What this mechanism is must, at present, remain

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This research poses something of a problem because in an earlier paper Martin (1960a) reports heightened skin resistance levels after administration of meprobamate. Perhaps this need not be taken too seriously as the method used to measure skin resistance was unbelievably crude. This persistence in the use of electronically unsophisticated measuring devices in studies of skin conductance is difficult to understand. In a very recent paper Grim and White (1965) report on the use of a measuring device which would not have been out of place in the latter part of last century. In view of the fact that most major scientific advances have come about through the development of more and more sensitive and sophisticated measuring devices (the telescope, the cloud chamber, the electron microscope) the lack of concern shown by many psychologists about the development of their own measuring techniques is, to say the least, surprising.

a matter for speculation, but it is presumably identical with the mechanism which controls performance on the rapid dotting task and on the pursuit rotor, though here also performance may, to some extent, be influenced by the greater incidence of blocks which seem to occur at low activation levels.

The lack of relationship between conductance level and the reaction times which were neither abnormally slow nor optimal can be explained only by assuming that these reaction times were 'indifferent'; that is they were produced at a time when the subject was not really trying. Most of them were not slow enough to have occurred during a block, though some of them might have, while most of them were not fast enough to reflect the subject's optimal effort; though, because of the arbitrary selection of the five fastest trials, some of these so-called 'indifferent' reaction times were obviously optimal just as some of the so-called 'optimal' trials were obviously 'indifferent'. The probable confusion introduced into this particular experiment by the complication of subjects who had periods of not trying, and even subjects who did not try at all, could have been reduced somewhat by informing subjects of their performance, just as they were informed in the pursuit rotor and dotting tasks. The confusion could have been reduced even further by introducing reward or punishment, and under such conditions one would predict a much closer relationship between basal conductance level and mean performance.

To conclude this stage of the experimental investigation one can probably say with a fair degree of confidence that basal conductance level is a reflection of the degree of attention a subject is capable of paying to a task upon which he is concentrating or focussed. It reflects his level of alertness which is taken to be synonymous with his level of arousal or activation as these terms might be narrowly construed.

There is no evidence that attention begins to decrease at high activation levels, though some tasks might appear to be performed worse, at least in the short term, because the method of assessing performance might obscure the very factors which heightened attention emphasises. This latter point is probably true of the experiment on the free immediate recall of uncommon words.

The question of basal conductance level and activation will be returned to in the wider context of the final Chapter. Now it is time to consider the last set of experiments.

c) Experiments using Anxiety and Neuroticism Scales

(i) The 'N' Scale of the M.P.I. and Examination Performance.

The title of this experiment might be taken to indicate a specific study. In fact it was the one significant result in an omnibus experiment. The reasons for using the M.P.I. and the M.A.S. have been fully stated in Chapter 2. As these scales were unfamiliar it was decided to carry out a multi-correlational study in order to assess their significance in the field of activation theory while at the same time trying to make more sense of the nature of basal conductance.

Furneau's (1962) study relating the 'N' and 'E' scales of the M.P.I. to examination performance has already been mentioned. Because of suggestions that they reflect activation or drive level it was desired to study the relationship of the M.A.S. and the 'N' scale to basal conductance level and mean G.S.R. For two reasons it was thought desirable to administer an intelligence test. The more obvious reason was that any effect of intelligence could be partialled out of any relationship between the M.P.I. and examination performance which might be observed. The second reason was that O'Connor and Venables (1956) had reported a negative correlation between intelligence and conductance level and this was thought to be worth further investigation.

Procedure: Subjects attended for two sessions. On the first occasion half of the subjects were given the high grade intelligence test A.H.5 and immediately afterwards their basal conductance level and mean level of response to a list of twenty words were measured using the apparatus described in Appendix II. The other subjects were given the M.A.S., had their conductance level measured, and were then given the M.P.I. During the second session the procedures were reversed and conductance level was

measured before intelligence, and M.P.I. given before M.A.S. Subjects were told that this was a purely experimental investigation and that none of the results would have any bearing upon their future academic careers. This was necessary because some subjects expressed alarm at the intelligence test and at the personality inventories. It was naturally emphasised that they should try to answer as well and as honestly as they could. Several of the subjects expressed considerable irritation over the questionnaires and on several occasions it was necessary to repeat the basic instruction that they should answer the questions or respond to the statements with the first response that occurred to them.

Apparatus: Conductance measuring apparatus was as described in Appendix II. The M.A.S. and M.P.I. have been described in Chapter 2 and are given in full in Appendix III. The A.H.5 is described by Heim (1955) and was administered according to the method outlined in that Manual.

Subjects: In all seventy-eight subjects were used. They were all 2nd year Psychology students and their ages ranged from 19 to 31. Because of lack of availability of some examination data not all subjects are included in the correlations reported below.

Results: Some of the results have already been reviewed in Chapter 2. A full list of individual results is given in Table 12.

Table 12

S.	B.L.R.	A.H.5	M.P.I. 'N'	M.P.I. 'E'	T.A.S.	EXAM.	MEAN G.S.R.
1	1.43	25	19	9	12	41	2.73
2	0.74	41	19	26	12	45	0.26
3	1.80	49	22	31	18	55.5	0.10
4	0.98	30	22	30	7	44.5	0.16
5	0.90	50	26	20	18	59	0.06
6	0.79	49	33	21	24	57.5	0.03
7	1.09	44	29	20	16	47.5	0.83
8	1.28	44	16	32	14	44	0.13
9	1.10	39	14	30	7	61	1.16
10	1.05	52	17	15	18	49	1.13
11	1.46	48	32	15	21	50	0.26
12	0.46	28	32	34	22	38	0.03
13	1.21	48	7	22	9	47.5	1.16
14	1.29	33	31	19	14	50	0.50
15	0.86	29	31	19	17	54	0.0
16	1.24	45	25	32	16	42.5	1.96
17	0.63	36	27	11	21	53.5	0
18	0.60	53	27	23	14	42.5	0.03
19	0.73	34	30	23	15	47.5	0.43
20	0.68	33	31	35	26	43.5	0.43
21	0.56	30	42	13	23	43	0.33
22	1.14	37	34	33	18	49	2.30
23	0.67	38	35	30	33	33.5	0.20
24	0.83	49	39	35	23	28	0.50
25	1.14	50	36	41	15	35.5	0.33

S.	B.L.R.	A.H.5	M.P.I. 'N'	M.P.I. 'E'	T.A.S.	EXAM.	MEAN G.S.R.
26	1.01	50	26	23	13	53	0.72
27	1.48	31	17	28	15	52	1.80
28	0.87	38	31	14	20	46	0
29	1.08	39	23	37	23	45.5	0.03
30	0.94	36	2	36	9	52.5	1.53
31	1.23	42	26	18	20	47.5	0.36
32	0.74	36	10	27	11	44.5	0
33	0.93	40	24	30	20	40.5	0.03
34	0.84	27	23	21	13	55	0
35	1.40	27	13	27	7	56	1.10
36	0.90	44	22	20	23	-	0.10
37	0.95	38	32	28	19	40.5	0.10
38	1.06	35	19	27	7	64	0.06
39	0.99	40	30	23	23	59	0.10
40	0.98	55	29	24	23	38.5	1.13
41	0.81	33	29	28	14	40.5	0.23
42	0.89	35	18	27	18	47	0.20
43	1.05	36	16	29	9	49	0
44	1.94	50	22	37	11	52	0
45	1.31	27	22	24	20	44	0.23
46	1.20	40	12	21	8	56.5	0.56
47	1.45	47	14	42	8	37.5	0.63
48	0.54	35	40	26	25	44.5	0.06
49	1.24	38	24	35	18	45	0.50
50	1.22	35	36	24	27	52.5	1.36
51	0.84	37	14	12	9	37	0.03
52	1.52	43	32	39	19	43.5	2.16

S.	B.L.R.	A.H.5	M.P.I. 'N'	M.P.I. 'E'	T.A.S.	EXAM.	MEAN G.S.R.
53	0.66	48	16	34	15	54	0
54	1.20	40	27	26	19	46	1.33
55	1.17	54	13	38	13	36	0.16
56	1.34	32	4	28	6	59.5	0.43
57	1.15	34	27	19	22	54.5	0
58	0.64	31	32	15	20	38.5	0.03
59	1.70	21	36	35	16	37	0.46
60	1.15	37	17	34	11	31	1.23
61	1.23	33	30	28	15	26	0.53
62	1.62	40	18	41	8	47	0.06
63	1.19	32	20	10	21	64.5	0.63
64	1.48	52	15	20	11	68	0.96
65	0.51	46	9	34	13	59.5	0.36
66	0.84	47	8	33	10	59	0
67	0.97	41	2	33	6	72.5	0.53
68	1.16	34	25	39	13	50.5	0.16
69	0.74	29	15	36	12	59.5	0.33
70	1.13	52	23	10	23	58.5	2.16
71	0.83	43	4	27	4	56.5	0.46
72	0.81	46	16	27	12	65.5	0.06
73	1.29	44	30	22	19	61	0.20
74	1.60	59	32	11	17	48	1.10
75	1.30	43	7	22	10	61.5	2.00
76	0.83	33	10	32	5	56	0.03
77	0.85	27	22	24	15	-	-
78	0.79	38	37	31	23	51	0.13

Apart from the relationships already referred to the main significant finding is a correlation of $-.43$ (significant at beyond $.001$ level, $N = 76$) between the 'N' scale of the M.P.I. and the average mark for the 1st and 2nd term class examination. In addition a significant difference ($t = 2.17$, d.f. 74, significant at beyond $.05$ level) was observed between the mean scores on the 'N' scale of those passing and those failing the final degree exam for the course; those failing having a mean of 25.1 and those passing a mean of 21.2. In the case of the degree examination results it was felt to be more appropriate to consider simply pass or fail as the performance criterion, as the marking of these papers is mainly aimed at determining whether a candidate has passed or not.

In addition, as one would expect from the correlation of $+.78$ between the 'N' scale and the M.A.S., there is a correlation of $-.28$ (significant at $.02$ level, $N = 76$) between the M.A.S. and class examination performance.

There was no significant relationship observed between:

M.P.I. 'E' scale and any other measure (N 's = 76-78)

M.P.I. 'N' scale and basal resistance ($N = 78$)

M.A.S. and basal resistance ($N = 78$)

A.H.5 and basal resistance ($N = 78$)

A.H.5 and examination performance ($N = 76$)

Basal resistance and examination performance ($N = 76$)

Mean G.S.R. and any other measure (N 's = 75-77)

Discussion: Apart from indicating a marked lack of relationship between autonomic measures of activation and the neuroticism and anxiety scales, these results do, rather surprisingly, indicate a strong negative relationship between the scales, in particular the 'N' scale, and examination performance.

This is in line with the large number of negative relationships between the M.A.S. and performance criteria listed in Chapter 2, section C i, but the negative relationship contradicts the findings of Furneaux (1962) who found that the failure rate, in written examinations, of students with above average 'N' scores was 28% and of those with below average 'N' scores was 41.5%. Calculating Chi - square for Furneaux's data gives a value of 2.9 with 1 d.f., which is significant at only just the .1 level of significance. This is not particularly convincing. Further, Furneaux found a negative relationship between 'N' scores and performance in an engineering drawing examination.

