

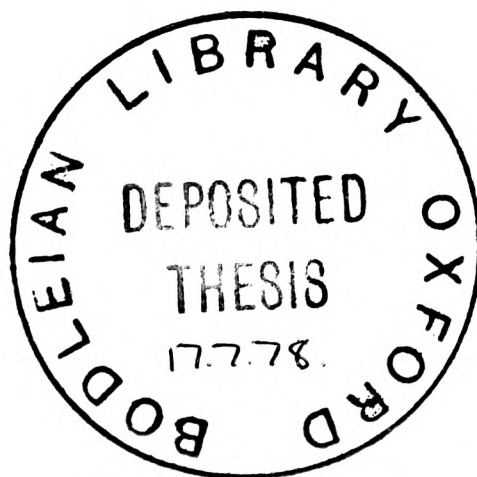
CRITERION CHANGES IN RELATIVE DISCRIMINATION TASKS

Thesis submitted for the degree of Doctor of

Philosophy in Psychology

by

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"There is one other difficulty in judging the length of lines which must be considered. This is the matter of the order in which the lines appeared..... It is very probable that the judgement of a given pair (of lines) is a function of the length of the preceding lines as well as of the two which are presented simultaneously at any time."

Telford 1931 p 31.

"..... but the decision as to what proportion in the sample is to be called heavier, what undecided and what lighter depends on a conscious act of the subject and can be varied if he is so disposed at his whim; and will vary with his mood at the moment."

Thomson 1920 p 307.

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Finally, I should like to thank my wife for aiding and abetting this whole project.

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ABSTRACT
of
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The thesis begins with accounts of various models of discrimination and of previous studies of sequential effects. It is mainly concerned with the study of discriminations in serial response tasks in which the difficulty of the discriminations varies from trial to trial. All the experiments reported were concerned with the discrimination between two lines of different length which were presented simultaneously in a display. There were always two levels of difficulty of discrimination, which were mixed in random sequence.

The first two experiments demonstrated that the RT's to displays were affected by the average difficulty of the preceding displays ("local difficulty bias effect"). In Experiments 3, 4, 5 and 7 it was shown that the overall difficulty of a run of displays also affected the mean RT to all the displays in a run ("overall difficulty bias effect"). It was seen that the direction in which the RT's were affected by local difficulty bias was sometimes different from the direction of the effect of overall difficulty bias, suggesting that subjects may be aware of both local and overall average difficulty levels.

Experiments 4, 5 and 6 showed that instructions to be fast modified the effects of bias, but that instructions to be cautious did not increase the effect of bias. Experiment 7 investigated the effect of increasing the actual difference between the easy and difficult discriminations - but was rather inconclusive.

In Experiment 8 the RSI was varied and showed no major effect on local difficulty bias, showing that these effects were due to other factors than the time between trials. It also supported an hypothesis considered in earlier experiments that when a criterion is not being used (either during the interval between decisions, or while another criterion is being used) it will drift towards a value that will give rise to more cautious decisions.

When displays were easy to discriminate subjects were able to track their speed to the errors they made, but they were not able to do so when the discriminations were more difficult. Whether the displays were easy or difficult, subjects were able to track their speed to the average difficulty of the displays. In general, as the average difficulty of the displays increased (as the proportion of difficult displays in a mixed run of easy and difficult displays increased), so the mean RT of the immediately following displays became slower (local effect). It was suggested that subjects' adjustments in performance which were induced by difficulty bias or instructions could be explained by changes in the values of criteria.

CONTENTS

		<u>Page No.</u>
	Summary	i
<u>Chapter</u>		
1	Introduction	1
2	Studies of Sequential Effects	37
3	The Influence of Difficulty on Criteria	59
	Experiment 1	59
	Experiment 2	71
4	The Effect of Overall Difficulty on Reaction Time	85
	Experiment 3	89
5	Instructions and Levels of Difficulty	99
	Experiment 4	102
	Experiment 5	108
	Experiment 6	124
6	How well do Discrimination Models describe Empirical Data?	132
7	Further Studies of the Effect of Difficulty	149
	Experiment 7	149
8	The Interaction between RSI and Difficulty	164
	Experiment 8	164
9	The Role of Errors in the Adjustment of Criteria	174
10	Final Discussion	191
	Bibliography	202
	Appendices	

Tables and Figures.

Tables and Figures referred to in each chapter are to be found separately (Tables first and then Figures) at the end of the particular chapter. They are numbered thus: Table x-y, or Figure x-y. x is the chapter at the end of which the page is to be found, and y is the number of the Table or Figure within the chapter.

Analyses of Variance and Calculations of Correlation will be found at the end. ANOVA's are numbered ANa-b, where a refers to the number of experiment and b is the number of the analysis for that experiment.

Note: There are no Tables numbered 9-4, 9-5, 9-6, or 9-7, nor Figure 9-3.

SUMMARY

of

CRITERION CHANGES IN RELATIVE DISCRIMINATION TASKS

Central to most of the theories attempting to describe the processes involved in discrimination tasks is the concept of a moveable criterion. Few attempts have been made to explain how these criteria are set, or how they are subsequently maintained by the subject. This research attempts to investigate the properties of criteria operating in discrimination processes and in particular how such criteria can be adjusted according to instructions from the experimenter to the subject, and according to the difficulty level of the discriminations.

The eight experiments described in this report were similar in many respects. They were concerned with the discrimination between two lines of different lengths which were presented on an Oscilloscope under the control of a mini-computer. The responses were always highly compatible and mostly involved subjects pressing a key on the side of the longer line - though some subjects had to respond to the shorter line as a methodological check. There were two formats of stimulus display. The simplest involved pairs of parallel vertical lines, with the upper ends of the lines in the centre of the screen. The lines had a common base and differed at the upper ends. The more complicated format included the displays just mentioned, but also had displays in which the lines were in the upper half of the screen, differing in length at their lower ends. In both formats the longer line would be to the left or right with equal probability. Except for Experiment 8, each trial comprised the presentation of a pair of lines, in either the

upper or lower half of the screen for three and a half seconds, during which the subject was meant to respond, followed by a 200 msec Response-Stimulus interval during which the screen was blank. The task was paced to the speed of the subject's responses. In Experiment 8 the RSI's were longer, and the displays could stay up for up to 40 secs, though subjects usually responded within about three seconds.

The shorter line in all displays was a standard length - the actual length varying slightly from one experiment to another. The longer line of a pair was one of two different lengths. The shorter length, when paired with the standard, led to a 'difficult' discrimination, and the longer line produced an 'easy' discrimination. One of the main features of the experiments was that displays that were easy to discriminate were mixed with displays that were difficult to discriminate.

In Experiment 1, all displays were in the lower half of the screen, and in Experiment 2 displays were in both top and bottom halves of the screen. In both experiments displays that were easy to discriminate and displays that were difficult to discriminate were mixed together in runs in equal proportions. The experiments demonstrated that RT's to both easy and difficult displays were affected by the average difficulty of preceding displays.

In order to demonstrate this, an analysis procedure was introduced, which was used throughout the experiments, this was called the Sample Size analysis. In the analysis each trial was categorised according to the trials preceding it. A sample window was set up to include one to six or eight previous trials. Within the window a count was made of the number of trials that had been easy discriminations. Thus, if the window covered three

trials, there could be 0, 1, 2 or 3 easy (3, 2, 1 or 0 difficult) trials.

It was found in Experiment 2 that as the proportion of easy trials in the larger window sizes increased, so the RT to both easy and difficult trials reduced. Subjects were speeding up after easy trials. In Experiment 1, RT's to easy trials were similarly affected, but RT's to difficult trials became slower where there were higher proportions of easy trials. It was felt that these results were consistent with the subjects of Experiment 1 using two criteria - one for each level of difficulty - and subjects in Experiment 2 using only one criterion for both difficulty levels.

Certainly both experiments demonstrated that the difficulty level of preceding trials affected the RT to later trials. This effect of the immediately preceding trials on the RT of a later trial was called the "local difficulty bias effect".

In Experiments 3, 4, 5 and 7 the proportions of easy and difficult displays in a run were not equal. The proportion of easy to difficult discriminations was either 3:1 (easy bias) or 1:3 (difficult bias). It was found that these changes in proportions affected the RT's to both the easy and difficult displays in a run. Only when subjects were responding quickly was there little or no effect of this "overall difficulty bias". For subjects who were responding more cautiously the latencies of responses to the two biases differed. The usual direction of the effect was that easy bias runs were on average faster than difficult bias runs. This overall or global effect of difficulty was seen to be separate from the local effect mentioned above, as subjects could be influenced in one direction by the average

difficulty level of immediately preceding trials, but in the other direction by the overall difficulty level of the run. For example, for one subject in Experiment 7, discriminations following a high proportion of easy trials within the run were faster than average, but easy bias runs were on average slower than difficult bias runs. This was taken to indicate that subjects were separately aware of the overall difficulty of a run and of the local difficulty within a run.

The effect of different instructions about speed or caution was examined in Experiments 3, 4, 5 and 6. It was found that subjects who responded very quickly showed very little effect of difficulty bias. When subjects were more cautious, it was found that the level of effect of overall difficulty bias was independent of the degree of caution shown by the subjects.

- We had seen that as subjects became more cautious, so
1. the RT's to both easy and difficult trials increased;
 2. the RT's to difficult displays increased by more than the RT's to easy displays;
 3. the difference in error rate between easy and difficult displays remained fairly constant, except for one or two subjects who were responding very quickly or very slowly.

In the former case it was assumed that the error rate would be increased by subjects guessing on some of the trials, and in the latter case it was assumed that it was because subjects made very few easy errors to easy displays.

These facts and the independence between caution and the effect of overall bias supported the hypothesis that subjects did not use the decision time of the discriminations as an indication of the difficulty of the discriminations. This was supported

further in Experiment 7 where the difference between easy and difficult displays was increased. For such displays the absolute level of bias effect increased slightly. This implied that subjects were in some way directly able to estimate the difficulty of a task independently of their knowledge of how long it took them to make the decision.

Analyses of variance were performed on the data to try to tease out the source of the local difficulty bias effect. The question was whether the effect was due to the subjects' awareness of the average difficulty level of preceding trials, or alternatively whether it was a small cumulative effect in which small alterations to average RT were being brought about by the difficulty level of the preceding trials. It was felt that the latter was the case, and that the trial which maximally influenced the latency of any trial n was found to be trial $n-2$. This was considered curious, since intuitively one would suppose that the level of difficulty on any one trial would have its maximal effect on the latency of the immediately following trial. The influence of the difficulty of trial $n-2$ was seen in several experiments. The preceding trial which had the greatest effect (not always significant at 5% - often at 10%) on the RT to a particular trial was always the trial $n-2$. This was even seen in Experiment 8.

In Experiment 8 the influence of long inter-trial intervals on local bias was studied. The Response-Stimulus intervals were increased from the standard experimental duration of 200 msec to 500, 1500 and 4000 msec. At both the shorter intervals it was the difficulty of trial $n-2$ that had the maximal effect on the latency of trial n . At the long interval, the difficulty of trial

n-1 had some influence on RT, and trial n-2 had no influence. This suggested that subjects adjusted to the preceding difficulty in small cumulative steps, but that it took some time for the adjustment to be made. When the RSI was long (i.e. at four secs) subjects could make some adjustment before the next trial.

The effect of local difficulty bias was as large in Experiment 8 as in the earlier experiments, indicating that it was unaffected by the long R-S intervals. This also supported the hypothesis that subjects were not remembering the average difficulty of preceding trials, but rather that the effect was a small cumulative one. If subjects had been remembering the previous difficulty of trials, it was thought that the memory of those trials should be poorer if the interval between successive trials was increased as they were here.

Finally, support for this "cumulative" hypothesis also came from Experiment 7. The direction of influence of the difficulty of trials n-1 and n-2 were compared with the local bias effect, and agreement was found to be greatest for trial n-2. This is a different point from that indicated by the result of the Analysis of Variance quoted earlier. Those analyses simply showed that the difficulty of trial n-2 had most effect on the latency of trial n, whereas this comparison indicated that the direction of the effect of trial n-2 and the local bias were generally the same.

The subjects in Experiment 5 were instructed either to be fast or to be very careful. The discriminations in Experiment 5 were rather easier than most of the other experiments, and it was found that subjects adjusted their speed after making errors to easy trials. Such adjustment has been described by Rabbitt and Vyas (1970) as "tracking" to errors. Subjects did not track

to the errors made to difficult trials. This implied that in the experiments where the discriminations were on average more difficult, subjects would not be able to track their performance to errors, and so must be tracking to the difficulty of the trials. This probability was examined further by re-analysing some of the data by a Sample Size analysis in which errors were excluded from the windows, i.e. trials were only considered when they had not been preceded by an error. The average preceding difficulty level was still found to affect RT's to later trials, demonstrating that subjects were able to adjust their performance to fluctuating levels of difficulty without making errors.

In Chapter 6 the predictions of the various models are examined with particular reference to the data of Experiments 3, 4 and 6, which involved quite large changes in caution by the subjects. Generally, the model which fitted the data best was Vickers' Accumulator Model (Vickers 1970). The models were also considered with reference to the Speed-Accuracy Tradeoff functions that were observed during the experiments. It was concluded that, although the models matched the observed changes in error rate and latency reasonably well for subjects who were responding fairly cautiously, they did not match the observed values when subjects were responding quickly. It was felt that the models needed supplementing with other postulates such as the fast guessing model proposed by Ollman (1966).

In conclusion the main findings were, first, subjects do adjust their performance to the difficulty level of a task. Second, the time taken to make discriminations is not used as a measure of their difficulty. Third, the RT's to displays in a serial response task are influenced quite separately by the

overall difficulty of the discriminations used, and by the difficulty of the preceding discriminations. Fourth, subjects are only able to track their performance by the errors that they make when the discriminations they are given are easy; when they are difficult, errors have very little effect on the latencies of sequent responses.

CHAPTER 1

INTRODUCTION

Psychologists have long been interested in how people discriminate between different alternatives. Early investigators studied 'just noticeable differences' and related these to the magnitude of the stimuli being compared. Such initial investigations were concerned with the measurement of such differences - the determination of difference thresholds. More recently the emphasis has moved away from the quantification of these values and towards the explanation of such events. Such studies fall into three areas. First, there is the detection of signals at a threshold level, where the discrimination being made is one between the presence or absence of weak stimuli. Second, there is the identification of a stimulus as a member of a set. Here the discriminations are generally between the stimulus that is presented and a set held in memory - such as the identification of letters or digits. Third, there are discriminations between two or more simultaneously presented stimuli. It is the simplest form of the latter task of relative discrimination which will be relevant to this report.

Theories that refer to the discrimination of two simultaneously presented stimuli evolved from earlier explanations concerned with the detection of threshold level stimuli.

Cattell (1893) and Solomons (1900) independently suggested that the central effect of a stimulus on the brain activity associated with a stimulus would vary. Thurstone (1927) was more explicit and indicated how the variable stimulus input could be matched on to variable response output. There would, he claimed, be variation

from trial to trial. Békésy (1930) then proposed a theory of hearing in which different sound amplitudes would excite integral numbers of states in the observer. When the signal as represented in this manner exceeded a criterion value then the observer would detect the signal. The criterion considered was a fixed one, and more or less remained so until the advent of signal detection and other related theories of threshold detection.

The notion of a criterion had thus been introduced, though as yet was really considered a fixture of the sensory system. In considering thresholds it was probably very easy to envisage fixed limits to the system that would regulate the flow of sensory input - rather like a weir across a river. When the level of sensory stimulation reached a sufficiently high level the criterion would be exceeded and detection would occur. The next step in the process was to exchange the fixed weir for adjustable floodgates. This was a much harder concept to envisage, i.e. that the criterion within the observer was movable. It would be much easier to envisage variable performance when considering decisions and discriminations, which are after all centrally mediated and may well be affected by whims and desires. Thomson (1920) discussed difference judgements with reference to lifted weights in which he allowed three categories of judgement: heavier, lighter and undecided. One of the conclusions that he came to was that the results depended on the "physiological make-up" and "mood" of the subject - which varied. In modern parlance we would consider that the criteria used by the subject in his decision could change.

Telford (1931) was also aware that criteria change and considered that the performance at any one time during a session would depend on the stimuli preceding it. This was a direct acknowledgement of the importance of context and of the way in which context can influence the criteria now being used.

Related to this attention to context are the reports by Volkmann (1936) and Rogers (1941) who considered the scales used by subjects in performing absolute judgements. They demonstrated that the same stimuli will be scaled differently when paired with different sets of stimuli. This notion that the value of the internal representation of a stimulus will be altered by the values of the set of preceding stimuli has been expanded into Adaptation Level Theory by Helson (1964) of which there is an account and extension by Broadbent (1971).

The concept that there are adjustable criteria which are used by observers to categorise their sensory input as a basis for discrimination is now widely accepted - at least as a working hypothesis. This can be seen by the inclusion of criteria in the large number of theories that have been proposed over the last twenty years. Some of the main summaries are to be found by Smith (1968), Broadbent (1971), Audley and Pike (1965), Audley (1971), Vickers (1972b, 1970, 1972a) and Teichner and Krebbs (1974).

A summary of some of the main theories of discrimination will follow with the aim of identifying how the various theories utilise criteria.

In the descriptions of the theories the following classification will be used:

1. Stimulus Orientated Theories

(a) Continuous Input

The internal representation of the signals is assumed to be continuous or approximately so.

- i. Stable criterion. Criterion assumed to be either fixed or with a fixed mean during a run of stimuli.
- ii. Adjustable criterion. Criterion assumed to move, subject to learning.

(b) Discrete Input

The internal representation of the signals is assumed to be one of a relatively small number of categories - usually two.

2. Response Orientated Theories

(a) Two, three and multi-state Models

Input to the system is assumed to be one of two, three or more alternatives. Concerned with the conditional probabilities of responses given the input state.

- (b) Tradeoff assumed between the relative preparedness of the two responses.

It is possible to classify theories of discrimination into two groups according to their main emphasis. They will be called for the moment Sampling Theories and Response Theories. All the Response Theories can be considered as learning theories in that they predict sequential effects based on response and stimulus history. Only one group of theories related to stimulus sampling attempts to explain sequential effects; mostly they are content to predict overall performance. Though the facility to allow change in performance is present there is no attempt to quantify how this change might occur.

1. Stimulus Sampling Theories

These are so labelled because they emphasise the data collection aspect of discrimination. Within the group there are two subdivisions which refer to whether the internal representation of the signal is considered to vary continuously with the external signal, or rather to be one of a small number of discrete categories.

1. (a) Continuous Data

These are the Signal Detection Models (SDT). Sensory input channels are considered to be noisy. This noise is normally distributed. When a signal occurs it is added to the noise to produce a second normal distribution. The mean difference between the two distributions will be the value of the signal, so at threshold levels the two distributions will be close. Subjects are considered to perform tasks of detection by selecting a criterion value which will be used by them to partition the input. Values above this criterion are considered as signals and those below as noise. The criterion is ideally located in a position

where the maximum likelihood ratio occurs - or where the ratio between the two ordinates of the normal curves - signal plus noise to noise - is optimal. Smith and Wilson (1953) produced such a model but it was rather overshadowed by the more complete accounts produced in various reports by W. P. Tanner and his colleagues. Summaries can be found in Green (1960), Licklider (1959), Swets (1961), Swets, Tanner and Theodore (1961) and Luce, Bush and Galanter (1963) who also consider something of the history of detection experiments.

Briefly SDT is as follows. When there is no signal at a sensory input, there is nevertheless considered to be a certain level of randomly fluctuating excitation. This is distributed as a normal curve and is referred to as the Noise Distribution. When a signal occurs, the excitation of the signal is added to the noise to form a new distribution with a new mean. Typically the standard deviation is assumed to remain the same as for the noise distribution but it is possible to allow for it to differ. This is called the Signal + Noise Distribution (S+N), see Figure 1-1. The difference between the means of the Noise Distribution and the S+N Distribution is the value of the signal. When the signal is weak, the difference will be small and the two distributions will overlap to a large extent. The subject is said to make decisions by choosing a cutoff point (criterion) at a particular level of excitation. Any level of excitation above this point is assumed to be the result of a signal, and below that point due to noise. If the criterion is placed midway between the two means, then when the signal is weak, there will be a considerable overlap of the two distributions and a high proportion of signal will fall below the cutoff and will be classified as noise. These resulting errors are known as misses. Conversely, on a similar

proportion of occasions the noise distribution will fall above the cutoff and the resulting state will be classified as a signal. These errors are called False Alarms. If the subject wishes to reduce the number of false alarms, he can do so by moving the criterion in the direction of the signal distribution. He will reduce his false alarm rate but at the same time will miss more signals. By examining the data for the proportions of the two types of error it is possible, with reference to normal curve tables, to estimate how far apart the two means are in standard units. This distance is called d' prime (d'). When the cutoff is moved by the subject in response to environmental variation, such as new instructions or different monetary payoffs, then it is possible to predict how the two kinds of errors will be traded against each other, again by reference to normal curve tables. This relationship was confirmed very early in SDT history by Smith and Wilson (1953) and many times later, for instance, Swets and Sewell (1963), Galanter and Holman (1967) and Shipley (1965).

The theory of SDT is quite easily adapted to one which will account for discrimination between the two alternative signals. The input to the subject's decision making machinery is taken to be a value that relates to the difference between the two signals. The mean of the difference is the difference of the mean of the two signals, and the standard deviation is taken to be the square root of the sum of the two variances. In the commonly assumed case in which the two variances are assumed equal, then

$$SD(\text{difference}) = SD(\text{signal}) \times \sqrt{2}$$

This is the standard error of a difference equation from statistical decision. If the task is to decide whether or not the two signals differ, then the situation is exactly like the one described above for Noise and S+N. Again the subject has to decide whether the difference which he observed came from a distribution of mean zero (no difference) or of mean other than zero (signals differ). The criterion can be moved as before to vary the proportion of false alarms and misses.

Predictions of choice take exactly the same form when two stimuli are presented simultaneously and the subject is asked to decide which is the 'longer', 'larger', 'nearer', etc. The only change is that the two difference distributions are now assumed to be symmetrically placed about zero. The unbiased position for the criterion is at zero. At that point the two alternatives are equally likely to be missed or detected. The situation is slightly simpler in some respects, since errors can no longer be classified as misses and false alarms. Any movement of the criterion from a central point will lead to a bias towards the identification of one stimulus. In the usual case, where there are two signals and two responses, this bias is confounded with response bias. This is the basic form of SDT applied to discriminations. Adaptations which are required to account for changes in the subjects' levels of caution in such tasks will be discussed shortly.

The important thing is that SDT supplies a framework in which criteria can be seen to be used and in which the effects of variations in criteria can be predicted.

Such theorising typically led to experiments in two alternative forced choice tasks (2AFC). In these, the subject is presented with two intervals of time. He is informed that the signal will occur in

one of the intervals, and must decide which. Green and Swets (1966) and McNicol (1972) have shown that when allowance is made for the difference standard deviation, estimates of d' under 2AFC conditions were similar to estimates using other procedures.

The criterion in SDT is considered to be stable over a run of observations, though Wickelgren (1968) has added the refinement that criteria can and will change constantly as normally distributed random variables, but with a variance much smaller than the noise or signal distributions. Holland (1968) and Gravetter and Lockhead (1970, 1973) consider a similar situation in which criteria vary according to the entire range of all criteria used in a multi-stimulus situation. With only two stimuli their model would be very similar to one assuming that criteria fall midway between equiprobable stimuli.

It has been shown that SDT can predict both how error rates vary as criteria are adjusted, and also the ratio between the two kinds of error which can occur - false alarms and misses. It has had very little success in relating such decisions to reaction time - a fact lamented by Thomas and Myers (1972) and commented on by Vickers (1970). It has been assumed by several authors (see Thomas and Myers op.cit.) that reaction time decreases monotonically with the distance of the signal distribution mean from the criterion. This is approximately the relationship that is normally observed, but there is no satisfactory explanation of the mechanism involved. Crossman (1955) suggested that subjects could make a whole series of such decisions/^{as}necessary, and average them in a statistical sampling manner in which the variance of the difference distribution would decrease directly with the size of sample. However, this approach leads to a prediction that reaction times vary as the square of the distance from the cutoff

which does not occur in practice. McGill (1967) and Luce and Green (1972) have also proposed solutions which are described below.

There remains one more problem when signal detection theory is applied to relative discriminations. The theory states that the observer examines a series of differences between the two alternative stimuli. It would seem a simple matter for him to compare the input with a criterion placed centrally between the two expected distributions. If there is only one criterion then the decision is straightforward - but what happens when the subject is required to be more careful? The observer cannot move his criterion without biasing his responses - and certainly not without moving it from a (probably already optimal) central position. He has two alternatives:

- (i) He may use two criteria. This has been suggested by La Berge (1962) and Cartwright and Festinger (1943). The two criteria classify the input into three states, see Figure 1-2. The two extreme states lead to responses, while the central area is neutral and leads to no decision. In this case a new sample is taken. Greater caution is achieved by moving the two criteria further apart.

However, as Vickers (1972b.) states:

In any case the theory can only deal with situations in which the stimulus exposure is not controlled by S: given the opportunity to make unlimited observations there is nothing in the theory to suggest why S should ever stop.

- (ii) Whether or not the observer uses two criteria he may not make a decision after one look at the data but may decide, having categorised it, to take a further sample and thus accumulate further evidence in support of the decision to

be made. This really takes the case away from SDT and into consideration of models using discrete signal classifications.

In SDT then there is no real attempt to specify how the criteria are set in the first place nor how they are later adjusted. The theory is merely concerned with the post hoc calculation of the position of criteria from observed performance. The next theories we review attempt, in this SDT framework, to predict how such adjustments can be made.

1. (a) ii. Learning from Continuous Input

Kac (1969) suggested that the criterion as used by SDT can be moved from trial to trial when errors are made. He assumes that the subject receives feedback from the experiment (E) on each trial instructing him whether or not he is correct, and he proposes that the subject then adjusts his criterion by a fixed amount in an appropriate direction to reduce the possibility of the kind of error which he just made. There are several other theories along similar lines, all of which assume feedback from the experimenter. Schoeffler (1965) allows for criterion movement after correct responses as well as after errors, as do the others. Dorfman and Biderman (1971) consider that the amount of criterion movement would be different following errors and correct responses. Thomas (1973) suggested that bias from payoffs would affect the amount of criterion shift. Ambler (1976) also allowed a response strength function that varied from trial to trial as the subject learnt the task.

Here in this group of theories there is the expectation that the criteria that are used to test the input will be adjusted to be more or less stringent according to the learning that has occurred

over the previous trials. Unfortunately, as stated above, all these theories consider the case in which the subject is informed whether or not he is correct on each trial, and assume that this is his only source of information. Most studies demonstrating sequential effects have not included feedback - or at least not immediate trial by trial feedback. Certainly the stimuli in such studies are fairly clearly discriminable (e.g. Bertelson (1961), Remington (1969), Kirby (1972), Williams (1966)), and in such cases it is certain that subjects become aware of most of their errors, (Rabbitt and Vyas (1970)). However, some estimate of the amount of feedback which the subject actually obtains in such a case would be necessary. Such an estimate would be difficult to achieve, especially in a task in which more indiscriminable stimuli were used, when subjects would not notice most of their errors, and it would be even more difficult to estimate the number of trials in which such feedback was available. The introduction of such a variable into the formulae would certainly be needed but it is not known by the present writer how feasible this would be and whether the formulae would become unwieldy.

Nevertheless here is the start of a theoretical framework that allows for quantitative adjustments to be made to criteria on the basis of immediately preceding information. The changes that occur are assumed to be directly related to the presence or absence of errors. Effectively, subjects are thought to track their RT's against errors, i.e. the suggestion is that they gradually get more reckless until an error is made when they will abruptly get more cautious and return to a slow response rate.

1. (b) Sampling Theories - Discrete Data

In the main, these theories are concerned with the counting up of evidence for and against the various alternatives. The input is assumed to be divided into discrete units which are each assigned to a particular alternative. If, for instance, the discrimination is between two lines, then each unit that arrives will be 'labelled' as evidence for one or the other of the lines. It is as though the subject had a series of bins into which the data items were thrown and the decision is taken on the basis of a comparison of the accumulated values in the bins. The number of samples taken, and the required relationship between the totals are specified by the criteria used in the models. Such models have in common the expectation that observations are made at a constant rate and that the subject can adjust his criteria to increase or decrease accuracy, or to bias towards one or other response. No mention is made of the initial sampling divisions. Observations are assumed to arrive clearly labelled, as though having passed a signal detection input stage with the required number of cutoffs to classify the data as required. Pike (1973 p 59) states:

The most obvious effect of the counting process is to increase sensitivity. If p and q are the predecisional probabilities so that $p > .5$ then the associated response probabilities will be P and $Q = 1-P$, where $P > p$. The overt decision is thus more accurate if a counting mechanism is present although the tradeoff in accuracy and time has now to be considered.

Runs Model

The data input is examined until a sequence of k items occur in one category. The value of k chosen at any one time constitutes the criterion. When such a run occurs the decision is made in favour of that category of events. Figure 1-4 shows the predictions of the model, Audley (1960). Clearly this has the advantage of being a simple concept and apparently produces a first approximation to some data. However, Vickers (1970) points out that latency probability functions, as plotted for cautious observers, are asymmetrical whereas in a Runs Model as the criterion k is increased so the symmetry should also increase (Pike 1968). This is really pointing out the empirical observation that errors tend to have longer latencies than correct responses when subjects are more cautious. In a Runs Model, as the criterion becomes more stringent, so latencies for errors and for correct responses become more similar to each other while at the same time the overall mean latencies increase exponentially.

Recruitment Model

La Berge (1962) proposed that incoming data will be divided into categories corresponding to the stimuli being presented and that there will also be one extra category which corresponds to a no-decision. Thus in a two-choice task, incoming data will be divided into three categories - one each for the two alternatives and one no-decision category. The concept in this model that some observations are ignored and do not contribute to the accumulations of evidence is quite different from assumptions in other sampling models. Data relating to the two alternatives is accumulated until one of the totals reaches a criterion level.

Vickers, Caudrey and Willson (1971) have suggest that k - the criterion - can fluctuate from trial to trial in a random manner. This does not apparently change the RT mean and variance, but does affect skewness and kurtosis in a manner which leads to a better fit of observed data.

Illustrated in Figure 1-5 are the predictions for a Recruitment Model when $P_0 = 0.0$, i.e. when there is no central no-decision area and $C_1 = C_2$. See Figures 1-2 and 1-3 for definitions of the terms used. Essentially RT's increase directly with k , increasing slightly faster for difficult than for easy trials. The effect of moving the two criteria C_1 and C_2 apart is to increase the slope of the resulting RT curves.

The important factor in calculating error rate is the value of PS where

$$PS = \frac{P_1}{P_1 + P_2}$$

As P_0 increases, so P_2 and P_1 reduce, but P_2 will reduce at a faster proportional rate to P_1 , so PS will also increase.

Cartwright and Festinger (1943) proposed a model which is, in effect, a special case of the Recruitment Model. They specify that incoming data are matched for value against two criteria. If the signal exceeds the upper criterion or falls short of the lower criterion, then the appropriate response is made. If not, i.e. if the signal is in a central no-decision area, a further observation is made. The criteria are symmetrical and assumed to vary randomly. This is essentially the case illustrated in Figure 1-2.

Vickers et al (op.cit.) conclude that the recruitment theory, while offering a first approximation fit to some data, does not afford sufficient account of the variability of reaction times - the variance as predicted from values of k is much too small to fit empirical data.

Random Walk Model

Stone (1960) proposed a Random Walk Model. It is suggested that stimulus differences are accumulated and when the summation of their differences exceeds a criterion value k the decision is taken in that direction. This is termed a Random Walk Model, since it can be pictured as the passage along a corridor of the accumulated evidence, moving from side to side as evidence weighs in favour of one or other alternative. When the evidence reaches one of the walls (absorbing boundaries) the decision is made in the appropriate direction. Incoming data are considered to contribute equal increments in movement towards one barrier or the other. Criteria are set by the initial distance of the absorbing barriers from the starting value - or in other words by the size of z , the required difference. Figure 1-6 shows the predictions of the Random Walk Model for different values of PS and z . In the calculations, starting point is assumed to be situated midway between the two absorbing barriers which are located at $\pm z$ from it. The increase in RT with increasing z is much faster for difficult than easy discriminations but not as rapid as in the Runs Model. Laming (1968) suggested that response biases could be introduced by moving the starting point towards one of the barriers. This adjustment allows error latencies to differ from correct latencies. Link and Heath (1975) point out that this assumption leads to the prediction that errors are faster than correct responses. The model as it originally stood predicted equal latencies for correct and error responses. In an attempt to explain fast errors in subjects who are instructed to respond rapidly and also, incidentally, to prevent unduly long latencies where the evidence continues meandering along the corridor away from the boundaries, Pike (1968) suggested that the subject could

reduce the value of k during a trial. Neither he nor Vickers et al (op.cit.) consider this particularly likely - the latter because it introduces an unnecessary complexity into the theory, which it would be difficult to quantify. Second, Pike suggested that the threshold might vary from trial to trial. It would certainly seem reasonable that if criteria vary in this way the overall distributions of latencies would comprise a mixture of fast and slow trials. The fast trials would be more likely to produce errors so that the net effect of the averaging would be to produce faster errors than correct responses when subjects attempt to respond quickly. However, this does not allow for the observation that when subjects try to be careful their errors are slower than their correct responses.

Link and Heath (1975) and Thomas (1975) have both considered how varying the starting point and allowing asymmetrical effects of the evidence on the walk can influence the mean RT. Link & Heath have effectively modified the Random Walk Model in the same way as Vickers (1970) (see later) modified the recruitment theory. He proposes that the values that are being accumulated are not unitary, but are the actual difference observed in each look at the stimuli. Thus, if the data are normally distributed and one value is larger than the other (say $B > A$ in Figure 1-2), then the mean distance of the 'correct' differences (to the right of C when $C = 0$) from 0 will be larger than the mean of the incorrect values, and so evidence will accumulate more rapidly. This means that errors will have longer latencies than correct responses. He concludes that this is sufficient to predict empirical observations of the asymmetry between correct and error latencies. Link and Heath (1975) have gone on to predict how sequential effects can be tied in with such a modification to the Random Walk Model.

They assume that adjustments are made to the distance of the two absorbing barriers following error detection.

Thus, in random walks we can consider at least two ways in which the decision can be influenced. The position at the start of the walk can be considered a response bias criterion adjusted in anticipation of the expected direction of response. Also, the distance of the boundaries (the value of k) can be adjusted with the result that the subject responds faster or slower.

Counter and Clock

Nickerson (1971) has proposed a theory which, strictly speaking, relates to same - difference judgements. The subject accumulates evidence of differences between two input states and if this accumulation exceeds a criterion C_d , then his decision is "different". If instead there is a "time up" neural signal before the C_d criterion is reached, he makes the decision "same". There are two criteria which can vary:

1. A value for a count of different (C_d) which can be set to require large differences to occur before a decision is made.
2. Total time allowed for inspection (C_t).

One problem with the theory is that it assumes that noise will lead to a constant accumulation of difference, so if the time criterion is set for a sufficiently long time, the subject will always decide that two input states are different.

Fixed Samples

Crossman (1955) and Hammerton (1959) suggested sampling theories in which the size of sample is fixed in advance. The subject would accumulate a fixed quantity of evidence and then decide on the basis of which alternative had the larger accumulation. The basis of Crossman's approach is statistical sampling theory. That is, as sample sizes increase so the variance of the means of the samples will reduce and the resulting discrimination will become more reliable. The criterion k equals the number of observations made and is set so as to obtain a particular error rate. One major disadvantage of a fixed sample theory, as indicated by Stone (1960) and Vickers (1970), is that the sample size is set in advance of each input event and so is not dependent on the stimulus. When easy and difficult stimuli are mixed together, fixed sample size theory would predict that responses to both should have the same mean latencies.

McGill (1967) has proposed a similar theory in which the sample size - which may be variable - is fixed in advance of each trial. However, he suggested that the rate of input of data for difficult discriminations will be slower than for easy ones, the rate varying with difficulty. He states (p 331) :

Mean reaction time is essentially the time required to obtain a fixed number of auditory impulses, and loudness is assumed to depend on the number of impulses per unit time. They should be inversely related.

More recently Luce and Green (1972) have suggested a more sophisticated version of this theory in which they assume that the output from a sense-organ is a set of neural pulses of negligible duration but of variable inter arrival times (IAT). A constant signal will lead to a common distribution, Poisson in form, with its mean decreasing monotonically with signal intensity. Sampling is terminated by a neural event subsequent to which the subject determines the average IAT, obtained by the sum of the IAT's divided by the number of samples taken. In a discrimination task the means of the two samples are directly compared.

This has a certain similarity to Vickers' (1970) Accumulator Model which will be discussed next. The criterion k can be increased to increase accuracy but only up to an upper value K which represents the limit to the system, this limit being divided between the J channels being considered.

Vickers' Accumulator Model

Vickers (1970) proposes that evidence is accumulated in two alternative categories, much as in the Recruitment Model. He assumes the input is a normal distribution of difference as in signal detection theory models. Separate accumulations are made of positive and negative events and the amount accumulated is the size of the difference observed. There is then no real need for any partitioning of differences into other than positive and negative events. Difficult discriminations will lead to smaller differences and hence will accumulate more slowly. The decision is made when the accumulation in any class reaches an amount C .

The value of $C = c \times sd$

where c lies typically in the range 0.5 to 5.

sd is the standard deviation of the difference distribution.

The predictions of this model can be seen in Figure 1-7 and are rather similar to the Recruitment Model which is conceptually similar. The graphed values were obtained by the author by a simulation of the decision process on the computer. The results were rather noisy so lines were fitted to the points by eye. The fine lines were obtained by interpolation and simple extrapolation rather than by actual calculation. The resulting plots were compared with the plots given by Vickers (1970) and give reasonable agreement.

Vickers goes on to demonstrate how his model can mimic the Runs, Recruitment and Random Walk Models respectively at different values of c . At values of the criterion around, say, five the latency - probability function is similar to the Recruitment Model; with intermediate values around 2.0 to 1.5 it resembles the Runs Model; with c values around 0.5 performance is rather similar to the Random Walk Model.

It is postulated that the criterion c is predetermined by the subject and that he is capable of adjusting it. Also, the tentative hypothesis is considered that under heightened cortical activity or high arousal there may be a number of random signals arising in the neural accumulators, so that the threshold is effectively reduced. During a prolonged vigilance task the neural activity might drop so that the effective value of c would increase. This would lead to an increase in mean latency over time.

As previously suggested, one problem of the Recruitment and Random Walk Models is that they predict similar latencies for correct and error trials. In Vickers' model as the criterion c is increased, so the latency - probability model becomes asymmetrical and longer error latencies are expected on average.

In these discrete sampling models, apart from Vickers' thoughts on heightened arousal partially filling the neural accumulators, and Laming's (1968) unquantified suggestion that the Random Walk starting point will be offset towards one or other of the two boundaries influenced by the preceding sequence of responses, there is no attempt within the models to describe how criteria are initially set and subsequently adjusted. Essentially these models allow for the possibility that criteria may shift, but they do not discuss what kinds of events will cause criterion movement, nor do they discuss the amounts by which criterion movement will occur from trial to trial during an experiment.

2. Response Based Theories

Next we describe theories which are concerned with response patterns. Several of these are concerned with sequential relationships between responses.

2. (a) Two State Theories

Blackwell (1953) proposed a simple two-state model in which the outcome of a stimulus presentation was either a detection or a no-detection. If the first, then the subject would respond accordingly; if the second, then the response to be made would be guessed with a certain probability attached to each response. This probability would be steady over a run of trials.

It is questionable whether the bias here can be considered as a criterion in the same sense as we have discussed above, but it is certainly a variable factor that divides the responses into two classes (changing the bias will act in much the same way as changing a criterion at the sensory stage). In the discussion of the theories that follow, it will usually be the response bias that is discussed. We shall consider that it can be adjusted in a similar way to a criterion.

Luce (1963) introduced a threshold theory for simple detection experiments in which he considered the relative probabilities of detection, no-detection, false alarms and misses as in signal detection theory. Ideally the probability of saying yes to a signal should equal the probability that the signal in fact occurs. The fact that this is not the case is due to a bias factor towards one of the alternatives. This bias factor affects the response probabilities, and is itself influenced by the probability of the signal, of the instructions and of the payoffs. He considered there are three classes of event that can influence learning - the subject's observations, his responses and feedback from one experimenter or one task. Varying the bias directly determines which responses will be made by increasing or decreasing their conditional probabilities. Shipley (1960, 1965) considers a similar model.

Atkinson and Kinchla (1965) consider a slight elaboration on the two-state model. They propose that there are three sensory states: S_0 = no detection; S_1 = detect signal 1; S_2 = detect signal 2. If S_1 or S_2 occur then the subject responds accordingly, otherwise he responds in accordance with the response bias present at the time. This bias is adjusted from trial to trial if the subject makes an error.

Friedman, Carterette, Nakatani and Ahumada (1968) performed a series of experiments in which they considered the Atkinson and Kinchla model. They agreed with its predictions and went on to conclude that the critical events related to changes in bias were the sensory events and feedback. The actual responses made by subjects were irrelevant. They also considered that subjects changed their bias when they had been uncertain about the signal, in which case the bias would tend towards the feedback they were given.

Three State and Multi-State Theories

Krantz (1969) proposed that sensory input was divided into three categories: non detections, weak detections and strong detections. Strong detections were always responded to correctly. There was however a certain probability that weak detections would be responded to as non detections and vice versa. These biases would be adjusted according to feedback.

Atkinson, Carterette and Kinchla (1962) suggest that sensory input is mapped on to a number of sensory states which are then matched to responses. In the event of failure, the response made is determined by the bias, which can either be set by previous feedback or, if there is no feedback, by the previous sensory pattern observed.

It should be noted that nearly all theories which deal with sequential effects required that there should be feedback. This idea allows the response bias to vary with the information received from the observed pattern of events. The changes in behaviour which are predicted by such theories are changes in response probabilities, and these changes are mostly assumed to occur when the subject is uncertain about the outcomes of his decisions.

2. (b) Response Preparation Models

Falmagne (1965) states that subjects will predict from the sequence of preceding stimuli which stimulus will be expected and hence which response will be required. That response will then be held in readiness. There are then two categories of response: those that are held in readiness and those that are not. The latter will be made more slowly on average than the former. Overall latencies will be the result of summing across these two RT distributions. The bias here is one that reflects predictions from previous response patterns as to which response to prepare for. Theios and Smith (1972) find a reasonable fit from this kind of model for their data from a two-choice response task.

Audley, Caudrey, Howell and Powell (1975) also suggest that when each event occurs the subject will be more or less prepared to make one particular response, and to the extent that he is, so he will be less prepared to make the alternative response. They term this an exchange function. This idea is really an extension of the view put forward by Audley (1971). In the 1975 paper the reaction times for one response are plotted against the reaction times for the other response and show an approximately linear negative relationship as predicted.

All these response models are effectively playing off one response against the other (in a two-choice task of course). The biasing that occurs from sequential effects goes towards increasing the probability and reducing the latency of the predicted responses.

It is hoped that this summary of theories has shown how criteria are thought to play a part in current theories. In discrimination, the relationship between stimuli and responses is thought to be channelled in at least two stages, corresponding with stimulus representation and response selection. Signal detection theory considers that criteria are used to divide up the original continuous sensory input into areas that can be identified with detection/non detection, or greater than/less than, which in turn can be mapped directly to the appropriate response in different kinds of tasks.

The counter (or counting) theorists consider that the input signal is already pre-divided - possibly by a mechanism which can be described in SDT terms (Vickers 1970) - into relatively clear categories which allow the input units to be assembled and counted. Decision criteria are then related in various ways to these counts.

Finally, the 'response theorists', like the last group, suppose that stimulus categorisation is already achieved before the mechanisms they describe come into operation. They apply their theory to the biasing of the subsequent responses - especially when the subject is presented with ambiguous input.

Sequential effects of bias are well considered by the 'response theorists' and apart from noting that this body of theory relates to sequential effects, and hence to context, we shall neglect it as being largely irrelevant in the present research. Taking note of the fact that bias affects RT's, our experiments will throughout be as far as possible devoid of overall response bias. Alternative responses are always equiprobable. We hope that bias effects will thus average across conditions.

Most of the later theories considered under 'Sampling Theories' allow for criteria which can be adjusted during a session to alter performance. Only the few theories in the tradition of Kac have attempted to quantify the changes in the light of immediately preceding stimuli, providing that feedback is given.

One of the aims of this research will be to see whether criteria can be shown to move without feedback in an equal bias condition, and whether any such movement is dependent on errors, or rather on display difficulty.

In several two-choice serial response tasks the difficulty of discriminations will be varied from one trial to the next. If subjects can adjust their criteria according to momentary changes in difficulty of displays, then changes in the average local difficulty within a run should be reflected by corresponding changes in the mean latencies to successive trials.

Overview of the position of criteria in the theories discussed.