The large number of negative relationships observed by other authors, together with the very marked negative correlation observed in this study, when compared with the relative lack of positive relationships which have been observed, except in the field of rate of eyeblink conditioning, leads one to question the assumption made by Spence and Taylor (1953) and Furneaux among others, that neuroticism/anxiety scales do, in fact, measure potential drive level. Admittedly one is at liberty to use the term drive in the context of a very narrow operational definition, i.e. drive is what neuroticism and anxiety scales measure, but this 'drive' is very different in its effects from the term which is current in common usage, and it is doubtful if an operational concept should be given a name which confuses it with a commonly understood, if vaguely defined, concept which is so different.

What does seem more or less certain from this experiment is that whatever it is being measured by neuroticism and anxiety scales it is not the same as the dimension of alertness or activation which is being measured by basal conductance.

In view of the large number of negative relationships which have been reported between these questionnaires and performance criteria it would seem more appropriate, in deference to common usage, to define the dimension which neuroticism scales measure as one of 'degree of inadequacy'. Consider the M.P.T.'N' scale (Appendix III). Items 3, 6, 13, 15, 19, 21, 23, 31, 33, 37, 39, 41 and 45, at least on face value, would seem to define an individual who finds it difficult to cope with the current situation. Truthful answers to these questions might well reflect a consistent inadequacy. It is true that this scale does differentiate between clinically defined neurotics and non-neurotics (Eysenck, 1959) but the overlap is very marked, many neurotics falling more than one standard deviation above the non-neurotic mean, and it could be that the observed difference between the mean scores simply reflects a tendency for neurotics to be less adequate in certain situations. In other words it seems likely that inadequacy could be one feature of some neurotics, but that inadequacy and neuroticism are very far from synonymous. If we consider the M.A.S. we find the same pattern. Items 2, 7, 9, 10, 11, 13, 14, 15, 16, 19, 23, 25, 26, 27, 29, 30, 31, 32, 33, 34, 35, 36, 37, 38, 39, 41, 42, 43 and 49, if answered in the 'anxious' direction, would all seem to indicate a degree of inadequacy. Again it is true that the M.A.S. differentiates between clinically defined neurotics and non-neurotics, though not between clinically defined anxious and non-anxious subjects (see Chapter 2, section C i), but again the overlap is considerable and the same arguments could apply as with the M.P.I. 'N' scale.

This interpretation of the two scales in this study may be an oversimplification. Certainly only just over half of the items in each scale

can be readily identified as items which relate to a measure of adequacy, but consistent answering of these would be sufficient to load the scales heavily in this direction even if other factors were being measured. The evidence leans quite heavily in the direction of such an interpretation as compared with the interpretation of the scales as measures of generalised drive or even neuroticism.

Before moving on to the next experiment a final piece of evidence can be considered. Of the seventy-eight subjects used in the experiment twenty-eight were potential honours students; of these sixteen finally obtained an honours degree and twelve were rejected or withdrew. The mean 'N' scale score for the sixteen honours degree subjects was 17.2, 7.5 points less than the mean for the remainder of the group ($M = 24.7$, $N = 62$) who were, or at least ended up as, ordinary degree students. This difference is significant at beyond the .01 level.¹

(ii) The 'N' Scale of the M.P.I. and Rote Learning at Different Presentation Rates.²

This experiment is included with some deference. ^{reference!} It was the last experiment of the entire series which has been presented in this thesis and for this reason there was insufficient time to carry out as comprehensive a study as is usually desirable. It is included because the results are quite striking and they have considerable significance in the attempt to interpret the dimension which has now been tentatively identified as one of ~~the~~ adequacy-inadequacy. The aim was to study high and low scoring

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The mean score for the rejected group was 25.5; so the difference is not merely due to a desire to take an honours degree. These findings, if pursued, might have interesting implications in higher education. Just how general they are is very uncertain. They may be quite specific to the class of students studied.

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Part of the actual administration of this experiment was carried out by an undergraduate student in the course of a supervised project. The conception, plan, and interpretations are, naturally, the author's. The author also carried out part of the experimental administration.

subjects on the 'N' scale of the M.P.I., in a task where their adequacy to deal with situations of different degrees of complexity could be compared. The task decided upon was rote learning of nonsense syllables and the complexity of the task was varied by altering the rate of presentation. According to Feldman (1964a) this is a variable which has not yet been systematically studied in the same experiments when studying the relationship between questionnaire 'anxiety' and performance.

One would predict that the performance of low scoring subjects will be superior to that of high scoring subjects at rates of presentation which represent a difficult situation. This somewhat limits the theory of adequacy to situations which are stressful and makes it comparable to the theory of Mandler and Sarason (1952) who imply that anxious subjects will tend to produce more incompatible responses in anxiety provoking situations. The only objection to maintaining the Mandler and Sarason hypothesis is that anxiety, as it is clinically defined, seems to be poorly identified by the two questionnaire scales which have been used in this investigation. It seems preferable to use the more relevant term 'adequacy' if only to avoid theorising which involves the concept of anxiety, either as if it were the same as clinical anxiety, or as if it were almost synonymous with drive. Sarason (1956) tends to use it in the latter sense in his well-known study of the interaction of motivation and anxiety. The above prediction could also be made on the basis of the Spence and Taylor (1953) theory which has already been discussed, but, because this drive-oriented theory tends to predict results which distort the common expectation of drive effects, there is ample reason for questioning the general value of such a theory. One has the feeling that the theory having been conceived

before the evidence was available, has been contorted to fit unexpected evidence rather than being based upon the evidence as it came to hand.

Procedure: The M.P.I. was administered to a group of forty students. Five whose 'N' scales were below 8, and five whose scores were above 25, were chosen as the low anxiety (LA) and high anxiety (HA) groups respectively. Five lists (A - E) were used, each of nine nonsense syllables of approximately equal associative value (Glaze, 1928). Five presentation speeds (1-5) were used, from one syllable per second to one syllable every five seconds. The order of presentation speeds was different for each subject but the order of the actual lists was the same for each subject. Table 12 shows the order of presentation at each rate for each subject. The HA and the LA group were given identical treatments.

	Subject					
	I	II	III	IV	V	
O	1A	2A	3A	4A	5A	
R	2B	3B	4B	5B	1B	
D	4C	5C	1C	1C	3C	4B refers to list B being presented at one syllable every four seconds.
E	5D	1D	2D	3D	4D	
R	3E	4E	5E	2E	2E	

Table 12

On arriving at the experimental session subjects were told that they had to learn a list of nonsense words in the right order by the method of serial anticipation. This was explained to them. They were told that each list had to be learned to a criterion of two correct full anticipations. Practice was continuous with no breaks between each presentation of the same list. They were then given fifteen minutes' practice on trial lists in order to familiarise them with the situation. Between each different list they were given a ten minute break during which time they played patience.

Apparatus: The lists were:

<u>List A</u>	<u>List B</u>	<u>List C</u>	<u>List D</u>	<u>List E</u>
LUJ	HEJ	ZIC	FUP	PAF
FEP	NAX	BEP	NOL	CUJ
JAT	ROJ	FUJ	ZIB	MEH
ZUN	PUK	NEF	POB	ZON
CEH	VID	MAF	RUQ	RIW
GUH	DUH	DEH	MEP	POZ
LEJ	GEB	XOW	HUQ	KEX
FAQ	NAL	LEJ	REH	FUJ
NIR	JOZ	GAW	CIB	GIC

Presentation was by means of a Stöelting Memory Drum.

Subjects: Subjects were first and second year male Psychology students. Selection was purely on the basis of 'N' scale scores. Age and intelligence was roughly controlled by the fact that all subjects were students under the age of 24.

Results: The most interesting result is the difference between HA and LA subjects at the different presentation rates. These are given in Table 14 below. Because syllables were perceived for different periods of time, according to the presentation rate, the number of trials to criterion in each case has been multiplied by the presentation rate in order to give a comparable time on task for each presentation rate.

PRESENTATION RATE	HA		LA
1	(81x1)	81	26 (26x1)
2	(19x2)	38	23 (11.5x2)
3	(13.7x3)	41	27 (9x3)
4	(10.2x4)	41	34 (8.5x4)
5	(6.4x5)	32	36 (7.2x5)

The difference between HA and LA at the fastest rate of presentation is significant at the .01 level on a one-tailed test. All other differences between the groups are insignificant. The F - ratio for the variance between presentation rates for the HA group is significant at the .01 level. Discussion: These results strikingly uphold the hypothesis that the differences between subjects on at least some so-called anxiety and neuroticism scales are due to differences in adequacy, particularly in confusing situations. The small number of subjects tested in this experiment would make any undue claims invidious, but at least the general hypothesis is upheld.¹

(iii) The Significance of the M.A.S. and the 'N' Scale of the M.P.I.

It might be said that by advancing a theory of adequacy to explain the results of these and other experiments, one is merely substituting one name for another. This is only partly true. The use of the concept of generalised drive level in relation to these scales merely leads to confusion when one is considering the results of deprivation and other experiments on drive. It would surely be absurd to imply that behaviour is always maladaptive under conditions of high drive induced by sustenance or stimulus deprivation. It is true that under extreme conditions of deprivation behavioural efficiency may suffer, but it is unlikely that this is always because of the extreme drive levels induced, and even when it is, it is unlikely that these drive levels will be comparable to the relatively mild levels which high scores on anxiety scales imply in normal subjects. And yet if we accept the drive level hypothesis we must accept that in almost

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Experiments along these lines are continuing. In a very recent experiment too recent to be included in this Thesis, a correlation of $+0.56(N = 25)$ has been observed between time to learn a list at the fast exposure rate and 'N' scores on the M.P.I. The correlational evidence is reassuring as it confirms that the effect is not confined to extreme scores.

all situations relatively high drive is going to produce a poorer, or at best, the same level of performance as low drive. The only exception is in the classical avoidance conditioning situation (Taylor, 1951) where high scoring subjects on the M.A.S. will condition faster than low scoring subjects. Even this can be explained in terms of adequacy as we shall see presently. Of even more relevance to the present thesis is the confusion which is caused when scores on these scales are confused with measures of activation. The use of the generalised drive level concept is bound to amplify this confusion.

The alternative to an interpretation in terms of generalised drive level might be to retain the terms anxiety or neuroticism. The objection to this has already been stated. These are clinically defined terms and the ability of the M.A.S. or the 'N' scale to identify anxious or neurotic patients is very limited. To continue to use these terms in such statements as 'Anxious or neurotic subjects tend to do badly in written class examinations at Edinburgh University' would be very misleading indeed if the statements were based upon the M.A.S. or M.P.I. scores of the students. The introduction of the concept of adequacy has several advantages. The most obvious are: (i) It avoids confusion with terms which might mean different things to different people. (ii) It fairly accurately reflects the observed behaviour of high and low scores on the scales in question.

The most striking feature of this adequacy dimension is that it appears to be independent of intelligence as measured by intelligence tests, even although the behavioural criteria are sometimes similar. In Experiment i c above it predicts examination performance much more accurately than the intelligence test.

It is not intended, in this thesis, to attempt to develop this concept very fully. Much more empirical evidence, collected against this rather different theoretical background, must be accumulated before one can do more than make vague general statements. It does seem likely, however, that adequacy is a valid personality dimension, independent of anxiety on the one hand and intelligence on the other, which is reflected in the way individuals react to situations, both intellectual and social, which require complex patterns of overt response. One emphasises overt response because the normal intelligence test often does not require more than underlining or the insertion of a figure or letter in a defined space. This is also a characteristic of the objectively scored multiple-choice examination, and in a comparison of 'N' scale scores and marks in such an examination made by 320 first year Psychology students no significant relationship was observed.

Adequacy, therefore, is revealed in situations which require active and positive interaction with the environment. The inadequate individual, defined in this context, will tend to avoid interaction if possible, and this may explain the results of the conditioning experiments, if we consider these as situations where the high scoring subject will tend to condition rapidly in order to avoid continued interaction.

It is doubtful if either the M.A.S. or the 'N' scale of the M.P.I. are truly ideal measures of this dimension. One feels that actual behavioural measures would be superior. One also doubts if the dimension is quite as simple as has been implied here. It could be approached and considered as the dimension which explains the difference in performance between individuals who have more or less identical competence models, in the sense that the linguists use the term; the degree of sophistication of

the competence model being dependent upon intelligence as it is normally understood. Formulated in this way it is possible to conceive of the adequacy dimension, properly measured, explaining much of the variance between intelligence and actual performance.

While the above speculations have fascinating implications, they go beyond the limitations of this section in which we are primarily concerned with the significance of the M.A.S. and the 'N' scale of the M.P.I., with particular reference to the concept of activation. In regard to this concept it can be more or less definitely said that these scales have no relevance whatsoever. Activation and adequacy are independent dimensions. This is demonstrated both by the fact that they are not correlated and that they relate to different aspects of performance. It must by now be clear that the inclusion of the scales of anxiety and neuroticism in the general field of activation theory has been partly due to the understandable but unfortunate tendency to extend this theory beyond its legitimate and useful bounds, and partly due to the confusion which has been caused by trying to explain the results of certain experiments in terms of a neo-Hullian S-R drive theory.

It is now time to try to state a coherent explanation of the experimental results which have been presented in this Chapter and to try to tie together the various loose ends which have been left hanging throughout the preceding pages.

IV DISCUSSION AND SUMMARY

a) Conclusions from Experiments

The most important conclusions that can be drawn from the above experiments are:

- i) Both basal log. conductance and the G.S.R. are indices of activity in central mechanisms. Variation in the level of this activity appears to affect the efficiency of the organism in tasks requiring long or short term alertness. In view of the fact that the skin conductance phenomena and the mechanisms which physiologists have identified as underlying cortical arousal appear to have a common neurophysiological basis, it seems reasonable to assume that basal conductance and the G.S.R. are at least adequate if not perfect, measures of long and short term activation respectively. Also, in view of the desirability of limiting the concept of activation to something which is usefully definable, it is necessary to assume that measures which have no relationship with the conductance phenomena are not measures of activation in any sense whereby that term can be sensibly used.
- ii) There is no obvious linear relationship between long and short term activation although, as we shall see, there is some basis for assuming a complex inter-relationship. They should, however, be regarded as two more or less separate activation dimensions.
- iii) Scores on anxiety and neuroticism questionnaires cannot be regarded as indices of activation. There is, however, a relationship with overt behaviour in a wide variety of situations and such scores might be regarded as indices of position on an 'intensive' dimension (i.e. Duffy's, 1962 use of the word 'intensive') which has been provisionally named 'adequacy - inadequacy'.

iv) Both mean G.S.R. and basal conductance are genuine and meaningful inter-subject variables at the time of measurement. This is particularly important in the case of basal conductance which some writers have suggested may be only a valid intra-subject measure (e.g. Malmö, 1958). The extent to which these two measures reflect long-lasting individual differences is not yet clear. It is likely that test - retest correlations will be significant over a period of weeks but not of the level required in, say, intelligence testing. This difference would not be a reflection of the reliability of the measuring method but would almost certainly reflect actual shifts in activation level.

b) The Overvaluation of the Concept of Activation

The theoretical implications of these basic conclusions must now be considered. The most obvious implication is the marked limitation which is placed upon the concept of activation, even with the two dimensional formulation which has been advanced. To some extent the concept is now seen to be relatively trivial.

The influence of variations in long term activation level appears to cover a fairly limited range of human activities, mostly those involving relatively prolonged alertness or vigilance where there is little redundancy. In fact very few activities come into this category and most of these have been invented by psychologists. There are some exceptions which occur in real life, such as watching a radar screen for very short-lived, indistinct signals; driving a racing car; proof reading at any sort of realistic speed; slalom skiing; and, possibly, quality control inspection situations. The influence ceases to be trivial only at very low activation levels where performance on a very much wider range of activities will be

affected. But we hardly require a sophisticated theory to tell us that a drowsy or sleeping man will tend to be inefficient. Here the theory is of value only if it tells us why a man is drowsy or asleep and it is no answer simply to say that his activation level is low. This is simple tautology. We must know more about the factors, both internal and external, which affect the level of activation, but so far these are imperfectly understood and no attempt has been made in this thesis to explore this considerable problem. One has been content to establish more firmly the nature and limitations of the phenomenon.

The influence of variations in short term activation can be less explicitly stated. The experiments in this field were fewer and the phenomenon is more elusive. The concurrence of a stimulus with a relatively large response, indicating a burst of short term activation, seems to result in the more efficient discrimination of the stimulus, a more efficient response to the stimulus,¹ and more efficient retention of the stimulus after a period of time (see Kleinsmith and Kaplan, 1964, in addition to Experiment a(i) above). But the differences are relatively minor and again the effect tends to be rather trivial. This is not to say that it is not theoretically important but one emphasises the triviality because of the over-valuation that has been placed upon the role of activation by some writers in the recent past.

c) Activation and a Theory of Attention

The emergence of a two-dimensional activation hypothesis from the experiments is hardly unexpected. Physiologists have postulated two types

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See Cowles (1965) who found that the mean reaction times to a light appearing at the peak of a G.S.R. tended to be faster than reaction times to the light when it appeared at a different time than the peak of the response.

of activation for some time. Jasper (1958), in referring to localisation of function in the non-specific reticulothalamic system, mentions the 'rapid, short-lasting or phasic activation of the cortex, in contrast with the slower longer-lasting, tonic activation'. This differentiation is further emphasised by Samuels (1959). Lindsley (1961) states 'These data on the reticular activating system suggest at least a bimodal theory of arousal, alertness and attention.' Observations of both subjective and objective responses in human subjects seem to provide further confirmation for such a concept.

'First we may think of a general arousal mechanism, capable of arousing from sleep or alerting to a general state of attention if the organism is already awake

'Secondly we may think of a specific attention mechanism, based on the neurophysiologically demonstrated interactions within the reticular system and on various sensory relays, which provide the mechanism for differential response to environmental stimuli, with suppression or ignoring of some and heightening of attention for others. This tendency for the development of a specific alertness or 'lock-in' with a given sense modality, to the exclusion of other modalities, seems to be fundamental in perceptual integration'.

It is likely that there is some interaction between these two aspects of activation, and a possible framework within which the interaction can be expressed is provided by Deutsch and Deutsch (1963). This theory relies upon an interaction between level of general arousal and 'level of importance' in a specific alerting system. Basically the theory is one of selective attention, which is something which has received little emphasis in this thesis,¹

¹ But see chapter II, section a,iv, where it is admitted that an explanation of selective attention must be included in any really satisfactory theory of the orienting or sensitising response.

but it conforms remarkably closely to some of the tentative theoretical points which have been raised at various places in the preceding text.

Incoming stimuli are weighted for importance. The specific mechanism whereby this is achieved need not, indeed cannot, concern us here. Deutsch (1960) suggests a possible model but the gap between current physiological knowledge and the formulations in this model is so great that the model is of doubtful value. However, the idea of some cortical, preawareness weighting of incoming stimuli has already been suggested by the author (see Chapter III, section a). The specific alerting system will respond according to the weighting of the stimulus, but the actual overt response will depend upon whether the level of the specific alerting system is raised sufficiently in relation to the general activation level to produce awareness. If activation level is high then comparatively small responses of the specific alerting system will produce awareness. This means that stimuli with relatively low weightings will be attended to. If activation is low then only heavily weighted stimuli will be attended to. The important point is, of course, that the response of the specific alerting system is independent of the level of general activation. This conforms particularly well with our observation that mean G.S.R. level is independent of basal conductance level.

The Deutschs' theory takes no account of individual differences and yet the whole body of experimental work in this thesis has demonstrated their importance. It would appear that there are differences in the size of response, by the specific alerting system, to identical stimuli. This is demonstrated by the results of Corteen's and Blackman's (1965) experiment. What is not clear is whether the differences are due to basic variability in the responsivity of the specific alerting system or to the different weightings attributed by different subjects to particular stimuli in particular

situations. In other words are we dealing with an individual difference which is centrally determined or one which is determined by the different way in which individuals assess the environmental factors? This can be clarified only by testing for the response from the same group in a wide variety of situations.

Another difficulty lies in determining what happens to the concomitant effects of a response by the specific alerting system when there is no awareness. As we are assuming that such a response is exactly synonymous with the sensitising response referred to in section a of Chapter III then we must also assume that the increase in cortical sensitivity which accompanies the response will occur whether or not actual awareness occurs as a result of the response. If this is not so then we cannot say that the sensitising or alerting response is truly independent of general activation level. The experimental demonstration of this would be difficult but not impossible. One would have to replicate Lindsley's (1958) experiment which studied critical flicker fusion in unanaesthetised monkeys, but the animals would have to have their general activation level varied by drugs in order to determine whether specific stimulation of the rostral reticulo-thalamic system would continue to produce the same changes in specific response to flashes under different levels of general activation. The main difficulty here would be in deciding upon drugs which would affect long, but not short term, activation.

It seems likely that there is some limited relationship between general activation level and the alerting response. Oswald (1962) reports the disappearance of G.S.R.'s at the onset of sleep, noting that they reappear later. This indicates that there is an adaptive inhibition of responses, either through the assignation of minimal weightings, or through a direct

inhibition of the response system itself. It seems more probable that the weighting mechanism is partly inactivated. Normally this would be adaptive in that minimal weights are assigned and the individual can go to sleep without being made constantly aware of extraneous stimulation. Sometimes, however, it would appear that the system breaks down and an inappropriate stimulus is assigned a heavy weighting. This would explain the exaggerated response which a half asleep individual might make to a trivial stimulus, such as someone coughing or a door opening. This contrasts with persistent insomnia which is probably explained by a failure of general activation to fall to a level where the adaptive mechanisms described above begin to function. We are, however, beginning to speculate beyond the limits of the evidence and the theory.

The general results of the experiments on basal conductance level seem to be fairly well explained by this theory.¹ If we assume that every relevant stimulus is weighted and produces a response of the alerting system²

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But see discussion on p. 141 below.

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With a fast sequence of stimuli each separate stimulus does not result in a G.S.R. To this extent G.S.R. may not be the ideal measure of activity in a sensitising or activating centre. When a G.S.R. occurs we can probably say that the centre has been activated but when it does not occur in a rapid sequence of stimuli then we cannot say that the centre has not been activated. It is possible that the slow arising peripheral manifestations of the sensitising response are inhibited by subsequent stimuli and that, therefore, in rapid sequences of stimuli they do not appear at all. There is another explanation. That is that G.S.R. is an inevitable concomitant of the sensitising, orienting or alerting response and that a rapid sequence of following stimuli will all have an identical value which was determined by the initial response. This would certainly be a more economic system. The only modification of the Deutschs' theory is to regard series of related stimuli in the same way as a single stimulus, the alerting level being determined by the basic orienting stimulus which may have been something like the word 'ready'.

then the higher the level of activation the more stimuli will the individual be aware of. The more stimuli he is aware of the more efficient his performance is likely to be. This is, of course, a general statement. The specific results of each experiment can be fully explained only by reference to the particular circumstances and this was done in greater detail in the preceding Chapter. The necessity to take into account the specific conditions of the experiment is most obvious when considering the results of the experiment on basal conductance and recall of uncommon words. A straightforward application of the theory to this experiment would lead to a prediction that high conductance subjects will recall more words.¹ The theory has nothing to say about the effect of effort after meaning and it is obvious that in a complex field such as verbal learning the theory is severely limited in its applications. This is hardly surprising, but it does illustrate just how much remains to be done. It is precisely in such fields that a general theory of activation might have its most important applications.