Ignoring the response orientated models, the theories which we have discussed so far have assumed that the subject uses one or more criteria in his discriminations. These take the form in SDT of one or more partitions that divide up the input signal into two or more channels. As the partitions are moved, so the proportion of input signal that passes on to each channel changes. In the various counting models the input is assumed to arrive pre-channelled, possibly having passed criteria of the form postulated by SDT, and counts are made of units of evidence until some required total is reached. The criteria in these cases comprise the totals that are required. In both cases the facility is provided for the criteria to be adjusted by the subject as a consequence of

instruction or observation. Though the theories do not state how the criteria are set or moved, they do attempt to predict the effects produced by criterion movement.

Having established what the theorists mean by a criterion, the question still remains as to how the criteria are set by the subject. If, for instance, subjects are instructed to be cautious this instruction will have different meanings for different individuals. Nevertheless we assume that subjects given such instructions will make fewer errors than subjects who are instructed to be fast and to ignore accuracy. The implication in the various theories is that the subject will set his criterion at the start of the experiment - even before the first stimulus is observed. In the laboratory, subjects are usually given practice at a task before they are expected to perform for the experiment proper. This allows them to try out different criteria for themselves in relation to the stimuli that are actually used. Then, when they are asked to be careful, they have some idea of what is expected of them. How do they in fact learn the effects of criterion changes? In many experiments, subjects are informed as they proceed how well they are doing, and their errors are indicated to them. In other experiments, they are given no feedback but clearly can recognise when they make errors without it. As they change their degree of caution, so the number of errors they make also changes. In such a case, the establishment of a suitable criterion entails a tracking process of performing at gradually faster rates until too many errors are being made and then slowing down sufficiently to reduce the error rate (Rabbitt (1970), Lappin and Disch (1972)). This is what is predicted by theorists who suggest that sequential effects influence criteria.

This is fine in the case of subjects receiving feedback from the experimenter, but what is the case when no feedback is given? Subjects in this case are again presumed to regulate their performance after errors, but now the subject must provide his own feedback. Evidence by Rabbitt (1969) of subjects slowing down after errors again supports the notion of subjects tracking their speed to errors. These results however were observed in experiments where the stimuli were highly discriminable. Under such circumstances errors are likely to be noticed, as fewer perceptual rather than motor errors will occur (Rabbitt 1970).

We are then left with the question whether subjects can adjust their performance and maintain a steady error rate when discriminations are difficult, i.e. when they are unlikely to be aware of many of their errors. If they can achieve a stable performance, is it merely by a passive process in which they set a criterion at the start and simply maintain it at a certain level throughout the session - perhaps with some drift, but basically without reference to the actual discriminations which they make? Or, is it by a more active process which can be influenced by the stimuli about which decisions are made? If this is the case, how is the subject aware of his level of performance if he is not aware of his errors?

One possibility is that subjects can judge how difficult a task is by noting how long they take to make decisions. This must surely lead to a conflict. Most experimental instructions require subjects not to take an undue time. If a subject finds that he is taking a long time over his decisions, does he speed up so as not to keep the experiment waiting, or slow down because

he intuitively feels that because he is taking a long time the likelihood of an error is high, so that he must be more careful? This latter strategy could possibly lead to unacceptably long latencies since, of course, by making his criterion more stringent, the subject will take even longer and consider the task yet more difficult, leading to more stringency, etc. Is it possible for subjects to decide how difficult a decision is without relying solely on the time taken to decide and falling into this vicious regressive circle?

Indeed, do subjects in fact adjust their performance to the difficulty of the task? If they do, one might ask over what period of time does such adjustment occur? When an error is detected, the subject immediately slows down on his next few trials. When a trial is observed to be difficult, does the subject immediately slow down, or is adjustment a more gradual process? It could be considered that if a subject can detect an error his confidence for error detection will be high, and so he will be sure that an immediate change is required. In contrast it may take several trials for the subject to become aware that displays are difficult, and in this case criteria would be modified much more gradually.

Methodological Considerations

One must next consider how criteria are to be measured. Unfortunately they cannot be measured directly - like many aspects of human behaviour they remain concealed within the 'black box' that constitutes the human observer. All that can be done is to examine variations in the behaviour of subjects and to relate such changes to comparable hypothetical movements of criteria. The particular behaviour that is relevant is the speed and accuracy with which tasks are performed. That these relate to the criteria that are being considered is no coincidence, since the theories which have been outlined attempt to predict speed and accuracy by the use of the criteria as hypothetical constructs.

In practice, two measures will be used to gauge subjects' performance - mean reaction time and percent correct. RT distributions are well known to be skewed and so many experimenters use the median of the scores as a measure of central tendency. However, in this set of reports the mean will be used throughout. There were several reasons for this.

First, the theories which will be considered and related to the data provide predictions of mean RT. Thus, direct comparisons between data and prediction can be made. Second, means are arithmetically manipulable. This allows the means themselves to be averaged to illustrate trends. Also, at times it will be convenient to estimate how much longer certain actions take than others, and so simple difference scores can be taken. For instance, at one point in the experiments instructions are changed from moderate to more extreme caution.

The difference between the two reaction time means will be calculated as one measure of the effect of the change in performance due to the change in instructions.

Limit

Third, the Central/Theorem states that when means which are taken are from samples of sufficient size they tend to lie on normal distributions, regardless of the shape of the underlying distribution. Thus, statistical operations can be performed in order to test whether means differ from one another, whether they are related by a common trend and whether correlations between them can be found. Further, it is possible to perform analyses of variance upon such data when the sizes of samples are sufficiently large, and to make valid assumptions concerning differences between means even when the populations from which the samples are taken depart considerably from normal (Hays 1963). This robustness does not hold up for random effects, but the variables considered in this research are all fixed effects.

One problem that must be considered in Analyses of Variance of reaction time data is that RT variance is usually related to the mean. Again, Hays points out that this restriction can be ignored if the numbers of cases in each sample are the same. As far as possible we have adhered to this principle but in some circumstances the very nature of the experimental procedure has led to different numbers of estimates in different conditions. In particular, this happened in Experiments 3 and 4 when the proportion of difficult trials in a run was varied from one run to the next. In these cases, the means were used. The number of means and subjects in each of the groups were of course the same. What differed was the number of reaction times which were summed together to make up the means. This would lead to the means from

the larger sample sizes having smaller error distributions than those from small samples. In fact, this sampling bias would show up when factors involving difficulty bias were involved, and none of those factors was significant. Since the difficulty bias was a regular one it would average out when other factors were averaged across bias.

A third assumption necessary when performing analyses of variance is that errors are independent of one another. This means that the data used must be based on independent observations - or that estimates from successive trials must be independent estimates and not related to one another. Hays again comes to the rescue by suggesting that subjects who are fairly well practised should produce data that conform to this restraint. At one stage in the experimentation, correlations were calculated between successive trial latencies. A few high correlations were found but basically the data examined were relatively free from them. Had the trials not been independent it is most likely that there would have been correlations.

Finally, it might be considered that RT distributions should be transformed into a more normal shape before being analysed, or that extremely fast or slow RT's should be dropped from the data. One requirement of theories of discrimination is that they will have to explain or at least describe the skewed distributions which actually exist. Incidentally, all theories initially described predict that RT data will be skewed. If extremes are to be removed from the data, then first suitable cutoffs (criteria?!) will have to be chosen, and second these will have to be justified. In fact, in the present studies RT's of less than 100 msec were largely ignored (there were only a few such responses) as certainly

not being reasoned decisions. No upper cutoff was used, but in practice RT's were limited except in Experiment 7 to a maximum of 4 secs, since that was the limit built into the programme. Trials in which no response occurred were not repeated. Again, there were not very many of these. When a subject did not respond for a few trials during practice, he was requested to respond within the required time and to guess if necessary. Examination of the histograms shows that this did not cause any apparent truncation of RT distributions.

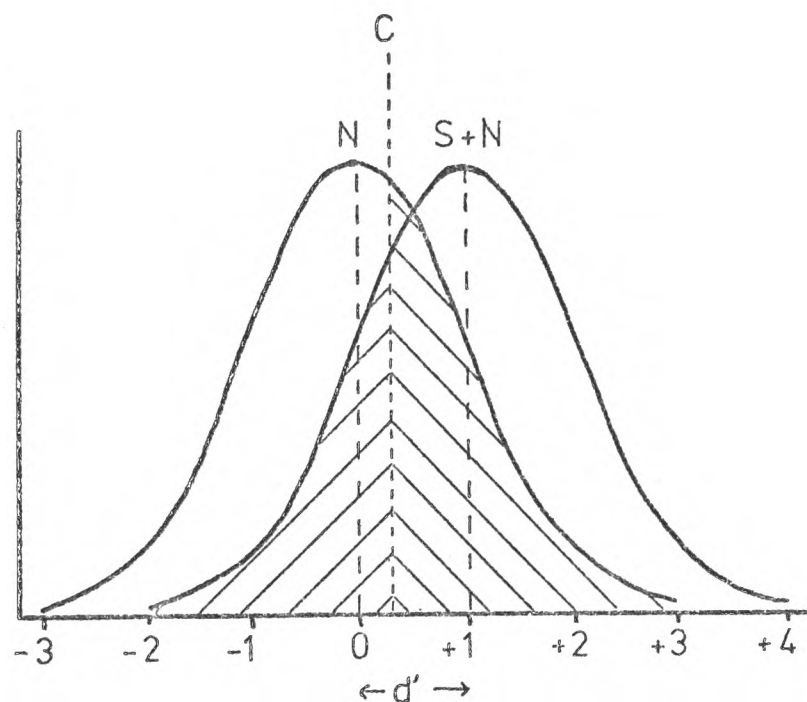
The second measure used was error rate. All the theories again predict how error rate will change with criterial movement, and so this is a very relevant measure. However, very often RT is a more sensitive measure than errors. Generally speaking, RT has to change by quite a large amount before error rates alter. This can be seen by studying a Speed/Accuracy tradeoff function. At the lower end of the function, there is quite a rapid change in error rate relative to change in RT. Over a range of 200 to 300 msec a change of 10 to 20% may be seen in errors. Subsequently the rate diminishes and the total improvement in error rate over the next several hundred msec of RT and may be around 5%. Most of the time subjects were observed to respond around or just above the point where the curve flattened out. Thus, fairly small changes in RT can occur with little or no measurable change in error rate. Because of this the main measure we examined was mean reaction time. Only when gross changes in error rate occur is it possible to say that a criterion has moved. For instance, if a sample of 1000 trials is taken, and the mean error rate at the time is 30% then the standard error of a proportion is 1.45%. The 95% confidence

limits are $p \pm 2.84\%$ which is quite a large change in error rate. The corresponding interval for reaction times at typical standard deviations of, say, 300 msec would be $m \pm 18.6$ msec.

Thus, there are the twin problems with error rate. First, very little change is expected when subjects operate on the upper part of a SAT curve, and second the standard error is relatively large compared to the expected change.

There is one more problem concerning the estimation of criterion movement that should be considered and this is one that has been relatively ignored in the present research. It has been assumed that changes in RT reflect changes in criterion position or value. In the case of the various stimulus sampling theories, the criterion is described as a value or count which must be reached. This count is reached by regular sampling of the signal and by the accumulation of various totals, as previously described. The prediction of change in RT due to a change in criterion is made by observing the number of units of stimulus input which are required by the theory. The relationship between the two is assumed to be linear. Two separate assumptions are being made here. First, all sample units taken during a particular discrimination are required to be of equal or nearly equal duration. The value has been estimated by some at around 100 msec and has been related to the perceptual moment hypothesis (Vickers, Nettlebeck and Willson 1972). Second, it is assumed that the reaction time to a signal can be calculated from this value to which may be added constants to cover overheads such as response selection and motor movements. One simple, but not very sensitive, test of this hypothesis is to compare SAT functions between different experimental conditions.

Any gross change in the length of time taken for each sample would show up as a faster acceleration of the curve at the start of the function. Second, if there were any real change in size of the constant overheads this would show up as a movement of the function to the left or right. The SAT functions of Experiments 2, 3 and 4 were compared (Figures 5-2, 5-3) and were very similar, so that this theory does not seem very probable.



Key:

C Criterion - placed in this instance to the left of the midway point between the two distributions.

N Mean of Noise distribution.

S+N Mean of Signal + Noise distribution.

d' d prime. In this case it equals 1.0. It is the distance between the two distributions in units of the Noise distribution standard deviation.



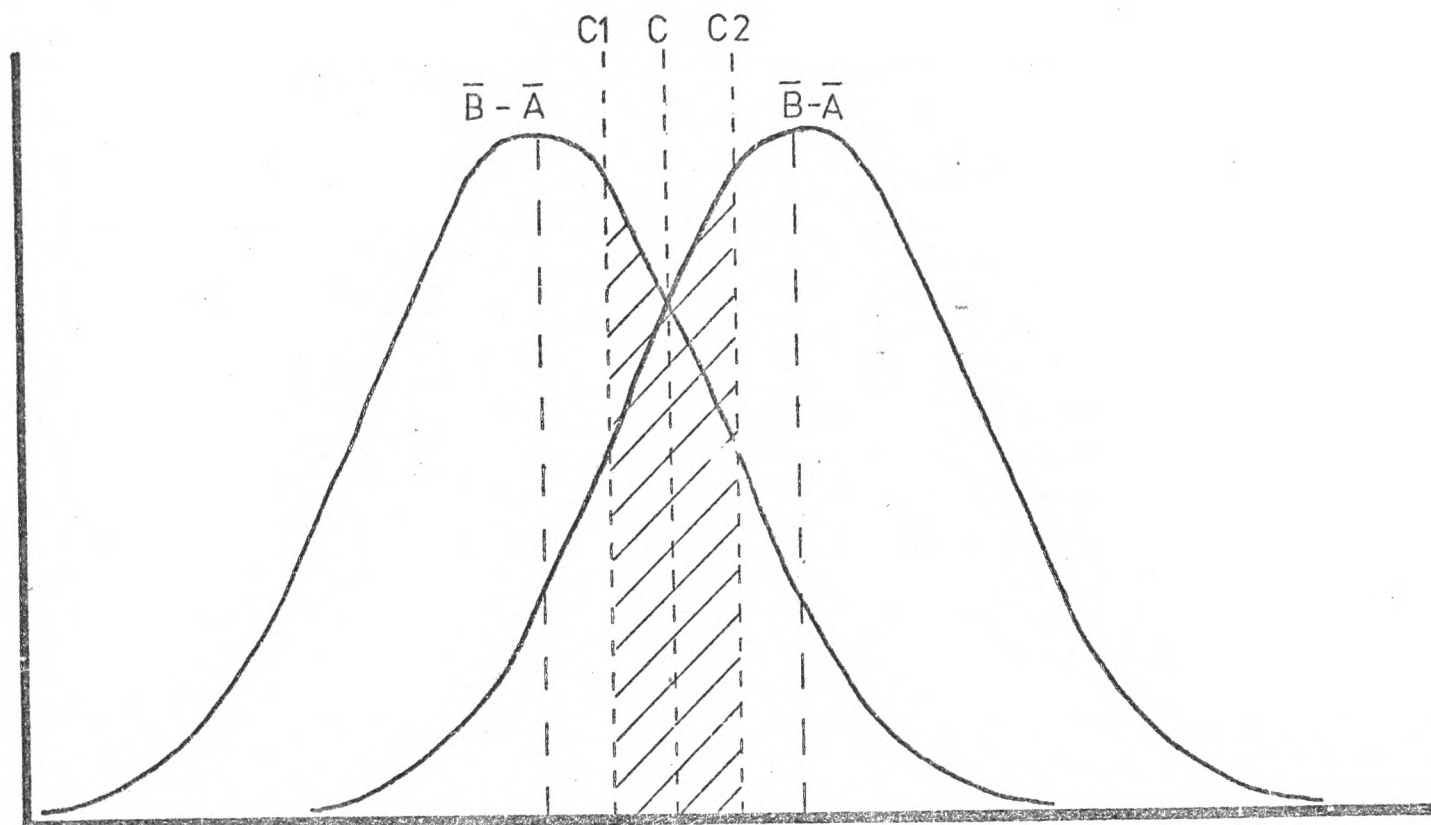
Area shaded thus represents the probability of missing a signal - i.e. of saying 'No' when there was a signal.



Area shaded thus represents the probability of a false alarm - i.e. of saying 'Yes' when there was no signal.

Diagram of signal detectability theory applied to the threshold detection of a signal S , given a Noise level N and a $d' = 1.0$. It can be seen that if C is moved to the right the probability of false alarms will drop, and the probability of misses will rise.

Figure 1 - 1.



Key:

C Criterion placed ideally midway between the two difference distributions.

C1, C2 Two criteria now being used instead of C in order to be more cautious. Detections to the right of C2 lead to the decision that B is longer than A. Those to the left of C1 lead to the decision that A is longer. The shaded area in between C1 and C2 represents occasions of no decision when the criteria C1 & C2 are being used.

Figure 1 - 2.

Diagram of signal detectability theory applied to the discrimination of two lines of length A and B. The two distributions represent the difference distributions of $\bar{B} - \bar{A}$. The righthand distribution occurs when $B > A$, the other when $A > B$. As C2 moves to the right, it becomes closer to the mean of that portion of the $B > A$ distribution that is to the right of C2. Simultaneously the area of the $A > B$ curve that lies to the right of C2 decreases proportionately faster than the area of the $B > A$ curve, so fewer errors are made. As the relationship is symmetrical, the reverse occurs as C1 moves left.

definition of terms used in the discussion of the various discrimination theories.
See figure 1-2 for diagrams of the distributions.

Given: Two alternatives A and B.

$\bar{B} > \bar{A}$ i.e. considering only the right hand distribution in figure 1-2.

Then:

P1 is the probability on any observation that the value of the observation will lie to the right of C2

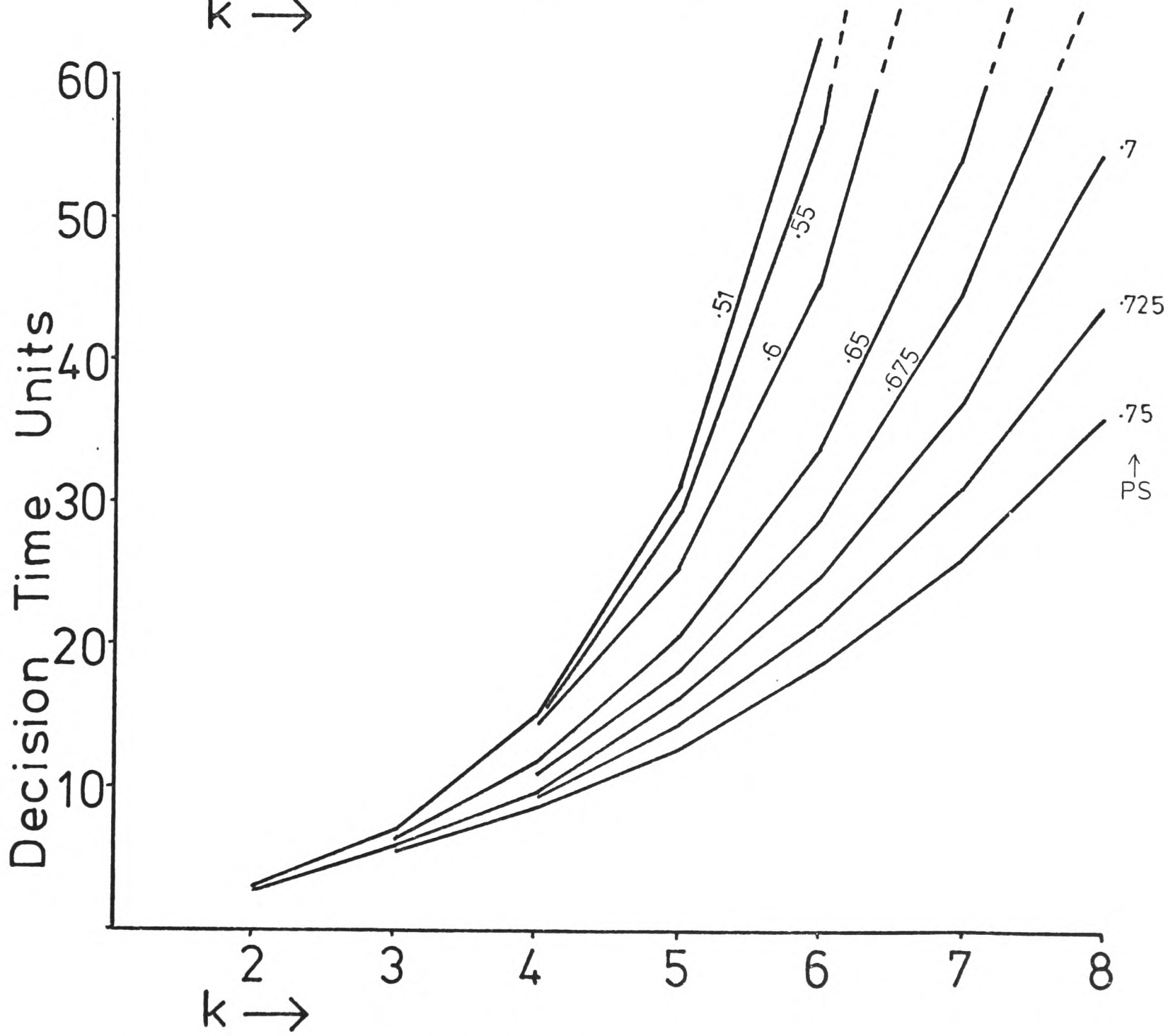
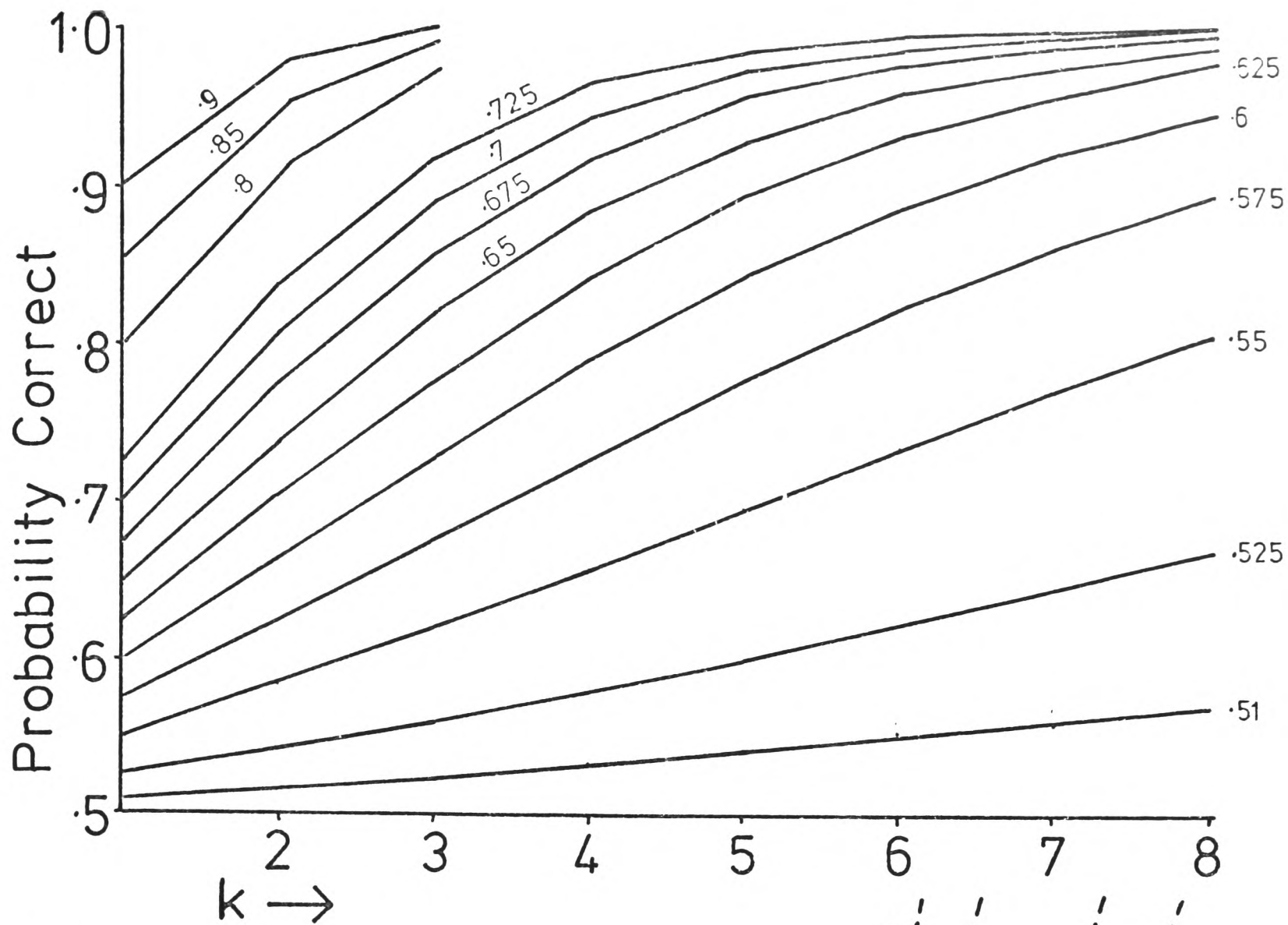
P2 The probability of the observation falling to the left of C1.

P0 The probability of the observation falling between C1 and C2.

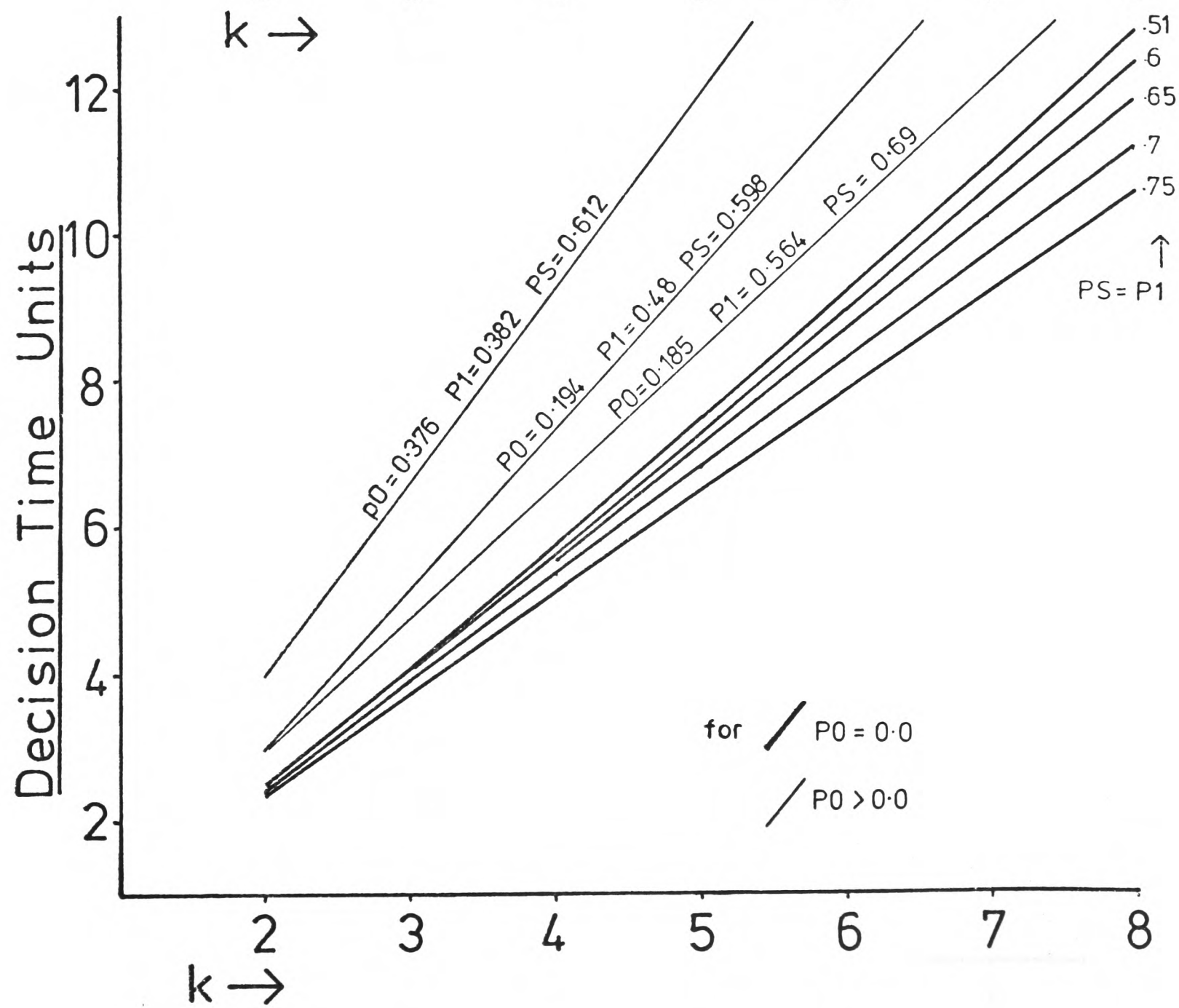
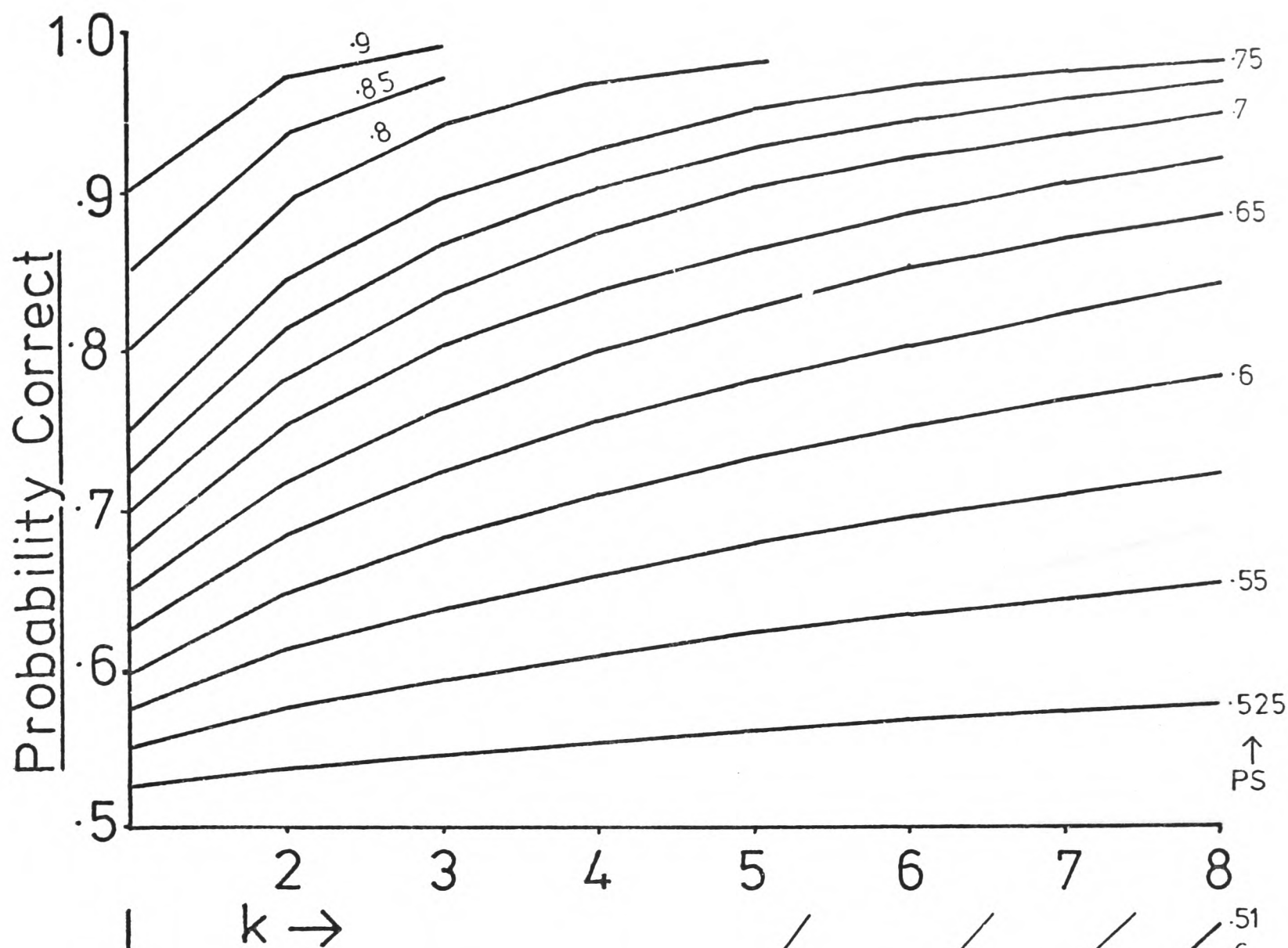
PS is the value $\frac{P1}{P1 + P2}$

i.e. it is the probability that if an observation lies outside the no-decision area, it will be to the right of C2.

Figure 1-3

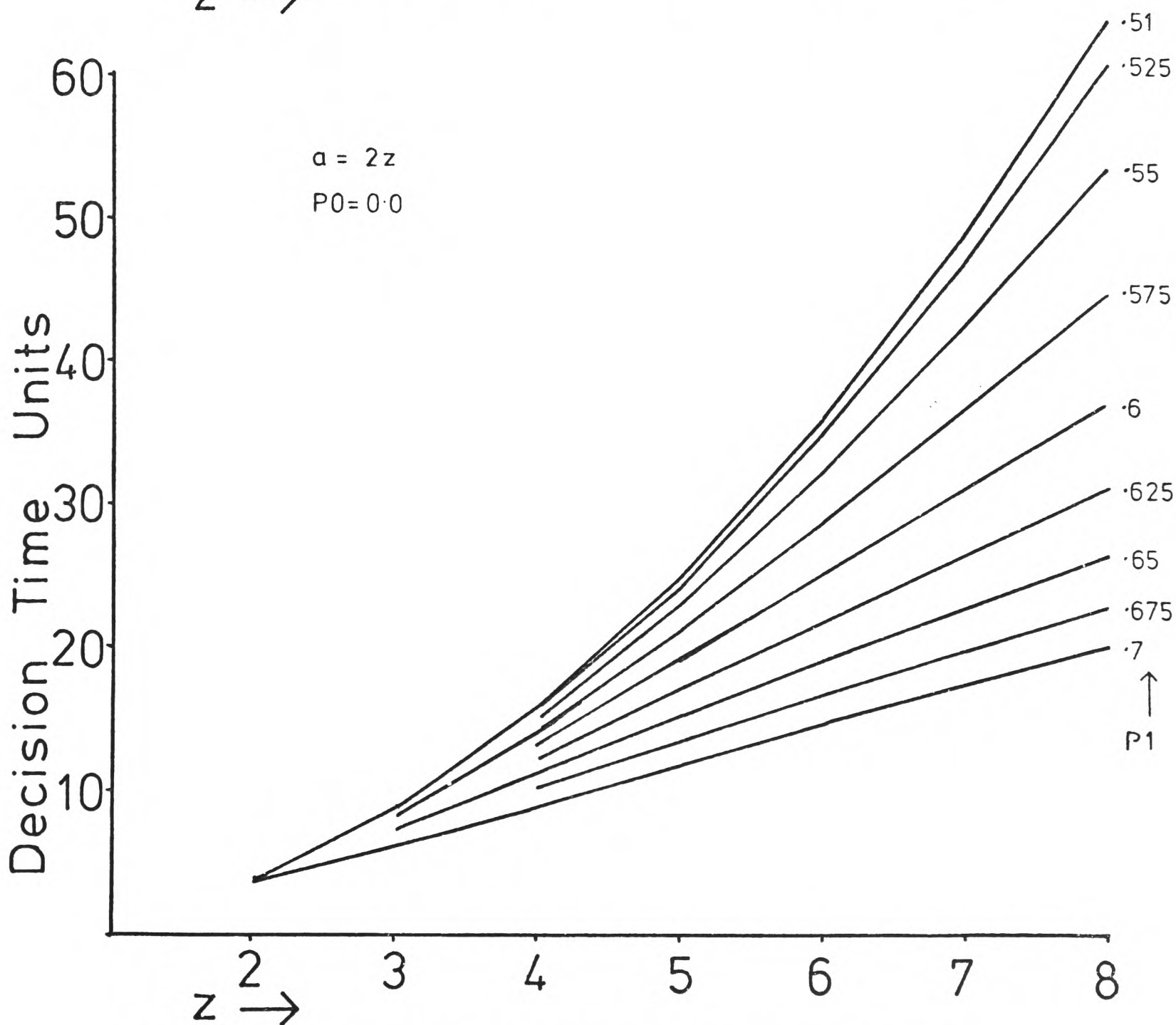
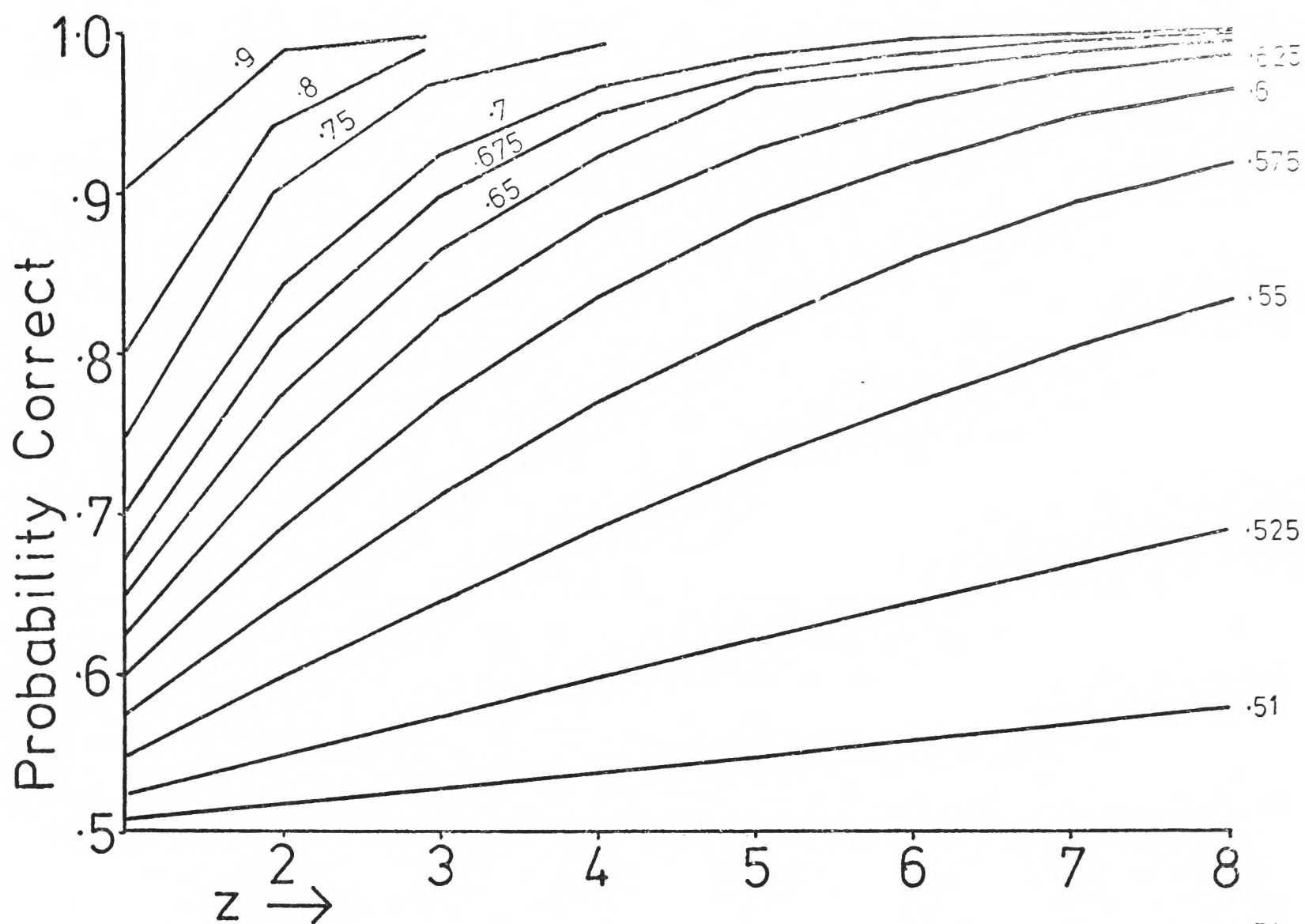


Runs Model Predictions
Figure 1-4



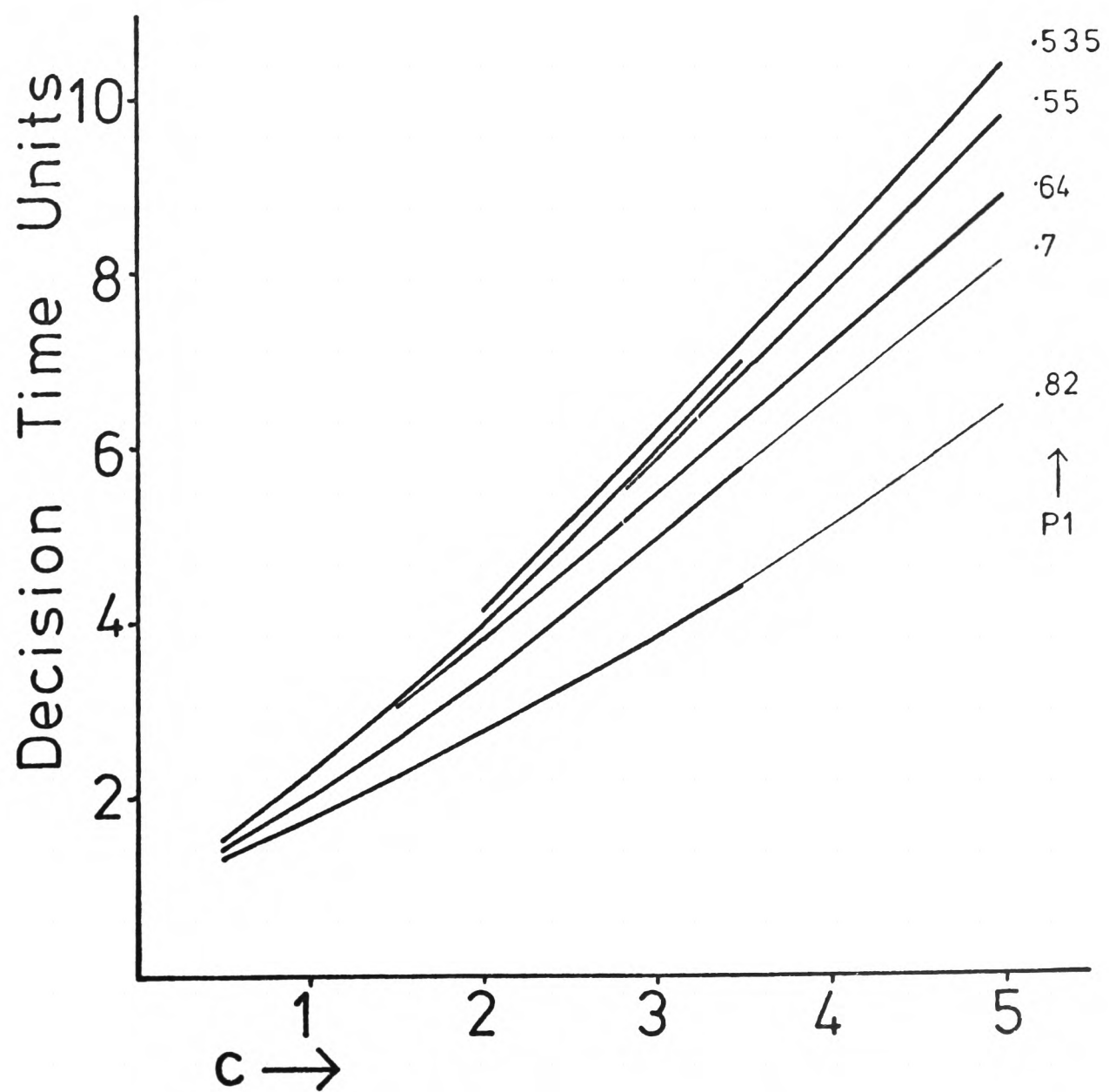
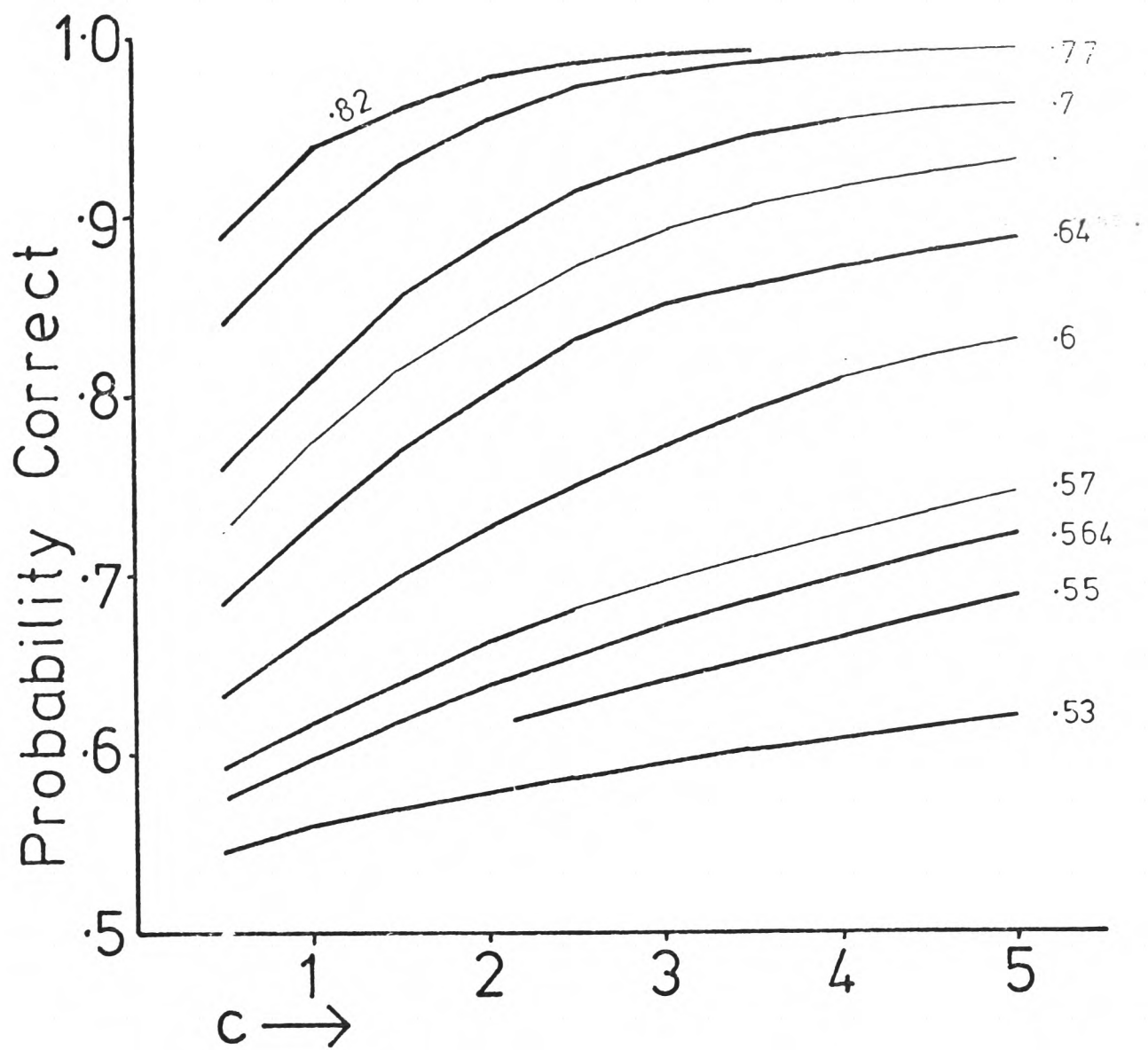
Recruitment Model Predictions