The result of the experiment on G.S.R. and word recall has already been discussed in some detail. The general theoretical framework advanced here adds little to the conclusions already put forward, namely that words associated with large responses will tend to be more thoroughly learned, this resulting in their more effective recall when the influence of short term memory has worn off. There is, however, one considerable difficulty raised by this interpretation, a difficulty which runs throughout this entire two-dimensional activation theory.

If a response of the alerting system raises a stimulus or series of stimuli into the level of awareness then are they all to be regarded as equal?

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But see discussion on p. 143 below.

Or will those stimuli which are raised well above the general activation level which is then current have preference, in terms of faster response, better recall, etc., over those which are raised barely above the general activation level? The results of the G.S.R. and word recall experiment suggest that the latter interpretation is the correct one. It is difficult to imagine that any of the subjects in that experiment were in such a somnolent state that some of the words did not reach the level of awareness. All the words must have reached such a level, and yet some were preferred over others in a significantly systematic way. Now, if there can be degrees of awareness defined by the height to which a stimulus is raised above a basal activation level, then the theoretical independence of alerting response and general activation in relation to performance is a myth. Activation level and alerting response must always summate. This means that if we hold activation level constant then the large responders¹ will, or should tend to do better on tasks relying upon awareness. This is true whether or not all stimuli reach the level of awareness. Similarly, if we hold mean size of response constant, the higher activation subjects should do better on precisely the same tasks. But this is just what was not found in the experiments. Not only was there no correlation between mean response level and basal conductance level, which would be predicted by the theory; there was no prediction of task performance from the measurement of one variable if there was prediction from the other, and vice versa. And yet the theory as stated in its simple form above, would predict that if one variable was related to performance then the other should be similarly related.

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In this context the reason for the large responses is unimportant. They could be centrally or environmentally induced; the consequences will be the same.

This is an obvious criticism of the theory if we interpret it in a completely straightforward way. There are, however, certain qualifications of the above interpretation. Let us consider the G.S.R. and word recall experiment. The relationships that were observed were the result of a tendency for some words to be recalled rather than others. As in the discussion of the results of the experiment on basal conductance and word recall we might postulate a limited memory store.¹ If we assume that more words reach the level of awareness than the store can cope with then all we are saying is that words associated with large responses will tend to be favoured in gaining access to the store and in recall situation. But the response are large only in relation to the other responses made by the individual. This was the reason for converting all responses to standard scores. Therefore, unless the memory store itself is influenced by activation or level of awareness, there is no reason to assume that high responders will recall more words than low responders. This would be true only if fewer words reached the threshold of awareness than the limited memory store could cope with. In this latter situation both large responders and high activation subjects would recall more words.² But in a situation where all words surpass the threshold of awareness the memory store is fully loaded and recall will be influenced only relatively, not absolutely, by the activation level associated with each word.

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We can hardly talk about a limited short term memory store in this content but it is perfectly conceivable to consider some limitation, independent of activation, upon the number of words stored over time.

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In other words we postulate that the size of the memory store is independent of variations in activation, but that, in certain circumstances around the threshold of awareness, activation level, both long and short term, will influence the number of words entering the store.

A similar interpretation can, and already has been put on the results of the basal conductance and word recall experiment. In this experiment, however, it was necessary to consider the possible additional factor of variations in the storage capacity required for each word as a result of differences in effort after meaning produced by differences in activation level. It could be that the heightened awareness of each word which the high conductance subjects would experience might explain the greater effort that they seem to have expended on each word. This, however, is a highly speculative interpretation which would need much more empirical support before it could be substantiated. It might be most effectively tested in a delayed recall experiment.

While one can maintain the relatively unmodified theory in explaining the results of the word recall experiments, the position is more equivocal when we come to consider the results of the three experiments relating basal conductance to various performance situations. Here we would quite definitely expect the mean size of response to have some influence upon efficiency. There are two possible explanations of the observed results which contradicted this expectation.

One is simply that while the mean size of response might have an influence, it was too slight to be observed in the experiments which were carried out. It might be necessary to carry out an experiment in which subjects are matched for basal conductance in order to determine any residual effects of response size. The numbers involved in the current experiments were too small to permit this.

The second explanation relies upon the assumption that general activation level is more than simply a passive base line upon which responses of the alerting system are superimposed. We might, for example, assume that the

level of general activation will determine the efficiency with which responses which rise above the awareness threshold will be dealt with. This would not clash with the findings in the word recall experiment, where the critical factor in determining the absolute number of words recalled is assumed to be the size of the memory store. In fact, if we accept that assignation of adequate meaning represents greater efficiency, at least in one sense, it might well go some way to explaining those results. There is also informal observational evidence to support this hypothesis.

The fact that individuals who are semi-awake tend to make inappropriately extreme overt responses to stimuli which rise above the awareness threshold has already been noted. The response of an individual who has been fully asleep, while less extreme, is even less efficient. The same points can be made about the responses of individuals who are extremely fatigued or suffering from the effects of alcohol. While part of their inefficiency can probably be explained by the number of relevant stimuli which fail to reach the threshold of awareness, some stimuli undoubtedly do reach well above this threshold and yet the overt response is often very inefficient.

This second explanation may be thought of as going too far in emphasising the role of general activation level at the expense of the specific activating response. And yet the evidence supports this position. The Corteen and Blackman experiment illustrates a relationship between G.S.R. and sensory sensitivity but it is assumed in the theoretical discussion of that experiment that the increase in sensory sensitivity and the G.S.R. are both parts of a specific alerting or sensitising response. In other words the relationship exists because the related phenomena are included in the same response. As for the G.S.R. and word recall experiment it has already been pointed out at some length that the effect is relative, that it occurs

in a situation where general activation level is ineffective in determining a difference in performance.

In situations where general activation level is effective it appears that response size is irrelevant, or at least, if we accept the first explanation advanced above, largely irrelevant. Assuming that the majority of stimuli reach above the threshold of awareness,¹ then in situations requiring continuous response it is the efficiency with which the stimuli are handled, as determined by general activation level, and not the actual level of awareness reached by the stimuli, which determines the overall efficiency of performance.²

To summarise: A two-dimensional theory of activation has been postulated. One dimension is general long term activation, the other short term activation as expressed in a specific alerting, sensitising, or orienting response. Various aspects of performance are related to these two dimensions of activation. Short term is influential in determining the relative efficiency with which items in a series of stimuli are handled when some limitation, such as a limited memory store, is placed upon the role of long term activation. Long term activation is influential in determining the absolute level of

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In the reaction time experiment some stimuli probably did not reach the threshold, thus resulting in the abnormally long responses which were noted. In this instance one would expect both long and short term activation to be relevant but unfortunately this was not tested for.

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In relation to a theory of selective attention, as well as in relation to the general theoretical position advanced here, it might be speculated that the inhibitory mechanisms suggested by Corteen and Blackman (1965) operate more efficiently at higher levels of activation. Thus at low activation levels there might be more awareness of irrelevant and disturbing stimuli. This, however, is a hypothesis which requires more thought and empirical evidence than can be afforded here.

performance in tasks requiring the efficient handling of a series of stimuli where no limitation is implied. The two dimensions will interact around the threshold of awareness, and there is probably some interconnection between them at the onset of sleep. One might also repeat the contention that the influence of both dimensions will be relatively trivial in most aspects of human behaviour carried out in a normal waking state. The influence might not be so trivial, however, in such states as extreme fatigue or alcoholic over-indulgence.

d) Drive and Activation

Many writers have identified a general drive factor with activation (Hebb, 1955; Lindsley, 1957; Malmö, 1958; Bindra, 1959; Morgan, 1959; Berlyne, 1960). The list is impressively long and weighty and one hesitates to criticise the position but one cannot help feeling that, while there must undoubtedly be some connection between motivational states and activation, a simple identification between generalised drive and activation is an over-simplification of a type which has bedevilled the whole short history of the activation concept.

Apart from the obvious lack of relationship between generalised drive and basal conductance (see correction of Eysenck and Warwick's (1964) statistics. Chapter III, section b (ii)), and the obvious lack of correspondence between D and basal conductance revealed in the discussion to experiment b (ii) above, one can offer only a brief defence of this critical point of view. An exhaustive review of the whole problem would require a considerable volume in its own right. But first it is necessary to summarise the position that is being criticised.

Those who identify drive and activation tend to assume that both internal and external determinants of drive, that is needs and motives, operate through a central mediating system, as well as specifically, to

arouse an organism to a state where the need or motive might be more effectively satisfied. The central mediating system is invariably identified as the reticular formation. Much emphasis is placed upon the maladaptive effect of very high drive levels or overmotivation. This is particularly true of Bindra (op.cit.) who includes a lengthy account of troops panicking under combat conditions.

This extremely brief summary does less than justice to some of the more sophisticated proponents of the theory, particularly Lindsley, but it does state the general position fairly adequately. The basic flaw lies in the almost exact interdependence of generalised drive and activation. If drive increases, activation increases; a diminution of drive leads to a fall in activation. This makes one or other of the concepts superfluous and yet that is precisely what neither of them is. Drive is a necessary concept to describe the effects of motives and needs, but activation is also necessary to describe the variation in alertness which the organism so obviously experiences. It is possible to conceive of an individual who is highly alert or activated, but who is nevertheless not experiencing any great need or motivation.¹ It is also possible to conceive of an individual who is highly motivated and yet who is at a fairly low level of activation.

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We can, of course, involve the need for stimulation which has been so admirably demonstrated by Butler (1953) but in this sort of situation it is pertinent to ask whether the need produces the heightened activation or the activation produces the need. An exhausted person is unlikely to seek stimulation, while an alert person might. It is necessary to distinguish between needs which are independent of the individual's state of fatigue and those which might arise in an alert person who has no immediate aim in view. This is complex, but the former needs relate to physiological requirements and maintenance of ego and super-ego integrity while the latter simply express the requirement of the organism for some level of non-redundant stimulation when it is fairly alert. The former needs will not be basically affected by changes in activation while the latter will tend to wax and wane with fluctuation in the fatigue level of the organism.

Naturally the two states will interact but this is not to say that they are identical. A possible and very rough theory to explain the interaction arises from the suggestion by Pribram (1961) that the level of activation might act in a way rather similar to the bias control on a homeostat. Thus, activation sets the level at which the organism will be in equilibrium and shifts from this level produced by external events or internal needs will result in negative feedback mechanism bringing the organism back to homeostasis.

This model successfully differentiates between drive and activation in that drive is seen to be the product of needs and motives which define the level which is related to the homeostatic level while activation is represented by the homeostatic level itself. The most effective feature of the model is the way in which it is successful in dealing with discrepancies both above and below the homeostatic level. In other words it can deal with the effects of boredom as well as the effects of high motivation. If the discrepancy results in regulatory response, then under, as well as over- motivation can be seen to be response producing.

As with most homeostats small adjustments are made more efficiently and smoothly than large ones. Thus when the discrepancy between actual conditions and ideal conditions is large (i.e. in states of extreme excitement or boredom) the organism will tend to behave inefficiently, and in extreme cases, unless there is adequate control over the regulatory mechanisms,¹ temporary breakdowns may occur. If efficiency is some more or less direct function of size of discrepancy then we would expect

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In most electrical homeostats the regulatory mechanism (e.g. a heater) is limited by the amount of power available. It is, however, possible to conceive of a system where the regulatory mechanism will be able to adjust its output in order to reduce the discrepancy as rapidly as possible. In such a situation too great a discrepancy will result in overloading and breakdown.

high activation subjects to cope with highly motivating situations more efficiently than would low activation subjects. This is because the discrepancy is less. Similarly, we would expect low activation subjects to cope with boring situations more efficiently than high activation subjects.

The most fundamental difficulty with this model is that a homeostat in equilibrium implies quiescence. If we identify the regulatory mechanisms with the organism's response, either overt or covert, then we imply that, when the level induced by the motivating conditions exactly coincides with the activation level, response will cease. This is probably true, except that in any normal situation the motivating conditions will be in a state of continuous change, producing a level which fluctuates around the activation level. This means that there will be continuous response, of an efficient nature, except, possibly, when the organism is in a state of deep sleep or coma.

This model requires some redefinition of the conventional situation. Needs and/or motives¹ will induce a drive state or level in the organism which will be compared to the current level of activation. Discrepancy between the two levels produces response in order to bring the system back into equilibrium. Needs and motives, therefore, do not result directly in response. They will do so only in relation to the current level of activation.

Activation level is independent of short term shifts in need or motivation, although it might respond to relatively continuous long term

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Need is taken to be the result of a physiological deficiency or pressure (e.g. need for food, water, sex, defecation, etc.) while motive is taken to be the product of an ego or super-ego integrative interaction between individual and environment.

motivating states. This seems to be a reasonable conclusion to draw from the all too scant evidence on the physiological basis of skin conductance which has been reviewed earlier. From this we might assume that activation level is the product of a biochemical balance and that it is to a large extent insulated from immediate environmental events.

This conclusion, which seems to fit both the model and the evidence, runs up against one major empirical difficulty. In all studies of basal conductance carried out by the author it was observed that conductance level dropped throughout the experimental session. With a small number of exceptions the drop was large and consistent. The immediate conclusion was that this represented a drop in activation as a result of increasing familiarity with the experimental situation. If true, this assumption would mean that activation level is directly influenced by environmental factors. There is, however, an alternative explanation which has on several occasions motivated the author to try different electrodes, without success. This is simply that the apparent fall in conductance is due to the drying out of the electrodes during the experimental session. Because this would tend to be more or less consistent for each subject it would not unduly affect the relative measurement of activation level, as can be seen from the results of the reaction time experiment. It would, however, make the absolute measurement of conductance over time impossible and it certainly confirms the conclusion that the high correlation between conductance and reaction time over the six sets of trials was the result of an artefact. This was, of course, more or less definitely confirmed by the observation of an insignificant correlation between change in basal conductance and change in mean reaction time.

To return to the model. Another particularly satisfying aspect is that it explains very adequately the U-shaped relationship between motivation and performance which has been observed, either directly or indirectly, and reported by so many workers (see in particular Malmö, 1959 and Bindra, 1959). As the motivationally induced level moves up to coincide with the activation level performance will increase to maximum efficiency; as it moves beyond the activation level performance efficiency will begin to decrease. This implies that there is no direct relationship between either drive and performance or activation and performance but that performance is determined by the interaction between the two. The results reported in this thesis reflect the probability that for most subjects the tasks produced a drive level above the activation level and that therefore those subjects with high activation levels would tend to perform more efficiently than those with low levels.

This theory or model presents certain difficulties of validation, the most obvious being the problem of defining drive level. Until this can be done a quantitative theoretical statement is out of the question. The best that can be done at present is to define it as the product of motives and needs and study the interaction with activation level in extreme cases. One might expect, for example, that someone with a low activation level would cope more efficiently with an intensely boring situation such as is found in certain stimulus deprivation experiments, while an individual with high activation would cope more efficiently with rather complex highly motivating situations. The latter point has to some extent been demonstrated in the current series of experiments.

This section has basically been written as a plea for an independent concept or dimension of long term activation. To confuse activation with

drive is to miss the value of each concept. The theory that has been presented is rough and incomplete. A full expression and evaluation would be well beyond the scope of this thesis. It seems to clear a certain amount of the confusion which currently surrounds drive and activation theory while at the same time emphasising the separateness of the two dimensions. Full empirical validation would be difficult although not impossible. As with all theories which introduce the concept of drive, the difficulty of operationally defining drive without reducing it to triviality is the main stumbling block.

e) The Dimension of Adequacy - Inadequacy

To some extent the work using anxiety and neuroticism questionnaires was a digression from the main aim of this thesis. It did, however, suggest a dimension of adequacy - inadequacy which was fully described at the end of the last chapter. It is briefly introduced here only because it might have some relevance for the theory which was outlined in the previous section. In a footnote on page 149 it was suggested that the regulatory mechanisms or responses which seem to reduce the discrepancy between drive and activation level might be adjustable in terms of output according to the size of the discrepancy. It was also implied, both in the footnote and in the text, that if these mechanisms were not adequately controlled when the discrepancy was great they might break down temporarily. This idea of control might be synonymous with the concept of adequacy.

In a highly motivating situation such as an examination one would expect the discrepancy between drive and activation level to be great. While part of the control of the regulatory mechanism will be accounted for by the skill of the individual (i.e. his level of knowledge and/or experience)¹

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This will be true of any situation. Skill has largely been ignored here not because of any disregard of its importance but because it has been assumed to be a randomly operative variable.

part may well be accounted for by the general adequacy of the individual. This would explain the observed results. One would, of course, expect the discrepancy to be less with high activation subjects, but the discrepancy would be large in any case and unless one measured activation just before, during, or after the examination it is unlikely that one would have a very useful measure of it to relate to the examination (see section f below).

One does not wish to make too much of this suggestion. The theory is tenuous enough as it stands. To extend it further without considerable empirical support would be foolish. This brief section was merely introduced in order to demonstrate the possibility of a coherent theory relating the intensive dimensions to performance.

f) The Activation Measures as Indices of Individual Difference

Before bringing this dissertation to a close, one short section must be included before the summary. This concerns the usefulness or reliability of measures of activation as indices of genuine individual difference. There is no real argument about this when the measures are taken at a given time. Some will have higher activation than others and this will be reflected in their performance on certain types of task. What we are concerned with here is the reliability or consistency of the measures over time. We would expect scores on an intelligence test, for example, to be much the same whenever we give the test to a set of individuals. This would certainly apply to relative if not to absolute scores. Are basal conductance and mean G.S.R. as reliable measures? Is it as meaningful to talk about someone's level of basal conductance level as it is to talk about their level of intelligence?

The practical importance of this should not be under-estimated. If we can make a once and for all measurement of basal skin conductance which

will specify an individual's general level of activation over months or even years then we will be in a position to make predictions about his likely behaviour in certain situations which we otherwise could not make with any degree of certainty.

Unfortunately there is little direct evidence to be gained on this point from a study of the experiments recorded in the previous chapter. All that can be said on the basis of the present study is that the measures seem to reflect a relative state of the organism which persists over a period of at least an hour.

If basal conductance level is a reasonable, if not perfect, measure of general activation level, and there is now every reason to assume that it is, then we would expect, at the very least, to find diurnal variations. It might, therefore, seem rather futile to consider the possibility of consistent individual difference in a measure which we accept as fluctuating. This, however, is not necessarily true. Provided the measures were taken at the same time of day we might find that some subjects were consistently higher, or lower, than others at that time. We might even find the pattern of diurnal variation to be consistently different in one person as compared with another. While this is speculative it would not be unexpected.

There is more reason to expect consistent measures of mean G.S.R. to standard stimuli. Unlike general activation the specific alerting response is probably less subject to long-term biochemical control. However, absolute differences in size of response seem to be less important than relative differences among the responses of a single individual. Minute distinctions in the ability to make sensory discriminations would not normally be an important factor to measure, although other more behaviourally significant relations may yet be observed.

There is very little definite evidence in the literature on this question of the reliability of individual difference. In general, when measures of basal resistance are made within a day of each other and at approximately the same time of day, correlations ranging from + .73 to + .99 have been observed (Elbel and Rankin, 1946; Obrist, 1948; Freeman and Griffin, 1939). Measurement in these cases was by crude but probably fairly adequate methods. It is likely that the methods used would tend to over- rather than under- estimate the size of correlation. When long intervals intervened between measurement correlations as low as + .07 have been observed (Obrist, op.cit.). The evidence on size of G.S.R. is even more indeterminate, although Freeman (1948) reports that G.S.R.'s remain fairly constant in a given individual when equivalent startle stimuli are given at different times. Corteen and Blackman (op.cit.) found a high correlation between mean G.S.R.'s measured at an interval of 45 minutes.

In general it would appear that differences are fairly consistent over short periods of time, at least up to a day, but as the interval between measurement increases the consistency shows a marked decrease. The reliability over a period of years appears to be negligible.

This is more or less what might be expected. The reliability of any measure of individual difference will tend to be low over long periods of time. With a variable like activation level, which is so prone, by its very nature, to fluctuation, any long term reliability would be unexpected. This topic, however, is so important, both theoretically and practically, that more work is definitely necessary before any final conclusions can be drawn.

g) Summary

As much of this chapter has already been in the nature of a summary it would be pointless to enter into a potentially endless regression by summarising what has already been summarised. This section is, rather, as clear and as brief a statement as possible of the final aims and conclusions of this thesis.

The main empirical aims became, after some preliminary indecision, the investigation of:

- i) The concept of activation through a study of four supposed measures of activation.
- ii) The relation of these measures to a wide range of performance measures.
- iii) The inter-relations existing among the presumed activation measures themselves.

The specific theoretical intention was to examine whether a uni-dimensional activation theory is still tenable and, if not, to consider possible ways in which activation theory might be revised.

The experimental investigation indicated quite strongly the need to consider both short and long-term activation, and that they must be considered as independent though interacting dimensions. It also showed that the straightforward equating of drive and activation effects is at best suspect, and that scores on anxiety and neuroticism questionnaires are not related to activation and may not even be very closely related to anxiety or neuroticism. A new dimension of 'adequacy-inadequacy' was tentatively proposed to explain the results of the questionnaire experiments.

Two general theoretical formulations, apart from the suggestion of the new 'adequacy' dimension, were advanced. In addition a fairly large number of ad hoc hypotheses was advanced in an attempt to explain particular difficult results. As far as possible an effort was made to fit the

hypotheses together and to fit them into the general theoretical framework, in as much as one can be said to exist. This rather ramshackle theoretical framework may be expressed as follows.

The size of the short term alerting responses which are required to reach the level of awareness is directly dependent upon the level of long-term activation. In a set or series of connected stimuli the size of the short-term activating or alerting response to the initial or warning stimulus might determine the weighting of the associated stimuli. When all, or most, stimuli in a connected or disconnected set reach the level of awareness then, all other factors being constant, the stimuli associated with the largest response will tend to be favoured. The actual level of long-term activation influences the efficiency¹ with which stimuli reaching the level of awareness will be dealt with, as well as influencing the actual number of stimuli which will reach the awareness level. The higher the level of activation the greater will be the efficiency, assuming that the task implies a drive level which is 'greater' than the activation level. This is because drive and activation, while independent dimensions, interact to determine the efficiency of performance. The greater the discrepancy between drive and activation levels the poorer the performance will be. Too great a discrepancy might lead to partial breakdown of the system. A tendency to partial breakdown may be reflected in the 'adequacy' dimension.

Looked at in this way it would appear that many psychologists have mistaken the behavioural effects of the drive-activation discrepancy for the direct effects of activation itself.