Figure 1-5



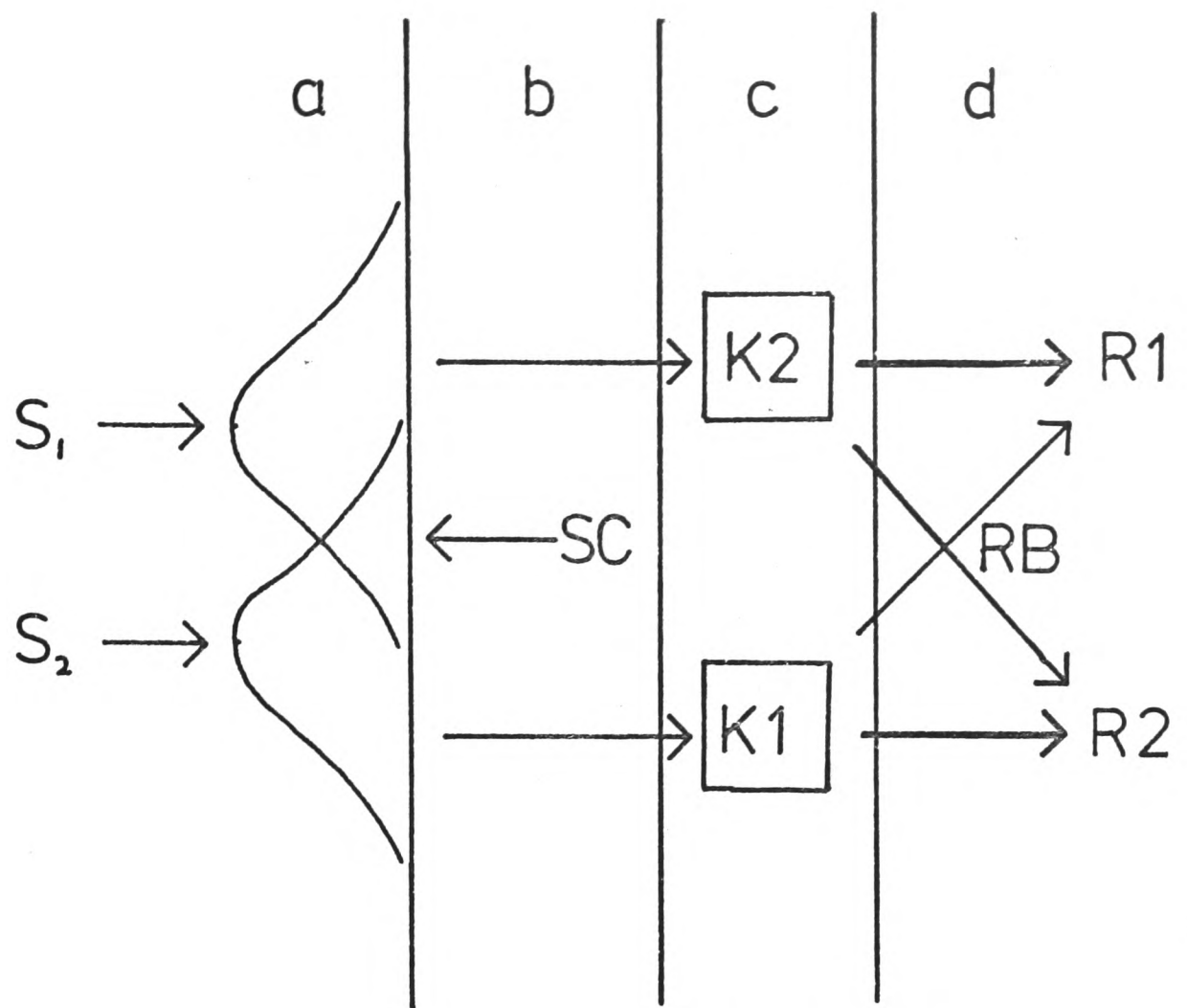
Random Walk Model Predictions

Figure 1-6



Vickers' Accumulator Model Predictions

Figure 1-7



S_1, S_2	Stimuli.	Stages:
SC	SDT Criterion.	a) Input of Signals.
K_1, K_2	Counters.	b) Division of continuous input into discrete pathways.
RB	Response Bias.	c) Counting of evidence for each alternative.
R_1, R_2	Responses.	d) Selection of Response.

Diagram of the stages of processing from stimulus input to response as considered in the text.

Figure. 1-8

CHAPTER 2

STUDIES OF SEQUENTIAL EFFECTS

There is a wide literature concerned with sequential effects which demonstrates that performance during a run is affected by preceding trials. One popular area for the study and illustration of such effects is concerned with the repetition and alternation of stimuli and response.

1.a. The Repetition Effect: What is it and how is it affected?

i. The effect of inter stimulus interval

One of the early workers to notice the repetition effect - sometimes abbreviated to RE - was Hyman (1953). He was primarily concerned with studying the relationship between information and reaction time, but at the same time noticed, especially in a four-choice and eight-choice task, that latencies to repeated stimuli were faster than to stimuli that had not been repeated.

Bertelson (1961) first really called attention to the effect. He used a simple discrimination between digits displayed on a Nixie tube. He found that when responses followed one another in a rapid series, responses that were repeated were faster. He found that the effect gradually disappeared as that response-stimulus interval (RSI) increased from 50 msec to 500 msec.

Hale (1967) required subjects to discriminate between the digits "1" and "2" and varied the RSI from 100 msec to 2 seconds. He also found a repetition effect at the shorter intervals which reduced and became a slight alternation effect at the longest interval.

Williams (1966) required subjects to discriminate between either a red or green light or a high or low tone and also found alternations to be faster than repetitions with a RSI of around 12 - 15 seconds.

The repetition effect is not however a simple function of RSI or ISI. Entus and Bindra (1970) with an ISI of 2 seconds, Remington (1969) with an ISI of 4 seconds and Shaffer (1965) with an ISI of 10 seconds and variable S-R mapping all showed repetition effects. Entus and Bindra (op.cit.) suggested that the RE was related to stimulus discriminability and that as discriminability decreased so the RE would reduce. They proposed that the effect of increasing ISI was to reduce the identification of successive stimuli as being identical. If a stimulus was less discriminable at the start, the RE would disappear sooner.

By and large the repetition effect is seen to reduce as the time between successive stimuli increases and at longer intervals an alternation effect is observed - Kirby (1972) with ISI of 2, 4 and 8 seconds, Entus and Bindra (op.cit.) with ISI of about 10 seconds, Shaffer (op.cit.) with ISI of about 10 seconds and fixed R-S mapping.

ii. The number of alternatives

Kornblum (1967) used as signals two arcs of neon lights. There were 8 lights in all and subjects were to respond to the light signals by pressing one of 8 corresponding keys. In different runs 2, 4 and all 8 lights were used. The

sequences were constructed so that the probability of a repeat or change of stimulus was known during a run with values of $\frac{1}{2}$, $\frac{3}{4}$ and $\frac{7}{8}$ for the probability of an alternation. Of interest at the moment are the runs where the probability was $\frac{1}{2}$. As the stimulus set size increased from 2 to 8, latencies increased and also the difference between latencies for repeats and alternations increased. This means that as the number of alternative signals increases, so the benefit of repeating a stimulus and response is greater.

iii. Stimulus-response compatibility

Bertelson (1963) presented subjects with 2 lamps to the left and right, or one above the other. Subjects would respond at different times and after much practice using different mapping rules. For instance, they would respond with the left hand to the left light and right hand to right in a "direct" mapping condition, or with left hand to right light, etc., in a "crossed" mapping condition. He found that latencies to the "crossed" responses were slower, but also that repeated responses were much faster than alternations compared with the difference in the "direct" condition. Keele (1969), Schvaneveldt and Chase (1969) and Shaffer (1965) all also find an increase in the repetition effect as stimulus-response compatibility decreases.

In conclusion, then, the repetition effect is seen to reduce as ISI increases, which may have the effect of reducing the discriminability of the signals; it is seen to increase as the number of alternative stimuli increase and as stimulus-response compatibility is reduced.

1.b. i. What causes RE? Anticipation?

The description however of these effects does not produce an explanation of the process. One possible explanation is that subjects anticipate the stimuli. Williams (1966) and Hale (1967) both required subjects to predict the stimulus they thought would come next and found correct predictions faster than incorrect ones. Hale also shows that the probability of the subjects predicting a "2" after one presentation of a "2" is around 0.57. This should lead to faster responses immediately following a single "2" but in fact these are seen to be slightly slower than the previous response. Similarly, Hale plots the RT as a function of run length of "2's" and it is seen to decrease as run length increases. However, at the same time, the number of predictions by the subjects of the occurrence of a "2" also decrease. This should imply that they are less prepared for the repetition and so should be slower. Thus, it would seem that simple anticipation would not explain the phenomenon.

Kornblum (1967) found, as did other workers, that repetitions were on average faster than alternations. He went on to study in the 2, 4, and 8-choice tasks, described earlier, the effects of varying the probability of alternations. With the probability of each signal kept at 0.5 he plotted the reaction time to repetitions and alternations as a function of the probability of each. He found that under these circumstances the reaction times for each were very similar when the probability was the same. Thus, it would appear that even if anticipation of the signal is not perhaps the whole answer there is a direct link between the observed probability of the transition between one trial and the next and the reaction time.

ii. Memory of previous stimulus + expectation?

Keele (1969) used in his third experiment 4 keys matched to 4 lights, with 2 sets of stimulus response mappings, representing compatible and incompatible sets. Subjects were required to anticipate responses. He found that correct anticipations led to a greater saving in RT for the incompatible set. This suggests that when a stimulus is anticipated the corresponding response is also anticipated, as, had only the stimulus been anticipated, the saving should be the same for both groups. For this to occur the response of the previous link must be recalled.

Williams (1966) in her fourth experiment used as stimuli a red and a green light. In an attempt to unconfound stimulus and response repetitions she required subjects to respond with one hand when a stimulus was repeated and the other when not. Thus, there could be repeats of signal and separately of response. A repeated signal would not imply a repeated response and vice versa.

In these circumstances, repeated stimuli led to slightly faster latencies and repeated responses to slightly slower ones - neither of these differences was significant. This suggests that overall the effect of repeating the stimulus and the response do not alone explain changes in latency. RT's to trials in which both stimulus and response were repeated or alternated were very fast. The implication is that there is a memory of the link between the previous stimulus and its response and also, as Williams suggests, that there is probably a fast initial test of whether the stimulus is the same as the previous one. If it is, then the same S-R chain is set into operation, and if not then the alternative response begins to be triggered.

The idea of a fast comparison of a stimulus with the memory of a previous one receives support from Nickerson (1965). Subjects had to respond 'same' or 'different' to 2 successively presented letters he displayed on a CRT. 'Same' responses were faster than 'different'. As he was afraid that this may reflect the relative probabilities of same and different matches occurring in practice, he repeated the study (1973) ensuring that the probabilities of same and different pairings were equal and confirmed the result. This suggests that a memory component of the previous stimulus plays a large part in such events.

Squires et al (1977) also considered a memory component paired with expectancy of the repeated signal. Repeats tended to reinforce the memory component. They used this as an explanation of their findings in examining scalp-recorded event related potentials. These were recorded while subjects counted the number of one kind of event 'a' in a sequence of binary alternations 'a' and 'A'. Squires et al related the P300 component of the trace to the unexpectedness of the stimulus and found that it was quite large when an alternation first followed a run of repeats such as 'aaaaA' or 'AAAAa'.

Further support for the notion that there is an immediate comparison of the stimulus with the previous one comes from a recent study by Rabbitt, Jordan and Vyas (1977). The stimuli used were the arithmetic operands + and - on a coloured background. Subjects were required to respond to the + and - and to ignore the colour of the background which could change from trial to trial. They found that subjects were fastest when both colour and sign were repeated, and slower when the sign was repeated but the background changed.

Posner and Snyder (1975) required subjects to decide if a letter pair presented simultaneously were identical ("yes") or different ("no"), by pressing one of two keys. The pair was preceded by a prime which could be neutral (a plus sign), or of benefit (the prime was one of the letters of the pair) or a hindrance when the prime was a conflicting letter. They found that when the prime was 80% likely to be the same as the target pair for a "yes" response, then the presence of the prime facilitated the "yes" response when identical, but hindered (caused inhibition) when it did not match.

When the probabilities of the prime matching the targets were less (50% or 20%) the benefit and cost both reduced. This is considered by them to support their notion that the recent use of neural pathways will facilitate their subsequent re-employment. Active attention by the subject to any one aspect or signal can, however, inhibit the processing of other similar signals. Such directed attention could be related to highly expected signals. This would lead to the possibility that when alternations are expected, the repeated channel becomes inhibited, and hence repetitions in such a case should be considered as slower rather than alternations as faster, unless the alternation response has occurred very recently and the neural pathway is still "warm".

In conclusion then it would appear that the repetition effect can be considered in large part to be due to the re-use of a recently used pathway with the actual benefit being related to the probabilities of that pathway being used.

iii. Is RE a repeated stimulus or repeated response?

The question as to whether the reduction in RT to repeated stimuli is mainly due to the repetition of the perceptual analysis or of the motor act still remains. It is unlikely to be resolved in view of the interdependency demonstrated previously. A rather easier question to consider is whether the repetition of the stimulus or the repetition of the response can affect subsequent responses.

Rabbitt (1965) used a task which subjects responded with either the hands or feet. Not only were repeated responses faster but alternated responses of the same kind of limb were faster than alternations between kind of limb. Rabbitt (1965) and Rabbitt, Vyas and Fearnley (1975) showed that when a different finger response was made using the same hand it was faster than a response using a finger of the other hand. Thus, there is evidence that the repetition of a motor act is facilitative.

Bertelson (1965) used as stimuli four digits displayed on a Nixie tube for which there were two responses. There was a 2:1 stimulus response mapping. With such an arrangement it is possible to have a repeat of stimulus and response (identical trials); an altered stimulus with a repeated response (equivalent trials) and different stimulus and response (different trials). It is postulated that the difference between identical and equivalent trials will demonstrate the importance of response repetition and that the difference between equivalent and identical trials will refer to stimulus effects. There was some effect due to stimulus repetition in two subjects but the main contribution was found to be made by repeated responses.

Theios (1972) attempted to resolve the issue in a Sternberg (1967) paradigm. Using the digits 0 - 9 he selected a target set of one, 2 or 4 digits with the rest of the digits non-targets. Subjects were required to respond with one hand to the targets and the other to non-targets. The analysis attempted to separate the effects of stimulus and response sequences and he found that the effect of repeating a stimulus was much greater than repeating a response. The same relationship was found for long (2 seconds) and short (0.5 seconds) RSI's.

The apparent conflict between this finding of the importance of stimulus repetition and Bertelson's finding of response importance can probably be resolved by referring to Rabbitt (1968). He used the digits one to 8 and several mappings: 8:1, 4:1 and 2:1; and the digits one to 4 with a 2:1 mapping. Early in practice the important repetition was that of the stimulus as equivalent sequence responses were nearly as slow as different sequence responses. Later in practice - after some 1200 to 1500 trials, the equivalent transitions became almost as fast as identical transitions. Thus it would seem that practice has shifted the main influence on the repetition effect from the stimulus to the response. This is presumably due to the subject learning to classify the stimuli more rapidly so that the effect of stimulus change or repetition is minimised, leaving the response effect.

Thus, it would appear that the repetition of both stimulus and response can separately influence RT on the repeated trial, and that the relative effects of the two vary with practice and with the task.

iv. What effect does RE have?

The important point is that particular sequences such as runs of repetitions facilitate one response. Audley et al (1975) have suggested that one response becomes traded for the other. In a two-choice reaction task they cued the subjects as to the following stimulus and varied the probability that the stimulus would be the same as the cue. They then plotted the RT to the one stimulus against the RT to the other and found an approximately linear negative relationship - as the RT to the one stimulus reduced, so the RT to the other stimulus increased. They acknowledge that it is not really possible to decide from the data the locus of the bias that occurs, though feel that Falmagne's (1965) response preparation model is a strong possibility. It is also possible to think of the exchange in the form of a bias towards one or other response, as the error rates also reflect the negative relationship - though not linearly. This bias could occur at the stimulus identification stage or at the response stage allowing a faster response when the expected stimulus occurs.

Falmagne et al (1975) report a similar exchange in a study where subjects responded to one or other of 2 triangles. They analysed the data to find the effect of runs and other sequences and found that RT and error rate dropped as runs of repeats continued. However, the first alternation after such a run was slower and more error prone.

Thus, it would seem reasonable to consider that repetition effects are in fact bias effects in which the bias is produced by a combination of anticipation and a memory component.

One common feature of the studies reported so far is that they have all involved stimuli that were clearly identifiable. Instructions to be careful lead to negligible error rates. Under such circumstances it is possible to think that it would be relatively easy to identify successive stimuli as being the same or different to the previous one both quickly and with confidence. It is likely that if stimuli become less identifiable then the subject will no longer be able to make use of this procedure which could possibly be thought of as a short cut. If the subject is confident of the response correctness of a particular trial (and Rabbitt (1969) has shown that subjects are usually aware of errors) and the stimulus is identified as being the same as previously (Nickerson (op.cit.) has shown the 'same' identification to be a fast one) then no further identification is needed and the previous response can be triggered again.

If the subject becomes less confident of his responses to stimuli as they become more difficult to discriminate, then it seems plausible that two things will happen. First, he is less likely to be able to identify successive items as being the same, though they may well appear to be the same. Second, even if the stimulus is considered to be the same, will the subject still tend to repeat the response that was not known to be correct? It is assumed that feedback was not given.

MacDonald (1974, 1976) performs a fairly complete analysis of sequential dependencies in a pair of more difficult tasks to see how they affect various measures of sensitivity and bias. The first task was a detection of a tone in noise in which the tone was detected 75% of the time. In the second task, subjects

had to discriminate between the presentation of a target tone of 1000 cps and another tone. Three other tones were used in different runs and were chosen so as to produce error rates of 40% (difficult), 20% (medium) and essentially 0 errors (easy). He found when using samples of the same size for comparison that there was no effect of transition on sensitivity measure such as d' of SDT or percent correct or Luce's η . He did find however, and this is relevant to the present discussion, that the criterion measures such as SDT β and Luce's b were affected by the preceding sequence.

MacDonald's analysis examined transitions between stimuli, between responses, and between the correctness of sequent responses. The stimuli and correctness transitions did not affect the bias (criterion parameter) but the response transitions did. Subjects tended to repeat their responses. Thus, it would seem that when stimuli are difficult to distinguish, it is likely that the subject will consider successive stimuli to be the same and so repeat his response. This may well be tempered by an increasing reluctance by the subject to continue repeating responses on relatively tenuous evidence.

v. Conclusions on RE

The term repetition effect thus properly refers to the speeding up of response latencies to trials where both signal and response are the same as on the previous trial and in tasks in which these stimuli are readily distinguishable from one another. It could appear at first sight that repetitions can lead to a shifting of a criterion leading to more 'risky' decisions. However, the studies mentioned especially Audley (1975) and Falmagne et al (1975) have shown that it is better explained as a bias being introduced towards one or other response with the alternate response suffering.

In terms of the models in the introduction the RE can be considered to have the following effects. In SDT the criterion C in Figure 1-2 would shift left or right towards one or other distribution. In the Random Walk Model a shift of starting point would occur towards one or other boundary. In Accumulator Models it is possible that one bin would be reduced in size while the other was enlarged or that the input would be divided differently so that more evidence would be passed to one accumulator and less to the other. The response bias theories incorporate such a bias change as an integral part of the theory as they were formulated with repetition effects in mind.

It would seem then that studies of RE will not help to explain how subjects adjust their level of performance during a run.

With regard to the repetition of responses there appear to be two factors. First, repeated responses appear to be faster than non-repeats, independently of stimuli. Second, if successive stimuli appear to be the same then there is a tendency for subjects to repeat their responses. This will occur when stimuli are difficult to discriminate.

This leads to the methodological consideration that in experiments in which stimuli are difficult to discriminate, a balance of alternations and repetitions must be kept so as to balance out this bias. However, since MacDonald (1976) has shown that neither response nor stimulus sequences affect overall error rate the constraint need not be too vigorously applied.

2.a. Sequential Effects of ISI and Foreperiod

Klemmer (1956, 1957) demonstrated that as the mean foreperiod in a simple reaction time task increased, so also did the reaction time to that stimulus. Drazin (1961), again in a simple RT task, varied the foreperiod between a 50 cps warning tone and a sequent neon light signal. It is fairly standard procedure to vary the foreperiod in SRT experiments in order that the subject can be seen to respond to the signal rather than to his time estimate of when he thinks the signal will occur. Drazin was interested in the relationship between successive foreperiods. He showed that as the foreperiod to the previous trial ($n-1$) became shorter, so the RT to the present trial (n) increased and vice versa. There was a similar, but much reduced, effect of the trial two back ($n-2$). This effect has been confirmed more recently, see Possamai et al (1973) for a summary. Possamai et al studied simple RT to an auditory signal in which they varied the foreperiod duration over 3 ranges. They found that RT was least when the ISI on trial (n) was the same as on ($n-1$). It increased when ($n-1$) ISI was greater than (n) ISI.

Granjon and Reynard (1977) report an experiment of simple RT in which they used 2 ISI's - 1.5 and 3.0 secs - mixed in a run. They found that the long ISI (ISI2) was relatively unaffected by previous sequences, but that ISI1 was faster after runs of ISI1 and slower after runs of ISI2. They considered that this reflected the preparedness of the subject for the shorter interval. They concluded that if an ISI1 follows a run of ISI1's the subject will prepare for a new signal at the end of that ISI. If the signal does not arrive, it is possible for him to partially re-prepare himself for the second ISI. That this is not completely successful is seen in the gradual rise of ISI2 latencies after a run of ISI1's.

The effect of varying ISI is to reduce the subject's accuracy of estimation of the time of arrival of the signal. It is not possible to determine from experiments on simple RT whether the consequence of this is to simply delay the start of the subject's decision processes or to make the task more difficult. This is an important difference as the second would imply a movement of the subject's criterion whereas the first would not.

Lappin and Disch (1973) studied the effect of varying foreperiods in a two alternative discrimination task where the stimuli were 2 illuminated circles to the right or left of the fixation point. There were 3 conditions. In the first condition there was a constant foreperiod of one second. In the second there were 2 foreperiods, which were intended to be indistinguishable, of 975 and 1025 msec. In the third condition, 2 foreperiods, which were intended to be discriminable, of 750 and 1250 msec were used. The results were plotted on a Latency Operating Curve (LOC) in which d' are plotted against RT in a similar manner to SAT functions, but in this case the expected relationship was linear. In examining the third condition they found that the 2 estimates from the 750 and 1250 foreperiods gave similar estimates of stimulus difference as measured by d' . However, the 1250 msec foreperiod gave (as did the long ISI² for Granjon and Reynard) faster RT's. This was coupled with more errors. This suggests that as the stimulus became more expected with the passage of time, so the criterion used in the discrimination became more 'risky'.

2.b. Conclusions on Foreperiods, etc.

The main effect of a long foreperiod, ISI or RSI is to increase the uncertainty of when the signal will appear and so lead to greater variability in RT. When the period of time before

a stimulus appears varies, subjects become more and more expectant of the signal as time passes, and this can lead to their responding with less stringency. With this in mind the experiments in this report will, with one exception, all have short RSI's with which there is no such problem of either varied or long intervals to complicate time estimation. In the one exception (Experiment 8) the RSI's will be held constant within each run, again in an attempt to minimise such effects.

3. The Sequential Effect of Stimulus Intensity

Very little experimentation on the sequential effects of the intensity of the signal used has been reported. Holland and Lockhead (1968) required subjects to make absolute judgements of 10 differently attenuated tones of 1200 cps. They looked at the judgements of signals related to the preceding trials, and found that the intensity of signals on up to 5 preceding trials affected the judgement on the current trial. Previous stimulus intensities tended to 'attract' subsequent responses towards them - in effect condensing the scale being used by the subject. Jestadt, Luce and Green (1977) have demonstrated, by using a multiple regression analysis of their data, that this effect is limited only to the immediately preceding trial ($n-1$). Nevertheless they do agree that there is a linear regression relationship between the log values of the previous stimulus and the latency of response for the current trial. These tasks involve the identification of one of several signals which lie on a scale. The effect of the intensity of the preceding stimulus has been thought of as its addition to a weighted average to an internal referent.

This is the essence of an explanation in terms of Adaptation Level Theory (ALT) first proposed by Helson (1947) which considers that when judgements are made along a continuum there is an internal representation which serves as an 'anchor point'. As long as this anchor remains stable, judgements will correspond to external values but if it drifts judgements will be biased accordingly. The anchor value is said to be influenced by the magnitude of the observed signals or stimuli. Hébert (1973) has proposed a theory which is an integration of ALT and SDT, and which is also relevant to the consideration of such sequential effects.

Hébert proposes that ALT can predict how the psychological representation - that is the internal representation - of stimulus values is anchored and how it can move. As a subject experiences signals of a particular magnitude so the anchor will drift towards it. The criteria of SDT theory divide the psychological continuum into as many categories as the task require. Thus, as the anchor point moves so it draws the psychological continuum with it, together with the criteria which are related to it.

If Hébert is correct it is likely that sequential effects in absolute judgements are due to movements of the adaptation level (anchor point) rather than of criteria as defined by SDT and similar theories.

John (1967) proposed a rather similar theory which, in simple terms, states that in serial simple RT tasks in which intensities may vary from trial to trial, the criterion used by the subject must take on a value that will reflect the stimulus

intensities as a whole. He reports an experiment in which subjects responded to one or other of 2 differently attenuated tones in which he found that latencies for trials following the more intense stimulus were relatively faster than those following the less intense stimulus. This is consistent with a SDT type of criterion related to threshold detection (Figure 1-1) in which the criterion is moved towards more caution (less chance of false alarms) when the observed stimulus is loud and the criterion is found to be (for that stimulus) rather 'riskily' placed. It is also consistent with the ALT/SDT prediction that the criterion is fixed, but the internal referent/adaptation level is shifted by the value of the previous stimulus.

This is one problem which is resolved when relative discriminations are used - the comparison item that is being used is always present and so internal adaptation levels are not relevant.

4. The Importance of Feedback to Sequential Dependencies

In most theories of discrimination that consider sequential effects it is assumed that the subject is given feedback in order to 'learn from his mistakes'. The subject is considered, as Rabbitt and Vyas (1970) and Lappin and Disch (1972) have suggested, to track his performance of errors. That is, he gradually speeds up his performance until an error occurs and then slows down again. There are 2 conditions under which this state of affairs is quite likely: First, in experiments where the subject is not given feedback by the experiment but the alternative stimuli are readily discriminable. It can be taken from Rabbitt and Vyas (1970) that under these conditions most errors made by subjects are not perceptual errors and are in fact detected by the subject.

When this happens it could be said that the subject is able to generate his own feedback and so to moderate his performance accordingly. To a certain extent this is the case for the first experiments reported which deal with the sequential effects of response repetition. In that case there was a constant adjustment of performance in which the 2 responses were traded off against each other.

Second, there is the condition in which the subject is given feedback and so should be able to track his performance relative to the feedback that is actually received. Friedman and Carterette (1964) studied the effect of feedback and demonstrated that when feedback was given to the subject by the experimenter the RT for the response on the current trial depends more on the current stimulus than it does on the response made on the last trial. Without feedback the reverse was true. The implication would seem to be that in the absence of feedback the subject uses his perception of the previous response as a form of feedback.

MacDonald (1974) reports 2 experiments in which the effect of feedback was related to task (detection/recognition), stimulus probability and a burst of noise between trials. It was found in the detection experiment that the presence of feedback significantly affected trials immediately following errors. There appeared to be learning from error trials with feedback. That is, in this case the subject would be made aware that his threshold level of detection was wrong. In the recognition task (identifying a 1000 cps signal) there was no sequential effect of feedback. There were 3 levels of difficulty. In the easy condition virtually no errors were made (no report of actual frequency) and so feedback would probably not be needed. In the difficult condition (where there

were 40% errors) the stimuli could probably not be readily distinguished so that it would not be possible to identify the difference between stimuli and there would be no distinguishing label to which the incoming information could be assigned. It would seem from this that feedback only helps the subject when he can identify the item to which the feedback refers. In the previous case of detection, the subject probably will adjust a criterion which will refer to incoming noise levels whereas in the recognition experiment he actually needs to be able to distinguish between 2 different internal representations of the signal.

Thus in a difficult task it would seem that feedback is no help to the subject. This could either be because the subject does not need the feedback or because he cannot use it (except to move criteria towards more or less sensitive settings). This leads on to the problem of how subjects can - or even if they are able to - adjust their performance when difficult stimuli are used and there is no feedback from the experimenter. MacDonald has already intimated that subjects in a difficult task cannot make use of errors to modify their performance - at least certainly not to amend performance on the immediately succeeding trial.

5. Where now?

Feedback generally implies that information about the preceding trial becomes available to the subject to give him some idea of the correctness of his current response. In the absence of feedback this has generally been assumed to be available either from further processing of the stimulus during the time between the conclusion of his decision and the termination of the signal, or from the monitoring of kinaesthetic feedback which allows the subject to

detect errors in response execution. When discriminations are difficult the proportion of perceptual errors will rise, and the proportion of motor errors will fall, so that kinaesthetic feedback from responses may bring little benefit. Extended monitoring of the signal will probably not help very much either since once a certain time has been taken for a decision, there can be very little gain in accuracy by further examination. This can be seen from a brief study of any SAT curve. The extra time of analysis available to the subject once the response sequence is set in motion is usually quite short especially if the signal is removed at the moment when the response is made.

Thus it has first to be established whether subjects are able to adjust their criteria and, if they can do so, how they do this.

One hypothesis is that subjects adjust their criteria according to the overall difficulty of all the discriminations which they experience. This is not to say merely that subjects will take longer on more difficult tasks. This can be predicted by all the theories we have discussed for cases when subjects perform differently on difficult than on easy tasks using the same criterion. The question is whether they can actually, meaningfully, move their criteria in response to changes in difficulty, since different values of criteria will be appropriate at different difficulties. If they can do so it may be that they can 'track' their performance to maintain speed error tradeoff according to difficulty in the same way as it has been proposed that they track their performance to their perception of errors.

The first two experiments will attempt to show that subjects can do this, later we shall see whether subjects can be aware (as demonstrated by their performance) of the overall difficulty level of a task.

CHAPTER 3

THE INFLUENCE OF DIFFICULTY ON CRITERIA

EXPERIMENT 1

We have seen that there are many variables which are known to affect the relationships between successive trials. Some of these have been mentioned in the previous chapters. It was stated that we do not know how, or whether, subjects can adjust their performance to the difficulty level of the task with which they are presented. Very strong biases due to response sequences have been described, as have the effects on criteria of the magnitudes of successive signals in absolute judgement tasks. We now propose to investigate whether there are similar dependencies associated with the levels of difficulty of successive trials.

Subjects are well known to take longer over difficult decisions than over easy ones. Decision theories allow for the prediction that criteria will lead to different error rates and latencies when maintained at the same value but used with different levels of stimulus difficulty. Generally, though, the decision models initially considered suggest different optimal settings of the criteria in order to balance the two factors of error rate and speed for different levels of difficulty.

As an example, in a discrimination task the criterion for an unbiased performance is probably best positioned around the midpoint between the two alternative distributions, (see Figure 1-1). In such a case if the discrimination becomes easier by the movement of one of the distributions - for instance, in a line discrimination if one of the lines is made longer - then to maintain a reasonable level of performance and in order not to

make too many false alarms the criterion should ideally be moved to the new midpoint. The question is whether in a difficult discrimination task the subject can or will make such adjustments.

Consider now the case in which the difficulty of the task varies from trial to trial in a two alternative forced choice paradigm. The two levels of difficulty could either:

1. both be decided using criteria of the same value. Differences in RT and error rate would be due to the different rates of information gain for the two alternatives, possibly in a manner predicted by the theories which we have discussed. This situation would presumably only apply when the two levels of difficulty could be considered to be on the same dimension as each other.
2. Criteria of two separate values could be used, one for each level of difficulty. This would imply that a particular criterion would be called upon each trial and that subjects would take two looks at each target, one to decide which criterion to use and the second to actually make the decision. Both parts of this decision process would take some finite time as would the process required to access the required criterion. If this were the case, then one could consider that the criteria for each of the two stages may or may not be independent. In either case there might be a certain time, which would probably have a fairly constant value, required to switch from one criterion to the other.

The criteria will have to be stored in memory and it is possible that the criterion that is not currently used will change due to memory loss. This would imply that its value would change while it is kept in storage, and this change could be random or systematic.

If the criterion movement due to such change were systematic then it could occur in one of the following ways:

- a. Towards the other criterion - difficult trials would be faster after several easy trials, but this trend would be offset by any time required to change criteria. Thus difficult trials would be slower after a run of other difficult trials, the time increased by any extra time required for criterion change.
- b. Away from the other criterion - difficult trials would now be slower after a run of easy trials, plus changeover time. Easy trials would be faster after difficult trials, but slowed by changeover time, as in (a) above.
- c. The criteria may drift towards more caution or more risk. Thus, the first use of a criterion following its non-use would be at a different level of stringency, but would return to a more normal level with use.
- d. The criteria may have to be reset for each trial and then an easy trial judged after a series of difficult trials might appear, on first inspection, to be relatively easy, and so a lax criterion might be used. This criterion would drift towards more caution as further easy trials occurred.

If the subject were to use one criterion for both of two difficulty levels, then any change observed in the latencies to one difficulty level should also be reflected in those to the other. Thus, if the subject became more rash with easy decisions this should also be seen for difficult decisions. A further implication is that if trials appear easy, and if the subject adjusts his criteria accordingly, then sequences of easy trials should be followed by relatively fast RT's for difficult trials and vice versa.

Of course it may be that subjects do not adjust their criteria in the light of previous difficulty. First, it is necessary to see whether they can, and then, if they do so, to see how their criteria actually change.

METHOD

Subjects:

9 male and one female aged approximately 20 to 30 years.

Procedure:

Apparatus - The subjects sat in a dimly lit room with their heads against a rubber face mask facing a cathode ray tube at a distance of 370 mm, see Figure 3-1. Their hands rested on keys (Figure 3-2) and they were required to press keys under each index finger to respond to signals shown to them. The screen of the CRT was coated with p11 phosphor (decay to 10% in 80 microsecs) and the stimuli were presented under the control of a LINC/8 computer.

Stimuli - The stimuli were pairs of parallel vertical lines presented centrally on the CRT. The lines were made up of points on the CRT. The shorter line was always made up of 60 points (18.5 mm) and the longer line of 61 points (18.8 mm) (difficult discrimination) or 63 points (19.4 mm) (easy discrimination). The longer line could occur with equal probability to left or right. The stimuli would appear for 3500 msec. If there was no response, then the stimulus would disappear and after 500 msec the next trial would begin. If there was a response during the 4 secs, the stimulus would disappear immediately and the next trial would start after a response stimulus interval of 200 msec, see Figure 3-3.

Testing - Each subject had 200 practice trials, 100 easy and 100 difficult discriminations presented in blocks in two runs. Then they each had three runs of 200 trials in which the easy and difficult stimuli were mixed together. Between each run they had a rest of 3-5 minutes.

Instructions - The subjects were instructed: "Respond with your index finger on the side of the longer/shorter of the two lines. There will always be a difference, so if you do not know please guess. You will have three and a half seconds in which to view the stimulus. They will disappear either after that time or when you make a response. The new trial will then start. Please try not to miss any responses. I am more interested in accuracy than speed, but while remaining accurate I would like you to keep going at a steady speed."

Five of the subjects were asked to respond to the longer and five to the shorter line.

Results:

The overall values for reaction time and error rate across subjects were:

Easy trials - reaction time 548 millisecs; error rate 4.3%

Difficult trials - reaction time 832 millisecs; error rate 27.9%.

These facts on their own do not tell us very much. They do, however, emphasize that the two levels of difficulty were quite distinct, producing different error rates and different response times. In order to answer questions about context a more detailed analysis, in which the trials are analysed according to the context, is needed and so a transition analysis was performed on the data. By this is meant that the data were divided up according to the

transition state between the previous and present trial. In the present data there are 16 such categories. The present trial (N) may be easy or difficult (E-D) and the longer line may be to the left or right (L-R) the same applies to the previous trial (N-1). The data from this analysis are shown in Table 3-1, an analysis of variance was performed on the data. See Appendix: AN1-1 and AN1-2.

Five of the subjects had been asked to respond to the longer, and five to the shorter of the two lines. This was a methodological precaution to check if there were any bias in this direction. No effect was found between these groups. ($F = 1.8$ and 1.9 df $1:8$ for the RT and % correct analyses respectively). This factor was then ignored and two separate analyses performed, one on percentage correct data and the other on RT's. See Appendix: AN1-1 and AN1-2. In both analyses of variance all factors and interactions were tested against the next higher interaction with subjects. Both reaction times and error rates to easy and difficult trials were predictably, significantly different.

The interesting interaction that was significantly different in the reaction time analysis was the four-way interaction involving previous trial easy and difficult x L-R x this trial easy/difficult x L-R. Four-way interactions are always very difficult to interpret and can often be considered meaningless; in this case there does seem to be some sense - a plot is shown in Figure 3-4. The four-way interaction for percent correct is shown alongside. Several general points seem to stand out and they will be considered under the difficulty transition levels that apply.

Case One - Easy trials preceded by easy trials: repeated responses were faster than alternate responses; in addition, right-handed (R) responses were faster than those using the left hand (L).

Case Two - Easy trials preceded by difficult trials: here there was very little effect on repeated trials, though where an alternative response was required a response by the right hand following one by the left hand seemed faster than the other way round. In both the above cases there seemed to be little effect on error rate though alternate responses seemed slightly more error prone than repeated responses.

Case Three - Difficult trials preceded by difficult trials: repeated and alternate stimuli seemed to lead to similar reaction times though repeated L responses seemed faster than repeated R responses and L leading to R seemed faster than R leading to L. It seemed as if responses following a L response were faster than those following a R response. Where the response required was repeated there seemed to be fewer errors. This implies that when a difficult trial followed a difficult trial the subject tended to repeat his response. This is easy to understand as the display had not altered very much.

Case Four - Difficult trials preceded by easy trials: here alternated responses were faster than repeated responses, with fewer errors, with the following provisos:

- a. L responses were faster than R.
- b. L responses gave rise to fewer errors than R responses.

It should be noted in this case that the stimulus had changed in the direction of the alternate response. Thus, there was a tendency to respond with the alternate response when going from

easy to difficult trials. Thus, to summarize - the following trends have appeared:

- a. When the difficulty level changed from one trial to the next there was a tendency for the subject to make an alternation response.
- b. When difficult displays followed difficult displays there was a tendency to repeat the response.
- c. When easy displays followed easy displays repeated responses were faster than alternation responses.

All three observations are consistent with the empirical studies reported in Chapter 2 in which if successive trials appeared similar there would be a tendency for the subject to repeat his response. Unfortunately, there is little here, in either analysis, to suggest that there is any effect of transition of difficulty alone. The only effects at this level interact with the side of the display on which the longer line appears.

Remington (1969) presented subjects with two lights which were turned on in a random sequence with either equal probability or at a ratio of 70 to 30. The data were analysed and showed a repetition effect, as previous workers had shown. However, he went on to analyse the data in a more detailed way and found that the effect could be shown to last for up to five trials - repeats becoming faster and faster.

Kirby (1972) has demonstrated the same influence in an experiment with longer ISI's (2, 4 and 8 seconds) in which there was a negative response effect, i.e. alternations were faster than repetitions.

The Remington and Kirby analyses demonstrated that the repetition and alternation effects, which are noticed immediately from one trial to the next, can be seen to be cumulative over a number of trials. It was felt that if there was an effect of difficulty, then it might have a more gradual impact. If this was the case, then an analysis of the kind performed by Remington and Kirby might be more sensitive and enable the influence of difficulty to be observed. It was decided to see if the results of the present experiment would show similar long-term effects. The following analysis was performed, which will be called the Sample Size analysis. Each trial was categorised according to the stimuli preceding it. A window was set up to include one to six to eight previous trials. Within the window a count was made of the number of trials of a particular difficulty. Thus, if the window covered three trials there could be 0, 1, 2 or 3 easy (3, 2, 1, 0 difficult) trials. The results of this analysis are plotted in Figure 3-5. Errors were included in this analysis - both in the counting within windows and in mean reaction times.

We can see that the RT's to easy trials seem to be quickly affected by preceding easy trials. With a window size of only one or two there is a pronounced effect which continues through to a window size of six. It is interesting to note that the plots for size 4 - 6 have a plateau. The reaction times seem to be affected by extreme probability sequences, whereas the sequences with a more average ratio of easy to difficult trials affect reaction time less. Linear regression lines through these points are significantly different from zero ($t = -3.0$ df 3000 at window size three). There is no attempt here to suggest that the relationship is linear, merely an attempt to indicate that there is a trend.

The difficult trials, as suggested by the analysis of variance, seem to be relatively unaffected by the difficulty of the immediately preceding trials - at least when the window size is small. However, when the window size is three or more there appears to be an effect. As the proportion of easy trials rises, so the reaction times of the trials slow down. Regression lines for window sizes four and five have t values of 1.15 and 1.55 ($df > 3000$) respectively, indicating some effect. It should be noted that the effects on reaction times to both difficult and easy displays are in opposite directions; difficult trials are slower after a run of easy trials, and easy trials are also slower after a run of difficult trials. The absolute difficulty level of the trials is obviously the same on different occasions, yet something has changed to make the subject take longer to respond.

It is assumed for the moment that the criteria mentioned in the introduction to this experiment have moved in directions which lead to the observed speeding up or slowing down of responses. The directions of these movements in this data seem to preclude the use of only one criterion for both levels of difficulty.