Most human behaviour in a normal waking state is unlikely to be unduly influenced by variations in either long or short-term activation. Activation

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This efficiency may not necessarily be reflected in the actual assessment of the performance which is decided upon by the investigator.

theory is no substitute for an adequate theory of drive. The most dramatic effects of variations in the activation dimensions are most probably seen in certain specialised tasks and under conditions of extreme fatigue. In such situations, and in certain fields of pharmacological research, a sound understanding of the activation dimensions could have important theoretical and practical implications.

Finally, to repeat the statement made at the end of the first chapter, the main value of the work presented in this thesis must lie in the experimental findings. They indicate the need for a drastic re-thinking of activation theory. The theoretical speculations advanced here represent little more than blind fumbblings towards such a re-thinking. Much more evidence is required before a precise formulation can be advanced and this demands freedom from preconceptions, such as the original activation theorists had, and, above all else, adequate measuring devices.

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APPENDIX I Apparatus for Measuring Skin Conductance and Skin
Conductance Change.

Description: The apparatus is described section by section in the order shown in the block diagram. The block diagram is used primarily to indicate the way in which standard components have been used. A more detailed circuit of the non-standard bridge unit is also given.

1.) Wien-bridge oscillator, battery powered and transistorised. This oscillator was built according to the circuit given in the Mullard publication "Transistor Circuits" (1960). It delivers a 15 cycle alternating current at 4 volts. A battery powered oscillator was chosen because it ensures freedom from mains interference and maximum subject safety. The oscillator is powered by a Mallory cell which has a long life with stable output.

2.) The bridge unit. This is a modified Wheatstone bridge. The fixed arms consist of two matched $12K \Omega$ resistances. The variable arm consists of a helical potentiometer (range 0 - $100K \Omega$) and a decade capacitance unit (range .01 -

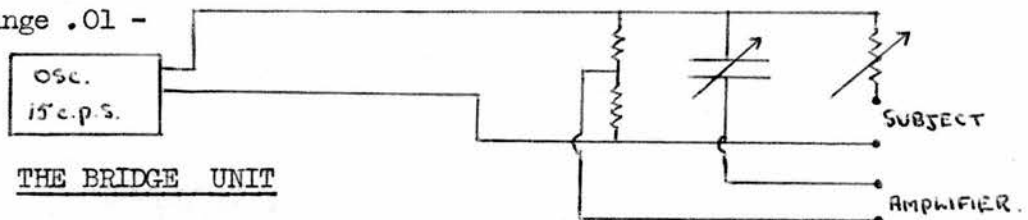


Fig. 1. THE BRIDGE UNIT

1.11) wired in parallel. The use of both potentiometer and variable capacitance is necessitated by the fact that the subjects skin can be represented by a circuit of resistance and capacitance in parallel. The aim of the bridge is to achieve a null and this can only be done if both the resistance and capacitance of the subject are matched in the variable arm of the bridge. The effect of matching capacitances is to bring the alternating current through the variable arm into phase with the current through the subject. The reasons for using A.C. rather than D.C. will be explained below.

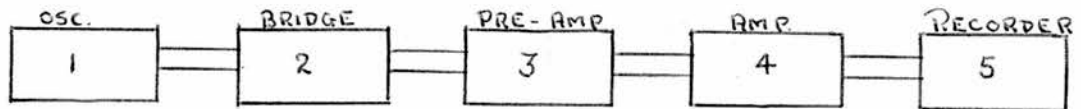


Fig. 2. BLOCK DIAGRAM

3, 4 and 5.) These three units of the apparatus are the pre-amplifier, amplifier and pen recorder which make up the Evershed and Vignoles pen recording system. QU/CRD 11. This system was employed because it gives high amplification at low frequencies and because the pen response is fast and silent, the latter attribute being due to the fact that an electrostatic rather than ink writing method is employed.

Procedure: The amplifiers should be switched on 20 minutes before the apparatus is required for use. This allows for warm-up and drift.

The subject is then connected to the electrodes. Because of the use of alternating current, electrode polarisation problems are eliminated. The electrodes used are silver chloride E.E.G. electrodes covered with a gauze pad and soaked in a Ringer's salt solution. The electrodes are set into the arm of a chair about one inch apart, the leads to the electrodes coming up through holes drilled in the chair arm.* The subject then places the palm of his hand on the electrodes and the hand is held firmly in place by a broad piece of elastic attached to the chair arm.

With the pre-amplifier set at minimum sensitivity the oscillator and pen recorder are switched on. The potentiometer of the bridge unit is then adjusted until pen oscillations are at minimum. Pre-amplifier sensitivity is then increased and residual oscillations are eliminated by

*

In some experiments the electrodes were attached by means of a rubber band and the special chair with mounted electrodes was not used.

adjustment of the decade capacitance dials. Once capacitance has been matched only minor adjustments should be necessary during the course of an experimental session with any one subject.

When the null has been found, this procedure taking only a few seconds with practice, one is ready to present stimuli to the subject. The presentation of stimuli can be recorded by pressing an event marker button set in the bridge control panel.* This will activate an event marker pen, which is incorporated into the pen recorder unit, by momentarily completing a circuit which will feed voltage to the pen.

Once the stimuli has been presented a figure representing the basal resistance of the subject prior to stimulation can be read off a digital indicator connected to the potentiometer. This figure is not in ohms but the basal resistance level in ohms can later be simply read off a linear conversion table.



Fig. 3 RESPONSES

The form of actual responses is shown in figure 3 at a, b and c. These responses are, of course, measured later. With this particular apparatus measurement is very easy because the paper is calibrated in millimeters. This is of undoubted assistance in making accurate measurements.

A rise in subject resistance is shown at d in figure 3. Normally there is no difficulty in distinguishing between responses, or falls in subject resistance, and rises in the basal resistance level. Sometimes,

*

In the reaction time experiment the marker pen was activated by the switch initiating the delay/light sequence.

however, some method of determining whether an increase in pen oscillation represents a rise or fall in subject resistance is found desirable. This is done in the present apparatus by wiring two alternative fixed resistances, one of 10 Ω and one of 100 Ω in series with the variable bridge resistance. When we are in doubt as to what an increase in pen oscillation might mean we bring one of these resistances into the bridge circuit by pressing a button. This has the effect of temporarily increasing the resistance in the variable arm of the bridge. If increasing oscillations represent a fall in subject resistance pressing the button will produce a sudden marked increase in pen oscillation because the bridge will be thrown further out of balance. A sudden decrease in pen oscillation will occur if the subjects resistance is rising. Two different resistance values are used to cope with different levels of sensitivity.

Advantages: The advantages of this system stem mainly from the use of alternating current; though certain additional advantages, some of which have been indicated already, stem from the use of highly developed components. The main advantages are:

- (a) The use of alternating current eliminates electrode polarisation problems. Simple electrodes with low resistance characteristics can be used, and basal resistance is accurately measured.
- (b) Random fluctuation, or 'noise', resulting from skin potential changes, do not affect the record. This noise is a feature of highly sensitive D.C. devices used for measuring skin resistance. Potential changes do come through, but only as movements of the recording pen to left or right of centre. Such changes can therefore be measured independently of the resistance changes, if this is so desired, or they can be ignored. In this respect the present device is a more sophisticated version of a device described by Tolles and Carberry (1960).

(c) The use of a wire-wound helical potentiometer with ten turns to its full range means accurate finding of nulls once capacitance has been matched. The digital indicator makes readings of basal resistance particularly easy.

(d) The instrument is completely safe. The most disastrous series of short circuits could only result in nine volts being fed across the subjects palm. This would not even be noticed.

(e) The elimination of random D.C. potentials means that extremely high degrees of sensitivity can be utilised. Changes in subject resistance as small as 10 ohms in 10,000 can be accurately determined and even smaller changes can be discerned, though accurate measurement is not possible. Resistance changes in response to subtle series of stimuli can be recorded, there is no need to shock the subject into giving a response.

APPENDIX II Skin Conductance Change and Sensory Discrimination

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Investigation was made of the relationship between mean log. change in conductance and sensory discrimination. Highly significant relationships were found with c.f.f. and two point tactile thresholds. No significant relationship was found with discrimination of a pure tone from background white noise. An attempt at explanation is offered in terms of a cortically controlled centre mediating a sensitising or orienting response of which conductance change is a peripheral manifestation.

Introduction Apart altogether from specific sensory defects individuals do vary in their ability to make sensory discriminations. There seems to be some evidence that this inter and intra-individual variability is a function of a common central process mediating all sensory reception (Duffy, 1962, Venables, 1962). Covian et.al.(1961) state that "activity in sensory systems is subject to modification by central regulatory mechanisms and background states, sufficiently pronounced to be capable of influencing perception and so to be of interest for psychology". Recent discoveries concerning the physiology of the nervous system, which will be briefly reviewed later, seem to indicate that this process is located in the non-specific reticulothalamic system. It therefore seems reasonable to assume that if we can measure some manifestation of relevant activity in this system we should find that it relates to sensory discrimination.

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A recent review by Duffy (op.cit.) and some reported research by Venables (op.cit.) seem to imply that sensory discrimination is related to general activation level. The evidence reviewed by Duffy is mainly indirect but on the basis of it she hypothesises a curvilinear relationship between activation and sensory sensitivity. Venables related resting skin potential to temporal visual discrimination and reported correlations of between $-.64$ and $-.85$, but only for subjects falling within a certain range of negative potentials. He found no evidence of a curvilinear relationship.

Resting skin potential has sometimes been observed to be related to resting conductance levels but there is also evidence that no such relationship exists (Tolles and Carberry, 1960). This is emphasised by the fact that Eysenck and Warwick (1964) found no relationship between basal skin conductance level and two-flash thresholds. The experiments to be reported here, however, were initiated on the assumption that a relationship would be observed between basal conductance level, generally assumed to be a measure of general activation level (Malmo, 1959), and sensory discrimination. In all cases measurement of resting conductance level was made with apparatus using an A.C. source, similar to but more sensitive than the apparatus used by Tolles and Carberry (1960), in order to eliminate problems of electrode polarisation.

In a pilot study, using 33 male subjects between the ages of 15 and 18, no relationship was observed between two-point tactile thresholds, measured on the nape of the neck, and basal skin conductance level. When, however, the mean skin conductance change (G.S.R.) in response to 20 words read to the subject was calculated, this measure was found to be significantly related to the two point threshold. A t-ratio of 3.5, d.f.31 (significant

at well beyond the .01 level) was calculated from the difference between the mean thresholds of the group with above average G.S.R. level and the group with below average G.S.R. level.

This finding was somewhat unexpected, particularly in view of Venables' findings and conclusion. An obvious assumption was that G.S.R. is a manifestation of the central mediating process mentioned above. But the experiment was not sophisticated enough to have any such far reaching conclusion based upon it, and it was decided to go ahead with a more thorough investigation utilising a more sophisticated determination of sensory thresholds.

The experiment to be reported in detail here was therefore designed to investigate relationships between:

a) Basal conductance level and visual, auditory, and tactile discrimination thresholds.

b) Mean G.S.R. (log change in conductance) to 20 words and visual, auditory, and tactile discrimination thresholds.

2. PROCEDURE

(i) Subjects: Sixteen male undergraduates from the Psychology Department of the University were used as subjects. Their ages ranged from 19 to 32 (average 22.5 years). None of them had any known visual or auditory defects.

(ii) Apparatus: The apparatus for measuring conductance level and change consisted basically of a Wheatstone bridge unit fed by an oscillator. The current from the bridge was amplified before passing into a quick response pen recorder. Two arms of the bridge were fixed resistances; the third arm was a variable resistance and capacitor in parallel, adjustable to match the impedance of the subject; the fourth arm was the palmar resistance

of the subject which in fact, with alternating current, showed the equivalent circuit characteristics of a resistance in parallel with a capacitor. The variable resistance was judged to be equal to that of the subject when the width of the oscillatory trace on the recording paper was at a minimum (theoretically zero, but practically a few mms. at maximum sensitivity). The G.S.R.'s were shown as characteristic changes in the width of the oscillatory trace. At maximum sensitivity the apparatus was capable of discriminating a change of 20 ohms on a basal resistance of 20,000 ohms.

Two silver-silver chloride electrodes were screwed into the arm-rest of the experimental chair, so that the subject could comfortably rest his right hand, palm downwards, on them. The electrodes were saturated with 3% NaCl solution before each recording, and were kept in clean water between experimental sessions.

The photic stimulator, used to measure the critical flicker frequency (CFF) consisted of a xenon-filled stroboscope lamp seated in a reflector covered by a perspex diffusing screen, and a control unit. The perspex screen was covered by a sheet of dull black paper with a centrally placed $\frac{3}{4}$ " diameter aperture. The position of the stroboscope unit was adjusted so that the aperture was at the subject's eye-level, and at a distance of approximately 36" from the subject. The flash length was a constant 65 usecs; the flash-rate was variable over the range 2-60 cycles/sec., and the intensity of the fused light was 180 ft. - lamberts. It should be noted that whilst the flash length was constant, the light/dark ratio varied directly with the flash-rate.

A Peters basic diagnostic audiometer was used for measurement of the auditory discrimination threshold. This instrument was set to generate a

pure 1,000 cycles/sec. tone which could be passed into either headphone, the intensity of the note being variable. An auxiliary unit also manufactured by Peters was used to generate white noise which was passed into both earphones. The audiometer is so calibrated that it measures hearing loss, not the actual intensity of the note passed through the earphones. The pre-set threshold intensities (without the white noise) are those just perceptible to a test group of subjects between the ages of 18 and 25 years with healthy ears when listening in completely silent surroundings. The white noise provided a constant background from which the single note had to be discriminated, and it served to mask the many extraneous noises which are inevitable outside a sound-proofed room.

The device used to obtain the two-point tactile threshold was a simple aesthesiometer. It was, in effect, a pair of compasses with the needles replaced by blunted ebonite pins at right-angles to the arms. A metal strip, calibrated to give the separation of the points in mms, was attached to one of the arms. The handle of the aesthesiometer was lightly clamped so as to allow freedom of movement only in the vertical plane. This ensured approximately equal pressure of the points on the skin over all the trials.

(iii) Method: The subject, after washing his hands, was seated in the experimental chair, and his right hand placed on the arm-rest so that the two electrodes were in contact with the palm. A strap was tied over the hand in order to minimize movement. The subject was asked to sit comfortably and relax whilst the experimenter adjusted the controls of the recording equipment. Light conversation was kept up, but questions as to the nature of the experiment were side-stepped, other than giving assurance that no "noxious stimuli" were to be used.

The following instructions were then given to the subject:

"I am going to read out a list of ordinary words to you. They will come at about 20-30 sec. intervals. You should not say anything in response to the words, just think about them. Please try and avoid moving your right hand. Any questions? Please close your eyes, now, and relax; do not speak, just think about the words as you hear them".

As soon as the subject's level of resistance had stabilised the first word was read out. The subject's response (a drop in resistance) was recorded, and the next word read out when the level of resistance had again stabilised. This was repeated for each of the twenty words in the list. The sensitivity of the preamplifier was adjusted to maximise the trace-width during the G.S.R. The variable resistance and capacitance components of the bridge were set before each word to give a minimum trace width.

The recorder was equipped with an event marker which was activated by the experimenter at the moment of stimulation.

Exactly the same procedure was carried out at the end of the experimental session, using a different list of words. In the intervening period, the subject's right hand was released from the strap, partly to prevent discomfort, and partly to avoid an increase in the temperature of the palmar surface.

The aim was to use neutral words as stimuli; those chosen were considered to have a minimal emotional connotation.

In the interval between the two sets of recordings, measurements were made of the subject's discrimination thresholds in three sensory modes.

(a) Visual: The critical flicker frequency (CFF) was chosen as the measure of visual sensitivity. The subject's task was to tell the experimenter, who was slowly increasing the frequency of flicker of the stroboscope lamp, when he thought the flickering light had fused. A similar procedure was

carried out with the frequency being slowly decreased from well above the CFF; the subject was asked to indicate when he could first distinguish regular flickering. Five trials were given in each direction, the first in each case being considered a practice trial and the result discarded. The subject's CFF was taken as the average of the eight remaining trials.

(b) Auditory: The subject's task was to discriminate a 1,000 cycles/sec. tone from a background of white noise. A constant level of white noise was fed into both earphones and the single tone into either the right or left earphone. Again the method of limits was used: the intensity of the single tone was decreased until the subject indicated that he could no longer hear it. It was then gradually increased from a sub-threshold value until the subject reported hearing it again. Four trials were given in each direction and for each ear, the results from the first trial in each case being discarded. The measure of auditory sensitivity used was the average threshold value over the remaining twelve trials.

The units of measurement here are to some extent arbitrary, as they relate to standards derived from measurement of absolute threshold, but this is immaterial in the present experiment where a relative rather than absolute measure is required.

(c) Tactile: The measure of sensitivity in this mode was the two-point threshold. This is the minimum distance of separation at which it is possible to discriminate two points touching the skin. The area of skin chosen was that at about 3" above the wrist on the inner aspect of the left fore-arm.

The following instructions were given to the subject:

"Please close your eyes. I will be placing either one or two points on your skin just above the left wrist. Tell me in each case whether you think it is one or two points".

Fifteen practice trials were given at different distances of separation between 0 & 60 cm. Then twenty-five trials were given at 1 cm. intervals centred around the threshold as estimated from the practice trials. The distances were given in a randomized order, and the two-point threshold was taken as that distance of separation which the subject judged to be 'one point' on 50% of the occasions.

The experimental room was kept at a temperature between 20° and 22°C. The experimental session lasted approximately 3/4 hrs.

3. RESULTS

Table 1 shows the average value of the G.S.R., expressed as the logarithm of the change in conductance and converted where appropriate to negative numbers, for the 16 subjects. Column III in Table 1 is the log of the average change of conductance computed from list 1 and list 2.

Table 2 shows the average threshold values in each of the sensory modes.

TABLE I: The Independent Variable

Subject	List I	List II	Av.
1	-1.135	-1.273	-1.198
2	-0.729	-0.337	-0.490
3	-1.222	-0.921	-1.046
4	0.129	0.360	0.223
5	-0.745	-0.436	-0.564
6	-0.486	-0.658	-0.563
7	0.031	-0.227	-0.079
8	-0.232	-0.160	-0.198
9	-0.608	-1.135	-0.798
10	-0.523	-0.824	-0.643
11	0.074	-0.384	-0.097
12	-1.222	-1.482	-1.331
13	-0.147	0.031	-0.049
14	-0.093	-0.714	-0.301
15	-0.796	-1.176	-0.946
16	-0.714	-0.083	-0.292

TABLE II: The dependent variables

Subject	<u>VISUAL</u> (c/sec)			<u>AUDITORY</u> (db)					<u>TACTILE</u> (Cms.)
	Inc. fr.	Dec. fr.	Av.	Inc. int.	Dec. int.	L. Ear	R. Ear	Av.	Av.
1	46.50	47.25	46.88	37.50	44.25	40.75	41.00	40.88	35.5
2	49.50	49.25	49.38	36.63	40.50	39.25	37.88	38.56	37.5
3	44.25	47.75	46.00	35.63	38.13	36.75	37.00	36.82	38.0
4	46.50	49.00	47.75	39.13	41.88	40.63	40.38	40.50	15.0
5	48.33	50.00	49.17	34.13	41.50	38.00	37.63	37.81	23.0
6	48.75	47.00	47.88	31.75	31.38	31.00	32.13	31.56	34.5
7	50.25	48.50	49.38	31.38	37.63	34.63	34.38	34.50	31.0
8	48.67	52.00	50.33	33.25	39.75	35.00	38.00	36.50	32.5
9	49.50	49.00	49.25	37.00	38.63	38.88	36.75	37.83	34.5
10	47.00	48.00	47.50	35.75	35.00	35.00	35.75	35.38	26.5
11	49.75	48.50	49.13	34.25	42.88	37.25	39.88	38.56	29.5
12	46.00	48.25	47.13	35.50	40.50	38.63	37.38	38.00	38.0
13	50.00	49.00	49.50	33.13	36.63	35.25	34.50	34.88	20.5
14	50.00	48.75	49.38	34.13	35.63	34.38	35.38	34.87	24.5
15	47.00	47.75	47.38	34.13	37.13	35.38	35.38	35.63	32.5
16	48.00	49.00	48.50	34.38	35.75	34.63	35.50	35.07	36.0

Analysis of Results

Pearson product-moment correlations were calculated after checking that there was no significant quadratic relationship between the sets of scores. There was no marked heteroscedarity in the distributions.

Variable 1 = Average Critical flicker fusion frequency

M = 48.41 Std. Dev. = 1.17

Variable 2 = Average Auditory threshold

M = 36.71 Std. Dev. = 2.33

Variable 3 = Mean two-point threshold (tactile)

M = 30.56 Std. Dev. = 6.64

Variable 4 = Mean log. change in conductance

M = -0.52 Std. Dev. = 0.43

The following correlations were calculated between variables 1, 2 and 3 and variable 4.

y	x	corr.	significance	best fit curves
1	4	+.650	.01	$y = 1.748 x + 49.323$ $y = -2.190 x^2 - .782 x + 49.013$ (significant linear relationship)
2	4	-.153	.05	no significant best fit curve
3	4	-.657	.01	$y = -10.043x + 25.308$ $y = -7.552 x^2 - 18.770 x + 24.236$ (significant linear relationship)

There were no significant correlations between measures of sensory discrimination.

Discussion. The results obtained indicate a relationship between sensory discrimination and mean log change in conductance only when the discrimination task is one which requires the subject to discriminate between two

or more spatially or temporally proximate stimuli. This fact seems to rule out explanation in terms of a common intervening psychological variable such as confidence level or willingness to risk a judgement. Explanation in these terms would fail to explain the insignificant relationship between mean G.S.R.¹ and the auditory discrimination task.

It would be equally misleading to attempt to explain the results in terms of general activation level. Malmö (1959) and others have pointed out the relationship of basal conductance level to activation but there has been no observed linear relationship between mean log conductance change and basal log conductance level.

It might be more reasonable to assume that the G.S.R. is a peripheral manifestation of a sensitising or orienting response (Berlyne, 1960) which occurs more or less independently of the general level of activation; though Oswald's (1962) observation that G.S.R.'s tend to disappear at the onset of sleep, only to reappear later, does indicate some non-linear relationship between the orienting response and general activation level.

The idea that such an orienting response might be related to sensory sensitivity is supported by several findings. Edelberg (1961) found that tactile sensitivity is enhanced immediately following a G.S.R. and Lowenstein (1956) demonstrated that sympathetic afferent activity increased the sensitivity of tactile receptors. These two findings led Martin and Edelberg (1963) to hypothesise that the G.S.R. may be part of a screening mechanism by which the organism can regulate its sensitivity in accordance with its tendency to reject or accept stimuli.

1

This common abbreviation for Galvanic skin response is widely used and widely understood as a substitute for the more cumbersome log change in conduction.