As mentioned earlier, it is possible to think of the subject either using one criterion for both levels of difficulty, or two criteria - one for each. In the event of him using one set of criteria, then any movement of that criterion, such as altering k in a Random Walk, Recruitment or Runs Model, will lead to both easy and difficult trials having shorter or longer latencies. In this experiment the latencies for easy and difficult trials have been seen to diverge. It is difficult to see how this can occur unless we consider that the subject is using separate criteria for the two difficulty levels. Also the two criteria may be

independent - they appear to drift towards more caution as more trials of the other difficulty level are presented. This could either be due to a general drift towards caution of an unused criterion, or to the possibility that it takes longer to switch in a criterion once it has been used. The latter possibility, however, should only be a once-only effect and should give the same increase in RT for easy trials followed by two difficult trials and easy trials followed by more than two difficult trials, with the proviso that the criterion may have to settle again after "recall" if it has drifted. Hence RT's to difficult trials after only one difficult trial should be the same as after two or more. The end points of the graph show that this could well be the case for both difficult and easy trials. Difficult trials after a sequence of two or more difficult trials have reaction times of 830 - 870 milliseconds. Easy trials after two or more easy trials have reaction times of 510 - 530 milliseconds. Conversely, RT's to difficult trials after a run of two or more easy trials vary from 830 - 940 milliseconds and RT's to easy trials after difficult trials from 560 - 610 milliseconds. RT's to difficult trials appear to be more affected than easy ones.

This could suggest, therefore, that:

- a. there are two criteria
- b. the easy criterion is less affected by preceding easy or difficult trials, but
- c. like the difficult criterion, takes longer to access after a period of non use, and

- d. both criteria are affected by previous ease or difficulty of trials and when a sequence of one difficulty occurs the other criterion becomes more cautious.

In conclusion:

1. The above analyses seem to suggest that there are two criteria being used. This could imply that, either the two levels of difficulty must lie along different dimensions or they must at least be readily distinguishable along one dimension. If this latter were the case, then it might be possible to reduce the difference between the two difficulty levels and thus get the subject to use only one criterion.
2. The criterion not being used seems to alter to a value of greater caution - or at least subsequent decisions using the criterion take longer to make.

EXPERIMENT 2

In the first experiment a relatively few readings were taken from each subject. This could either lead to unreliability of the data - or that the effects observed may only apply to relatively naive subjects. With those factors in mind it was decided to replicate the study, collecting more data per subject. The parameters of the next experiment differed from the first in the following ways - the basic paradigm being similar.

1. Easy trials were made more difficult. They were now more similar in difficulty to difficult trials and it was felt that they might then be less easily distinguishable and so subjects may find it harder to use a two-look method. By a two-look method is meant that the subject first inspects the stimulus with the intention of deciding which set of criteria to use, i.e. in this case whether the trial is easy or difficult. When the stimuli are made more similar as they are in this experiment it becomes harder for the subject to decide which difficulty level he is confronted with on a particular trial, if indeed he is aware that there are two such levels.
2. Apart from the two binary variables Left/Right and Easy/Difficult, a third variable was introduced. It is well known that repetitions and alternations of stimuli and of responses have strong effects on RT. In order to try and make some allowance for this and also to allow for possible estimation of the effect later on if required, the stimuli were "mirrored" in the top and bottom half of the screen. In the first experiment, the vertical lines differed at their upper ends. Now the lines either "grow" up from the bottom

of the screen like stalagmites, or down like stalactites. In both cases the difference between the lines was in the centre of the screen.

METHOD

Standard experimental setup.

Apparatus - The subject sat in a dimly lit room with his/her head resting against a face mask maintaining a fairly comfortable view of a LINC/8 cathode ray tube at a distance of approximately 370 mms (Figure 3-1). The scope was coated with phosphor p11 which decays to 10% luminence in about 80 microsecs. The scope was approximately 80 mm high and 97 mm across the display area, and mounted in a 19 inch rack. The screen was surrounded by a black cardboard mount to reduce distractions and reflections to a minimum. The subject's hands rested on a keyboard in front of him that had 10 keys arranged as shown in Figure 3-2. The subject was required to respond to stimuli by pressing one of the keys under his index finger. These were the only keys used.

Stimuli - The stimuli presented to the subject consisted of a pair of vertical lines, one of which was longer than the other. The shorter line was always 18.5 mm long (2.86 degrees) and was drawn with 120 points. The lines were 200 mm apart (3.1 degrees).

The subject's task was to decide on which side was the longer line. During the experiment half the subjects were required to respond on the side of the shorter line as a control, but there was no difference between the two. There were two levels of difficulty:

Easy - where the longer line was 123 points long (2.93 degr).

Difficult - where the longer line was 121 points long (2.88 degr).

Timing - A trial consisted of the presentation of a stimulus (a pair of lines) and the subsequent response. The stimuli would appear for 3500 msec. If no response occurred during that time, the stimulus would disappear and after a pause of 500 msec a new trial would begin. If a response occurred at any time during the stimulus presentation or the subsequent pause, the stimulus would be turned off (if it had not already disappeared) and a new trial would begin after a period of 200 msec. Thus the task was self paced with a response-stimulus interval of 200 msec. Trials in which no response occurred were not repeated, but there were very few of them (about 0.7% in Experiment 3).

In one run a subject was presented with a series of 200 trials. This would take approximately 5 minutes, and between each run there would be a break of several minutes. At the start of each run the following instructions would appear on the screen:

RESPOND TO LONGER LINE

PRESS KEY TO START

When a foot switch was depressed the display

READY

would appear for one second and then the trials would begin.

At the end of the display:

END OF RUN

would appear. The timing of trials is illustrated in Figure 3-3.

Nomenclature - Described above are four alternative stimuli. They will be referred to as:

EL , ER = easy discrimination, respond left or right.

DL , DR = ditto difficult discrimination. Later another dimension was added in which half the stimuli appeared to the top of the screen and half in the bottom of the screen. There are now eight conditions, with the letters T and B added for "top" and "bottom" the codes now become:

TEL	TDL	TDR	TER.
BEL	BDL	BDR	BER.

Each stimulus was equally likely to occur at any time, except as specifically described in the biased experiment. The programme generating the random sequences did not balance for frequencies, or for transitional effects and so did sometimes produce slightly unbalanced frequencies, though not far from ordinary random expectations.

The experiments were computerised and run on line. The computer generating the displays and calculating response times was a LINC/8 computer. The programme controlling the experiments was PACE which was written by the author as were the analysis programmes on the same computer.

Subjects:

6 subjects, 3 male and 3 female. Their ages are not known exactly but they are in the range 20 - 30 years. All were right handed.

Procedure:

Each subject attended for four sessions of about one hour. The first session was treated as practice. The setup was as in

standard experimental details above. Each session was made up of five runs of 200 trials. Between each run a five minute rest was given during which the subject was informed as to his/her overall performance.

Instructions:

Three of the subjects RS, JJ, GA were asked to press the key under the index finger of the hand on the side of the longer line. The other three subjects SV, MR, DS were asked to respond on the side of the shorter line. They were asked to respond fairly quickly but to be accurate.

The subjects were instructed: "Respond with your index finger on the side of the longer/Shorter of the two lines. There will always be a difference, so if you do not know please guess. You will have three and a half seconds in which to view the stimulus. They will disappear either after that time or when you make a response. The new trial will then start. Please try not to miss any responses. I am more interested in accuracy than speed, but while remaining accurate I would like you to keep going at a steady speed."

Results and Discussion

Easy and difficult trials were, as planned, less different from each other than in Experiment 1, though they were still distinguishable from one another by error rate and by RT. The error rates and RT's averaged across the six subjects and the last three sessions, but omitting trials where the RT was less than 100 msecs, were as follows:

Easy trials	RT 1154 msecs	errors 11.7%
Difficult trials	RT 1378 msecs	errors 31.7%

As in the previous experiment, a transition analysis was performed. In this case there were 64 different categories. Each trial could be one of eight alternatives:

TLE, TLD, TRE, TRD, BLE, BLD, BRE, BRD

and each could be preceded by the same eight trial types. The data was collected separately for the four sessions and six subjects. A full analysis of variance on the data would require one capable of accepting nine factors and 1536 data items. Unfortunately, the analysis programme available was only capable of handling seven factors and 990 data items. In order to get around this difficulty two things were done:

1. The data were averaged across sessions 1 and 2, and also across 3 and 4. It was hoped that this would still leave enough information for the effect of practice to be estimated. In fact of course it prevents detection of effects changing between sessions 1 and 2 - a time when probably most change, due to practice, will occur. In order to cover for this, a separate analysis was performed solely on sessions 1 and 2. This only showed an interaction in which, essentially, alternate responses settled down in the second session to being faster than repeated ones, with left and right responses being more similar. In the first session, left responses were faster than right in both repeated and alternated responses - the amount being larger after a difficult trial. As the main interest was in difficulty levels it was felt possible to accept the averaging as there was no interaction otherwise of practice and difficulty.

2. The transitions from Top/Bottom to Top Bottom were treated as one factor of four levels, as was the transition from L/R to L/R. It is possible that this could reduce either main effects or interactions, but it was considered a small risk when allowance is made for the increased degrees of freedom in the resulting variance ratios.

Initially the design of analysis was treated as a split plot design (Kirk 1968) with five factors within subjects:

A : Session 1,2 vs 3,4 B : This trial easy/difficult
C : Trial (N-1) easy/difficult D : LL, LR, RL, RR
E : TT, TB, BT, BB
and one factor between subjects
G : Respond to "longer" or "shorter" line.

The analysis was performed separately on RT's and on error scores. See Appendix: AN2-1 and AN2-2.

There was no effect of, nor interaction with the factor G - whether to respond to the longer or shorter line. This had been included purely as a methodological precaution, and the analysis was repeated without this factor, which increased the degrees of freedom of the error terms. See Appendix: AN2-3 and AN2-4.

In the first analysis and in both analyses on error rates, a three-way interaction was significant at the 5% level. This was the interaction A x D x E (Session x handedness of response required x transition of Top/bottom). These interactions are plotted in Figure 3-6. This is basically an interaction between T/B and L/R in which, if the stimuli following each other are both at the top or the bottom of the screen, the alternate responses (thin solid line in the figure) are faster than repeated

ones (thick solid line). The other responses are similar to each other except that stimuli to the bottom left of the screen are faster and more accurate. In the first session repeated left top and right top responses were faster than the same in the second session.

The interaction does not seem particularly easy to understand and appears mainly to relate to the transition between physical locations in the display, and their interaction with practice. If anything, it appears to suggest that there may be a slight asymmetry of the display as the bottom left stimuli were faster and more error free. This was not examined in detail and later analyses will be averaged across this and so not be influenced by it.

These analyses do not support any effect of the transition of difficulty from one trial to the next. Trials seem to be unaffected by the difficulty level of the previous trial.

Further Analyses and Discussion

Next a sample size analysis was performed on the data from sessions 2, 3 and 4 and across subjects. Trials producing RT's below 100 msec were ignored, as were two runs in which subjects made erroneous responses. This analysis covered 17469 responses - 8707 easy and 8762 difficult ones. The number of trials omitted, either through no response or because of the fast RT, was 43 - less than $\frac{1}{4}\%$.

Again there is no immediate effect of the difficulty of previous trials. However, in the larger window sizes (see Figure 3-7) RT's for both easy and difficult trials were faster after a high proportion of easy trials and slower after difficult ones. Regression lines fitted to the points were non-significantly

negative. At a window size of 8, for instance, the t value for difficult trials was -0.8 and for easy -1.68 . The latter value, while not being significant does suggest some relationship.

If this speeding up and slowing down of RT's could be related to criteria, then the shifts of these criteria are sluggish and do not occur until a relatively high proportion of easy or difficult trials, and only over the larger window sizes. In Experiment 1 the change in RT was a little more marked and this may well be due to the greater ease of the easy condition. In that experiment the difference between easy and difficult RT's was 289 msec - difficult RT's being 54% longer than easy ones - whereas in this experiment the difference of 224 msec represented an increase of 19%. The differences in error rates were 24.1% and 20%.

The sample size analysis shows that both easy and difficult trials are similarly affected by preceding trials. Unlike Experiment 1 the criteria appear to be moving in the same direction. This suggests a different mechanism from the first experiment. It would seem not unlikely that the two levels of difficulty are now being judged using the same or related criteria. If we consider the use of only one criterion, a slight difficulty arises - theories of decision making, such as Random Walk and Accumulator Models, would predict that any change in criterion should affect difficult decisions more than easy ones. In fact we have seen in this experiment that the RT's to easy trials were affected rather more than RT's to difficult trials.

The regression lines through the data points at a window size of eight gives slopes of -4.22 for difficult and -6.8 for easy trials. See Appendix: REG-2. This means that on average every repeat of an easy trial decreases the reaction time to succeeding stimuli by 4.2 msec when the next trial is difficult and by 6.8 msec when it is easy.

It should be noted though that:

- a. The two slopes do not differ significantly from each other.
- b. Neither do they differ from zero, though the easy regression line has a value of $t = -1.68$ with above 8000 df which would be sufficient for a one-tailed test at the 5% level.
- c. There is no claim being made that these slopes are linear - the values are being used as indicators of possible trends and as some estimate of their magnitude.

The slope thus measured gives a prediction of a total change in RT of around 35 msec for difficult trials and just over 50 msec for easy trials across a window size of eight. Thus, the adjustment being made to the criterion is a fairly fine one.

This would be quite reasonable as, after all, the subject is expected to maintain a fairly consistent performance. Any gross change in criteria at this stage would lead to quite large changes in performance which would be unacceptable within the larger framework of the run or session.

When these changes in RT are matched against histograms of RT and error rates, they keep within areas where the error rates remain fairly constant for quite a wide range of RT change. This explains why there is very little mention here of the effect of error rates - there is virtually none.

There would appear at first sight to be a conflict between the two analyses reported so far. The first - a straight transition analysis suggested that there is no immediate effect of difficulty. The second - the Sample Size analysis - suggests there is an effect of difficulty. Presumably the effect is long term and either:

1. the effect is gradual - too slight to pick up immediately in the transition analysis - but enough to have an effect over a period
2. or possibly the effect of difficulty occurs belatedly - after two or three trials. Again it may be slight and cumulative.

In an attempt to examine this in more detail another analysis was performed called 2SEQU. This analysis allowed a trial to be classified according to immediately preceding trials - rather like the other analyses - but this time specific sequences of previous trials could be studied. The trials could be classified into one of four categories and then trials labelled according to a particular sequence of those categories.

In this particular case only two categories were used. Trials were simply classified according to difficulty. Thus, there were four trial types in each level of difficulty, TLE, TRE, BLE and BRE were easy and the other four difficult. Trials were then labelled according to a sequence of three previous trials - there were then eight groups: those following three easy trials (EEE_x) (where x represents the present trial which may be easy or difficult), those following two easy and one difficult (EED_x, EDE_x, DEE_x), and so on.

The data were collected separately for each session of each subject and an analysis of variance performed.

This analysis had six factors:

- A present trial easy/difficult
- B trial N-1 e/d
- C trial N-2 e/d
- D trial N-3 e/d
- E Sessions
- F Subjects

It was treated as a within subjects design. In the error score analysis, apart from the current trial difficulty (trial e/d), there were no significant effects. See Appendix: AN2-5 and AN2-6.

In the RT analysis two interactions were significant. The interaction between the difficulty of the current trial (N) and the difficulty of trial N-2 was significant at the 5% level (see Figure 3-7). This suggests that if the current trial is easy then the level of difficulty of trial N-2 influences the RT to the present trial. If N-2 was easy then the current trial is faster. There seems little effect on difficult trials.

A second interaction was significant at the 1% level. This was the interaction in level of difficulty of trials N-2 and N-3. This showed that the present trial, regardless of difficulty, would be faster if both N-2 and N-3 were easy, and slow when N-3 was difficult with N-2 easy. Thus, the RT of the current trial is affected by the level of difficulty of preceding trials. This effect is not immediate, but occurs only after an intervening trial. It is as though the subject, while responding to a trial, is adjusting his criteria according to previous information, but this setting either takes time, or the criterion value being used cannot be adjusted in this fine way while it is being used. This may of course be a function of the speed at which the experiment was being run. A later experiment examines this position further by slowing down the presentation rate of trials.

In the light of the experiment by Rabbitt (1969) in which he used response-stimulus intervals of 20, 120 and 220 msec and found that the trials following an error were affected even at the shortest interval this would appear to be unlikely. Perhaps

it is a function of something akin to confidence in which the subject in Rabbitt's experiment would be fairly certain that he had made an error, whereas in this experiment the subject may not really know how difficult a particular trial is, and only after several difficult ones does he begin to appreciate that they are in fact difficult.

It must be remembered that the difficult trials are being shifted in the reverse direction to that observed in Experiment 1. This may be due to one of three reasons:

1. Chance.
2. The extra factor of Top/Bottom of the screen.
3. The change in difficulty level.

As already indicated, the change in difficulty level of the easy trials made them more similar to the difficult ones, and so it could be that the subjects were using one criterion or one set of criteria for both sets of difficulty. These criteria, then, were influenced by the difficulty of the preceding stimuli, which is reflected in the similar changes in reaction time to both easy and difficult trials.

If, in fact, the answer lies in the change in difficulty level then it may be possible to replicate these results. This will be examined in the course of the next experiments.

It was stated above that the two regression lines did not differ significantly from zero. This of course may either be that there is no effect or that the one that is present is too small to be detected within the noise of the experiment. It has already been agreed that the amount of adjustment is small - the problem remains as to whether it exists at all.

In conclusion, these two experiments have shown that a subject's performance is affected by the difficulty of the immediately preceding displays. In the first experiment this was consistent with the subject using two sets of criteria, one each for easy and difficult displays. The term 'set of criteria' is used to cover the possibility of the true situation being like the SDT model which was modified for discrimination by the introduction of an extra criterion, as illustrated in Figure 1-2. In the second experiment where the displays were more similar, it appeared that the subjects were only using one set of criteria for both difficulty levels. This is apparent in the Sample Size analysis from the way in which RT's to both easy and difficult trials were affected in the same direction by the difficulty of the preceding displays. This is in direct contrast to the case in Experiment 1 in which the two were affected in opposite directions, and is consistent with the greater similarity in this experiment between the two levels of difficulty. The data are consistent with the subject relaxing his criteria to a less stringent level after a run of easy displays, thus when the subject subsequently faces a difficult decision, he tends to be faster than he would if he had previously received a series of difficult ones.

From:	LEFT		RIGHT	
	EASY	DIFFICULT	EASY	DIFFICULT
To:				
LEFT				
EASY	534	566	557	575
DIFFICULT	851	831	809	877
RIGHT				
EASY	545	557	517	574
DIFFICULT	848	849	887	883

Table of Transistions between handedness and
difficulty in Experiment 1. RT's.

Table 3 - 1.

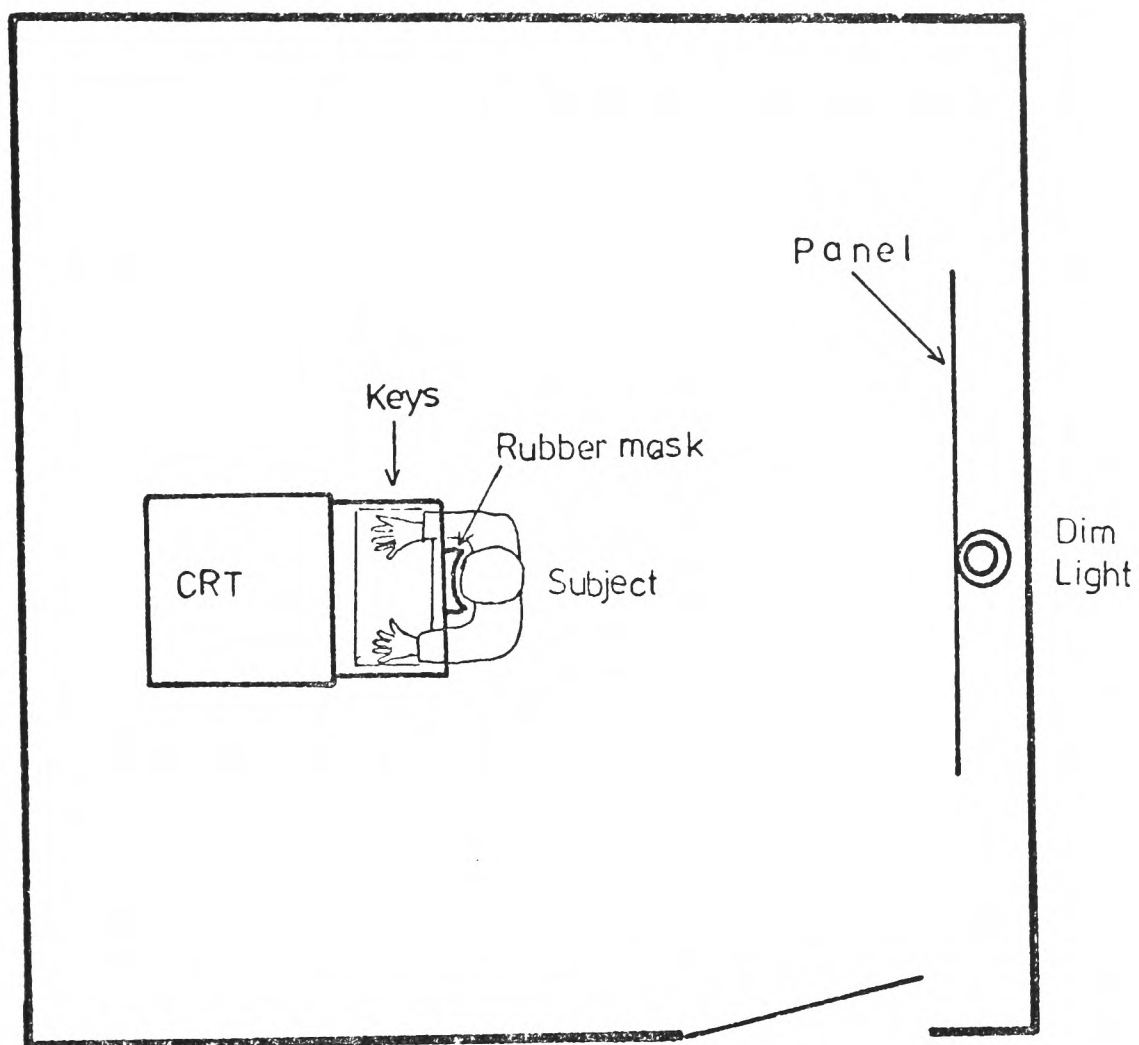


Figure 3-1 Subject Testing Room

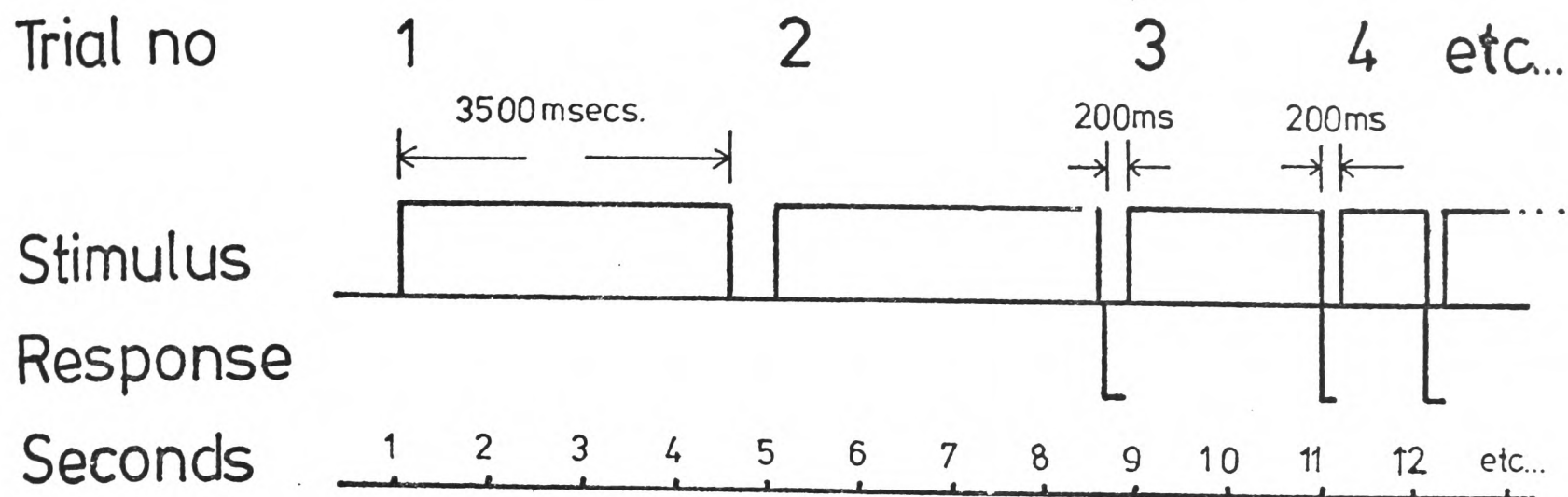


Figure 3-3 Experimental Timing

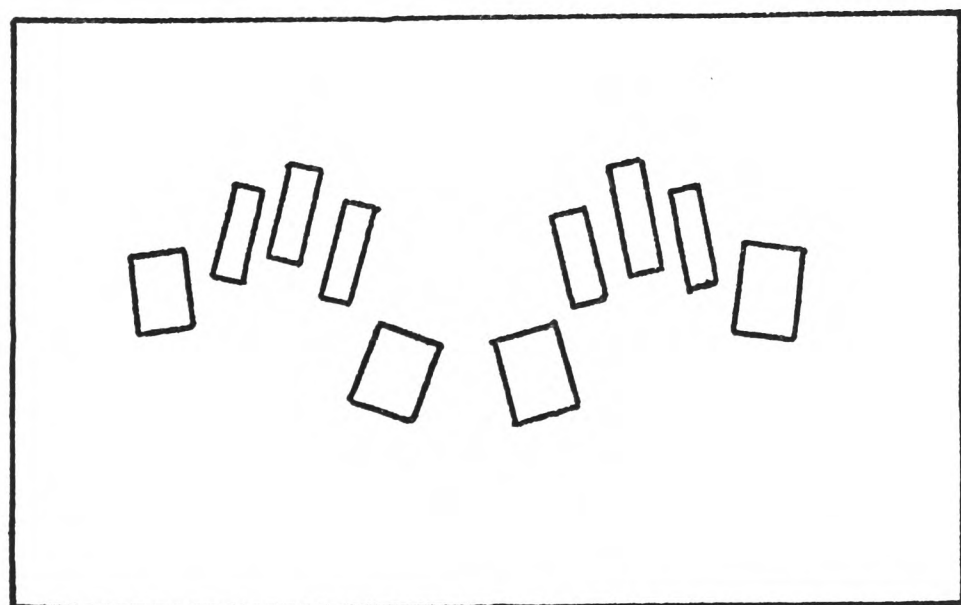
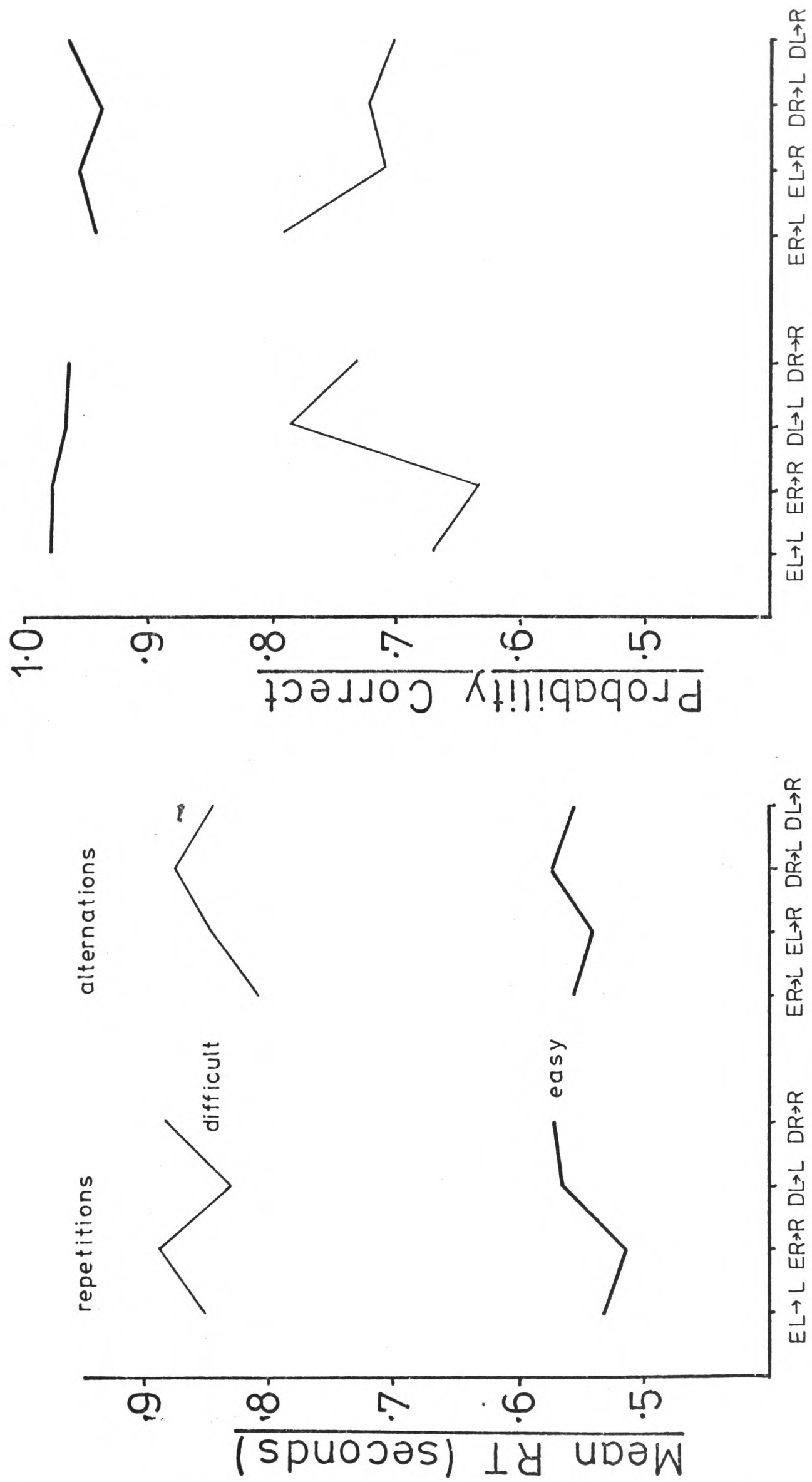


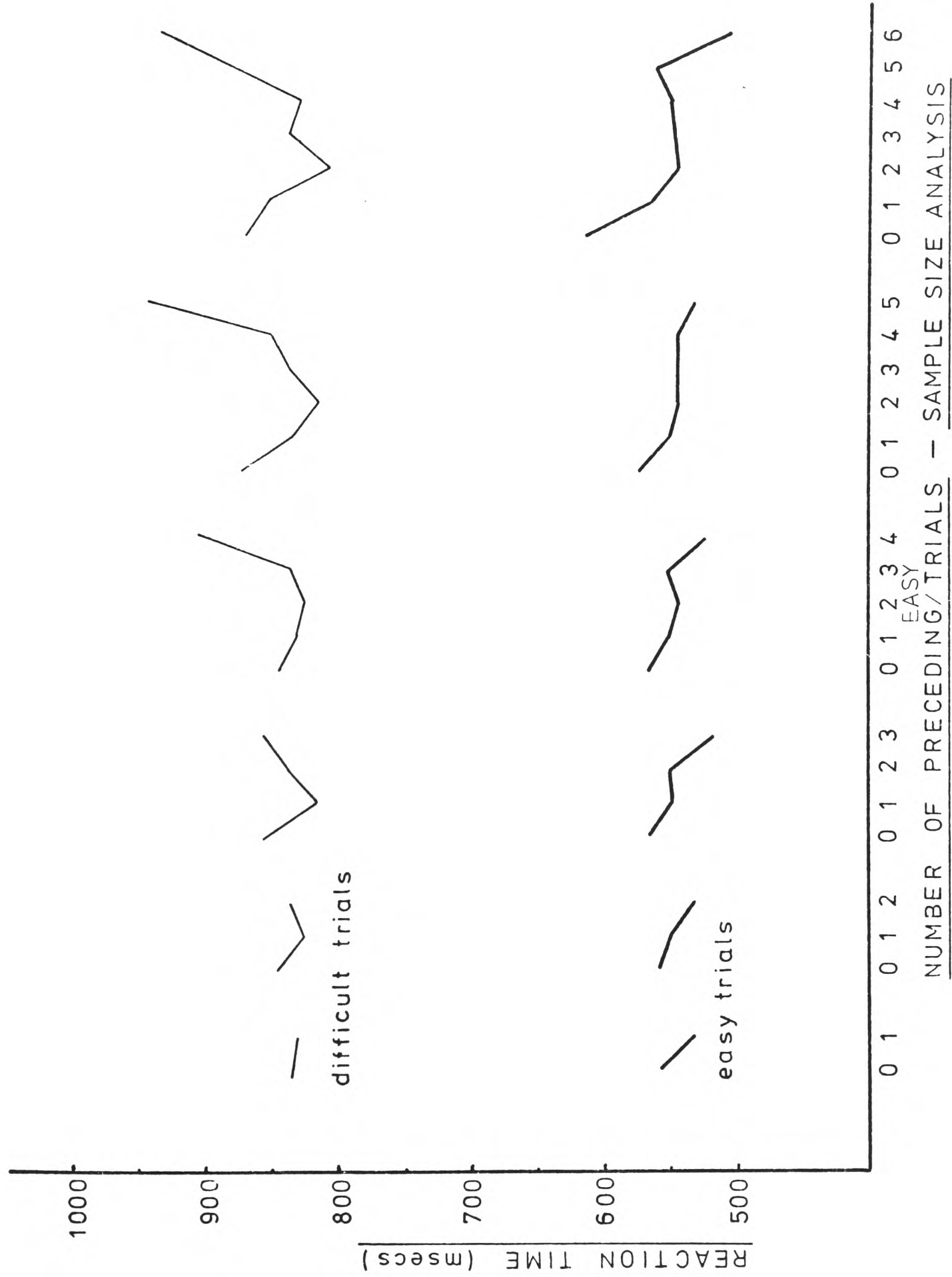
Figure 3-2

Plan of response keys.

Plot of 4-way interactions of RT and Percent Correct in experiment 1.

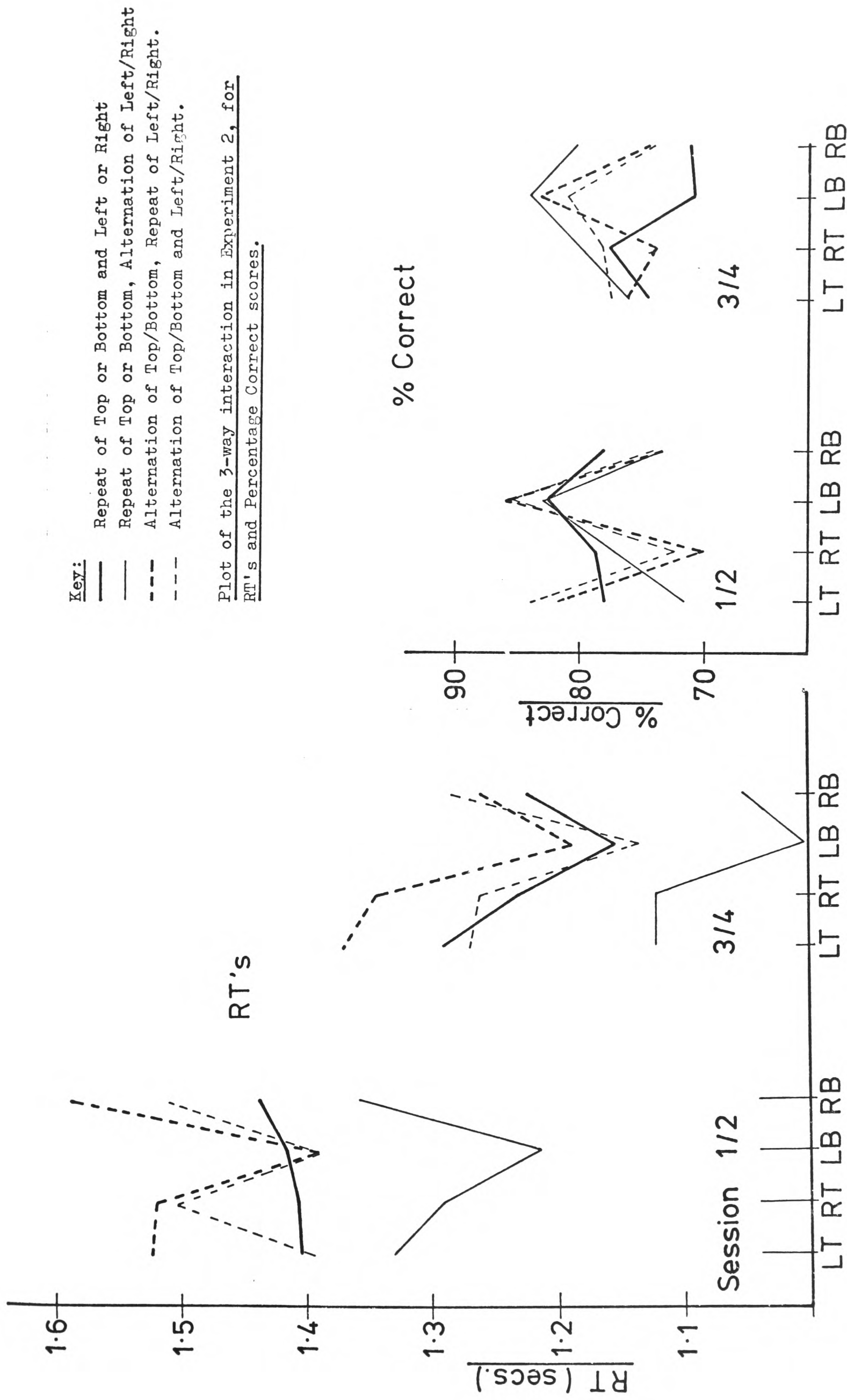
Figure 3 - 4





EXPERIMENT 1

Figure 3-5



- THIS TRIAL -

Figure 3-6

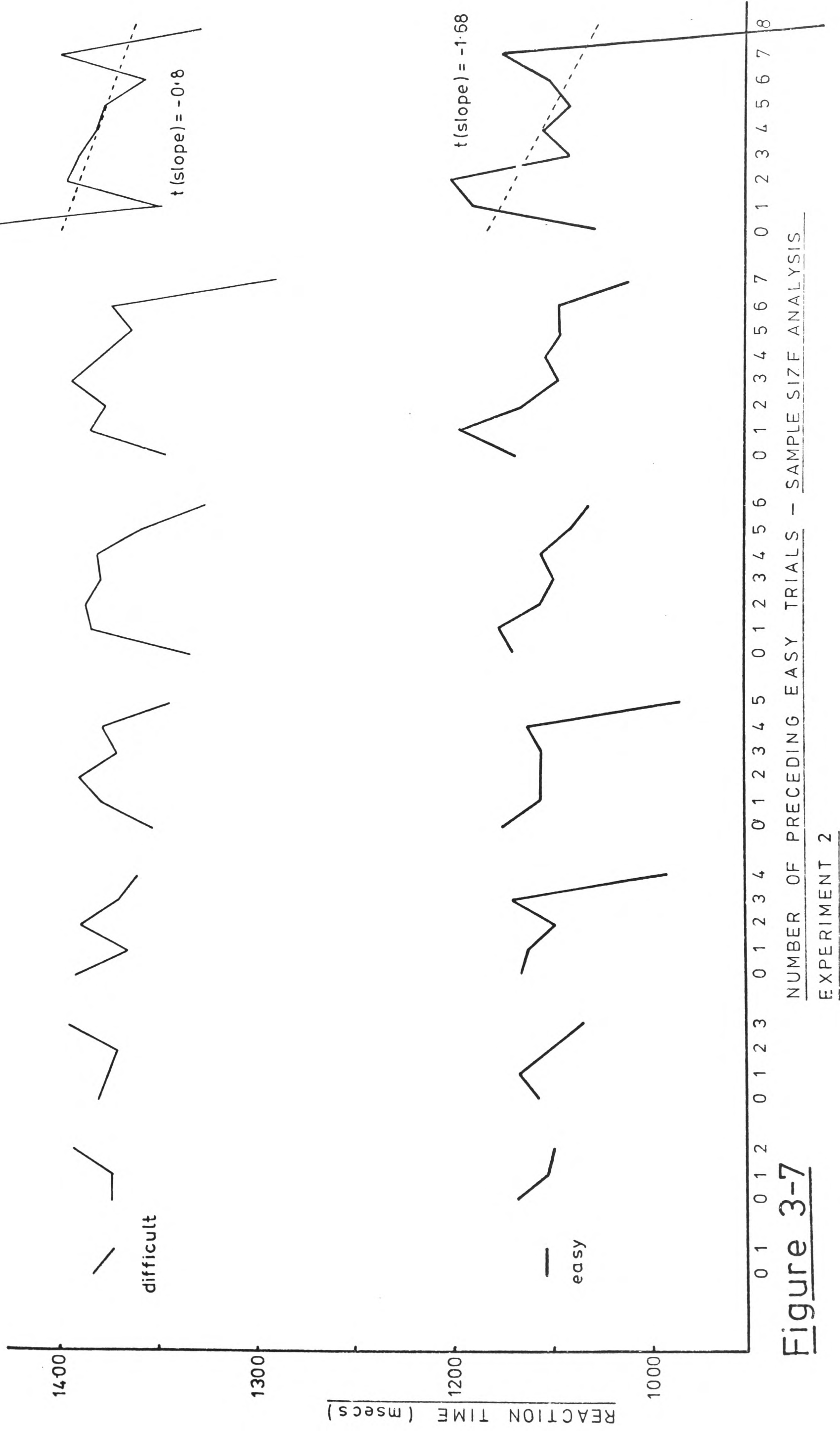


Figure 3-7

CHAPTER 4

THE EFFECT OF OVERALL DIFFICULTY ON REACTION TIMES

We saw in Chapter 2 that the repetition effect was probably associated with, or at least affected by, the subject's anticipation of the signal. The likelihood of a signal being repeated is directly related to the probability of the signal occurring in the first place.

The probability of a signal has been shown to affect subjects' responses. Falmagne (1965), for instance, presented subjects with the digits 1 - 6 in random order with a 1-1 stimulus response mapping. During the first five runs the probabilities of the stimuli were equal, but for the last 11 runs the probabilities ranged from 0.01 to 0.56. He found that the responses to the more probable stimuli were faster, and showed that the speeding up of RT's to repeated signals was proportionately greater for the less probable signals than for the highly probable ones. By this is meant that the difference between the latencies of repeated and non-repeated signals was greater for the less probable signal. According to Broadbent (1971) he also showed that the responses to the more probable signal were more likely to occur as errors. Laming (1968) reports similar findings.

This supports the proposition that probability and the repetition effect work rather similarly in biasing the subject towards one or more responses and away from other less likely ones. Furthermore, the subjects were able to adapt their performance to the bias that they observed rather than one that they were aware of in advance, so the probability of the different responses matched the probabilities of the corresponding signals.

It is not intended in this report to attempt to consider the question of whether the important aspect is the repetition or the probability. The point being established is similar to one that was made before, that when stimuli are clear and unconfusable, subjects become aware of the probabilities of the different signals that have been presented to them and adjust their responses accordingly - trading the expectancies of responses in line with their likelihoods. Atkinson, Carterette and Kinchla (1962) and Grundy (1961) have also shown that subjects are well aware of the different stimulus probabilities without being given feedback.

The situation is altered however when one considers signals that are not so easily discriminable. Parducci and Sandusky (1970), for instance, used two bursts of random noise, the one attenuated by 1dB relative to the other. One of the two noise bursts was presented two seconds after a warning noise burst and the subject either had to discriminate between the warning burst and the target ("louder" or "softer") or to identify which of the two targets it was. The louder signal occurred with probabilities of 0.2, 0.5 or 0.8 in different runs. The task was a difficult one and they found that the proportions of "louder" judgements were independent of the stimulus probabilities. Thus, subjects were unable in these circumstances to determine the probability of the signal. It could be argued that the overall error rate in the recognition experiment of around 45% would lead to such inconclusiveness anyway. MacDonald (1974) reports that Kinchla (1966) showed that when difficult stimuli of unequal probability were used, subjects only matched the probability with their responses when they were given feedback.

MacDonald went on to report his own experiments (described earlier) in which he also varied the probability of less difficult signals. He found some effect of signal probability on response bias as measured by Luce's b (1963) and Atkinson, Bower and Crothers' b (1965). The effect was greater when feedback was given. Thus, it would appear that in the absence of feedback subjects are less able to match the actual probabilities of the stimuli. In the absence of instructions from the experimenter to the contrary they are likely to assume that both responses are equally likely, and so tend to respond with probabilities nearer chance.

If subjects are aware, as they appear to be, of the difficulty of the task they are performing, then it should be possible to persuade them to alter their criteria not only as has been shown, in a way that trades one response for another, but also towards more or less stringency. In this report we shall distinguish between the presence of response bias (where as one response is preferred, so the other one will suffer) and difficulty bias (where criteria are shifted towards greater or less stringency and all responses get faster or slower). In Experiments 1 and 2 we saw that the repetition of easy or difficult trials - or rather the proportion of each - affected the reaction time to succeeding stimuli. RT's to easy displays were faster in both experiments after a high proportion of easy displays, as were RT's to difficult displays in Experiment 2. RT's to difficult displays after an easy sequence were slowed down in the first experiment. If this change is due to the nature of the stimuli, then it should be possible to replicate the result of Experiment 2 by using the same stimuli again.

Furthermore, if the local difficulty bias does influence RT, it should be possible to bias RT's over a whole session or run by altering the stimulus probabilities. If, say, easy trials are more prevalent in a run than difficult ones, then both easy and difficult trials will be more likely to follow an easy run and should both be faster. This should mean that the average RT for a run will be faster if there are more easy trials.

Christ (1970) reports a similar situation in which subjects responded to the digits 1, 2, 3 and 4 in a 2 or 4 choice paradigm. The stimuli were made difficult by using short exposures - 50, 100 and 150 msec. An "S" group received stimuli of 50 and 100 msec while an "L" group received stimuli of 100 and 150 msec mixed together in a random sequence. He then compared the RT's to the 100 msec digits and found that the "L" group were faster. It would appear that they were treated as easier stimuli because of the context in which they appeared.

It is important to distinguish between two areas of difficulty bias. Leonard, Newman and Carpenter (1966) used as stimuli the digits 1 to 5. Four had probabilities of 0.08 and the other of 0.68. They found that subjects were influenced both by local "short-term sampling effects" and also separately by the general overall probabilities of the signals.

The short-term sampling effects can be identified with the local variations noted in the previous two experiments. The question now is whether subjects can also be aware of the overall difficulty level of the whole run; and whether the short-term effects merely generalise to the whole run, or if subjects can treat the two separately.

EXPERIMENT 3

METHOD

Subjects:

1 male, 5 females, age range approximately 18 - 25.

Apparatus and Stimuli:

As in Experiment 2.

Procedure:

Each subject attended for four sessions, each of which lasted less than an hour. During each session they were presented with six runs of 200 stimuli. The difference between this experiment and Experiment 2 was that in half the runs - easy bias runs - there was a ratio of 3:1 easy to difficult trials. The other half of the runs had the reverse bias 3:1 difficult to easy. Thus, in an easy bias run of 200 trials the expected number of easy trials was 150. In practice, because the random number generator was not given any absolute constraints the actual ratios varied slightly from this. Of the 14,400 stimuli in the difficult conditions, 3,500 were easy and in the easy conditions, 3,607 were difficult - the expected value should be 3,600 in each. In any session the first three runs were of one bias and the last three of the other bias. The order of biasing was counterbalanced across subjects (see Table 4-1). The subjects had a few minutes rest between each run which took about five minutes.

Results:

When the data are averaged across all six subjects and all four sessions, RT's were faster in the easy bias runs than in the difficult bias runs (see Table 4-3). The Z values quoted have degrees of freedom in excess of 12,000. The differences - as

implied by the sluggish way in which the error in RT's changed in the previous experiment - are small - 25 and 29 msec. The differences in error rate (0.25% and 0.55%) were essentially non-existent. This is not really surprising as at this kind of performance level, RT would have to change by a large amount to be sufficient to alter error rate.

The difference in RT was slightly larger for difficult trials. The difference is not at all significant, but would be consistent with the use by the subjects of one criterion for both levels of difficulty which, as it is altered, affects both levels of difficulty similarly.

However, what has been shown is that the subject's criterion seems to be set according to the general difficulty level of the run that is being experienced. Thus, RT's to both easy and difficult trials in an easy bias condition are faster than the same trials in a difficult bias condition. This was predicted by the trends shown in the previous experiment. An analysis of variance was performed on the data to try and tease out the source of the bias effect. See Appendix: AN3-1. As in the previous experiment, the data were sorted into categories related to the previous two trials. Thus, there were eight categories within each bias. This trial could be easy or difficult, trial N-1 and trial N-2 could also be easy or difficult. The data used were averaged across sessions. The counterbalancing of the order of biasing meant that by taking these averages, the effects of practice - which were large - would be reduced.

The only factor of significance was trial difficulty, see Table 4-2 for the values. There seemed no effect of bias. The data were examined more carefully subject by subject, and

it was found that there was individual variation. Four of the subjects showed a clear bias effect, as has been mentioned; one subject MA showed virtually no effect of bias (the differences for easy and difficult trials under the two biases have t values of -0.08 and -0.97 $df > 2300$). The last subject showed a reverse bias. FC was clearly faster ($t = +1.69$ and $+4.55$ $df > 2300$) in the difficult bias condition. No special factors idiosyncratic to this subject explained this. The subject did not perform particularly faster or slower or at a greater or lesser error rate than the other five subjects. Clearly a different mechanism was at work. One possible explanation that emerged later was that the subject may have felt that the easy bias runs, since they were easier, were worth being careful for, whereas in the difficult runs, since errors were inevitable she may as well respond quickly. Unfortunately the difference in the subject was not noticed until too late to question her about it.

The analysis was re-performed omitting this subject and again the only significant effect was that of trial difficulty. See Appendix: AN3-2. However, bias did account for about 20% of the within subject variance, though it was not significant, as the effect varied more from subject to subject than trial difficulty, which also accounted for a similar amount of variance. There appeared to be no effect of the difficulty of trial N-1 or N-2, nor any interaction of any of these effects.

Interestingly, the effect of bias on RT for the five subjects who did show some effect of bias was the same as the effect due to trial difficulty, see Table 4-3.

This relationship, it is felt, is certainly coincidental as the individual subjects do not show this consistently - as demonstrated by the non-significant F-ratio for bias. This did however lead to the consideration that there may be some relationship between the effect of trial difficulty for a subject and the effect of the bias itself. A correlation coefficient was calculated of this relationship in the following manner: for each subject the effect of changing bias was found for easy and then difficult trials by subtracting the easy bias RT's from the difficult bias RT's. Next the effect of changing the trial difficulty was found in a similar manner for each of the biases. (Compare Tables 4-3 and 4-6). The effect of bias on RT's to easy trials was then matched with the effect of trial difficulty on overall RT within easy bias runs. Similarly the effect of bias on RT's to difficult trials was matched with the effect of trial difficulty on over-all RT within difficult bias runs, see Table 4-6.

The correlation between these two sets of figures was found to be 0.82. The t value for this is 4.098 (df 8; 1% sig). Thus, it would seem that the subjects who showed the greatest difference in RT between easy and difficult trials also showed the greatest effect of bias.

In order to maintain a reasonable balance of performance, subjects require some knowledge of their current progress and generally this would appear to be accomplished by attending to errors, as suggested by Rabbitt (1970). Without feedback and with an imperfect knowledge of the errors he has made, the subject is left to determine how he is doing from his observations of the

stimuli and his own feelings of confidence concerning his previous responses. It would seem reasonable to assume that the important factor is the subject's opinion rather than the absolute difference between the stimuli. The subjects who are more aware of the difference between the two levels of difficulty are also more likely to be aware of the difference between biases and so be more affected. It is thought that this is really what is reflected in the significant correlation coefficient.

There is no evidence from the analyses of variance that the difference in bias is caused by the immediately preceding trials. Remember, in Experiment 2 there was some evidence that the difficulty of trial N-2 had some effect on the RT to the current trial. In order to investigate this further, a sample size analysis was done for each of the biases. This was averaged across the four sessions and over the five subjects, omitting subject FC. See Figure 4-2. The graph indicates some tendency for both difficult trials in the difficult bias runs, and easy trials in easy bias runs to be slower after several preceding difficult trials.

RT's to difficult displays in easy bias runs and RT's to easy displays in difficult bias runs are slower after several easy displays. These last are counter to the trends seen in Experiment 2 and also against the overall trends seen in this experiment. That is, if RT's to difficult displays were slowed by preceding easy displays, then RT's to difficult displays would be slower in easy bias runs than in difficult ones but this was not the case. The same argument applies for easy trials in the difficult bias runs.

This points then to two adjustment procedures being adopted by the subject. On the one hand, there are local fluctuations of RT brought about by the ease or difficulty of preceding trials, and second there is a longer term adjustment based on the overall difficulty of a run.

A further attempt was made to tease out where the overall difficulty bias effect was coming from and to try and see how quickly subjects were affected by bias. During each session subjects were given three runs of each bias, the first three of one bias and the last three of the other. This gave subjects plenty of exposure to each bias in a block. Maybe the effect of overall difficulty bias was due to a learning of the bias during a run that was then reinforced over the next two runs - thus, the information on how difficult a run was need only be assimilated slowly. This could be checked by examining only the first runs of each bias. The differences in bias are already apparent and significantly so, see Table 4-5. The difference in RT between easy and difficult biases on easy trials is in fact greater than over the sessions in general (see Table 4-2) and it would seem that easy trials are more affected by bias early on - the effect reducing as time and trials pass. Indeed, if this first run under a bias is compared with the second and third runs, the only mean that is different is the easy trial - difficult bias. In the first run on a difficult bias, RT's to easy displays are considerably slower than in later runs. Otherwise, the mean values appear to stabilise fairly rapidly. This implies that subjects are constantly assessing the difficulty of the runs they are performing and attaining a working speed fairly rapidly. Once attained it is only slowly affected by local changes in average difficulty.

It has been noted that although the expected effects predicted by the results of Experiment 2 are present, the trends shown by a sample size analysis are less well represented. The stimuli used were - apart from the bias - the same as used in Experiment 2 and so a comparison of the results was made between Experiments 2 and 3. It was noted that the performance of the subjects in this experiment was much poorer - they were more error prone.

Experiment 2	Easy trials	88%	Difficult trials	67%
--------------	-------------	-----	------------------	-----

Experiment 3	Easy trials	82-83%	Difficult trials	62-63% correct.
--------------	-------------	--------	------------------	-----------------

This could be explained by the fact that they were performing on average about 400 - 500 msec faster. It was decided to bring back the subjects from this experiment and have them repeat the experiment with different, more stringent, instructions to the effect that they were to be more careful. It was thought that this might enhance any sequential effects as subjects may well then take more notice of preceding trials.

Unfortunately, only five of the six subjects were available for re-testing. The one subject whose results were relatively deviant (FC) in the preceding experiment was not available. The experimenter was not aware at the time of re-testing that the subject had been different as the analyses of the preceding experiment were not complete at the time. Had it been known, then more strenuous efforts would have been made to attempt to locate her.

Discussion

This experiment demonstrates that when different difficulties of signals are mixed together in proportions other than 50:50, subjects treat all the trials as being of greater or lesser difficulty according to the direction of bias. This is a bias

of the criteria used towards either more or less stringency and is independent of any bias that there might be between responses. This demonstrates that subjects are tracking their performance somehow, as they are aware of the different biases. Subjects reported during the experiment that they noticed the varying difficulty of the runs. They were generally not aware that there were two levels of difficulty and sometimes not aware that there were stimuli of different difficulty during the run. Subjects were not informed that there would be mixtures of difficulty.

Furthermore, it would appear from the analyses that subjects were affected by two different levels of difficulty bias. First, there was the level of local bias, as shown in the first two experiments, and to some extent in this one, and second there was an effect of the overall bias of the session. Neither effect was very large and this seems consistent with subjects attempting to maintain a fairly consistent performance and in doing so, matching their criteria to the apparent difficulty level.

We have seen that some workers have considered combining signal detection theory with adaptation level theory (see Hébert, 1972). This has only been suggested for absolute judgements or, essentially, the identification of items along a scale. Can we think of the changes of RT and error rate seen in this experiment being caused by a change in anchor point rather than by a change in criterion? On the face of it, it would appear to be a plausible suggestion that difficult stimuli gradually pull an anchor point one way and easy stimuli pull it another. If the differences are considered to be, as they generally are, on a continuum including both positive and negative differences, as drawn in Figure 1-2,

then an ALT concept is not compatible. In order to explain an unbiased increase in caution, it is possible to think of two criteria (C1 and C2) moving apart symmetrically. ALT would only allow the shift of the criteria (both together) in the same direction unless the criteria were also altered independently.

ALT therefore would only help to explain movement in one direction. One possible way of including such a suggestion in the present circumstances would be to suggest that subjects look at differences between the two stimuli using the two alternately as standards. Thus, all the differences that the subject would be interested in would be positive. Any negative differences would be ignored. If the adaptation level lay between the two distributions, then the difficult distribution trials would draw the level towards it, which would move the criterion further from both distribution means and so speed up responses, whereas easy trials would tend to lead to slower responses (see Figure 4-1).

Thus, it would seem that ALT cannot readily be applied to such discriminations. This is not very surprising as the task is not an identification task but a relative discrimination task in which a standard line for comparison is being given each time. The process described in the previous paragraph could be used as a possible explanation for a contrast effect in which easy trials after a series of difficult ones appear to be much easier than after easy ones. It would appear that this is not happening in these experiments.

Conclusions

In Experiments 1 and 2 local difficulty bias was noticed. It had been thought that if whole runs of trials contained different proportions of easy and difficult trials, then the local bias might extend to the whole run. For five of the six subjects it was found that there was some change in mean RT between the two bias conditions that was consistent with this prediction. However, it was noted that some of the local bias effects went the other way and so it was considered that two separate estimates of the probabilities were being used - one, of the local variations in difficulty and the other a more long term awareness of the overall difficulty of the run.

This ties in with other observations on the ways in which subjects distinguish between the occurrence of runs of repetitions of particular events in a sequence, and the overall bias within such a sequence towards occurrences of one type of event rather than another. For example, Granjon and Reynard (1977) who studied the sequential effects of repetition and bias of foreperiod durations in simple RT tasks found that RT, when the same foreperiod was repeated, not only depended on the number of preceding repetitions (local effect), but also on the number of repetitions which occurred during an experimental run (long term effect). This is likely to be directly related to the present study as when the stimuli are biased as they were in this experiment, the length of run of different difficulty levels will be directly proportional to the different probabilities involved.

	- Session -			
subject:	1	2	3	4
CM	D	E	D	E
SW	E	D	E	D
JW	D	E	E	D
MA	D	D	E	E
FC	E	D	D	E
TC	E	E	D	D

Key:

D = a session in which the first 3 runs were all of difficult bias and the last 3 runs of easy bias.

E = a session in which easy bias runs preceded difficult bias runs.

The order in which the bias was presented in Experiment 3.

Table 4 - 1.

	EASY TRIAL			DIFFICULT TRIAL		
BIAS:	easy	difficult		easy	difficult	
CM	670	721	(+51)	692	760	(+68)
SW	591	611	(+20)	628	641	(+13)
JW	777	810	(+33)	844	859	(+15)
FC	827	794	(-33)	961	859	(-102)
MA	635	635	(0)	649	660	(+11)
TC	834	911	(+77)	875	1040	(+165)
Mean:	722	747		773	802	
CM	79.6	78.4		60.1	58.5	
SW	83.8	84.6		65.6	62.9	
JW	94.0	94.4		70.0	72.2	
FC	81.4	80.2		63.6	61.7	
MA	71.2	69.2		56.7	57.0	
TC	85.4	88.7		60.9	63.9	
Mean:	82.6	82.6		62.8	62.7	

RT's (msecs) and percent correct for both biases in Experiment 3. Figures in brackets are the effect of bias on reaction time.

Table 4-2.

	EASY TRIALS			DIFFICULT TRIALS		
bias	$\bar{E}$	E sd	$\bar{E}$ se	$\bar{D}$	D sd	$\bar{D}$ se
easy						
$\bar{e}$	722	285	2.25	773	345	5.76
difficult						
$\bar{d}$	746	318	5.38	802	372	3.58
$\bar{d} - \bar{e}$	24.5		6.04	28.7		6.78
z	4.04			4.24		

Key:

$\bar{E}$ & $\bar{D}$ Mean latencies for easy and difficult trials.

$\bar{e}$ & $\bar{d}$ Mean latencies for easy and difficult bias.

sd,se Standard deviation, Standard error.

z Normal curve standard units.

Mean latencies for experiment 3 averaged across all subjects and partitioned for trial and bias difficulty.

Table 4 - 3

bias	$\bar{E}$	$\bar{D}$	$\bar{D} - \bar{E}$
$\bar{e}$	701	738	37
$\bar{d}$	738	792	54
$\bar{d} - \bar{e}$	37	54	

Key: as above in Figure 4 - 3

Mean latencies for the four pairings of trial difficulty and bias averaged across 5 subjects of Experiment 3.

Table 4 - 4.

bias	$\bar{E}$	E sd	$\bar{E}$ se	$\bar{D}$	D sd	$\bar{D}$ se	$\bar{D} - \bar{E}$	se	z
$\bar{e}$	718	290	4.9	774	364	10.5	55.7	11.5	4.8
$\bar{d}$	761	313	9.3	806	375	6.2	45.0	11.1	4.0
$\bar{d} - \bar{e}$	42.4		10.5	31.7		12.2			
z	4.0			2.6					

Key: see figure 4 - 3

Mean latencies averaged across all subjects for the first runs of each bias in Experiment 3.

Table 4 - 5

	Effect of bias on trials (X)	Effect of trial difficulty within bias (Y)	
CM	51	22	Sum X 454
	68	39	Sum X ² 42384
SW	20	37	Sum Y 453
	13	30	Sum Y ² 30307
JW	33	67	Sum XY 32581
	15	49	
MA	1	14	r = 0.823
	11	25	
TC	77	41	$t = \frac{r \sqrt{n-2}}{\sqrt{1-r^2}} = 4.098 \text{ (df 8)}$
	165	129	

Values used for the calculation of the product moment correlation coefficient between the effect of trial difficulty and bias on latency for Experiment 3.

The values in the table are the difference in latency between either easy and difficult trials within each bias, or the difference in latency between easy and difficult bias for easy or difficult trials.

Thus the first value of 51 under (X) means that, for easy trials, subject CM, difficult bias was on average 51 msec slower than easy bias. The corresponding value under (Y) of 22 means that under easy bias difficult trials were on average 22 msec slower than easy ones. The second line refers to the effects on difficult trials and difficult bias.

Table 4 - 6

It should be noted that the correlation calculated here is a rather weak test for the interaction involved as it is related to the base level RT's of EASY trial/easy bias and DIFFICULT trial/difficult bias conditions. Also it should be noted that in the text on page 113 the correlation is considered to be spurious.

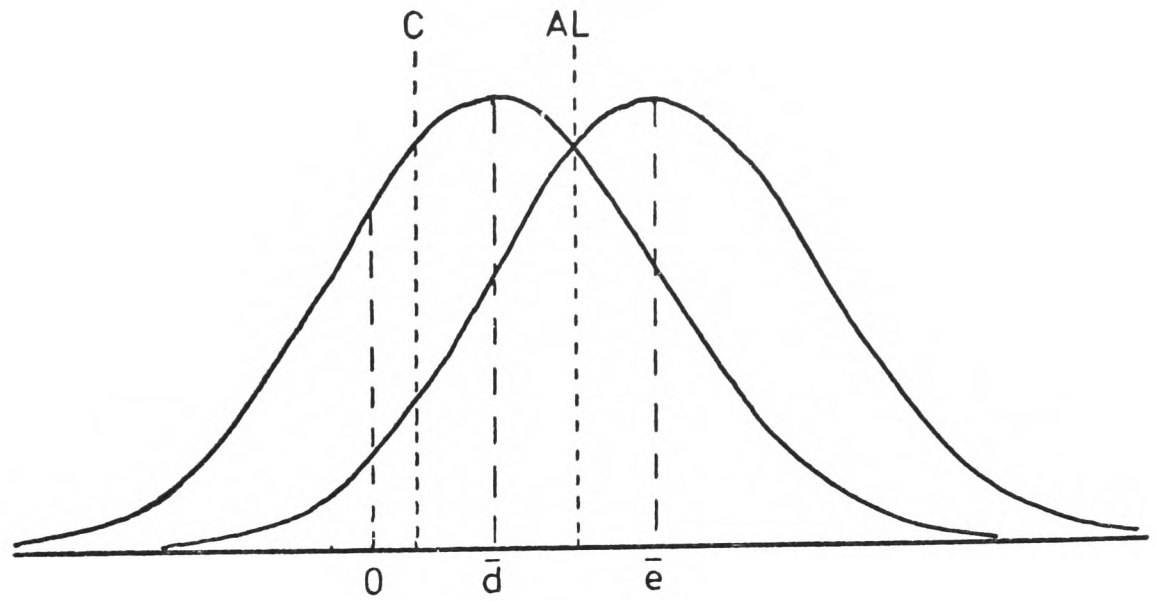
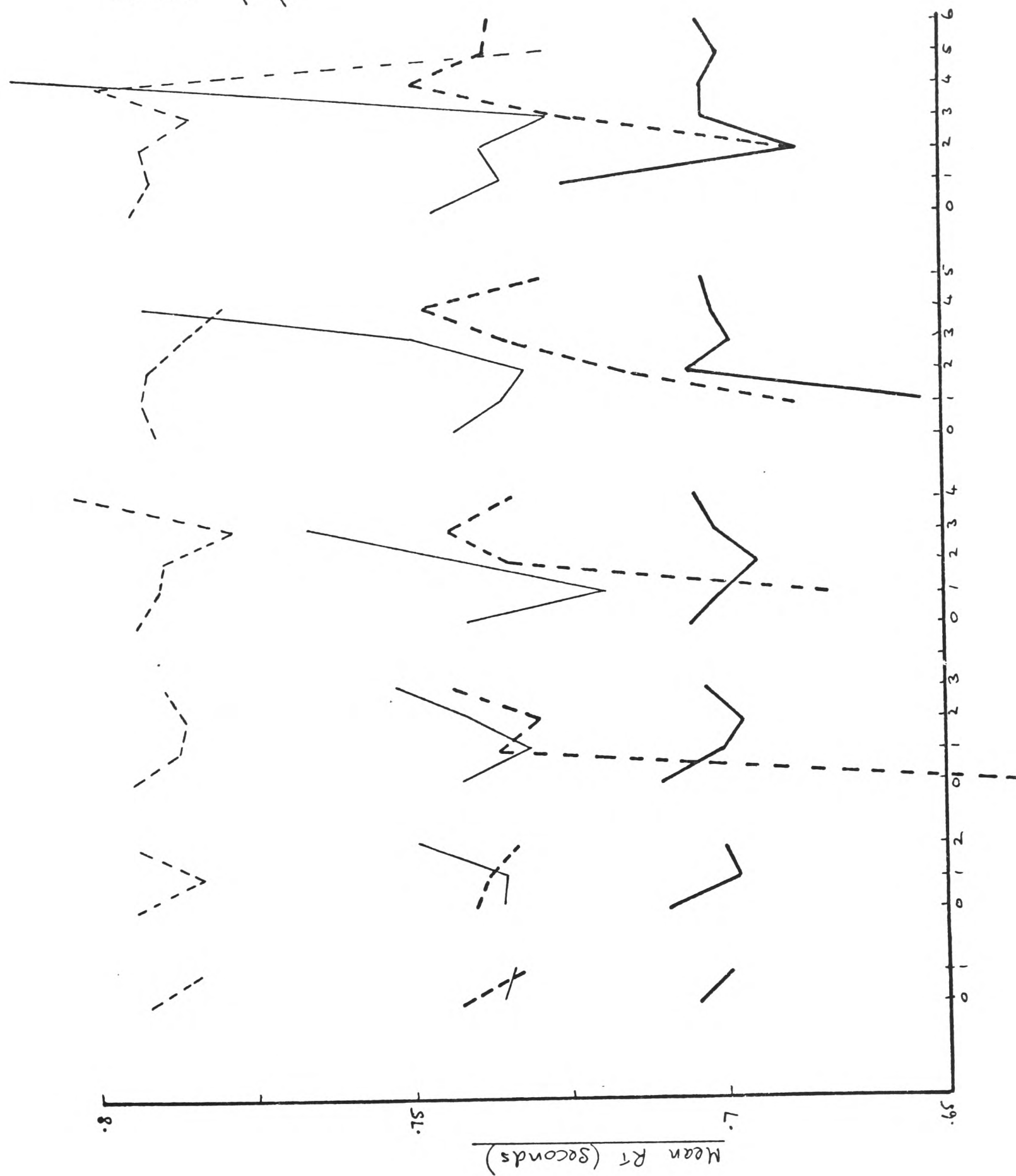


Diagram of the case considered in the text where the subject examines absolute differences between easy ($\bar{e}$) and difficult ($\bar{d}$) stimuli. When more easy stimuli occur the Adaptation Level (AL) is drawn to the right, which will also move the criterion to the right (C). This would lead to slower easy and difficult latencies as C is now closer to both distribution means.

Figure 4 - 1.



Sample Size Analysis for window sizes 1 to 6 and for both biases in Experiment 3.

Subject FC omitted.

Figure 4 - 2

CHAPTER 5

INSTRUCTIONS AND LEVELS OF DIFFICULTY

So far all the experiments have used a common set of instructions which were intended to induce performances of moderate caution. The three experiments reported in this chapter are concerned with the effect on local and overall difficulty bias of instructions that are given for greater caution or speed.

Experiment 4 uses the same subjects as Experiment 3 and so will allow some comparison between the effect of the previous instructions to be fairly cautious and the present instructions to be extremely careful. Experiment 5 essentially replicates Experiment 4 but has a more balanced design and instructions to be fast in addition to ones of much greater caution. Experiment 6 examines the interaction (within subjects) between instructions which were intended to produce differing levels of caution and local difficulty-bias effects.

Bush, Galanter and Luce (1967 p 90) state of instructions:

These have at least the following two roles.

First, they inform the subject about the nature of the stimulus presentations and background (usually by example); about the response set R and how responses are to be made; and about the outcome set O , the generation of the complex outcome schedule $\omega \in \Omega$ and the outcome function ϕ . Second, they attempt to convey to him the judgement it is desired he make or, what is the same thing, what significance his responses will have to the experimenter.

It is the second role which will be important in these experiments in that subjects will be required to perform with different degrees of caution. The advent of signal detectability theory (SDT) in the 1950's gave psychologists a theoretical framework to explain how subjects may alter their performance in line with such instructions. SDT allows the prediction of how false detections and misses can be traded for each other by the subject moving the criterion he is using towards the noise distribution or towards the signal + noise distribution.

Smith and Wilson (1953) demonstrated this relationship between misses and false alarms. Swets and Sewell (1963) showed how estimates of d' remained relatively unchanged under different monetary rewards. Galanter and Holman (1967) and Shipley (1965) have shown how SDT is useful for describing the effects of varying monetary pay-offs. All these experiments have typically been concerned with error rates and did not study the effect of these changes of criterion on reaction time.

Psychologists have long been interested in relationship between reaction time and accuracy - the Speed Accuracy Tradeoff function (SAT). If the reaction times of subjects are partitioned into time bands and the percentage of correct response plotted against each band, accuracy is seen to increase with RT to an asymptote, (Schouten and Bekker (1967), Wickelgren (1977)). Ollman (1966) and Yellott (1967) have suggested that subjects mix two strategies in their performance. One strategy is of reasoned decision, which is mixed with a number of fast guesses. Subjects achieve speed by increasing the number of fast guesses that they make. Other authors prefer to consider a tradeoff between the two that is more or less continuous, for instance, Schouten and Bekker (1967), Smith (1968), Hale (1969), Swensson (1972).

Certainly the theories considered at the start of this report predict that as the criteria for a task become more stringent, so latencies will increase and accuracy will rise though it is acknowledged that there is a limit to the accuracy attainable. Wickelgren (1977) has suggested that SAT functions should replace the usual reports of RT in experimental reporting. While acknowledging the importance of SAT, Schmitt and Scheirer (1977) argue for the retention of RT reports. Certainly this author considers that a SAT plot can be a useful descriptive tool, but it might not be easy to determine from it some aspects of the underlying data. For instance, any successful theory of discrimination will have to explain how RT's of errors and correct responses often differ. If we consider a subject using a criterion which produces correct responses of a given average latency, then the latencies to error responses may well be slower. Such a prediction is made by a Recruitment Model and by Vickers' Accumulator Model with a high criterion value. Finally, if the data from this output are partitioned into RT bands, as suggested above, such partitioning could well divide the RT's to correct and error responses made with the same criterion into separate groups. The error proportions within each band would then be composite events, with the errors from one criterion level being mixed with those from another. Admittedly this will also occur when reaction time alone is reported, but in this case it is known. In the case of SAT it is not known and caution must be shown in interpreting the curve.

Nevertheless it is possible to think of changes in instructions from speed towards accuracy as moving subjects' criteria in such a way as to shift their responses along a SAT function. This possibility will be studied.

EXPERIMENT 4

The stimuli used in Experiments 2 and 3 were identical. The only difference was the varied proportions of easy and difficult trials. If however the results are compared, it can be seen that the subjects in Experiment 3 generally responded faster and with more errors than the subjects in Experiment 2. It was decided as a first step in the investigation of the effect of instructions to re-run the subjects of Experiment 3 under a different instruction set. Although these subjects showed an effect of bias between runs, they showed very little local bias effect when compared with the subjects of Experiment 2. One possibility for this change was the fact that they were responding faster. It was felt that if the subjects were asked to respond with more caution the local bias effect might appear more as it had previously.

METHOD

Subjects:

The same subjects were used as in Experiment 3 with the exception of one female (FC) who was not available.

Apparatus and Stimuli - These were the same as previously described in Experiment 2. The biasing of difficulty was the same as in Experiment 3 : for half the runs the ratio of easy to difficult displays was 3:1 and for the other runs the ratio was reversed (1:3).

Instructions - Subjects were informed: "The experimental set up is exactly as before and the stimuli will be the same. This time however I wish you to be more careful. I want you to be more interested in being accurate than in being fast. I would still rather you guess than miss a response."

Procedure - The subjects each attended for two sessions of approximately one hour duration. During each session they were presented with six runs of 200 stimuli. The first three runs would be of one bias and the last three of the other bias. The two biases were as previously:

- a. Easy bias 75% of trials easy, 25% difficult
- b. Difficult bias 25% of trials easy, 75% difficult.

The order of presentation of the biases for the subjects is shown in Table 5-1 in which the order for the previous experiment is also given. The order of counterbalancing for the last two sessions was not ideal as for four of the subjects the order was the same in both sessions. As the subjects in the last experiment typically became faster from session to session and from run to run during the session, it was realised that this would affect estimates of overall bias but not any estimates of local bias where trials are compared with other trials in the run rather than with trials in different runs.

Results:

The mean latencies and percent correct scores are shown in Table 5-2. The figures in brackets give the difference in latency between the easy and difficult bias runs. A negative latency means that the difficult bias latencies were faster than those for easy bias.

If the mean reaction time over subjects is taken then the situation is much as in Experiment 3, with the difficult bias sessions being slower than the easy ones. However, when the data are examined more closely, it appears that this effect is

largely due to one subject TC who showed a massive 280 to 300 msec difference between the two biases. For three of the subjects there was effectively no difference between the latencies of the two bias conditions, while one subject SW showed a reverse effect (difficult bias conditions were faster than easy ones).

Analysis of variance on the effect of preceding trials

An analysis of variance was performed on the data in which the values were obtained by summing across the two sessions, see Appendix: AN-4. The difference between reaction times to easy and difficult trials was not significant at the 5% level. ($F = 6.825$ df 1:4). There was, as expected from the first inspection of the data, no effect of bias. ($F = 0.68$ df 1:4). In the circumstances it was not surprising that there was no effect due to the difficulty level of trial (N-1) nor (N-2). However, there was an interaction between present trial difficulty and the difficulty level of trial (N-2) ($F = 9.37$ df 1:4; Sig 5%). This is in the same pattern as was seen in the last two experiments - significantly in Experiment 2 and almost so in Experiment 3. Again, it appears that the difficulty of the immediately preceding display had no effect on the RT to the current display, but the difficulty of the display two before did affect the RT to the current display. The actual effect in this experiment was that if a display was easy, then it was faster if trial N-2 was difficult; whereas if a display was difficult it was faster when trial N-2 was easy. This is different from the effect in Experiment 2 where difficult trials were not affected and easy trials were faster when trial N-2 was easy. We cannot say exactly what was happening, but it would appear that the influence of the difficulty of trials on later trials was delayed.

Possible causes for the loss of effect

One possibility is that the trials occur in quite rapid succession, and so there was not time for any adjustment by the subject on an immediate trial basis. If this were the case, then slowing down the trials and having longer RSI's would remove the effect, or transfer the effect to the preceding trial. This will be studied in Experiment 8.

The effect of the instructions to be cautious has been to change the influence of bias on the reaction time of both easy and difficult trials. For one subject the effect was enhanced, whereas for the other subjects the effect was reduced or reversed.

In the case of the subject for whom the effect was reversed, it is possible that this was due to a strategy of the following kind: The subject had been told to get as many correct as possible. When the trials were easy the subject was determined not to make any mistakes and so was relatively careful. However, when the trials were difficult, there seemed little sense in being extremely careful as, subjectively, there was little chance of improving performance.

This kind of reasoning was not expressed directly but was inferred from conversations with this subject and others in a later experiment. It is only put forward as a very tentative answer, and is not born out by a reduction in error rate for the easy bias runs.

Whatever else is considered, the bias effect has been severely disrupted in this experiment.

First, it is possible of course that the loss of effect may be due to practice - that is, the subjects had by now performed for five or six hours at the task. This was felt to be unlikely as the subjects had two to three weeks rest between the two parts of the experiment.

Second, it is possible that there may have been some interaction between the two sets of instructions, as all subjects had four relatively fast sessions followed by the cautious sessions.

Third, it is possible that it may be due to poor counterbalancing of the order in which the biases were presented during the two sessions of this experiment. In several cases, the subjects had the same order of presentation of bias in the two sessions (see Table 5-1).

Generally speaking subjects responded faster towards the middle and end of a session than at the start and so counterbalancing is important in order that this practice effect is not confounded with the bias effect for which we are searching. However, SW and TC, who both had both sessions starting with easy bias, showed opposite effects of adaptation to bias.

Of the three remaining subjects who showed no effect of bias, two had both sessions starting with the difficult bias, which could have led to an increase in the "normal" bias effect, in which RT's to displays in an easy bias run were on average faster than the RT's in a difficult bias run.

Thus, it would appear that although there was an error of counterbalancing it did not have any systematic effect on the data and cannot be blamed for the changed biasing behaviour.

The next experiment is an attempt to investigate further this loss of effect due to instructions.

Finally, it would seem quite likely that the loss of effect was due to the instructions. In being careful, the subjects must pay much more attention to the individual displays. This concentration on the present display may cause them to forget earlier displays, in which case the subjects would be much less affected by local bias, as each trial would then be treated alone, out of context. In the absence of local bias there might very well be very little overall bias as it would seem possible that one would lead to the other.

EXPERIMENT 5

In the last experiment we saw that instructions to be cautious reduced the effect of bias. Two methodological points may have contributed to this. First, the 'caution' instructions were introduced to all the subjects after they had already received four sessions of Experiment 3, and so were very well practiced. This could have led to an interaction between the instructions and the effect of practice - subjects who are well practiced may well interpret such instructions differently from subjects who are new to the task. It could also have led to an interaction between the two sets of instructions - for instance, when the subjects were instructed to be much more careful, having already performed for several sessions under one set of instructions, they may take the new instructions much more seriously, feeling perhaps that they must have failed to perform as required in the first run. The second methodological point noted was the poor counterbalancing for some of the subjects. In this next experiment an attempt is made to remove these doubts by using fresh subjects and an adequately counter-balanced design.

It has been suggested by some experimenters that when variables are altered within subjects one may get unwanted effects that are due to the very fact of changing the variable (Poulton 1973). That is in this case there may be some interaction, as mentioned, between instructions if both are given to one set of subjects. Accordingly it was decided to use two sets of subjects with the instructions of the two different extremes given separately to each group of subjects.

Subjects:

5 males and 5 females aged approximately 18 - 26 years.

None had taken part in any of the earlier experiments.

Apparatus - The subject sat in a dimly lit room of dimensions eight feet square and eight feet high facing a Tectronix 604 screen at a minimum distance of 400 mm. There was a face mask at 400 mm but some subjects apparently sat a bit away from it as they found it slightly easier. The screen phosphor was p24.

Stimuli - The stimuli were pairs of vertical straight lines drawn on the 'scope as a number of points under the control of a PDP8/e running a programme PIPER written by the author.

There were three lengths of line - the standard line of 93 points (18.4 mm) a difficult discrimination line of 94 points (18.6 mm) and an easy discrimination line of 96 points (19 mm). At a viewing distance of 400 mm these correspond to angles across the length of line of approximately 2.64, 2.66 and 2.72 degrees. They were thus similar in length to the lines of the previous experiments. The line separation was 13 mm (1.86 degrees) rather less than the previous 20 mm, rendering the lines rather more discriminable, but at a similar ratio. The lines were refreshed every 20 msec.

Timing - This was also similar to the previous experiments. A trial consisted of the presentation of a pair of lines for up to 3.5 seconds during which the subject was required to respond. If no response occurred within four seconds of the start of a trial, the next trial commenced. Trials to which there was no response were not repeated. If a response was made, then the display immediately disappeared and the next trial commenced after a response-stimulus interval of 200 msec.

Procedure:

The ten subjects were assigned randomly to two groups of five and each group received instructions concerning either speed or accuracy. After the nature of the task was explained and the stimuli demonstrated, the subjects were instructed either:

"I want you to go as fast as you can without actually guessing" or "I want you to try to get each trial correct. Do not take too long; that is don't take more than three and a half seconds. If you do then the display will disappear and the next trial will begin. Try not to miss any trials. If you have to, then please guess rather than miss a trial. I am more interested in you being accurate than fast."

Each subject attended for three sessions. During each session, the subject would observe two runs of one bias (easy or difficult) and then two runs of the other bias.

There were 252 trials in each run. The first 12 trials comprised three runs each of the four kinds of stimulus in random sequence. These 12 trials were omitted from the data analysis. The 240 trials of the main part of the run comprised approximately equal numbers of displays with the longer line to the left or right. In half the runs the bias of trials was 3:1 easy to difficult and for the other half the reverse bias was given, see Table 5-3.

At the end of these runs each subject was then run for two runs of only easy or difficult displays. The results will be reported later. They were run under the same instructions.

Results:

The first session was treated as practice and the results quoted are the average across two sessions, see Table 5-4. As previously, the figures in brackets are the differences in RT between the two bias conditions. A negative value indicates that the difficult bias latencies were faster than those for the easy bias.

The average latencies for the "fast" group indicate that there was no influence of bias. When the individual subjects are considered, however, there was some variation, as in Experiment 4. For two subjects there was a "positive" bias effect - the RT's in the difficult bias condition were slower than the RT's for the easy bias condition. Two subjects showed no effect of bias. The remaining subject showed a "negative" bias effect - the RT's for the difficult bias condition were faster than the RT's for the easy bias condition. It was again suspected that this subject may have been using a strategy in which he was determined to make as few errors as possible in the easy bias condition. This was not supported by the evidence, as the slower latencies of the easy bias condition were not matched by reduced error rates.

The "slow" subjects were much more consistent, and, with one exception, were slower in the difficult bias condition. The exception was subject JL, for whom RT's to difficult trials were 11 msec faster in the difficult bias condition than in the easy bias condition.

The relationship between bias and difficulty

In Experiment 3 there was a correlation between the effect of bias on RT and the observed difference in RT between easy and difficult trials. This indicated that as subjects became faster, so the difference in latency between easy and difficult trials reduced, and, simultaneously, the difference in latency between easy and difficult bias conditions also reduced.

Again a correlation was calculated between these variables for the subjects in this experiment. A separate calculation was performed for the "fast" and "slow" groups, see Table 5-6. It was considered that if a correlation existed it would be positive, as in Experiment 3. The t value for a one-tailed test to reach significance at 5% is 1.86 with 8 df. The two correlations (0.42 for the fast group and 0.113 for the slow group) were nowhere near significance. (t values of 1.3 and 0.32 respectively).

There was no correlation either in Experiment 4, so we have seen that for subjects who were responding quickly or slowly, there was no relationship between the two variables. It is apparent from the RT's of the "fast" subjects that they did not respond differentially to easy and difficult displays, so it is not surprising that they did not respond differently to easy and difficult bias conditions. For the "slow" subjects, on the other hand, there were differences in RT between easy and difficult displays, and differences in RT between easy and difficult bias conditions. If there were a relationship between the two, it should have shown up in the correlation calculation for the "slow" subjects. The conclusion must be that there is

no relationship between the effect on RT of differences between displays and the effect on RT of the differences between biases.

The correlation that was observed in Experiment 3 was probably due to the fact that the subjects spanned two levels of performance. If a correlation is performed across subjects, some of whom are responding quickly, showing no effect of bias, and some of whom are responding more carefully, showing an effect of bias, a correlation will be seen. This can be seen in the present experiment. If the values in Table 5-6 are used in one single correlation, instead of dividing the data into "fast" and "slow", then the correlation value is 0.406. The t value for this is 1.88, which with 18 degrees of freedom is significant at 5% using a one-tailed test. Thus, it is felt that the original correlation was spurious - and that it did not demonstrate a real correlation of the two variables with each other, but rather a correlation of each of them with the simple presence or absence of any effect. It may be that there is a correlation between the effect of bias and trial difficulty over a limited range, but it is probably a by-product of the strategy of the subject, rather than a direct relationship.

It certainly implies that the subjects were not using the difference in decision time between the easy and difficult displays as a measure of the difference in difficulty between the two. A tentative extension of this conclusion is that subjects were not using the length of decision time for responses to displays as a measure of their difficulty. This hypothesis will be examined further and will receive some support in Experiment 7, where the difference in difficulty between easy and difficult displays will be greater.

Local bias effects

The next question considered was whether the difference in overall bias effects observed between the two groups was also to be found in the local effects that had been seen in earlier experiments. Sample size analyses were performed for each subject. The results are shown in Figure 5-1. It can clearly be seen that there were local bias effects, and that most of them were positive (sequences of difficult trials were followed by relatively slow responses). The analysis was repeated across all subjects and the results shown in Figure 5-1a. In this figure there appears to be a difference in local bias between the "fast" and "slow" groups. The "slow" group were much more affected by the local bias than the "fast" group and, within that group, RT's to difficult displays were more affected than RT's to easy displays.

We have seen that as subjects became more cautious, so the difference in RT to easy displays compared with difficult displays increased. At the same time it may be that subjects become more aware of the difficulty of the displays being judged. If so, they may have been able to judge both the difficulty of the display and the level of riskiness with which they made their decision. Criteria would not necessarily be changed simply because of the difficulty of the display, but because the subject thought that the criterial value being used was wrong for that difficulty level.

For example, say, a subject has made a response that took 1000 msec. If the subject recognised the display as an easy one, he may feel that he was too slow, and so speed up his subsequent responses. Conversely, if he recognised the display as a

difficult one, then he may feel that he was too fast, and slow down.

In this experiment the displays were readily discriminable - especially for the "slow" group - and so it provides a test for this hypothesis, as simple repetition of a difficulty level should not lead to a change in latency. The results show that subjects did take note of the difficulty in setting their criteria, and did not have separate standards for easy and difficult displays.

This conclusion, together with the absence of a correlation between the effect of bias on latency, and the effect of trial difficulty on latency, again suggests that subjects do not judge the difficulty of a display by the length of time it took them to make the discrimination. This point will be further discussed after Experiment.7.

Introducing the concept of slow guessing

Subjects have shown in these experiments that they can operate at varying levels of accuracy - with the accuracy being traded for speed. There would appear to be a continuum of such performances and it is felt that at the fast end of the scale there is probably a gradual increase in the number of guesses that the subject makes - as suggested by Ollman (1966). One main difference between the original suggestion of fast performance being obtained by a mixture of fast guesses and deliberated responses is that it is likely that when discriminations are difficult - as they are in these experiments - there may be a number of slow guesses as well. For instance, if a subject feels that he is taking too long over a decision he may precipitate the response before adequate information has arrived. This is rather similar to the suggestion put forward by Laming (1968 p 85) :

There must obviously be some limit to human ability to discriminate the signals used in these experiments. It seems reasonable that this should take the form of an effective limit to the time over which a subject can integrate information to reach any one decision. If a subject on any trial fails to collect sufficient information to satisfy any of the response criteria of Axiom 4 his best strategy is to make that response which has the greatest a posteriori probability.

If this were the case, longer duration decisions would not necessarily continue to show an improved performance after reaching a certain level, and in fact could decline. This would probably apply both to fast and deliberated decisions if the latter in fact have a time limit as was the case in these experiments.

This possibility could have implications for the estimation of how well different models of discrimination explain observed behaviour. Generally, models are used to predict behaviour in relatively "pure" conditions in which guessing is not considered to occur, except when either a fast guessing model is being tested or occasionally, as with Audley et al (1975) who use RT's adjusted for fast guessing. Most of the theories of discrimination predict a variable response time to any given set of criteria. If the subject guesses when the latency gets too long, it could be that corrections for "slow" guessing are also in order. The relationship of the data presented here to the theories outlined at the start of this report will be considered in the next chapter. What is intended now is an examination of the SAT functions of these last experiments in the light of this discussion.