Jung (1958) found that repeated thalamic and reticular stimulation increased the c.f.f. of specific neurons by 10 to 15 c.p.s. The same author also found that stimulation of the non-specific thalamic system led to an increase in the amount of neuronal discharges and the number of neurons responding to retinal afferents in the visual cortex. These findings by Jung indicate that the central structure largely involved in this sensitising or orienting response is located in the rostral part of the non-specific reticulo-thalamic system, that part which, according to Jasper (1958) "seems to mediate the rapid, short-lasting, or phasic activation of the cortex". That this area is also involved in facilitation of the G.S.R. is confirmed by Wang (1958).

The role of the cortex in the orienting response should not be underestimated. It is likely that it plays a selective inhibitory role upon the strength of the response. This is indicated by the evidence of Wang et al. (1956) and Wang (1958) who noted that the G.S.R. of a cat is enhanced after removal of the forebrain, and by Jouvett (1961) who found that habituation of the orienting response either did not occur or was irregular in neo-decorticate cats. The intervention of the cortex might in part explain the large range of individual differences which are commonly observed in G.S.R. experiments, particularly where meaningful material is involved.

The insignificant correlation between mean G.S.R. and performance on the auditory discrimination task can probably best be explained by referring back to Jung's (op.cit.) finding relating non-specific thalamic stimulation to amount of activity in the visual cortex. If we assume that a similar effect occurs in the auditory area of the cortex then activation of the sensitising or orienting response will result in facilitation of both the pure tone and the background noise with no consequent improvement in discrimination.

The fact that the G.S.R.'s and the measures of sensory sensitivity, although taken within a short time of each other, were not taken simultaneously, indicates that we are dealing with a sensitising system which behaves with reasonable consistency, at least within the time interval involved in the experiment. In other words, response to a variety of stimulus situations is fairly consistent within a given individual over a period of at least 45 minutes. It is, however, certainly not possible, on the basis of this experiment, to state that this is a genuine individual difference which remains consistent over months, weeks, or even days.

In general this experiment indicates that the G.S.R. can best be thought of as a peripheral manifestation of sensitising or orienting response which is basically facilitated by a cortically controlled centre in the rostral part of the non-specific reticulo-thalamic system. The response of this system is reasonably consistent over short periods, but without further study it is impossible to say whether measurement at any one time will provide a long-lasting basis for differentiating one individual from another.

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APPENDIX III

Scales of Anxiety and Neuroticism

Iowa Manifest Anxiety Scale (Taylor 1953)

1. My hands and feet are usually warm enough.
2. I work under a great deal of tension.
3. I have diarrhea once a month or more.
4. I am very seldom troubled by constipation.
5. I am troubled by attacks of nausea and vomiting.
6. I have nightmares every few nights.
7. I find it hard to keep my mind on a task or job.
8. My sleep is fitful and disturbed.
9. I wish I could be as happy as others seem to be.
10. I am certainly lacking in self-confidence.
11. I am happy most of the time.
12. I have a great deal of stomach trouble.
13. I certainly feel useless at times.
14. I cry easily.
15. I do not tire quickly.
16. I frequently notice my hand shakes when I try to do something.
17. I have very few headaches.
18. Sometimes, when embarrassed, I break out in a sweat which annoys me greatly.
19. I frequently find myself worrying about something.
20. I hardly ever notice my heart pounding and I am seldom short of breath.
21. I have periods of such great restlessness that I cannot sit long in a chair.
22. I dream frequently about things that are best kept to myself.
23. I believe I am no more nervous than most others.
24. I sweat very easily even on cool days.

25. I am entirely self-confident.
26. I have very few fears compared to my friends.
27. Life is a strain for me much of the time.
28. I am more sensitive than most other people.
29. I am easily embarrassed.
30. I worry over money and business.
31. I cannot keep my mind on one thing.
32. I feel anxiety about something or someone almost all the time.
33. Sometimes I become so excited that I find it hard to get to sleep.
34. I have been afraid of things or people that I knew could not hurt me.
35. I am inclined to take things hard.
36. I am not unusually self-conscious.
37. I have sometimes felt that difficulties were piling up so high that I could not overcome them.
38. I am usually calm and not easily upset.
39. At times I think I am no good at all.
40. I feel hungry almost all the time.
41. I worry quite a bit over possible misfortunes.
42. It makes me nervous to have to wait.
43. I have had periods in which I lost sleep over worry.
44. I must admit that I have at times been worried beyond reason over something that really did not matter.
45. I am a high-strung person.
46. I practically never blush.
47. I blush no more often than others.
48. I am often afraid that I am going to blush.
49. I shrink from facing a crisis or difficulty.
50. I sometimes feel that I am about to go to pieces.

b) MAUDSLEY PERSONALITY INVENTORY (Eysenck, 1959)

(Underlined items indicate 'N' scale items, others are 'E' scale items).

1. Are you happiest when you get involved in some project that calls for rapid action? Yes ? No
2. Do you sometimes feel happy, sometimes depressed, without any apparent reason? Yes ? No
3. Does your mind often wander while you are trying to concentrate? Yes ? No
4. Do you usually take the initiative in making new friends? Yes ? No
5. Are you inclined to be quick and sure in your actions? Yes ? No
6. Are you frequently "lost in thought" even when supposed to be taking part in a conversation? Yes ? No
7. Are you sometimes bubbling over with energy and sometimes very sluggish? Yes ? No
8. Would you rate yourself as a lively individual? Yes ? No
9. Would you be very unhappy if you were prevented from making numerous social contacts? Yes ? No
10. Are you inclined to be moody? Yes ? No
11. Do you have frequent ups and downs in mood, either with or without apparent cause? Yes ? No
12. Do you prefer action to planning for action? Yes ? No
13. Are your daydreams frequently about things that can never come true? Yes ? No
14. Are you inclined to keep in the background on social occasions? Yes ? No
15. Are you inclined to ponder over your past? Yes ? No
16. Is it difficult to "lose yourself" even at a lively party? Yes ? No
17. Do you ever feel "just miserable" for no good reason at all? Yes ? No
18. Are you inclined to be overconscientious? Yes ? No
19. Do you often find that you have made up your mind too late? Yes ? No
20. Do you like to mix socially with people? Yes ? No

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|------------|---|-----|---|----|
| <u>21.</u> | Have you often lost sleep over your worries? | Yes | ? | No |
| 22. | Are you inclined to limit your acquaintances to a select few? | Yes | ? | No |
| <u>23.</u> | Are you often troubled about feelings of guilt? | Yes | ? | No |
| 24. | Do you ever take your work as if it were a matter of life or death? | Yes | ? | No |
| <u>25.</u> | Are your feelings rather easily hurt? | Yes | ? | No |
| 26. | Do you like to have many social engagements? | Yes | ? | No |
| <u>27.</u> | Would you rate yourself as a tense or "highly-strung" individual? | Yes | ? | No |
| 28. | Do you generally prefer to take the lead in group activities? | Yes | ? | No |
| <u>29.</u> | Do you often experience periods of loneliness? | Yes | ? | No |
| 30. | Are you inclined to be shy in the presence of the opposite sex? | Yes | ? | No |
| <u>31.</u> | Do you like to indulge in a reverie (daydreaming)? | Yes | ? | No |
| 32. | Do you nearly always have a "ready answer" for remarks directed at you? | Yes | ? | No |
| <u>33.</u> | Do you spend much time in thinking over good times you have had in the past? | Yes | ? | No |
| 34. | Would you rate yourself as a happy-go-lucky individual? | Yes | ? | No |
| <u>35.</u> | Have you often felt listless and tired for no good reason? | Yes | ? | No |
| 36. | Are you inclined to keep quiet when out in a social group? | Yes | ? | No |
| <u>37.</u> | After a critical moment is over, do you usually think of something you should have done but failed to do? | Yes | ? | No |
| 38. | Can you usually let yourself go and have a hilariously good time at a gay party? | Yes | ? | No |
| <u>39.</u> | Do ideas run through your head so that you cannot sleep? | Yes | ? | No |
| 40. | Do you like work that requires considerable attention? | Yes | ? | No |
| <u>41.</u> | Have you ever been bothered by having a useless thought come into your mind repeatedly? | Yes | ? | No |

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|------------|---|-----|---|----|
| 42. | Are you inclined to take your work casually, that is a matter of course? | Yes | ? | No |
| <u>43.</u> | Are you touchy on various subjects? | Yes | ? | No |
| 44. | Do other people regard you as a lively individual? | Yes | ? | No |
| <u>45.</u> | Do you often feel disgruntled? | Yes | ? | No |
| 46. | Would you rate yourself as a talkative individual? | Yes | ? | No |
| <u>47.</u> | Do you have periods of such great restlessness that you cannot sit long in a chair? | Yes | ? | No |
| 48. | Do you like to play pranks upon others? | Yes | ? | No |

APPENDIX IV. Some Notes on Methodological Problems

From time to time in the text and in appendices I and II reference has been made to problems found in the measurement of skin conductance. These, together with some that have not been specifically referred to elsewhere, are summarised here. While most of these problems have been reported by other workers in the field at least one is peculiar to the apparatus used while others persist despite apparent solutions which might be appropriate for less accurate and sensitive apparatus.

- a) **Electrodes.** Wet electrodes were used and this introduced the problem of electrode drying, resulting in a spurious rise in resistance. Dry clip-on, suction, and plate electrodes were tried without marked success, the main difficulties being capacitance matching and high basal resistances with loss of sensitivity. It is possible that suitable dry electrodes could be evolved and these should make for a great improvement in the measurement of activation changes over time. The use of electrode jelly or other abrasive pastes is not recommended as it results in artificial changes in resistance which would probably not be uniform for all subjects.
- b) **Electrode Placement.** After extensive trials it was decided to place both electrodes on the palm of the subjects left hand. This ensured that only palmar resistance was being measured and it resulted in much more sensitive measurement of conductance changes. The main problem arising from this placement was that minor movements of the subject's hand produced massive interference with the record, sometimes resulting in pen breakage. Attempts to fix the

electrodes to a wooden base and to strap the subject's hand on top of them were only partially successful. After prolonged use the base became saturated with salt solution and the subject was short-circuited out. An improved base might be the answer, together with a cut-out device to prevent overloading of the pens.

- c) Measurement during Performance. Because of the sensitivity of the measuring device to movements of the subject (see above) it proved impossible to measure conductance during performance of any but the most passive task. With active tasks conductance measurement had to be made either before or after task performance. This may not be a very serious limitation when task performance is of short duration but if one desired to measure performance over a prolonged period the limitation could be serious. A possible solution to this problem is the placement of electrodes on the sole of the foot, perhaps using a specially designed shoe.
- d) Capacitance Matching. This was difficult and time consuming. In many cases completely accurate matching was impossible and a true null trace could not be obtained. There is no obvious solution to this problem. Switched condensers have to be used, as a variable condenser in the microfarad range would be grotesquely large. Use of a backing-off voltage was contemplated but it was decided that it would be difficult to limit its effect to the residual produced by phase difference.
- e) Extraneous Influences on Basal Conductance. Measurement of basal conductance may not always be a true reflection of activation level. Hemphill (1942) has emphasised the influence of dehydration (e.g. after a brine bath) and has pointed out that abnormally high

resistance levels are recorded from females during menstruation. This latter point is probably the most important and one should avoid using as subjects females who are menstruating. It is also true that certain forms of diabetes can result in partial or complete sympathectomy. This is bound to have an influence on conductance measurement and diabetic subjects should be avoided. The easiest way to eliminate unsuitable subjects is simply to ignore any results obtained from them.