In order to investigate the possibility of the data falling on SAT curves, the data from Experiments 3, 4 and 5 were partitioned into 100 msec intervals and the SAT functions calculated and drawn, see Figures 5-2, 5-3 and 5-4.

When the data from Experiments 3 and 4 are compared, the results are compatible with the suggestion that the effect of instructions is to move subjects along the SAT function. The SAT functions for both experiments and both biases appear similar. The difference lies in the shift of the frequency distribution. The probability of error at a particular RT band seems to remain constant, but as subjects become more careful so the proportion of faster responses reduces. As the plots were drawn in this case from averages across subjects, it was considered that there might be a similar function for the subjects of Experiment 2 who were presented with the same displays. The similarity is marked, in spite of the two sets of subjects being completely different. Here is support for Wickelgren's (op.cit.) concept of a descriptor of the data that is relatively unaffected by different subjective criteria, and agreement with Schouten and Bekker (op.cit.), who found that subjects differing in error rate and RT for stimuli have similar SAT functions. A SAT function thus allows a general description of the data, not an explanation of what is occurring. The relationship between SAT functions and theories of discrimination will be considered later.

If the upper ends of the curves for Experiments 3 and 4 are examined though, there appears to be a divergence. The curve for the experiment in which subjects were more careful continues upwards for a long time. If there is an asymptote it would seem to be around 1500 to 1700 msec. This occurs with both bias

conditions. It is possible to think of three explanations. First, there could be some "slow" guessing going on in the faster of the two experiments, this would lead to a reduction in correctness at long latencies. Second, it is possible to predict this behaviour with some discrimination models. For instance, Vickers' Accumulator Model, a Random Walk Model and an Recruitment Model all allow that for a particular level of criterion used the reaction times will vary without a commensurate change in error rate. Thus, if the subject has an upper limit of criterion that he uses, his accuracy will tend to asymptote at a certain level. If the criteria are then changed - allowed, for instance, to become less stringent - there will be a new asymptote. This will be discussed again in Chapter 6. Third, it is possible that the effect is due to a differential change of criterion shift by the individual subjects. This would occur as follows: If the subjects who have a lower error rate and are possibly slower on average to start with, slow down more than more error prone subjects, the longer latencies will contain a higher proportion of their responses and so will on average have a lower proportion of errors. This would lead to the observed increase at longer latencies. If it were the case, the faster end of the SAT should have suffered in the reverse manner as the correct responses were removed from it. In fact, the shapes of the two SAT functions are very similar at the lower end. Also, both SAT functions as stated match the functions for Experiment 2.

In order to investigate this further, SAT functions were calculated for each subject separately in the difficult bias of Experiments 3 and 4, see Figure 5-5. The data for easy trials

are very unreliable as most of the cells of the analysis had very few entries. This was because of the difficult bias. The difficult trials again appear to echo the finding of a divergence of the curves from the two experiments. Had the SAT for the second experiment merely been higher, rather than diverging, from the curve of the former, then it would have suggested that the difference was one of greater accuracy, possibly due to practice.

In a similar manner, the SAT functions for Experiment 5 were calculated for both sets of subjects and both biases. It is immediately apparent from the sharp rise of the SAT function and the high plateau that the experiment is a much easier one. We can still see a difference between the two sets of instructions - the SAT curves for the "slow" group are higher than those for the "fast" group. In this case there is no possibility that the difference was due to practice as two different groups of subjects were involved, one for each level of instruction. The two results together suggest that, in these cases at least, the SAT curves tend to have common origins but to diverge when subjects become really careful.

It was suggested earlier that the possibility of "slow" guessing should be considered. This idea receives some support when the individual data from the subjects SW and CM are examined. The SAT function for them for difficult trials in Experiment 3 tends to decline over time - indicating that they were making more errors for longer latencies. In the curve for SW it is also apparent during Experiment 4. It is not possible to estimate at this stage how many responses in these experiments would be affected by such guesses (if in fact they occur).

It is certainly possible that subjects were guessing, as the instructions indicated that subjects should guess rather than miss any responses. However, time pressure from the duration of the trial is not sufficient to explain such a strategy. First, the trends observed in Experiment 3 occurred at latencies of 1500 msec or faster, whereas the displays remained visible for 3500 msec. Second, such time pressure would be much greater in Experiment 4, where the timing of trials was the same as in Experiment 3, but subjects were taking much longer to make their decisions. If time pressure alone were causing subjects to "slow" guess then it should have been more evident in Experiment 4, but was not.

It is probable that when subjects were trying to respond consistently, they were aware of the rhythm at which they were responding, and thus also aware when decisions were taking longer than average. When this happened they might guess. Such an hypothesis might predict that "slow" guessing would be more likely to occur when subjects were responding fairly quickly and might be reluctant to allow RT's to be long.

It is difficult to know how one could tease out the effect with displays as difficult to discriminate as the ones used in this experiment. Subjects often commented at the end of sessions that they considered that they were guessing for much of the time, and they were often surprised to find out how well they had performed. At such times it would be very difficult for them to identify during a run which responses were really guesses. One problem is of course to define what is actually a guess, as there could be considered to be degrees of guessing. If a subject

decided that he was taking too long over a decision, he could either make a pure guess or else make the decision on the evidence that he already had available in, say, partially filled accumulators. This would effectively be the same as altering his criterion during the run, as suggested by Pike (1968). Thus the decisions would be based on evidence of variable reliability, depending on the time that had been spent sampling and the relative rate of acquisition of the relevant information. At this stage the concepts of confidence and response bias could be considered. If the evidence that had been accumulated was not particularly conclusive, then a subject might be more influenced by any response bias that was present. In trials where the accumulated evidence was more conclusive, the subject might be more confident and there would probably be a reduction of the effect of response bias.

This would lead to the consideration that response bias could play a part in the responses at very fast response times when the subject was "fast" guessing and also at longer response times when he was "slow" guessing. There should then be a relatively bias-free area between these two extremes. The presence of response bias should show as excessive repetitions or alternations. It would be necessary to examine repetition and alternation errors at different points along the SAT function, to see if either error was more prevalent at short and long latencies than at medium latencies. On the other hand, if subjects were using the available evidence in making their decisions, there should either be no response bias, or else, if there were any, it should be similar for different parts of the curve. This analysis was not available at the time of writing this report therefore the hypothesis remains untested.

If a subject were to "slow guess" then the guess could be initiated by some internal time reference or clock. In such a case more difficult trials would be guessed than easy ones, as the difficult trials would more often exceed the time limit. It has already been stated that guessing will affect easy trials more than difficult ones, and so, it is hard to make any real predictions as to exactly what would occur with the tradeoff of these two factors which lead to opposite effects. If, however, there were a time limit, the trials that occurred beyond that limit would be equally likely to be guessed and so the SAT curve would take on a symmetrical appearance with, once again, easy trials suffering more than difficult ones towards the upper end. The subject, though, may only guess on a proportion of these longer trials and so the effect would not necessarily be as great as at the lower end of the curve. When the SAT curves for Experiments 3, 4 and 5 for individual subjects are examined, although there is evidence for the deterioration of performance for some subjects, there is no real evidence that the easy and difficult curves are converging at their upper ends. At first sight then there does not appear to be evidence that, if there is "slow" guessing, it mimics the "fast" guessing in the way predicted.

Conclusion

So far the experiments in this chapter have shown how instructions to be fast disrupt local and overall difficulty-bias effects. The disappearance of the bias effect in Experiment 4 can probably be considered to be due, in part at least, to the poor experimental design, as the replication of such instructions in this experiment have failed to remove the effects of either overall or local bias.

The theories mentioned at the start of this report predict that as criteria become more risky so the latencies of easy and difficult trials will become more uniform. A relationship was found between bias and the difference in latency between easy and difficult trials in Experiment 3, and there might have been a hint of this in the correlation mentioned earlier for the "fast" group ($r = 0.42$). It was considered that there could be a continuum along which the effect of bias operated. The following relationship was considered: At very fast response times the subject will be less aware of the difference between easy and difficult trials so there is very little chance of his adjusting his performance with regard to difficulty. As he becomes more cautious so he becomes more aware of differences in difficulty and so can regulate his criteria more meaningfully. The evidence for this so far has come from data averaged across different subjects. It was decided to see if the transition between little or no overall bias effect for subjects who were responding quickly, and some effect for more cautious subjects could be replicated within subjects.

The next experiment studies subjects who are performing very quickly compared with the same subjects performing at two different levels of caution. This should give three different conditions in which the interaction between local bias and instructions can be compared.

EXPERIMENT 6

METHOD

Subjects:

There were just two subjects: the author (SF) aged 34 and another male (PR) aged 43. Both were very experienced in reaction time experiments, and the author was of course experienced in these particular experiments.

Apparatus - The subject sat in a very dimly lit room facing a Tectronix 'scope with P11 phosphor at a distance of approximately 400 mms. Their hands rested on keys that were to be pressed as responses. The room was rather dimmer than in the other experiments.

Stimuli - These were pairs of parallel vertical lines presented centrally on the CRT. One line was always longer than the other and could occur with equal probability to the left or right. The lines differed at their upper end as in Experiments 1 and 5. The shorter line was always made up of 120 points (18.5 mm) and the longer line was either of 121 points (18.65 mm) or 123 points (18.95 mm). The timing of the trials was exactly as in the standard setup as described in Experiment 2.

Instructions - After being shown the apparatus, the subject was told: "Please respond with the hand on the side of the longer line. I want you to establish three levels of performance - fast, normal and slow. At the start of each run which will be of 200 trials, I shall tell you at which speed I wish you to go for that run."

Procedure - The subjects each attended for six sessions. During each session they were presented with six runs. Each run was made up of eight initial trials which were ignored in the later analysis, and 200 trials in which each of the four types of trial were equiprobable. At the start of each run the subject was instructed at which speed to respond. The order of these instructions was counterbalanced as shown in Table 5-8. Subject SF also performed extra runs at a more cautious level in pairs of runs together after runs 4, 5 and 6 and at three other occasions during the days of these later sessions.

Results:

The means and correct scores can be seen in Table 5-9. It can be seen that the RT's for both subjects when asked to be fast were essentially the same for both easy and difficult trials. At the slower speeds, the difference between the two increases.

The main purpose of this experiment was to examine how, within subjects, the local effects can vary with the instructions that were given. Accordingly, sample size analyses were done of the two subjects for each of the instruction groups. These are shown in Figure 5-6 for sample sizes of 2, 4 and 6.

For subject PR the two curves for easy and difficult trials are essentially identical, both having a very slight positive slope for fast instructions. The "normal" curves are also similar, but the two are now separated by about 50 msec and showing possibly a slight tendency to separate. The two "slow" curves show more signs of separation with the easy trial curve appearing to drop and the difficult one appearing to rise. These would indicate that easy trials were faster in the "slow" condition when preceded by several easy trials, whereas difficult trials were slower after preceding easy trials.

The picture for SF is similar except that both the medium and fast curves are similar to each other. The "medium" curve is in fact rather similar to PR's "fast" curve and the "slow" curve rather like PR's "medium" one. It was for this reason that SF was given another set of runs in which he was much more careful. It must be remembered that these were not given in balanced order as were the other runs and so cannot be directly compared with them. They are in fact rather slower than the "medium" runs but not really any more accurate. They do continue the trend towards the separation of the two curves for easy and difficult trials, so both subjects are showing the same trend in which easy trials are slightly negatively influenced by trial difficulty in the faster runs, but positively influenced when the subject becomes more cautious.

Discussion:

The tendency towards the separation of the two levels of difficulty as seen in this experiment as subjects' criteria become more stringent could well be the same as the case observed in Experiment 1. Then subjects were all performing at a moderate level of caution and the same effect was observed.

This is compatible with a suggestion made earlier, that as subjects become more careful so they are more able to discriminate between the two levels of difficulty. If this were the case it is feasible to think of the subject using two criteria - one for each difficulty of trial. This is obviously only possible if the subject can discriminate easily between them. It would seem therefore that both subjects in their most cautious runs could be

considered to be using two criteria. If the local bias effects are examined in Figure 5 - it will be seen that the curves for easy and difficult trials diverge - the easy trials getting faster and difficult trials getting slower after increasing numbers of easy trials. This can also be stated as trials of a particular level of difficulty getting faster after previous trials of the same level of difficulty. If the subject were only using one set of criteria, as described in the models outlined at the start, it is only possible to make one prediction from them; that is that any movement of the one set of criteria will have the same direction of effect on RT's to both easy and difficult displays.

Subjects may be able to identify the ease or difficulty of the trial independently of their knowledge of the decision time, in which case they might be able to prepare themselves to respond at different expected times. This decision could occur at a relatively simple level, such as deciding whether the difficulty of the current display was the same or different from the previous one. Whenever the difficulty changed the subject would automatically become more cautious. A change of difficulty after a long run of displays of one level of difficulty might be easier to detect than after only a short run and so lead to a greater change in caution.

It will be shown in the next chapter that the latencies and error rates by these two subjects - especially PR - can be fitted very well using the assumption of only one criterion. This adds weight to the consideration of an explanation involving only one set of criteria, and is difficult to reconcile with the first suggestion above, of subjects using two sets of criteria. The question is left open for the moment, but certainly some change in strategy has been observed as subjects become careful.

The fact that the slopes of the sample size curves for RT's of easy and difficult displays are very close for the "fast" runs and also show a relatively small effect of previous difficulty could be explained by several strategies.

Possible strategies

1. First, it is possible that the subjects deliberately do not adjust their criteria as this would slow them up and they would fail to do as instructed. They may be operating on a time-based rather than a criterion-based system. This is borne out by the identity of the latencies for both easy and difficult trials.
2. It could be that subjects take only one sample of the data on which to base their guess. If this were the case then, as stated elsewhere, the responses at chance level cannot be accounted for. If the subject did take only one look at the data then he should always do better than chance.
3. It is therefore possible that the subject guesses on a proportion of trials. How does he choose which trials to guess on?
 - a. He may have - as in Cartwright and Festinger's model - a central no-decision area. If the one observation that is made falls in this area, then instead of taking another observation, the subject may guess. In this event, however, the trials on which the subject guessed would be longer and more error prone. This does not match the evident increase in correctness as latencies increase - especially in the "fast" condition. Also, the trials that would be more

likely to lead to guessing would be the difficult trials, and so difficult trials would be more affected by guessing; by this it is implied that as subjects became faster more errors would be made on the difficult trials than easy ones. Again this is not the case, so it can be assumed that this is not the subjects' strategy.

- b. The other possibility, if subjects are guessing, is that they decide in advance which trials they will guess on and perform fast guesses. If this were the case, then an equal proportion of easy and difficult trials would be guessed. Since easy trials are less error prone than difficult trials this should lead to a greater deterioration in easy trial performance than difficult. For instance, if the average error rate for easy trials is 10%, then guessing half the time will increase this to 30%; if the average error rate for difficult trials is 30%, then guessing half the time will lead to an error rate of 40%. There are two separate facts that support this.

- i. The SAT curves for easy and difficult trials meet as latencies get faster - as RT's decrease, so the error rate for easy displays increases faster than the error rate for difficult displays.
- ii. The error rates of easy and difficult trials for the "fast" runs are more similar than those for the other runs, see Table 5-8. This greater similarity in error rate would occur if subjects were guessing more in the "fast" runs than in other runs.

There is therefore strong support for the subject guessing part of the time, especially when responding quickly.

"Slow" guessing

It was conjectured earlier that subjects may have been "slow" guessing in earlier experiments. It is proposed that this would be very likely in a "fast " run if one of the more deliberate discriminations was taking longer than required. There does not appear to be any real support for this idea in the SAT for the fast runs, though the SAT functions for the slower runs of both subjects do show some signs of an increasing error rate as reaction time increases. It would seem therefore that subjects are using both fast guessing and slow guessing to maintain their speed of responding unless the fall off in accuracy at longer intervals can be explained by any of the theories. The Runs Model, Random Walk and Vickers' Accumulator Models all predict that as latencies at one particular criterion setting increase, so accuracy will remain fairly steady. The Recruitment Model, however, does predict a falling off of accuracy as latency increases (see next Chapter) so it is possible that, either the subjects are "slow" guessing, or they are using a recruitment style of decision making. A more detailed study of the predictions of the various models will be made in the next chapter.

Conclusion

These experiments have shown that it is possible to think of instructions for speed or accuracy moving a subject along a continuum of performance. At the fast end the difference between easy and difficult trial latencies is small or absent altogether, though there is still some difference in error rate. At that level the local difficulty-bias effects are also minimal or non-existent.

As the subject becomes more cautious, so the local effect becomes apparent and then changes as, with the subjects being fairly careful there is a separation of the two curves. These facts suggest that at the faster speed of responding, either the subjects are unaware of the difference in difficulty of trials - which would imply that their judgement of the difficulty of the trial was related to the time of the decision process in some way - or that they are aware, but do not make use of the information. This would happen for instance if the subject was deliberately ignoring the difficulty of the trials and using an internal clock for making his decisions instead of some mechanism as described in the various models discussed earlier.

The effect of difficulty-bias on latencies was found to change by rather a small amount when compared with the difference in latencies for easy and difficult trials, which implies that the effect of difficulty-bias is relatively independent of decision time, and may in fact be more dependent on the actual difference between the two stimuli. This possibility will be examined in Experiment 7.

- Session -						
subject	1	2	3	4	5	6
CM	D	E	D	E	D	D
SW	E	D	E	D	E	E
JW	D	E	E	D	D	E
MA	D	D	E	E	D	D
FC	E	D	D	E	-	-
TC	E	E	D	D	E	E
experiment	3			4		

Key: D = a session in which the first 3 runs were all of difficult bias and the last 3 runs of easy bias.

E = a session in which easy bias runs preceded difficult bias runs.

The order in which the bias was presented in Experiments 3 and 4.

Table 5-1.

	EASY TRIAL			DIFFICULT TRIAL		
bias:	easy	difficult		easy	difficult	
CM	945	942	(-3)	1009	1010	(-2)
SW	741	685	(-56)	839	792	(-48)
JW	1752	1794	(+42)	1977	1954	(-23)
MA	669	669	(0)	655	672	(+17)
TC	881	1181	(+300)	997	1215	(+278)
Mean:	998	1054		1095	1129	
CM	88.5	87.4		68.9	64.0	
SW	91.1	93.0		66.0	66.7	
JW	99.8	99.7		80.9	81.4	
MA	76.4	74.0		57.1	62.0	
TC	91.3	88.7		67.9	66.3	
Mean:	89.4	88.6		68.2	68.1	

RT (msecs) and percent correct for both biases in Experiment 4. Figures in brackets are the effect of bias on reaction time.

Table 5 - 2.

	Session		
"Fast"	1	2	3
RB	D	E	D
MM	E	D	E
SJ	E	E	D
AK	D	D	E
JD	E	D	E

"Slow"			
JL	D	E	D
TJ	D	E	D
JG	E	D	E
NS	E	D	E
PA	D	E	D

For each session the letter gives the bias for the first three runs of the session. The second half of the session was of the other bias.

E stands for easy bias; D for Difficult bias.

Table 5 - 3

Experiment 5.

	EASY TRIALS			DIFFICULT TRIALS		
bias:	easy	difficult		easy	difficult	
"FAST"						
RB	570	514	(-56)**	663	543	(-120)**
MM	504	539	(+35)*	567	582	(+15)
SJ	591	590	(-1)	731	729	(-2)
AK	538	600	(+62)**	661	718	(+57)*
JD	549	536	(-13)	572	573	(+1)
Mean:	550	556	(+6)	639	629	(-10)
"SLOW"						
JL	632	655	(+23)	1003	992	(-11)
TJ	1050	1231	(+181)**	1473	1613	(+140)**
JG	858	895	(+37)	1532	1560	(+28)
WS	606	637	(+31)	700	728	(+28)
PA	518	561	(+43)**	664	723	(+59)*
Mean:	733	796	(+63)**	1074	1123	(+49)*
"FAST"						
RB	88.3	89.9	(+1.6)	67.8	65.6	(-2.2)
MM	91.6	92.8	(+1.2)	67.6	72.1	(4.5)
SJ	94.9	94.1	(-0.8)	72.1	72.9	(+0.8)
AK	97.0	96.0	(-1.0)	75.2	77.2	(+2.0)
JD	83.2	66.7	(-16.5)**	64.3	60.5	(-3.8)
Mean:	91.0	87.9	(-3.1)	69.4	69.7	(+0.3)
"SLOW"						
JL	100	100	(0)	90.2	90.6	(+0.4)
TJ	99.4	95.9	(-3.5)	82.3	78.1	(-4.2)
JG	100	100	(0)	87.8	89.1	(+1.3)
WS	95.1	94.2	(-0.9)	68.7	75.3	(+6.6)
PA	98.7	99.5	(+0.8)	85.4	83.8	(-1.6)
Mean:	98.6	97.9	(-0.7)	82.9	83.4	(+0.5)

Mean latencies (msecs) and percent correct under both conditions of bias in Experiment 5. Figures in brackets are the effect of the bias on latency or error rate.

Difference signif. at: * 5%; ** 1%

Table 5-4.

	Effect of bias on trials(X)	Effect of trial difficulty within bias(Y)		
CM	-3	64	Sum X	505
	-2	68	Sum X ²	175319
SW	-56	98	Sum Y	861
	-48	107	Sum Y ²	120815
JW	+42	225	Sum XY	39121
	-23	160		
MA	0	-14	r = -0.052 (not sig)	
	+17	3		
TC	+300	116		
	+278	34		

Values used for calculation of product moment correlation between the effect of trial difficulty and that of bias on latencies for Experiment 4.

The values in the table are the difference in latency between either easy and difficult trials within each bias, or the difference in latency between easy and difficult bias for easy or difficult trials.

Thus the value -3 (under X) means that, for easy trials, subject CM, difficult bias was on average 3 msec faster than easy bias. The corresponding value (Y) of 64 means that under easy bias, same subject, difficult trials were on average 64 msec slower than easy trials. The second line refers to the effects on difficult trials and difficult bias.

Table 5-5

	Effect of trial difficulty on bias (X)	Effect of bias on trials (Y)		
"FAST"				
RB	93	-56	Sum X	808
	29	-120	Sum X ²	85180
MM	63	35	Sum Y	-22
	43	15	Sum Y ²	26254
SJ	140	-1	Sum XY	7834
	139	-2		
AK	123	62	r = 0.42 (not sig)	
	118	57		
JD	23	-13		
	37	1		
"SLOW"				
JL	371	23	Sum X	3346
	337	-11	Sum X ²	1537444
TJ	423	181	Sum Y	559
	384	140	Sum Y ²	62239
JG	674	37	Sum XY	199977
	664	28		
WS	94	31	r = 0.1137 (not sig)	
	91	28		
PA	146	43		
	162	59		

Values used for the product moment coefficient correlation calculation between the effect of trial difficulty and that of bias on latencies for Experiment 5.

(see table 5-5 for further details)

Table 5-6

	EASY	DIFFICULT - EASY		
"FAST"	(X)	(Y)		
RB	570	93	Sum X	5531
	514	29	Sum X ²	3069155
MM	504	63	Sum Y	808
	539	43	Sum Y ²	85180
SJ	591	140	Sum XY	457028
	590	139		
AK	538	123	r = 0.719	
	600	118		
JD	549	23	t = 2.928	(df 8)
	536	37		

"SLOW"				
JL	632	371	Sum X	7643
	655	337	Sum X ²	6339549
TJ	1050	423	Sum Y	3346
	1231	384	Sum Y ²	1537444
JG	858	674	Sum XY	2826074
	895	664		
WS	606	94	r = 0.589	
	637	91		
PA	518	146	t = 2.06	(df 8)
	561	162		

Correlation calculation between mean latency of Easy trials and the difference between easy and difficult latencies in Experiment 5.

Table 5-7.

subject:		PR	SF
SESSION :	1	F M S S M F	M F S S F M
	2	M S F F S M	S M F F M S
	3	S F M M F S	F S M M S F
	4	S M F F M S	F M S S M F
	5	M F S S F M	S F M M F S
	6	F S M M S F	M S F F S M

Order of instructions to go Fast (F)
Medium (M) or Slow (S) for the two
subjects in Experiment 6.

Table 5-8.

subject	EASY TRIALS		DIFFICULT TRIALS	
	$\overline{\text{RT}}$	%	$\overline{\text{RT}}$	%
PR 'FAST'	420	69.4	418	63.2
'MEDIUM'	511	86.7	565	66.4
'SLOW'	634	92.8	735	72.4
SF				
'FAST'	384	74.3	393	59.7
'MEDIUM'	468	90.3	479	68.8
'SLOW'	596	97.7	629	78.3
'V.SLOW'	744	98.4	897	77.8

Latencies in msec and percent correct scores averaged across all sessions for both subjects in Experiment 6.

Table 5 - 9

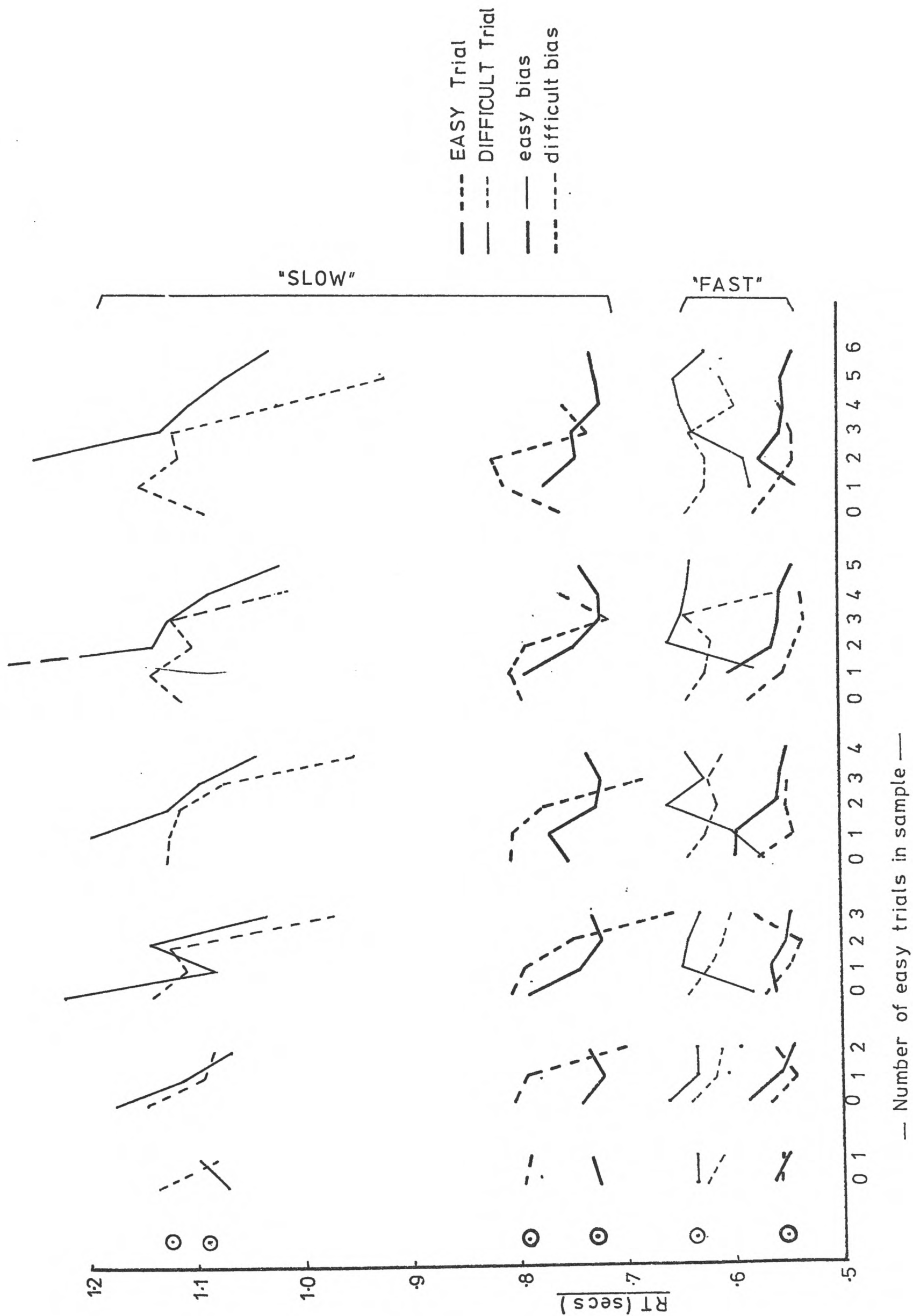
	EASY TRIALS			DIFFICULT TRIALS		
bias:-	easy	cf	difficult	easy	cf	difficult
"SLOW"						
JL	—	—	//	//	—	—
TJ	\	//	—	//	//	—
JG	\	—	//	—	—	/
WS	—	/	/	—	/	//
PA	—	//	/	—	//	/
"FAST"						
RB	//	//	/	—	//	//
MM	//	//	—	—	—	/
SJ	//	—	—	//	—	//
AK	//	//	//	—	//	—
JD	\	—	/	\	—	\

An attempt to illustrate the varying effects of bias in Experiment 5. The columns under 'easy' and 'difficult' show the effect within easy or difficult biases, and the column between shows the effect between biases.

Key:

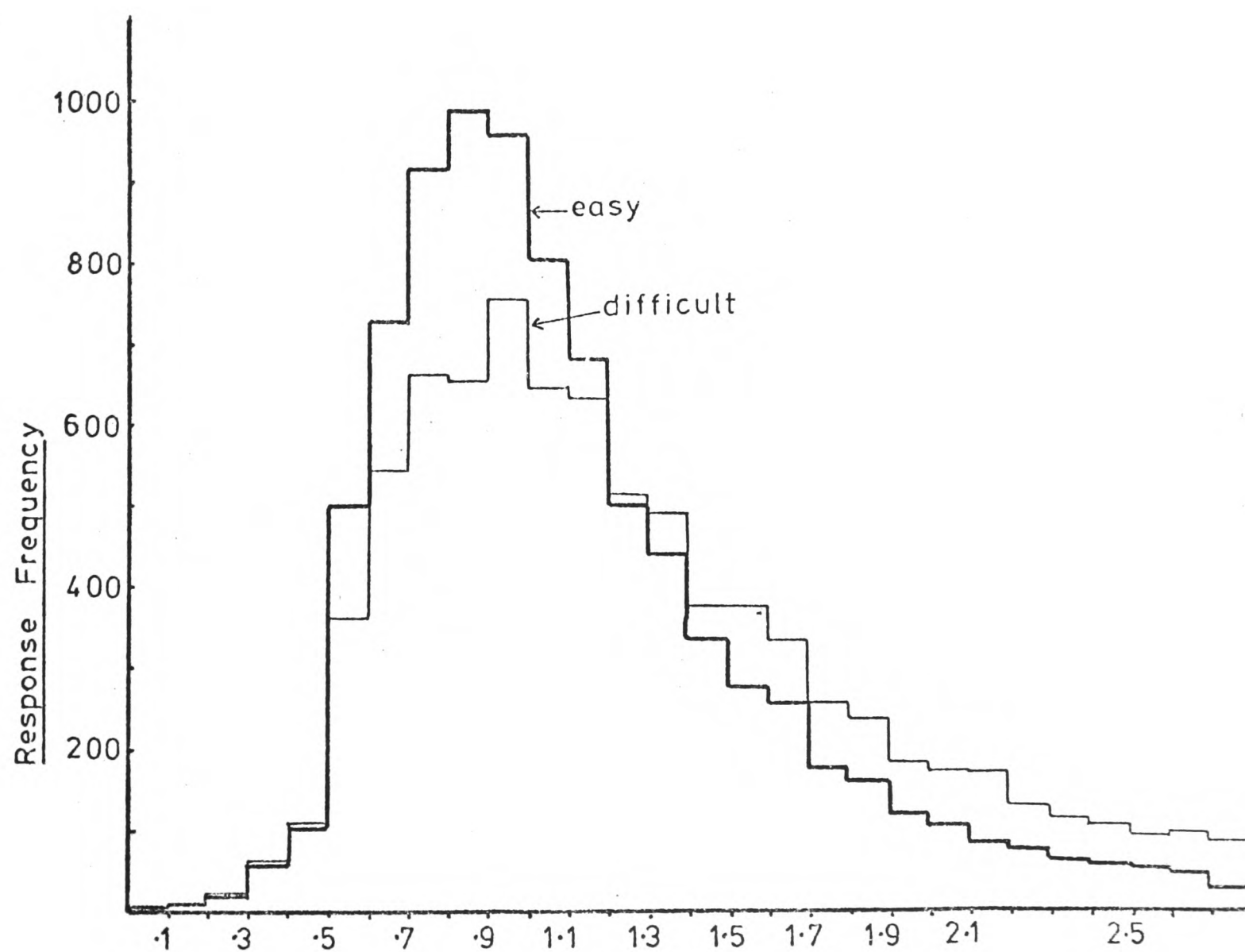
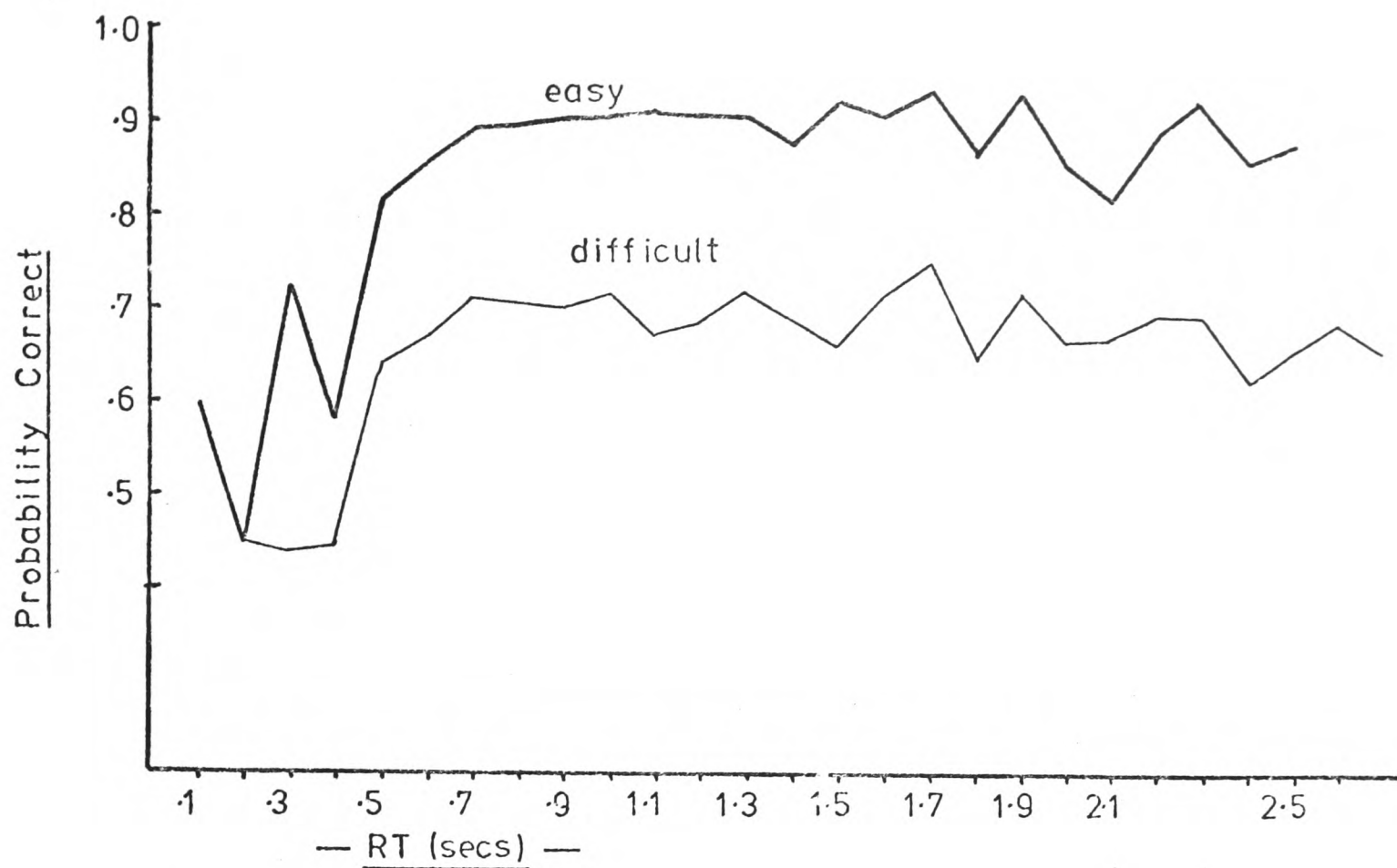
- / +ve effect:- Difficult trials/bias slow down RT's
- \ -ve effect.
- / \ (one line) t value of difference or slope: $1 < t < 1.5$
- // \ (two lines) $1.5 \leq t < 2.0$
- // // (three lines) $2.0 < t$

Figure 5-1

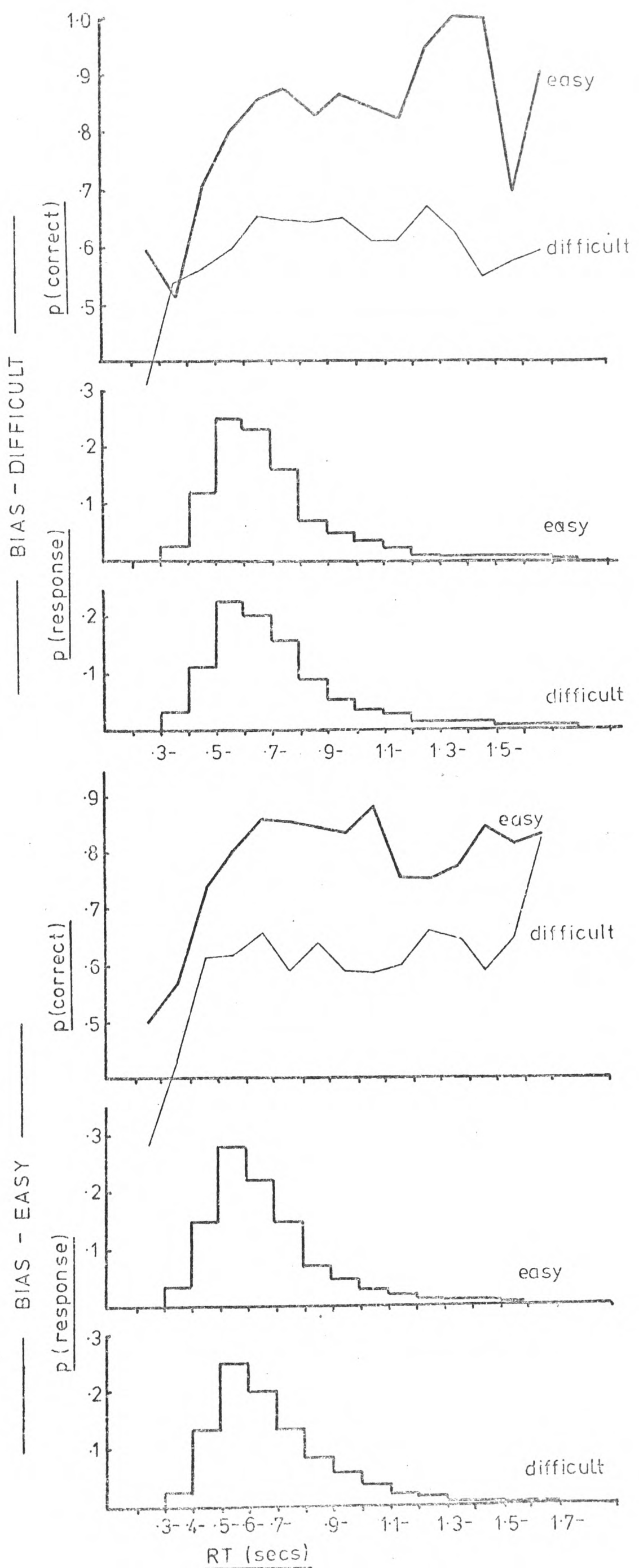


Sample Size Analyses for both "FAST" and "SLOW" groups in Experiment 5, averaged across subjects.

Figure 5 - 1a.

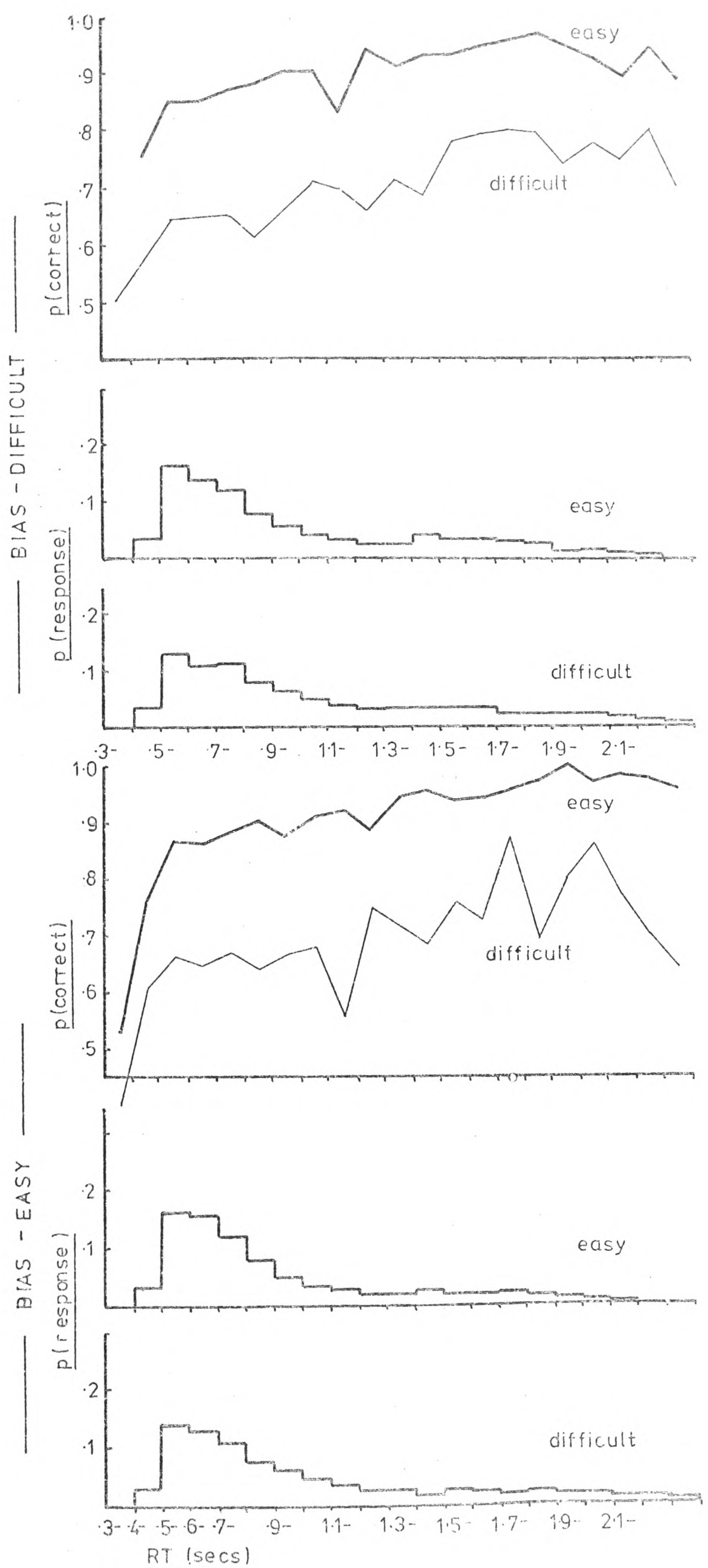


SAT & Histogram Experiment 2



SAT Plots and Histograms of Response Probabilities. Experiment 3

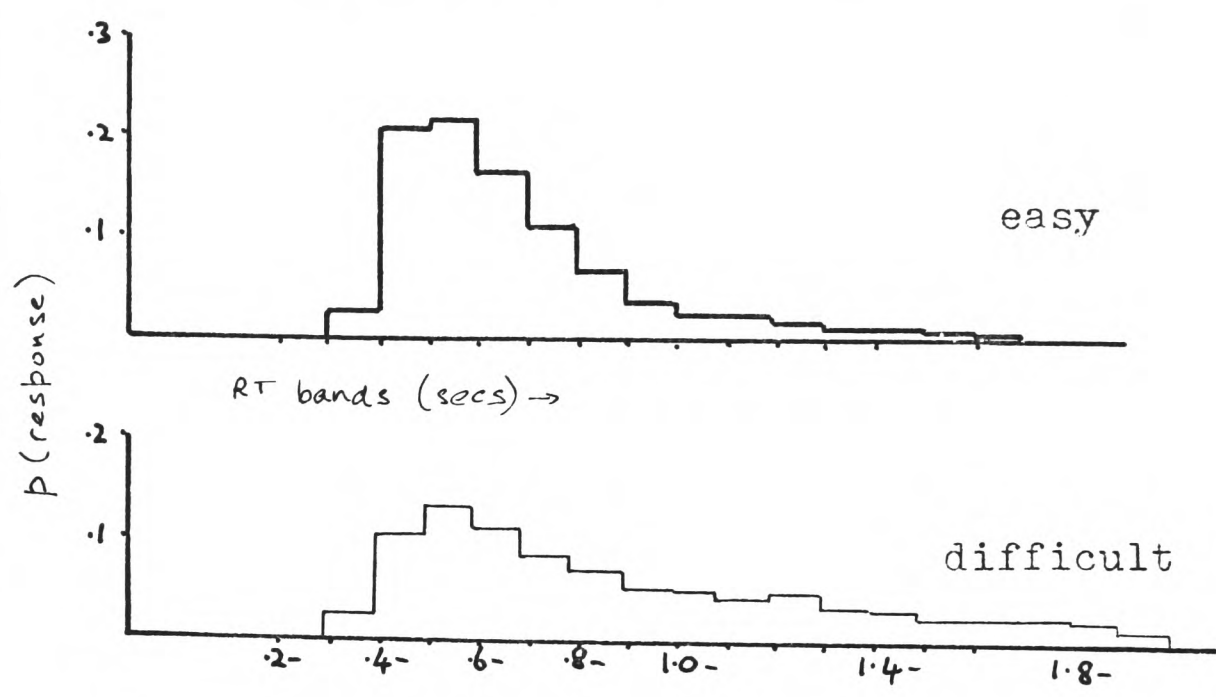
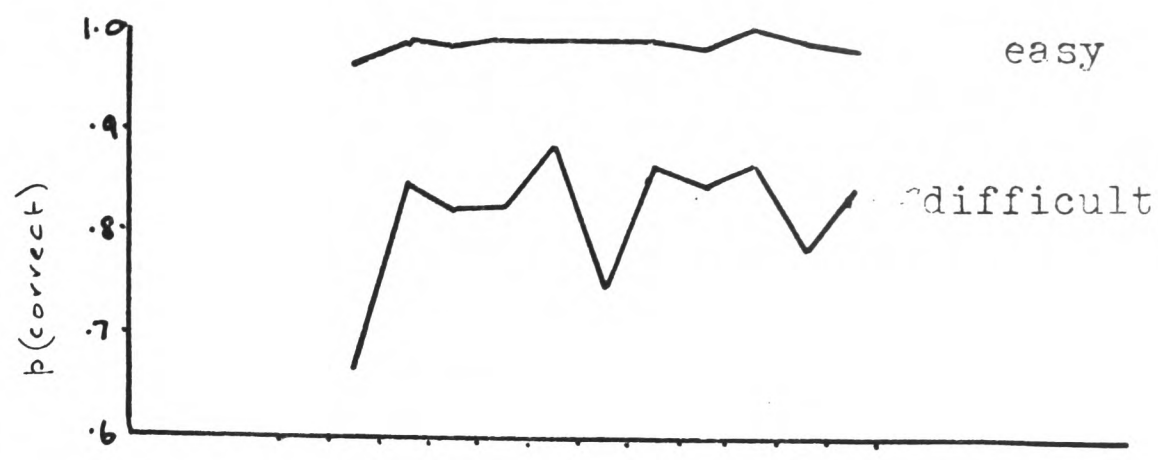
Figure 5-2



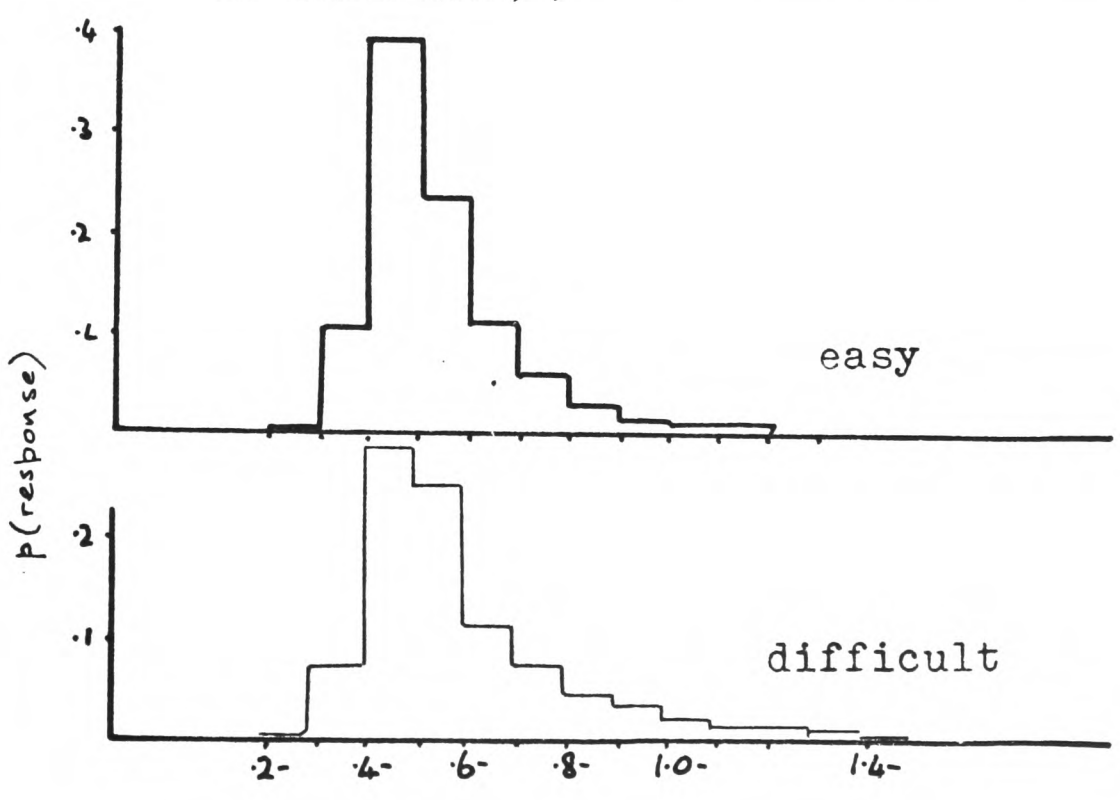
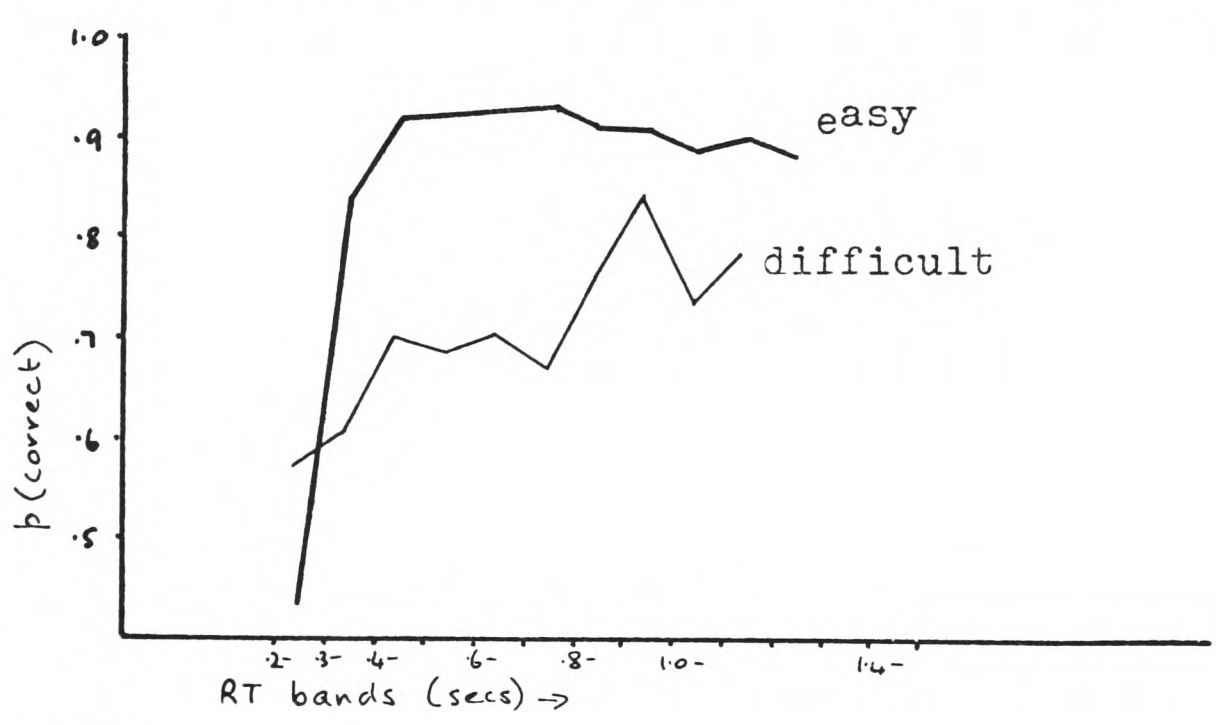
SAT Plots and Histograms of Response Probabilities. Experiment 4

Figure 5-3

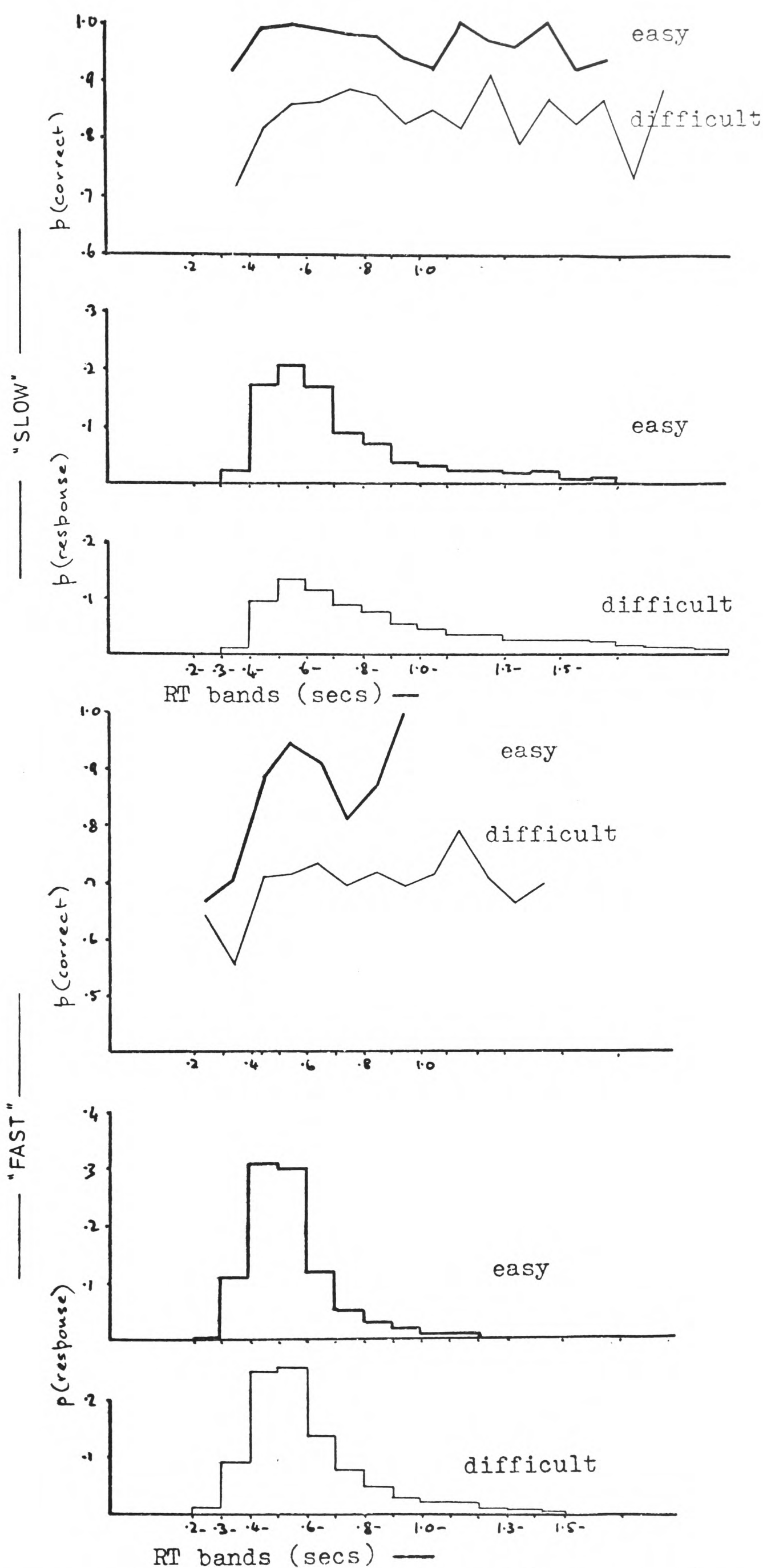
"SLOW"



"FAST"

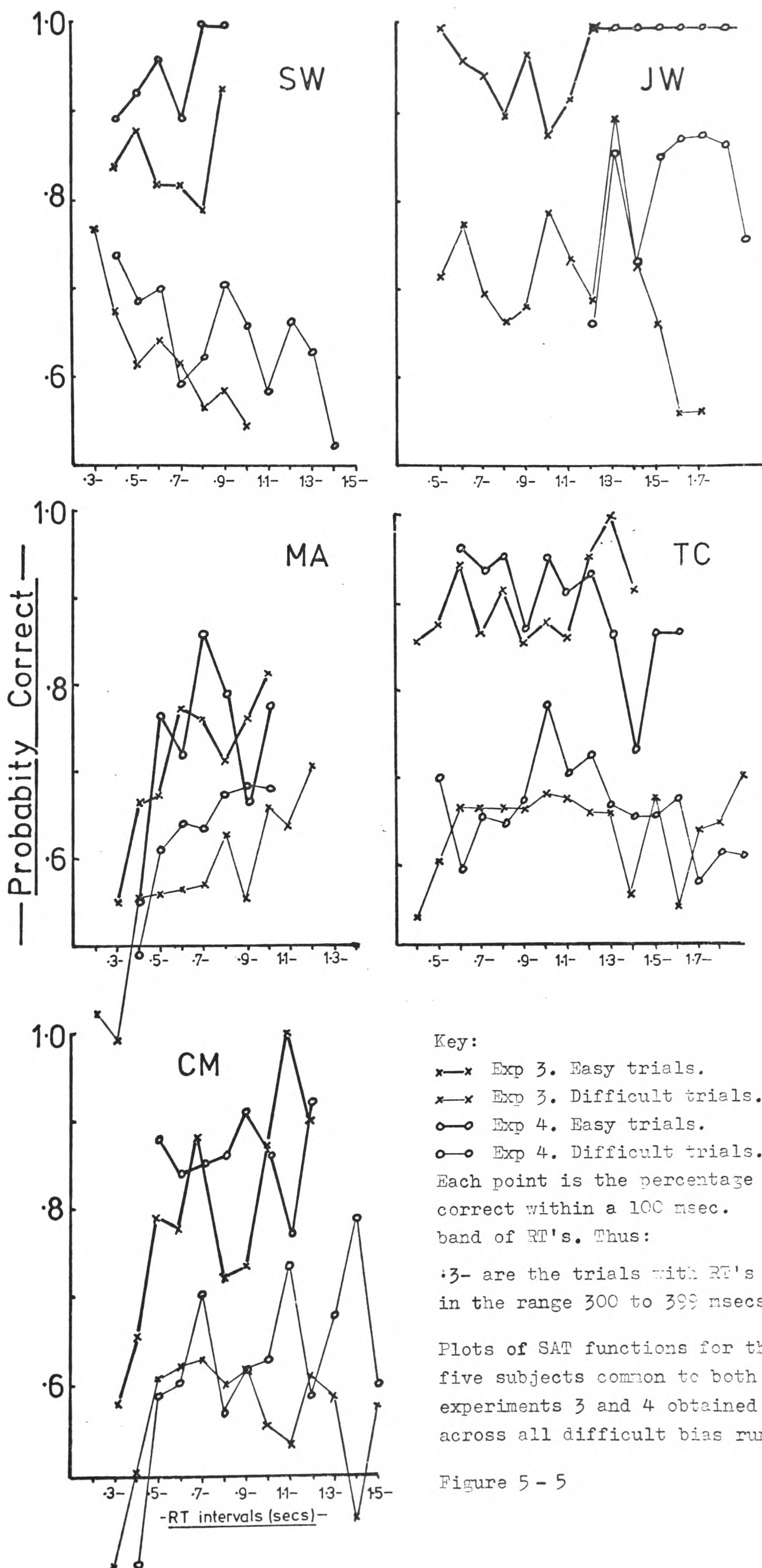


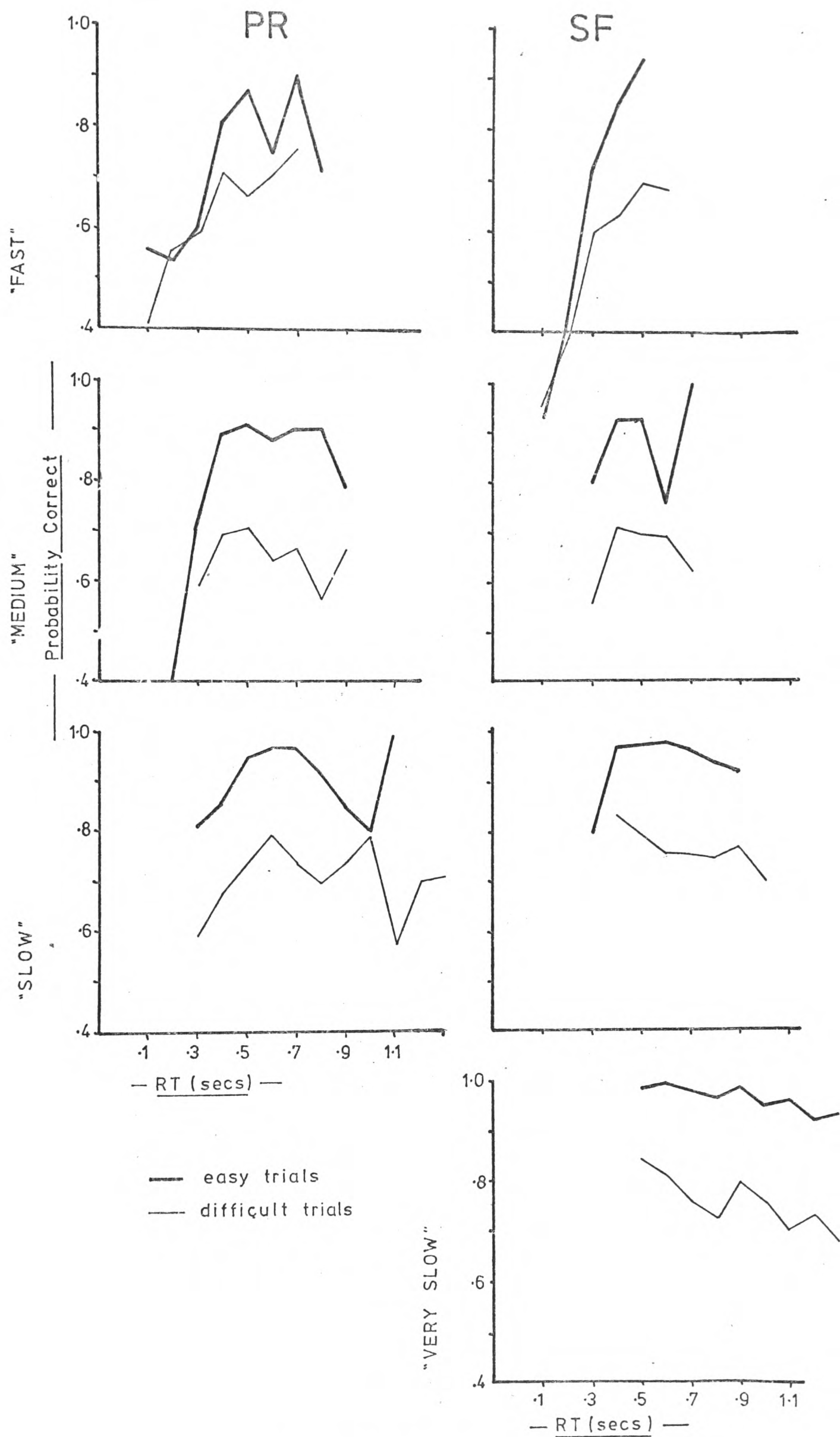
Experiment 5. SAT Plots + Histograms of Response Prob.
Figure 5-4a EASY Bias.



Experiment 5. SAT Plots + Histograms of $p(\text{correct})$.

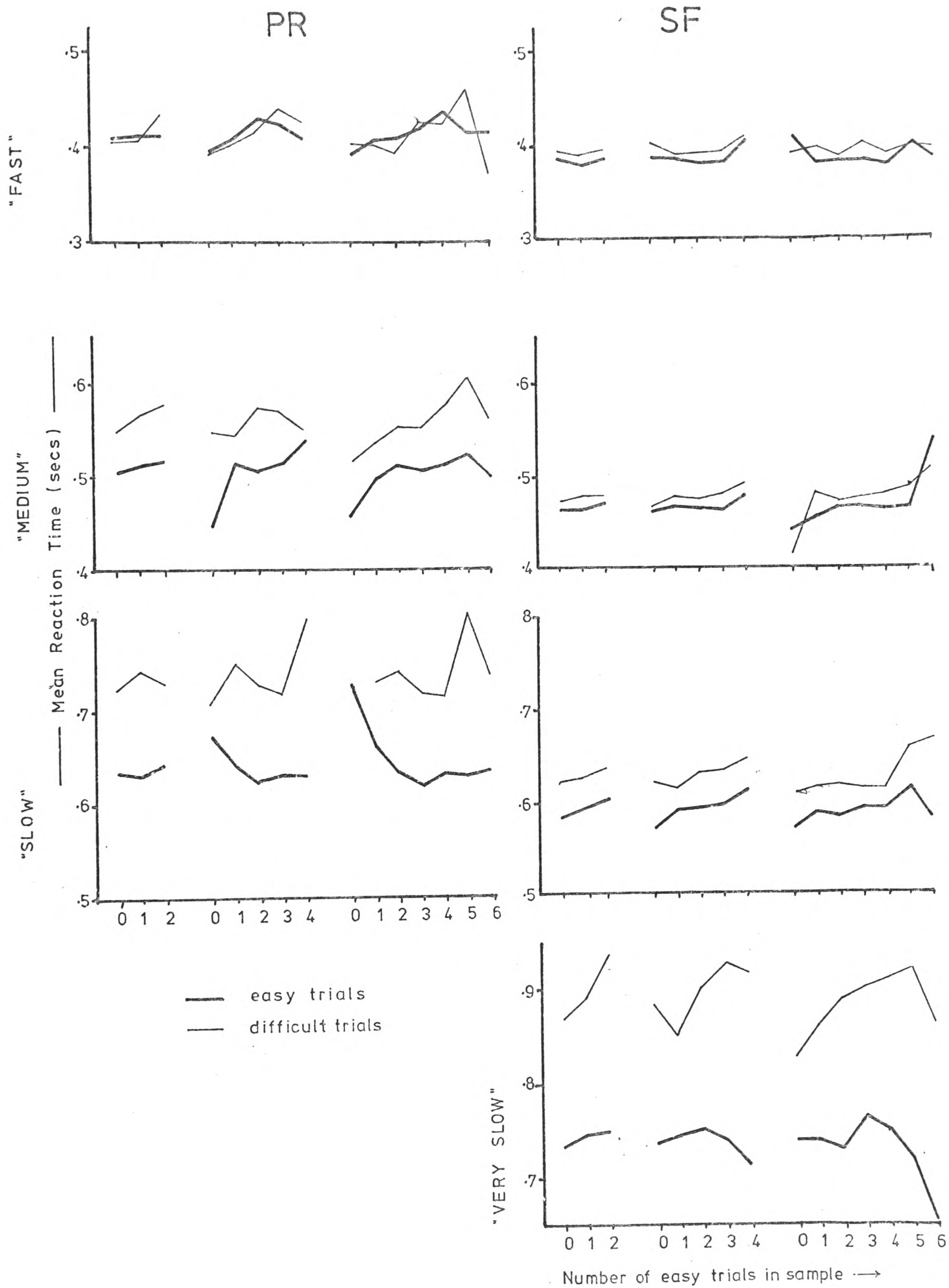
Figure 5-4b DIFFICULT Bias.





SAT functions for both subjects
Experiment 6.

Figure 5-6.



Sample Size Analysis - window sizes 2, 4 & 6

Experiment 6.

Figure 5-7.

CHAPTER 6

HOW WELL DO DISCRIMINATION MODELS DESCRIBE EMPIRICAL DATA?

At the start of this report, a number of discrimination model processes were outlined. An attempt will now be made to examine how these models relate to the data in the experiments we have discussed. It is clear that the varying instructions used caused great changes in subjects' performances. It was assumed that the instructions brought about movements of the subjects' criteria. If this was the case, it should be possible to examine the data in the light of the models put forward in the literature and see if they can account for the changes actually observed.

None of the models considered here actually predicts how far criteria are likely to move under various conditions - nor should it be necessary for them to do so, as subjects will interpret conditions differently. It should be possible however for them to predict how changes in criteria will affect tasks of differing difficulty. In this particular case, if the instructions, say, to be cautious, cause subjects to move their criteria, but otherwise not to change their strategy, (for instance, from using one criterion to using two criteria), then the effect on RT and error rates for both easy and difficult trials should be related. We shall examine the theories to see which, if any, can predict these various relationships.

At this time it is not proposed to prove or disprove any of the theories - it is quite possible that several may be appropriate, each in its own context. What is intended is that if any of the theories can indeed demonstrate a link between RT

and error rate in these experiments, there would be some reasonable grounds to consider instructions as affecting the latencies and error rates through their influence on criteria, rather than by some change in strategy.

By a change in strategy the writer is thinking of examples such as changing from using one criterion to using two; of starting to guess on some of the trials; or of concentrating on, say, the easy trials to the detriment of the difficult ones. All these changes would lead to alterations in error rate and latency that would not necessarily be predictable by the models outlined in the introduction.

Initially it is intended to examine each model and see how it can relate the data of Experiments 3 and 4. In Experiment 3 it will be remembered subjects were given fairly conventional speed-accuracy instructions, but in Experiment 4 the same subjects were given instructions to be much more cautious. A brief summary of the latencies and correct percentages is given in Table 6-1. The formulae for calculating the tables of values for the Runs, Recruitment and Random Walk Models are given in Table 6-7.

Several points should be noted. First, it is being assumed that the same criterion is being used for both easy and difficult discriminations. Second, it is assumed that the subjects do not change their strategy between the two experiments. Third, it is assumed that the overheads to the overall reaction process, such as response selection and the motor response itself, will remain fairly constant. Thus, the extra time taken to make more cautious decisions should be predictable by the models.

The Random Walk Model

The standard Random Walk Model predicts that for a fixed distance of the absorbing barriers, the RT's for errors and correct responses will be the same. We have already seen that this was a difficulty for the theory when we discussed errors. Conversely, the probability of success at a given time is constant. That is, for a given value of k (the distance of the absorbing barrier), and given that the rate of data acquisition is constant, then the probability of reaching one barrier A rather than the other barrier B after n units of time is the same as the probability after $n+1$ units of time. This in turn implies that the Speed-Accuracy tradeoff functions is rectangular, and that there is no increase in accuracy for longer latency decisions for a particular value of k . An increase in k will lead to both longer latencies and fewer errors. By mixing sequences in which k varied it is possible to obtain a SAT function, as lower values of k will lead to shorter latencies with higher probabilities of error. Thus, if the random walk style of model is to explain the variations observed in much of the data, it is the contention of this author that the criterion being used must vary from trial to trial, and cannot be considered to be fixed during a run.

Consider now quite large changes in the criterion, such as were brought about by the instruction changes between Experiments 3 and 4. Using the formulae from Feller (1968 pp 345, 348) estimates of correctness and latency were calculated, table 6-2. In the experiments, the difference

in error rate between the two levels of difficulty was typically around 20%. The average correct rate for Experiment 3 was 62.8% correct for difficult trials and 82.6% correct for easy trials. These rates changed to 68.1% for difficult and 89% for easy trials in Experiment 4, an increase for both of around 5 to 6%.

Interpolation in the table of random walk values points to one or two possibilities. For instance, for the probability of a sample coming from one signal $p(s)$ of 0.675 and values of k of 4 and 6, $p(\text{correct})$ goes from .812 to .9 approximately. For the same values of k , $p(s)$ of .575 gives $p(\text{correct})$ of .647 and 0.712. A value of $p(s)$ around .56 would probably give results slightly nearer to those observed. The change of criterion from a value of 4 to 6 causes changes in the predicted RT values of from 3.56 to 6.85 and 3.9 to 8.5. These are values that relate to units of processing time, and each unit will take an unspecified number of msec. Each of these units may be defined as the taking of a particular data sample, perhaps the completion of a single "look" at the evidence in a display. If, say, a time of 90 msec is tied to this unit, then the increase in RT would be of the order of 296 msec for the easy discrimination and 412 msec for the difficult discrimination. The first estimate value is similar to the observed difference between the two experiments, but the second is rather large. If the second value is adjusted by taking each unit as 75 msec, then the two values become almost 250 and 343. Now the first value is a little low. The disparity increases if larger values of k are taken. For instance, comparing $k=6$ and $k=8$ for $p(s)$ of 0.625 and 0.55 at units of 65 msec gives RT increases of 298 and 421 msec. Thus, it is possible to give a moderate fit to the data using standard random walk theory.

Runs Model

The standard Runs Model predicts that for a fixed value of k , (the length of run of one type of evidence required to make a decision) errors will be slightly slower than correct responses. This leads to the prediction by the model that when decision times are long, the result is more likely to be in error, given of course that the criterion has remained fixed. A Speed-Accuracy Tradeoff function, therefore, for a fixed criterion would on average have a negative slope. Again, the only way to obtain a SAT function as usually observed is by mixing criteria.

As with the Random Walk Model it is possible to examine a table of predicted probabilities in order to see what changes would be required to match the data observed, table 6-3. Again the values under consideration are the change from 62.8% to 68.1% (difficult) and from 82.6% to 89% (easy). As the run length required increases, so predicted RT increases exponentially, the rise being much greater for lower probability sampling levels (difficult discriminations) than for higher probabilities (easy discriminations). This was one reason for restricting the search for possible fits to a short run length. There are two possible matches - within general approximations.

One such pair of possibilities lies at $p(s)$ of .55 $k=3,4$ and $p(s) = .65$ $k=3,4$. The changes in probability correct from .82 to .885 and .62 to .658 are similar. The RT changes however are much too large. First, the relative change is great, as decision times are doubled by the criterion change. This would suggest that the overheads to the system (response selection, motor response) are of the order of 420 to 460 msec, as if the change in RT of around 300 to 350 msec represents a doubling of the decision RT,

then those are the orders of magnitude of the residual response times. Since choice RT's can fall well below 400 msecs, e.g. Rabbitt (1969), it seems unlikely that the overheads alone should take this long. Second, the change between predicted RT's are in the ratio of 5.447 to 7.627 (easy:difficult). If one unit is considered to take 55 msecs, then the changes in RT would be 300 msecs (easy) and 420 msecs (difficult). The last is clearly too large.

A second possibility for a fit lies at $p(s) = .575$ and $.725$ with k changing from 2 to 3. Predicted probabilities changes are from 0.837 to 0.917 (easy) and 0.624 to 0.676 (difficult). The predicted change is a little high for the easy trials and about right for the difficult ones. The decision times are again doubled (2.7 to 5.4 and 2.97 to 6.77) and so the previous problem arises. The increases are in the ratio of 2.7 to 3.8, so taking one unit as 110 msecs leads to predicted changes of 293 and 419 msecs. Again, the predicted change for difficult trials is high. (The easy change was used as a standard).

One point should be made. If the 'overheads' are different for the two experiments - as indeed they might be - it would seem likely that they would be greater for the more cautious condition. If this were the case, and if the value was the same for the two levels of difficulty, then the ratio of the difference RT's would increase. In order to produce a ratio of 2.7 to 3.8, it would be necessary to subtract 200 msecs (approximately) from each difference. This would leave actual increases in decision time of 100 and 140 msecs. This would predict that one unit of

decision time would take approximately 38 msec. The total decision time for the difficult trials when being cautious would then be around 257 msec - leaving about 840 msec of overheads! This is, of course, possible but does not seem realistic.

It is interesting to note that the ratios predicted by the Random Walk Model for $p(s) = 0.575$ k 4,6 and $p(s) = 0.675$ k 4,6 were, when using 90 msec per unit, 296 and 412 msec. This is very similar to the above and so it is not really possible to choose between these two models on this basis.

Recruitment Model

At this point we shall consider a simple version of an accumulation of evidence into two bins, without the extra no-decision group as proposed by La Berge (1962), see Table 6-4. As with the Runs Model, for any given value of the criterion k (the amount that must be accumulated), as the length of time for the decision increases, so the probability of selecting the correct response reduces. In fact in this case, at the maximum time predicted for a response, which is $2k-1$, the probability of selecting a signal which has $p(s) = p$ is in fact p . The time at which the highest probability of being correct occurs is k . Thus, there is a reducing function of the probability of being correct as time passes. This leads to a predicted SAT function with a negative slope. Again, it is only possible to predict standard SAT's by mixing criteria, though with all values reducing at time $2k-1$ to $p(s)$, it is very likely that the result of a mixture will give a bow shaped curve at best. A simple mixture for $p(s) = 0.6$ and $k=3,4$ gives probabilities of being correct over increasing time of 0.7714, 0.734, 0.675, 0.693, 0.6. Note that the longest time possible under the model prediction (7 time units) has a probability of 0.6 of being correct.

Again, the exercise of attempting to match probability predictions and reaction times to the observed data of Experiments 3 and 4 was attempted. Two possible fits immediately emerge. One is at $p(s)=0.75 \text{ k } 2,3$ and $p(s)=0.6 \text{ k } 2,3$. The probabilities do not match exactly but the changes of just over 5% and just under 3.5% are not so different. Throughout the table a particular change in criterion causes a greater change in error rate for easy compared with difficult signals, except when considering high probabilities. Another similar fit occurs for $p(s)=0.7 \text{ k } 3,5$ and $p(s)=0.575 \text{ k } 3,5$. The error rate changes are about 6% and 4%. These percentage changes are similar to those predicted by the Random Walk Model, but provide a better fit to the observed data than those predicted by the Runs Model.

To continue with the present comparison, the predicted changes in RT for the two sets of values are very similar. The second set ($p(s)=.7 \text{ k } 3,5$ and $p(s)=.575 \text{ k } 3,5$) predict differences in RT of 2.96 units for easy trials and 3.34 units for difficult trials. If a value of 100 msec for each unit is taken, then these translate into changes of 296 and 334 msec for the two respectively, which virtually match the observed data.

Of the three simple versions of models studied here, it would appear that the Recruitment Model fits the changes in percentage correct scores and RT's the best, so possibly a model based on similar concepts would be best suited for predicting behaviour in this kind of discrimination task.

One interesting point to emerge from this last analysis was the time per unit decision. The value that gave a good fit was 100 msec. This is compatible - possibly rather coincidentally - with the concept of a perceptual moment of duration 100 msec, as discussed by Vickers, Nettlebeck and Willson (1972).

Signal Detection Theory

As previously mentioned, there are problems considering SDT in relation to discriminations if only one criterion is considered. It is much more convincing to think in terms of two criteria. It has been accepted by various workers that RT will vary directly in proportion to the distance of the mean of the distribution from the cutoff point. Thus, when considering two distributions which represent the differences between easy and difficult discriminations, (see Figure 1-2), as the subject becomes more cautious so the two criteria will move apart. As this happens, so the means of the two distributions to the right of the rightmost criterion will both increase. However, if the two distributions are as drawn, with the criteria to the left of the means, so the mean of the easy distribution (the mean further from the criterion) will move less far than the other mean. This implies that after the criterion moves it will be relatively closer to the easy mean than the difficult mean. This in turn suggests that if RT does in fact vary directly with the distance of the mean of the distribution from the criterion, then as the criterion moves so correct easy RT's should increase by more than correct difficult ones. At the same time the means to the other side of the criteria will change in reverse proportions. This will suggest that RT's for errors to difficult displays will change more than RT's to easy displays. If the averaged data from Experiments 3 and 4 are examined, it is found that the converse actually happens. That is, RT's for correct responses to difficult displays change by more (+347 msec) than RT's for correct responses to easy displays (+298 msec). At the same time RT's for errors to difficult displays change by less (+374 msec) than those to easy displays (+497 msec).

It is possible that these results could be biased by the readings of one subject (JW) who only made three errors in the easy trials of Experiment 4. The data were re-analysed without his data, and the differences were found to be less, but in the same direction (Table 6-1a).

Vickers' Accumulator Model

A programme was written by the author to simulate Vickers' model as described in his 1970 paper. A similar approach to Vickers was taken. A normal distribution function was placed into an array and sampled from using a random number generator. Similar, though less variable, latency-probability functions were obtained. 1000 decisions were taken at each of many combinations of $p(s)$ and criterion values. Each decision in this case consisted of sampling from the normal distribution until the criterion had been reached or exceeded. The probability correct, RT for error and correct responses and SAT function were recorded. The plots of RT and percent correct against $p(s)$ and criterion are shown in Figure 1-7.

The SAT functions calculated from this are essentially flat when the criterion is fixed. Judicious mixing of criteria gave SAT curves that increased more like the generally observed SAT, but without enough of a dip at the fast RT end. This means that the theory does not predict chance behaviour for fast responses.

Attempts to find values that would fit the observed differences in Experiments 3 and 4 were reasonably successful. Figure 1-7 was examined for points at which the observed correct scores occurred at the same criterion for the two experiments. At $p(s)=0.65$ and $p(s)=0.55-0.56$ approximately the following probability correct were noticed: For $c=2.1$, $p(c)=0.83$ and 0.63

for the two curves respectively. For $c=3.9$, $p(c)=0.89$ and 0.68 for the two curves respectively.

The reaction time units associated with these values are:

For $c=2.1$ Units = 4.0 and 4.2

For $c=3.9$ Units = 6.8 and 7.4 for the two curves.

These give differences of 2.8 and 3.2 units between different instructions. The actual differences in RT were 301 and 342 approximately. These are in the same ratio and if each unit is thought of as being 106 msec, the two give values of 297 and 339 msec. These are remarkably close to the observed data.

One point of weakness is that these predict differences between easy and difficult trials of just over 20 and 40 msec instead of the observed differences of 50 and 100 msec.

The value of 106 msec per unit of time as used by the model is close to the value used by Vickers (1970) himself (i.e. 100) which is related to "the perceptual moment hypothesis" as proposed by Stroud (1956) and by Vickers, Nettlebeck and Willson (1972). It is considered that each sample will take up the time related to one such perceptual unit. The idea is not unattractive especially since it relates so well to the observed data.

Using the $P(s)$ values of 0.555 and 0.65 gives values for z on a normal curve of around 0.14 and 0.39. These are close to a ratio of 3:1 which was the actual physical ratio of the presented line lengths.

It would appear that Vickers' model is a better predictor than the other models which we have considered and that it can also relate directly to the values of the stimuli used.

One problem common to all the theories just considered is that of predicting behaviour at very fast reaction times - at the area of the SAT curve where it crosses the 50% correct mark. With all of them when the criterion is at a minimum, the probability of being correct approaches the probability of an individual item being sampled from the correct channel. In the notation used earlier, the probability approaches $p(s)$. Thus, the theories will only predict chance performance under two conditions. First, when the probability that any one unit of information coming from the input represents an error is 0.5. In this case however, the probability of being correct at any criterion level will also be 0.5. Second, when the required criterion is zero, or in other words the response is made without any evidence having been collected, i.e. the subject guesses. It is not enough to say that the evidence collected is not sufficient as any collection of evidence will tend to bias the final response towards the correct one, and will lead to the case of subjects always performing better than chance on average. This suggests that all models of the kinds we have examined must be extended by theorists to allow for fast guessing of the form proposed by Ollman (1966) and Yellott (1967). Of course when the signals are difficult to differentiate, then $p(s)$ may approach 0.5. When this happens, and if the proportion of fast responses is small, then the variability of the probability will be quite high and values will quite often be observed below 0.5 simply because of sampling error. The situation being referred to will mainly be evident when stimuli are fairly discriminable.

A third explanation may apply, but no group of theorists has yet dealt with it explicitly. It may be necessary to combine theories from different stages of processing. As a subject

responds more quickly, the speed of his response selection and the time necessary for motor control increasingly set limits to the accuracy of his performance. Certainly, response repetitions and alternations are well known to affect reaction time, and if biases of this kind are mixed with the stimulus sampling theories, then it may be that, since responses based on sequences of previous responses are likely to be at chance level, the number of errors is likely to increase.

It was with these possibilities in mind that the data for Experiment 5 were re-examined. The subjects had been asked to perform at three levels of caution and it was wondered if these could be represented by any theory as three positions of criteria. Since Vickers' Accumulator Model gave a reasonable fit to the grouped data for Experiments 3 and 4, it was the first one examined.

A very reasonable fit was found for the first subject's (PR) data for the two more cautious conditions. It was obtained in the following manner by interpolation from the prediction curves plotted in Figure 1-7. The four levels of percent correct were plotted out and curves searched for that would join the two easy scores and the two difficult scores at the same criteria. Such curves were found between the curves $PS=0.7$ and 0.77 for easy (around $PS=0.74$) and just below the curve for 0.6 (around 0.59). Thus four points were found which matched the correctness scores of the subject. The z values for these $p(s)$'s were:

$$z(p(s)=.74) = 0.645 \text{ approx.}$$

$$z(p(s)=.59) = 0.23 \text{ approx.}$$

These values were fairly near to a ratio of 3:1 which was the ratio of the difference between the signals, a prediction of the theory. The standard deviations of the difference distributions would be around 0.7.

Given these values, the criteria used were found to be $c=1.25$ for the middle caution level and $c=2.25$ for the more cautious. The number of reaction time units that correspond to these were, for the easy displays 2.2 and 3.5 a difference of 1.3 (this matched a difference in RT of 123 msec); for the difficult displays the values were 2.7 and 4.4 - a difference of 1.7 (this matched a difference in RT of 170 msec). Thus, there was a very close correspondence between the predicted and observed difference RT's when one unit was taken to be 100 msec.

This matching of RT's also applied to the difference between easy and difficult trials. Using the more cautious criterion ($c=2.25$) the predicted difference in RT between easy and difficult trials was 90 msec (compared with 101 msec). The predicted difference using the middle caution criterion ($c=1.25$) was 50 msec (compared with 54 msec).

Thus, for subject PR there was a very good prediction of the various differences in RT for the middle and cautious RT's.

When the prediction was attempted for the fast runs however, there was not a good match. If the same curves were used, then the easy stimuli at the observed speed produced too many errors. The observed RT's would occur when c had a value of around 0.4, at which value the observed correct scores are predicted to be 0.77 and 0.61 (compared with 0.69 and 0.63 as observed values). When subjects were responding quickly we would expect a certain amount of guessing which would increase the proportion of errors to both easy and difficult display. PR stated that during the fast runs he was aware of going very fast at times, (at which

he felt was a chance level of performance) while at other times he was more careful. This would certainly appear to be a change in strategy for the fast runs when compared with the slower ones. However, if he were guessing, it would be predicted that both easy and difficult trials would suffer. It was observed that easy trials did indeed suffer as though he were guessing, but that the difficult trials did not.

Attempts to fit the other subject to the models were not really very successful. It was felt that this could be due to the subject being highly practiced which led to low error rates for the easy trials. This, combined with relatively fast reaction times, led to difficulty in applying any model realistically. For instance, the values for fitting Vickers' model need to be around $p(s)$ 0.9 and 0.7 with $c=0.25$ and 1.25. At this level it is very difficult to make any real estimates of the model predictions from the data available. Other than saying that the predictions appear to be of the right order of magnitude for the middle and slow runs little more can be said. The Recruitment Model does as well in predicting his performance with $PS = 0.9$ and 0.7 ; $k = 1, 2$. This is assuming that $P_0 = 0.0$; if $P_0 > 0$ then the RT's would increase at a faster rate than observed and so would not fit as well.

The Runs and Random Walk Models predict either, for the RT's involved, a smaller increase in correctness than observed for the difficult trials, or a greater increase in RT for the observed increase in accuracy.

One Criterion or Two?

There is a problem that must be considered when looking at the data of PR. As we have seen his data are matched very well by Vickers' Accumulator Model. This match included the assumption that he was using only one criterion which he adjusted as different instructions were received. This is, in fact, quite a reasonable assumption. The problem arises when one now remembers the Sample Size analysis reported in the previous chapter. This showed that as the proportion of easy trials that immediately preceded the present trial increased, so the present trial RT would be faster if it was an easy trial, or slower if it was a difficult trial (Figure 5-7). It was felt that this divergence of mean RT for easy and difficult trials as the proportion of easy trials immediately preceding the present trial increased could only be explained by the presence of two criteria - one for each level of difficulty. If only one criterion had been used, then both curves would be expected to move together, as in Experiment 2 (Figure 3-7), and the slow subjects of Experiment 5 (Figure 5-1).

There is then a conflict which the author at present cannot resolve, though it is felt that the evidence from the Sample Size analysis would be hardest to refute. If the subject was using only one criterion, then any strategy, such as guessing or slowing down after easy trials and speeding up after difficult ones, would affect both easy and difficult trial latencies in similar directions.

Conclusion

None of the models completely predicts the form of the SAT functions observed. It would seem that the upper part of such curves, or rather the performance of subjects that relates to that part of the curves, can be fairly reasonably accounted for by the models - in particular by Vickers' Accumulator Model. The lower part of the curve cannot be predicted by any of the models, so either the models have to be rejected or they must be extended by supplementary postulates akin to the fast guessing model of Ollman (1966).

We cannot claim that any of these models exactly describe the internal mechanism used by the subject to make his decisions. However, when a model such as Vickers' can match the data so well using just one criterion, then this points to the possibility that subjects also have a single reference value which they use as a criterion when examining the outside world. On the other hand, there is the conflicting evidence, in particular of subject PR in Experiment 6, in which his data overall indicate the use of one set of criteria, whereas when the data are examined in a more detailed sequential way there is evidence that two criteria are being used.

This really indicates the complexity of the system that is being studied and that the explanations will not be simple.

In general, the changes in latency and error rate observed under different instructions are best matched by Vickers' (1970) Accumulator Model in conjunction with a change in strategy to include guessing at faster speeds.

experiment	EASY TRIALS	DIFFICULT TRIALS
3	711	779
4	1012	1121
difference	301	342
3	82.6	62.8
4	89.0	68.1
difference	6.4	5.3

Summary Table of RT's in msec (above) and
Percent Correct (below) in experiments 3 & 4.

Table 6 - 1.

TABLE OF RUNS MODEL VALUES

PROBABILITIES:

K=	2	3	4	5	6	7	8
P+0.900	+0.979	+0.997	+1.000	+1.000	+1.000	+1.000	+1.000
P+0.875	+0.967	+0.993	+0.999	+1.000	+1.000	+1.000	+1.000
P+0.850	+0.952	+0.988	+0.997	+0.999	+1.000	+1.000	+1.000
P+0.825	+0.935	+0.981	+0.995	+0.999	+1.000	+1.000	+1.000
P+0.800	+0.914	+0.970	+0.991	+0.997	+0.999	+1.000	+1.000
P+0.775	+0.891	+0.956	+0.985	+0.995	+0.998	+1.000	+1.000
P+0.750	+0.865	+0.939	+0.975	+0.991	+0.997	+0.999	+1.000
P+0.725	+0.837	+0.917	+0.962	+0.984	+0.993	+0.997	+0.999
P+0.700	+0.806	+0.890	+0.943	+0.973	+0.987	+0.994	+0.998
P+0.675	+0.773	+0.857	+0.918	+0.956	+0.977	+0.988	+0.994
P+0.650	+0.738	+0.820	+0.885	+0.930	+0.960	+0.977	+0.987
P+0.625	+0.702	+0.777	+0.843	+0.894	+0.932	+0.957	+0.973
P+0.600	+0.663	+0.729	+0.791	+0.845	+0.888	+0.921	+0.946
P+0.575	+0.624	+0.676	+0.729	+0.779	+0.824	+0.862	+0.893
P+0.550	+0.583	+0.620	+0.658	+0.698	+0.736	+0.771	+0.804
P+0.525	+0.542	+0.560	+0.581	+0.603	+0.625	+0.647	+0.669
P+0.520	+0.533	+0.548	+0.565	+0.583	+0.601	+0.619	+0.637
P+0.510	+0.517	+0.524	+0.533	+0.542	+0.551	+0.560	+0.570

TABLE OF RUNS MODEL VALUES

REACTION TIMES

K=	2	3	4	5	6	7	8
P+0.900	+2.2967	+3.7050	+5.2391	+6.9347	+8.8167	+10.9075	+13.2306
P+0.875	+2.3684	+3.9153	+5.6408	+7.5958	+9.8252	+12.3720	+15.2823
P+0.850	+2.4384	+4.1390	+6.0886	+8.3538	+11.0086	+14.1289	+17.7990
P+0.825	+2.5062	+4.3755	+6.5871	+9.2259	+12.4054	+16.2523	+20.9130
P+0.800	+2.5714	+4.6235	+7.1411	+10.2319	+14.0634	+18.8383	+24.8012
P+0.775	+2.6336	+4.8811	+7.7540	+11.3940	+16.0417	+22.0117	+29.7022
P+0.750	+2.6923	+5.1457	+8.4278	+12.7360	+18.4124	+25.9355	+35.9403
P+0.725	+2.7471	+5.4136	+9.1615	+14.2811	+21.2613	+30.8200	+43.9571
P+0.700	+2.7975	+5.6804	+9.9498	+16.0482	+24.6845	+36.9324	+54.3532
P+0.675	+2.8431	+5.9403	+10.7806	+18.0445	+28.7790	+44.5967	+67.9317
P+0.650	+2.8835	+6.1871	+11.6339	+20.2543	+33.6172	+54.1694	+85.7197
P+0.625	+2.9184	+6.4136	+12.4793	+22.6233	+39.1985	+65.9512	+108.8890
P+0.600	+2.9474	+6.6125	+13.2763	+25.0413	+45.3653	+79.9715	+138.3770
P+0.575	+2.9702	+6.7763	+13.9759	+27.3300	+51.6942	+95.5735	+173.8370
P+0.550	+2.9867	+6.8987	+14.5258	+29.2502	+57.4181	+110.8680	+211.5870
P+0.525	+2.9967	+6.9744	+14.8784	+30.5426	+61.5052	+122.5540	+242.6400
P+0.520	+2.9979	+6.9836	+14.9219	+30.7056	+62.0349	+124.1190	+246.9550
P+0.510	+2.9995	+6.9959	+14.9804	+30.9259	+62.7560	+126.2680	+252.9430

TABLE OF RECRUITMENT MODEL VALUES

PROBABILITIES:

K=	2	3	4	5	6	7	8
P+0.900	+0.9720	+0.9914	+0.9973	+0.9991	+0.9997	+0.9999	+1.0000
P+0.875	+0.9570	+0.9839	+0.9938	+0.9975	+0.9990	+0.9996	+0.9998
P+0.850	+0.9392	+0.9734	+0.9879	+0.9944	+0.9973	+0.9987	+0.9994
P+0.825	+0.9188	+0.9595	+0.9790	+0.9889	+0.9941	+0.9968	+0.9982
P+0.800	+0.8960	+0.9421	+0.9667	+0.9804	+0.9883	+0.9930	+0.9958
P+0.775	+0.8709	+0.9211	+0.9502	+0.9680	+0.9792	+0.9864	+0.9910
P+0.750	+0.8437	+0.8965	+0.9294	+0.9511	+0.9657	+0.9757	+0.9827
P+0.725	+0.8147	+0.8684	+0.9040	+0.9289	+0.9468	+0.9598	+0.9695
P+0.700	+0.7840	+0.8369	+0.8740	+0.9012	+0.9218	+0.9376	+0.9500
P+0.675	+0.7518	+0.8023	+0.8393	+0.8676	+0.8900	+0.9081	+0.9227
P+0.650	+0.7182	+0.7648	+0.8002	+0.8283	+0.8513	+0.8705	+0.8868
P+0.625	+0.6836	+0.7248	+0.7570	+0.7834	+0.8057	+0.8248	+0.8415
P+0.600	+0.6480	+0.6826	+0.7102	+0.7334	+0.7535	+0.7712	+0.7869
P+0.575	+0.6117	+0.6385	+0.6604	+0.6791	+0.6956	+0.7104	+0.7238
P+0.550	+0.5747	+0.5931	+0.6083	+0.6214	+0.6331	+0.6437	+0.6535
P+0.525	+0.5375	+0.5468	+0.5545	+0.5613	+0.5674	+0.5729	+0.5781
P+0.520	+0.5300	+0.5375	+0.5437	+0.5491	+0.5540	+0.5585	+0.5626
P+0.510	+0.5150	+0.5187	+0.5219	+0.5246	+0.5271	+0.5293	+0.5314

TABLE OF RECRUITMENT MODEL VALUES

REACTION TIMES

K=	2	3	4	5	6	7	8
P+0.900	+2.180	+3.319	+4.439	+5.554	+6.666	+7.778	+8.889
P+0.875	+2.219	+3.400	+4.559	+5.709	+6.855	+7.999	+9.142
P+0.850	+2.255	+3.480	+4.682	+5.870	+7.053	+8.232	+9.410
P+0.825	+2.289	+3.558	+4.804	+6.036	+7.259	+8.477	+9.693
P+0.800	+2.320	+3.634	+4.927	+6.204	+7.472	+8.732	+9.989
P+0.775	+2.349	+3.706	+5.047	+6.373	+7.688	+8.996	+10.298
P+0.750	+2.375	+3.773	+5.163	+6.540	+7.907	+9.265	+10.616
P+0.725	+2.399	+3.837	+5.274	+6.703	+8.123	+9.535	+10.940
P+0.700	+2.420	+3.895	+5.378	+6.859	+8.333	+9.801	+11.263
P+0.675	+2.439	+3.947	+5.474	+7.004	+8.533	+10.058	+11.579
P+0.650	+2.455	+3.993	+5.560	+7.137	+8.718	+10.299	+11.879
P+0.625	+2.469	+4.033	+5.634	+7.254	+8.883	+10.517	+12.153
P+0.600	+2.480	+4.066	+5.697	+7.354	+9.025	+10.706	+12.393
P+0.575	+2.489	+4.091	+5.747	+7.434	+9.140	+10.860	+12.590
P+0.550	+2.495	+4.110	+5.783	+7.492	+9.224	+10.974	+12.737
P+0.525	+2.499	+4.121	+5.805	+7.527	+9.276	+11.044	+12.827
P+0.520	+2.499	+4.123	+5.808	+7.531	+9.282	+11.052	+12.838
P+0.510	+2.500	+4.124	+5.811	+7.537	+9.290	+11.064	+12.853

Table 6-3

TABLE OF RANDOM WALK VALUES

PROBABILITIES:

A=	4	6	8	10	12	14	16
P+0.900	+0.9878	+0.9986	+0.9998	+1.0000	+1.0000	+1.0000	+1.0000
P+0.875	+0.9800	+0.9971	+0.9996	+0.9999	+1.0000	+1.0000	+1.0000
P+0.850	+0.9698	+0.9945	+0.9990	+0.9998	+1.0000	+1.0000	+1.0000
P+0.825	+0.9569	+0.9905	+0.9980	+0.9996	+0.9999	+1.0000	+1.0000
P+0.800	+0.9412	+0.9846	+0.9961	+0.9990	+0.9998	+0.9999	+1.0000
P+0.775	+0.9223	+0.9761	+0.9929	+0.9979	+0.9994	+0.9998	+0.9999
P+0.750	+0.9000	+0.9643	+0.9878	+0.9959	+0.9986	+0.9995	+0.9998
P+0.725	+0.8742	+0.9482	+0.9797	+0.9922	+0.9970	+0.9989	+0.9996
P+0.700	+0.8448	+0.9270	+0.9674	+0.9857	+0.9938	+0.9974	+0.9989
P+0.675	+0.8118	+0.8996	+0.9490	+0.9748	+0.9877	+0.9940	+0.9971
P+0.650	+0.7752	+0.8650	+0.9225	+0.9567	+0.9762	+0.9870	+0.9930
P+0.625	+0.7353	+0.8224	+0.8853	+0.9278	+0.9554	+0.9728	+0.9835
P+0.600	+0.6923	+0.7714	+0.8350	+0.8836	+0.9193	+0.9447	+0.9624
P+0.575	+0.6467	+0.7124	+0.7701	+0.8193	+0.8598	+0.8924	+0.9182
P+0.550	+0.5990	+0.6461	+0.6905	+0.7317	+0.7692	+0.8029	+0.8328
P+0.525	+0.5499	+0.5745	+0.5988	+0.6226	+0.6458	+0.6683	+0.6901
P+0.520	+0.5399	+0.5597	+0.5794	+0.5987	+0.6178	+0.6365	+0.6548
P+0.510	+0.5200	+0.5300	+0.5399	+0.5498	+0.5597	+0.5696	+0.5793

TABLE OF RANDOM WALK VALUES

REACTION TIMES

A=	4	6	8	10	12	14	16
P+0.900	+2.439	+3.740	+4.998	+6.250	+7.500	+8.750	+10.000
P+0.875	+2.560	+3.977	+5.329	+6.666	+8.000	+9.333	+10.667
P+0.850	+2.685	+4.239	+5.703	+7.140	+8.571	+10.000	+11.429
P+0.825	+2.812	+4.528	+6.129	+7.686	+9.229	+10.769	+12.308
P+0.800	+2.941	+4.846	+6.615	+8.317	+9.995	+11.665	+13.333
P+0.775	+3.071	+5.194	+7.170	+9.053	+10.896	+12.723	+14.544
P+0.750	+3.200	+5.571	+7.805	+9.918	+11.967	+13.987	+15.995
P+0.725	+3.326	+5.977	+8.528	+10.938	+13.254	+15.520	+17.763
P+0.700	+3.448	+6.405	+9.347	+12.144	+14.815	+17.407	+19.955
P+0.675	+3.563	+6.850	+10.263	+13.565	+16.721	+19.762	+22.726
P+0.650	+3.670	+7.299	+11.265	+15.223	+19.048	+22.729	+26.293
P+0.625	+3.765	+7.737	+12.329	+17.114	+21.860	+26.475	+30.943
P+0.600	+3.846	+8.143	+13.402	+19.182	+25.158	+31.130	+36.996
P+0.575	+3.912	+8.494	+14.408	+21.285	+28.785	+36.628	+44.609
P+0.550	+3.960	+8.767	+15.244	+23.172	+32.309	+42.410	+53.243
P+0.525	+3.990	+8.940	+15.803	+24.512	+34.985	+47.129	+60.838
P+0.520	+3.994	+8.962	+15.873	+24.685	+35.343	+47.783	+61.932
P+0.510	+3.998	+8.991	+15.968	+24.920	+35.833	+48.689	+63.468

Table of some calculated values for a Runs model.

p=0.6 k=2.

Time (t)	Possible Sequences	p(sequ)	At Time (t) p(decide A)	
2	AA	.36	.6923	Mean p(a)= 0.663
3	BAA	.144	.6	Mean RT = 2.947
4	ABAA	.0864	.6923	
5	BABAA	.03456	.6	
6	ABABAA	.020736	.6923	
6	BABABB	.009216		
5	ABABB	.02304		
4	BABB	.0384		
3	ABB	.096		
2	BB	.16		

p=0.6 k=3.

3	AAA	.216	.7714	Mean p(a)= 0.729
4	BAAA	.0864	.6923	Mean RT = 6.6125
5	BBAAA	.0864	.6923	
	ABAAA		.74703	
6	AABAAA	.072576		
	ABBAAA			
	BABAAA			
7	AABBAAA	.0539136	.70909	
	BABBAAA			
	BAA BAAA			
	ABABAAA			
	BBABAAA			
8	BAABBAAA	.034836	.62376	
	ABABBAAA			
	BBABBAAA			
	ABBA BAAA			
	AA ABAAA			
	BABABAAA			
	ABAABAAA			
	BBAABAAA			
8	(vv above)	.021012		
7		.0221184		
6		.024576		
5		.0384		
4		.0384		
3		.064		

Table 6-5

Table of some calculated values for a Recruitment Model

<u>p=0.6 k=2</u>				
Time (t)	Possible Sequences	p(decide at time t)	p(decide A) at time t	
2	AA	.36	.6923	
3	ABA BAA	.288	.6	RT (A)=2.44
3	ABB	.192		RT (B)=2.54
2	BAB BB	.16		
<u>p=0.6 k=3</u>				
3	AAA	.216	.7714	
4	BAAA ABAA AABA	.2592	.6923	
5	BBAAA BABAA BAABA ABBAA ABABA AABBA	.20736	.6	RT (A)=3.987
5	vice versa above	.13824		RT (B)=4.234
4		.1152		
3		.064		
<u>p=0.6 k=4</u>				
4	AAAA	.1296	.83505	
5	(BAAA)A	.20736	.7714	
6	(BBAAA)A	.20736	.6923	
7	(BBBAAA)A	.165888	.6	RT (A)=5.648
7	v.v above	.110592		RT (B)=5.812
6		.09261		
5		.06144		
4		.0256		

Table 6-6.

Given: World in state A or B. Sample from the environment, in which case the probability that a sample value represents state A = p. Accumulate such evidence. $q=1-p$.

RANDOM WALK Model Increment a counter for each unit of evidence for A; decrement the counter for each unit for B. Decide A if reach value $2Z$, or B if reach 0. Start with value of Z .

$$p(\text{decide A}) = \frac{(q/p)^{2Z} - (q/p)^Z}{(q/p)^{2Z} - 1}$$

$$\text{Duration of decision} = \frac{Z}{q-p} - \frac{2Z}{q-p} * \frac{1 - (q/p)^Z}{1 - (q/p)^{2Z}}$$

RUNS Model Decide A if get run of k items of A. Same for B.

$$p(\text{decide A}) = p^{(k-1)} * \frac{1 - q^k}{p^{(k-1)} + q^{(k-1)} - p^{(k-1)} * q^{(k-1)}}$$

$$\text{Duration of decision} = \frac{(1 - p^k) * (1 - q^k)}{qp^k + pq^k - p^k q^k}$$

ACCUMULATOR Model (perfect memory)

Decide A/B when have accumulated k items of A/B.

$$p(\text{decide A}) = p^k \left(\sum_{i=1}^{k-1} q^i (k+i-1) C_i \right) + p^k$$

$$\text{Duration of decision} = k + \sum_{i=0}^{k-1} i p_i$$

$$\text{where } p_i = (p^k q^i + p^i q^k) * (k+i-1) C_i$$

Formulae used in the prediction of performance under three models discussed in chapter 1.

CHAPTER 7

FURTHER STUDIES OF THE EFFECT OF DIFFICULTY

EXPERIMENT 7

When subjects are not performing quickly, we have seen that the difference in latency between RT's to easy and difficult bias runs is relatively independent of both the mean RT of the run and of the difference in RT between easy and difficult trials. For the experiments in which this was observed the difference in error rate between easy and difficult trials remained fairly constant at around 20%. This led to the question of whether the relative stability of this overall bias effect was a function of the actual difference between displays.

This could be tested by increasing the difference between the easy and difficult displays. If the effect of overall bias is attributable to the actual difference between easy and difficult displays, then the effect should increase. It is also possible that the effect may disappear if the subject is then able to treat the two levels of difficulty separately - perhaps using two criteria, one for each level of difficulty.

The difficult stimuli used in this experiment will be the same as in Experiments 3 and 4 where the effect of overall bias was first noticed, so it may be possible to compare performances in the two experiments.

There is another point to be explored concerning the difference in direction of the effect in Experiments 1 and 2. It may be remembered that in Experiments 1 and 6 the difficult trials were

slowed down by preceding easy trials, whereas in Experiments 2 and 5 they were faster when preceded by easy trials. This difference may have been due to the introduction of extra lines to the top and bottom of the screen in the second experiment. However, the effect was still seen in Experiment 5 where there were only stimuli in the lower half of the screen. The actual difference between easy and difficult line lengths was greater in Experiment 1 than in the later experiments - as they will be in this experiment - and it has already been suggested that this might have led to the subjects using two criteria as it was felt they did in the first experiment. If so, then the effects of the two bias conditions may be similar. The configuration of this experiment will be like Experiment 2, but the difficulty levels will be more like those in Experiment 1, so a comparison should be possible.

METHOD

Subjects:

2 male and 2 female, age range 20 - 30 years old. None had taken part in any other of the experiments and all were ignorant as to the purpose of the experiment.

Apparatus and Stimuli - As described for Experiment 2 under General Experimental Setup, with the exception that the easy stimuli were now 125 points (19.3 mm) long. In Experiment 2 the easy stimuli were 123 points long. Difficult stimuli and the short standard line are as previously - 121 and 120 points long respectively.

Instructions - After being shown the stimuli and apparatus, the subjects were instructed: "Please press with your index finger the key on the side of the longer line. Please respond fairly quickly but do not make too many errors. Try not to miss any trials."

Procedure - Each subject attended for four sessions and during each session was presented with six runs of 200 trials. As in Experiment 3 and 4 there were biases towards easy and difficult displays. The first three runs in a session would be at one bias and the last three at the reverse bias. Thus, each subject contributed some 2400 responses in each of the bias conditions. The order of easy and difficult bias runs was counterbalanced across sessions, see Table 7-1.

Results and Discussion:

When the data were examined overall, across subjects, there appeared to be little effect of bias, see Table 7-2. There is no difference in reaction time to easy trials between the two conditions and the 20 msec difference for difficult trials is not significant. Neither is there any difference in error rates between the two biases.

However, the situation is somewhat different when individual subjects are examined. Two of the subjects (JD and JC) show a clear effect of bias, as has been seen previously, and the other two (MM and ET) just as clearly show the reverse! All the differences are significant at, at least, the 0.5% level (Z values start at 2.4, $df > 3000$), the differences ranging from 48 to 171 msec.

There were slight changes in accuracy, none of them significant. Of the small changes observed, half were similar to previous

findings in which a faster response led to more errors, and half suggested the reverse.

All the subjects' responses were consistent over easy and difficult trials; that is, if easy bias runs led to faster RT's to easy trials then they also led to faster RT's on difficult trials. For one subject the effect was about the same for easy and difficult trials; for the others the effect was two to three times greater for difficult trials than for easy trials.

In short, the two subjects who were faster in the easy bias condition showed a positive bias effect and the two who were faster in the difficult bias condition showed a negative bias effect. In an attempt to determine the reason for the difference between the two pairs, the following factors were examined:

1. Experimental design - the order of presentation of the biases was examined to see if there was anything here that could explain the difference. The two groups both received balanced sequences.
2. The overall reaction time and the proportion of errors was examined to see if either pair was faster or more accurate than the other pair. No real difference could be found. On average the two from the negative bias effect were slightly slower than the other two, but
 - (a) by only 80 msecs and
 - (b) this was really only one of the subjects. The other subject was only 40 msecs slower in the difficult bias.

Of course it may be that these two subjects were being very slow by their own standard - but this is conjecture

and there is no reason to suppose after the last two experiments that this would be the case. Also, if it were the case one would expect it to show up in the error rates.

3. The error rates in the different bias conditions were compared and also the difference in error rate between the two pairs. No differences were found.
4. The reaction times to easy and difficult trials were compared and no differences were found.
5. Sample Size analyses were performed on the data separately for each subject, see Figures 7-1 to 7-4.
 - (a) Positive Bias pair. All trends seen in the Sample Size analysis, except one in one subject, show a speeding up of trials after a number of easy trials. There was one exception - one subject showed a slight increase in RT for easy trials following a run of easy trials during easy bias runs. The same subject showed fairly sluggish effects of preceding difficulty in the difficult bias condition.
 - (b) Negative Bias pair. One subject had negative slopes (RT increased with increasing numbers of easy trials) in the easy bias runs, and little effect during difficult bias runs. The other subject showed quite a small positive effect for three of the conditions, but difficult trials in difficult bias runs were slower after easy trials. Hence, there is no consistency here.

6. An analysis of variance was performed on mean RT's classified by the difficulty of the two preceding stimuli. See Appendix: AN7-1. There were thus four categories (trial (N-1) or (N-2) easy or difficult) of sequence preceding the present easy or difficult trial. The difficulty of the present trial was significant at the 5% level ($F = 12.0$, $df 1:3$). Nothing else was significant at this level, though because of the effect shown by trial N-2 in earlier experiments the interaction between that and the current trial was examined, see Table 7-3. The F ratio of 7.76 ($df 1:3$) is in the 10% region (10% value for $df 1:3 = 5.537$; 5% value 10.13). Taken with the other experiments this is further support for the effect of trial N-2 on the current trial. This also suggests a moderate amount of consistency between the four subjects which would not be expected from the earlier observation of overall bias effects. The local response is similar, yet the overall bias effects are very different. This is a further indication that RT's to any one display are influenced by two quite separate factors - one is the difficulty of the immediately preceding displays, the other is the overall level of difficulty of the display.

The Source of Local Difficulty Bias Effects

We have seen that RT's are affected by the average level of difficulty of the preceding trials (local difficulty bias effect). We have also seen that the trial which has most influence on the current trial RT is the trial n-2. What, if any, is the relationship between these two observations?

The appearance of the local difficulty bias effect was a gradual one. The question now is whether it is produced by the subject adjusting his performance to the average difficulty level of recent trials, or whether it can be attributed to a slow cumulative effect over a number of trials. Jesteadt, Luce and Green (1977) have shown, for example, that the sequential effect of the amplitude of signals reported by Holland and Lockhead (1968), which had been considered to last over many trials, could in fact be attributed to an accumulation of the effect of trial $n-1$. Since in the present experiments the main trial to influence RT was trial $n-2$, it was decided to examine the direction of that influence first. If the local effect of difficulty is cumulative, then the direction of influence on the individual trials should correspond with that observed from the average difficulty of preceding trials. If these two do correspond, then if trial $n-2$ being difficult slows down the current trial, a high proportion of preceding difficult trials should also lead to slower trials.

The direction of the trends shown by the Sample Size analyses was compared with the effect of the difficulty of trial ($n-2$) on the latency to the present trial. Generally the two corresponded fairly well. For instance, if the present trial was slower when the trial two back was difficult, then trials after a high proportion of difficult trials would also tend to be slower. This corresponds to the positive bias effect.

For JC and MM the local trends and the effect of trial $n-2$ corresponded exactly. For JC if trial $n-2$ was difficult then the present trial was slower than average; he also showed strong

positive bias effects. For MM the same was true for three of the conditions, but for difficult trials in a difficult bias run there was a small negative bias effect and if trial $n-2$ was difficult, the present trial was slightly faster than average.

For JD, three or four such comparisons matched. The one that did not concerned difficult trials in a difficult bias where if trial $n-2$ was difficult the present trial was faster (negative effect) but trials following a high proportion of difficult trials were slower than average (positive effect).

For ET, easy trials were affected in the same direction by the difficulty of trial $n-2$ and the average difficulty of the preceding sequence. However, difficult trials in the easy bias condition were slower after trial $n-2$ was difficult, but faster on average when preceded by a high proportion of difficult trials.

In the analysis of variance trial $n-2$ was seen to have some effect on the latency of the present trial. It has been seen here that the direction of that influence corresponds quite well with the average difficulty level of trials preceding the present one. It would seem, therefore, that the local difficulty bias effect can probably be accounted for, in this experiment at least, by a gradual accumulation of the effect of trial $n-2$. It was mentioned earlier that other workers have found trial $n-1$ to be an important influence in sequential effects - for instance, Drazin (1961) in studying the sequential effects of foreperiods, Bertelson (1961) and many others in the repetition effect, and Jesteadt, Luce and Green (1977) when re-analysing the data of Holland and Lockhead (1968) on the sequential effects of loudness. In view of this, it was decided to examine the relationship between the effect of trial $n-1$ and local difficulty bias in the same way as with trial $n-2$ above.

Only for subject JC was there any complete correspondence in the direction of the two factors. It has already been noted that this subject showed a strong positive effect of local difficulty bias. If trial n-1 was difficult, then the present trial was slower than average.

For subject ET, three of the four comparisons of the effects of trial n-1 and local difficulty bias showed a similar direction of effect. The one that did not match was for easy trials in difficult bias runs, in which there was a slight negative local bias effect, but if trial n-1 was difficult then the present trial was slower than average.

For the other two subjects there was less agreement. JD in difficult bias runs was slower after a difficult trial n-1 in both easy and difficult trials, and also showed a positive bias effect. For the easy bias runs the direction of influence of trial n-1 and average difficulty did not agree. For MM there was only agreement in one condition - easy trials in easy bias runs, in which trials were slower when preceded by a difficult trial at n-1 and there was a positive local bias effect.

We can conclude from this that there is a greater correspondence between the observed effects of local difficulty and trial n-2 than with trial n-1. The relationship can be put as follows: in only 10 of the 16 matchings of trial and response difficulty (four for each subject) can the direction of the local bias effect be predicted from the influence of trial n-1, whereas 13 can be predicted from the effect of trial n-2.

This adds further weight to the hypothesis that it is the influence of trial n-2 that is critical in the appearance of local difficulty bias.

In summary therefore, the difficulty of the immediately preceding trial (n-1) has no consistent effect on the current trial. Trial (n-2) if difficult slows down difficult trials but has relatively little effect on easy trials. If samples of preceding trials are taken, the current trial (n) is generally slower if preceded by several difficult trials. Finally, the overall bias of the run will affect mean RT over the run, but in this experiment it has been shown that the effect may be either way - that is, when the mean RT's for the difficult and easy bias runs are compared, some subjects are faster in the difficult bias runs. There seems no way of predicting from local effects within the run, which way the overall bias will affect RT.

- . Finally, in searching for differences between the positive and negative groups, it was noticed that the subjects who had the positive effect were male, the other two female. This, however, is considered to be an unlikely cause of the effect as no such difference was noticed in the previous experiments.

It was decided at this point to compare the absolute effects of bias observed in this experiment with those in the other experiments in which there was a bias of stimuli. Various statistics of the experiments are shown in Table 7-4. When the absolute values are compared, it can be seen that for each experimental group there is just one subject for whom the absolute difficulty bias effect exceeds that of the subjects of this last experiment. When each group is compared with the present one using a Mann-Whitney test, the probabilities of each one being as different from the results in Experiment 7 by chance are around 0.1. Thus, each one on its own does not

suggest significance, but when taken as a whole there would seem to be an indication that the present subjects do on the whole show larger effects of bias than the other subjects.

It had been noted in Experiment 3 that there was a correlation between the difference in latency of easy and difficult trials and the magnitude of bias. This was not observed in the later experiments, so it was felt that perhaps the relationship was a spurious one because some of the subjects were performing quickly, and since there was very little difference in RT's between easy and difficult trials when subjects respond quickly, very little bias would be expected. It is possible that the correlation noted was confounded with a second factor that is correlated with reaction time at that speed, i.e. the difference in accuracy between easy and difficult trials. It can be noted that the differences in accuracy have remained fairly steady over quite wide ranges of instruction. The exceptions have been subjects performing very fast, and those who are performing near to an asymptotic level, as in Experiment 5, when asked to be slow.

In this experiment there has been a fairly large increase in the difference between the two difficulty levels, the average now being around 29% compared with differences of around 20% for all the other experiments except the first where the difference was around 24%. On the other hand, the difference in latency between easy and difficult trials has been matched and exceeded before without a commensurate increase in bias. Thus, if any factor alone can be related to the increase in absolute bias, it would seem to be the actual difference in difficulty.

Discussion

It had been hoped that this experiment would give a fairly clear cut result with regard to the relationship between bias and the actual measurable difference between easy and difficult stimuli. The results were however rather ambiguous. If the absolute values are considered, the differences in RT between easy and difficult bias runs are greater on average than those in Experiment 3 - in which the difficult stimuli were the same as in this experiment, so a comparison can be made. It is clear that, as in the other experiments, subjects did adjust their performance according to some perception of the bias.

That these strategies do not necessarily correspond with the local effects noted, again points to the presence of separate adjustments in criteria for local and global levels of difficulty. This distinction between local and global effects was noted previously in Chapter 4, and resembles a difference found by Granjon and Reynard (1977) between two parameters governing effects of repetitions of foreperiods on subsequent trials in simple RT experiments. Subjects were influenced by the immediate repetition of the same foreperiod (a local effect) but also by their knowledge of the probability of repetition (a global effect).

We have seen that increasing the difference in difficulty between the two kinds of displays also increases the absolute level of the effect. The direction of the effect however is more variable. In Experiment 1 the direction of the local effect on difficult trials was the reverse of that on the easy trials. This was noticed in this experiment. In fact many of the local effects (12 out of the 16) were not negative.

Of those that were negative (RT's faster after difficult trials) only one affected difficult trials. It would appear therefore that the negative effect of Experiment 1 was not due simply to the difference in difficulty of the two stimuli.

We do not know how the strategy employed by the two "negative effect" subjects differs from that used by the others. We can only conjecture, following a comment made by one subject in Experiment 5, that in the easy bias condition they were determined not to get any responses wrong and so responded more slowly and carefully. This strategy should lead to a greater degree of accuracy in their easy runs. This was noticed for subject MM but not for ET - who, in fact, made more errors to difficult displays during runs of easy bias than during runs of difficult bias. It is then certainly possible that this was the strategy of MM, but she was not asked as the problem had not been noticed at that time. It is interesting to note that if this were MM's strategy, it did not affect her being influenced quite strongly by local bias, in which three out of four of her sample size analyses showed positive bias effects.

The data for subjects JC and ET are consistent with the hypothesis that they are using one criterion for both levels of difficulty. It would also seem that JD in difficult bias runs was using one criterion. In all these cases the trends for the sample size analyses are moving in similar directions -

as RT's to easy trials become faster so also do RT's to difficult trials. For the other two biases in these two subjects the reverse is true, though the trends are reversed. For JD in the easy bias runs, difficult discriminations were made relatively faster and easy discriminations slower when following a high proportion of easy discriminations. For MM, in the difficult bias runs, easy discriminations were relatively faster but difficult discriminations slower following a high proportion of easy discriminations. This is the same trend that was seen in Experiment 1, which it was felt was due to the greater difference between easy and difficult displays. It was proposed that the greater difference would make the two more discriminable and so subjects would find it easier to use two criteria.

In the later experiments where the displays were more similar, it would be harder for the subjects to decide which criterion to use on a particular trial. It would appear from this experiment that if there is a greater difference between easy and difficult displays this is not sufficient to induce the subject to use more than one criterion, since six of the eight conditions are consistent with the use of only one criterion.

Conclusion

The difference in difficulty between the easy and difficult displays was greater in this experiment than in any of the previous ones. Also, the absolute difference in latency between the easy and difficult bias runs was greater. It can be tentatively suggested that increasing the difference between the two levels of difficulty in this line judgement task has had a greater effect on the difference in latencies between easy and difficult bias runs than the instructions to be cautious did in the earlier experiments.

This in turn could imply that subjects may well be separately aware of the difficulty of the trials that they observe without using the information of how long they take to make each judgement. This is assumed because subjects do not appear to utilise such information in altering their criteria in the earlier experiments where caution was urged.

There appeared to be no greater use of two criteria (one for each level of difficulty) with the greater difference between the difficulty levels of displays. It may be that displays have to be readily discriminable by the subjects in order for them to use two criteria, but the fact that they are made more discriminable does not on its own make them do so.

Finally, results gave rise to further evidence that the local difficulty bias effect could be caused by an accumulation of a succession of small changes in latency which are influenced by the difficulty of trial $n-2$. The direction in which latencies were affected by trial $n-2$ was generally the same as that observed in the local difficulty bias effect.

Subject	Session -			
	1	2	3	4
ET	E	D	E	D
JD	D	D	E	E
MM	E	E	D	D
JC	D	E	D	E

Experiment 7 Counterbalancing of order of difficulty within sessions.

E = 3 runs of easy bias, then 3 of difficult.

D= 3 runs of difficult bias, then 3 of easy.

Table 7.1

	TRIAL EASY			TRIAL DIFFICULT		
bias:	easy	difficult		easy	difficult	
ET	818	767	(-51)	1300	1210	(-90)
JD	643	704	(+61)	818	877	(+59)
MM	741	685	(-56)	943	772	(-171)
JC	644	692	(+48)	805	911	(+106)
Mean:	710	712		963	943	
ET	99.2	99.14		70.86	73.36	
JD	96.81	97.2		64.0	63.28	
MM	93.48	90.9		64.86	62.1	
JC	95.44	96.98		67.92	68.02	
Mean:	96.0	96.26		66.7	66.9	

Reaction times and percent correct for both biases in experiment 2. Figures in brackets represent the effect of bias on reaction time. RT in msec.

Table 7 - 2

		EASY TRIALS			DIFFICULT TRIALS		
		(n-2)			(n-2)		
bias		E	D		E	D	
JD	easy	649	630	(-19)	789	868	(+79)
	diff	693	698	(+5)	883	869	(-14)
JC	easy	650	657	(+5)	814	829	(+15)
	diff	665	682	(+17)	880	901	(+21)
ET	easy	839	795	(-44)	1234	1309	(+75)
	diff	778	740	(-38)	1230	1198	(-32)
MM	easy	749	762	(+14)	916	999	(+83)
	diff	673	689	(+16)	777	771	(-6)
Mean	easy	721	711	(-10)	938	1001	(+63)
	diff	702	702	(0)	942	935	(-7)
	mean	712	706	(-6)	940	968	(+28)

Summary of the three way interaction between the level of difficulty of trial (n-2), trial (n) and bias in experiment 7.

Table 7 - 3

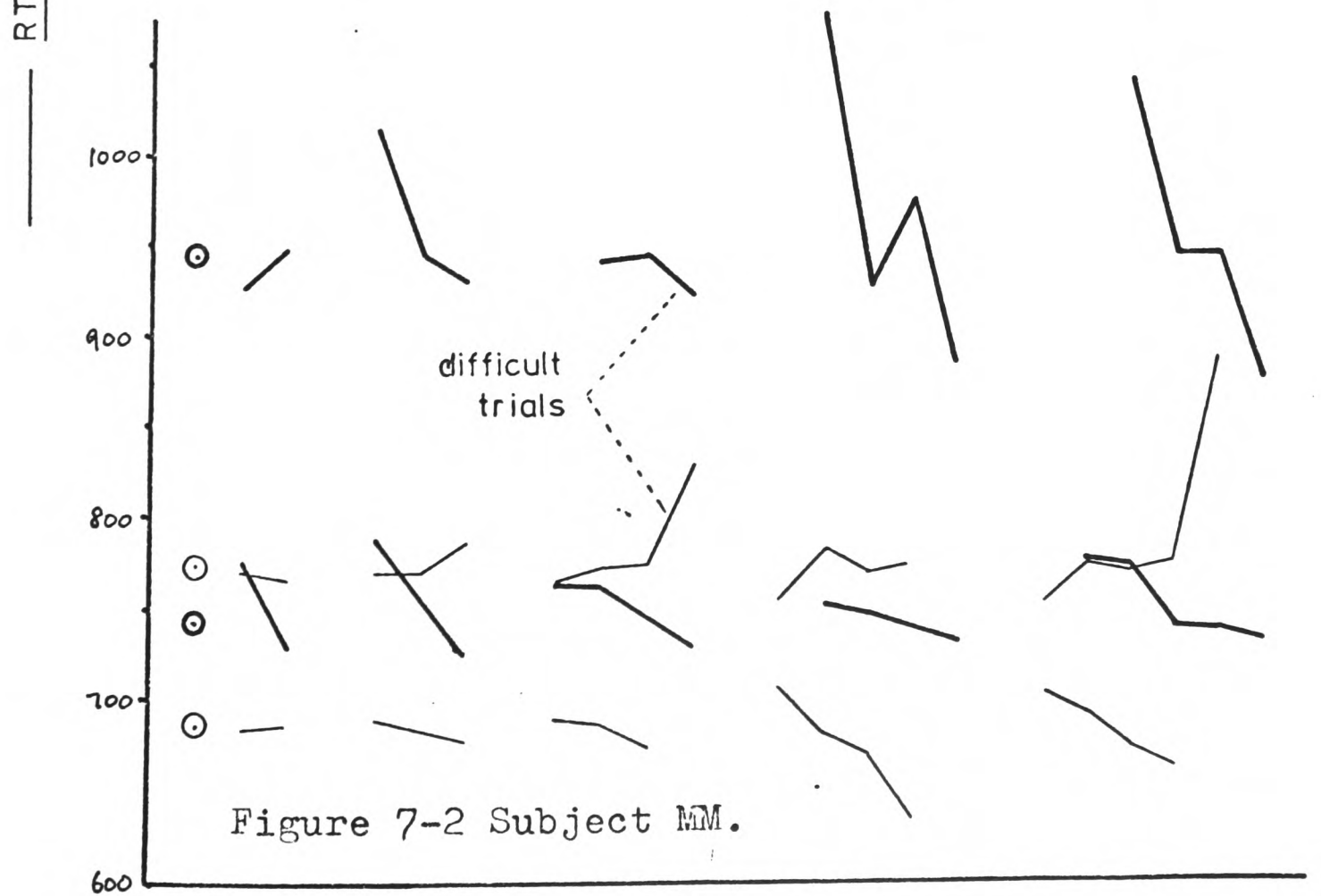
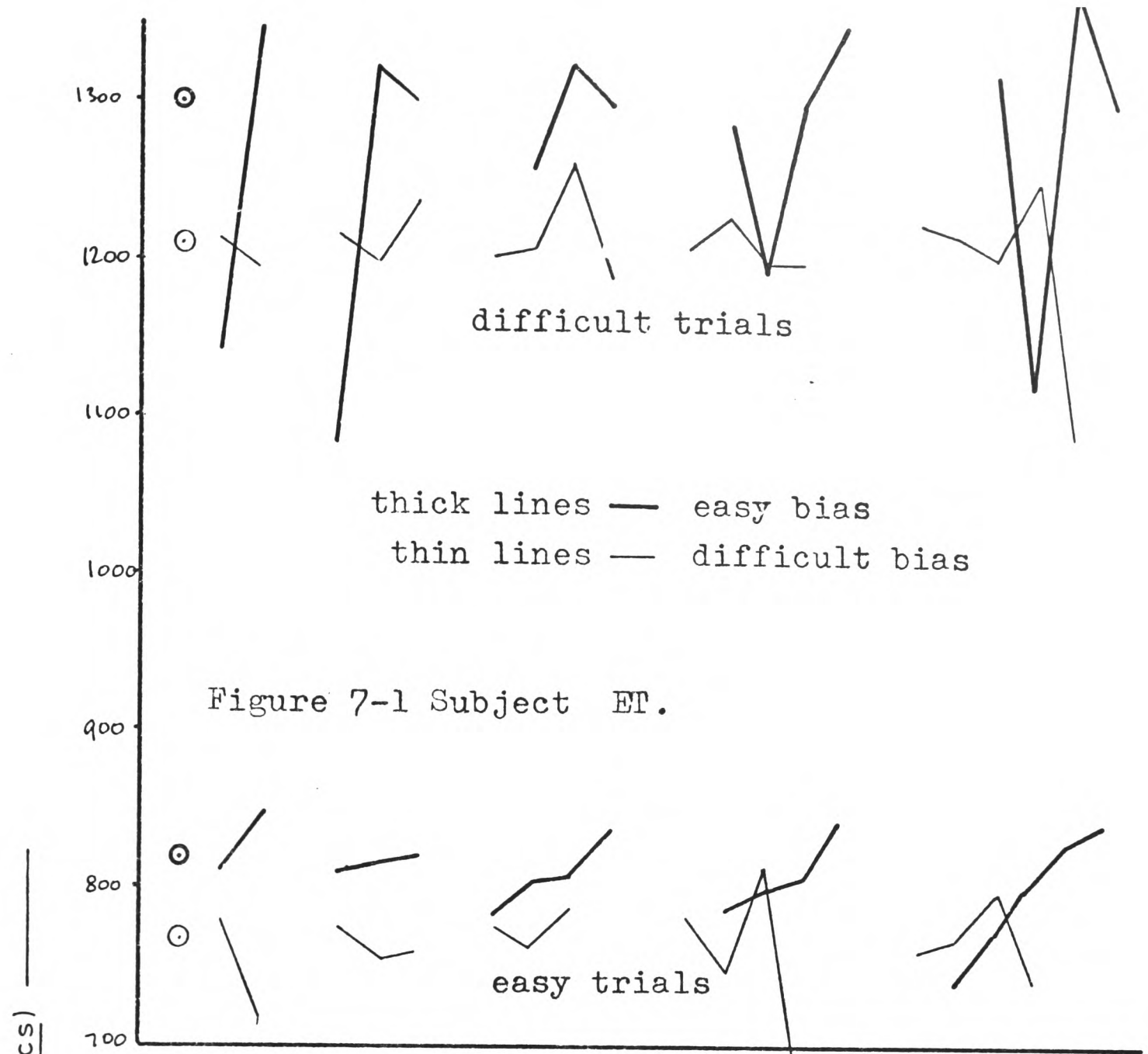
subj		(1)	(2)	(3)		(4)		(5)
		b/t	b/t	e	d	e	d	e/d
exp 3	MA	1/14	11/25	635/635		14.5/12.2		71/69
	SW	20/37	13/30	591/611		18.2/21.7		84/85
	JW	33/67	15/49	777/810		24.4/22.2		94/94
	CM	51/22	68/39	670/721		19.5/20.0		78/78
	TC	77/41	165/125	834/911		24.5/24.8		85/89
exp 4	CM	-3/64	-2/68	945/942		19.6/23.4		89/87
	MA	0/-14	17/ 3	669/669		19.3/12.0		76/74
	JW	42/225	-23/160	1752/1794		18.9/18.3		998/99.7
	SW	-56/98	-48/107	741/685		25.1/26.3		91/93
	TC	300/116	278/34	881/1181		23.4/22.4		91/89
exp 5	SJ	-1/140	-2/139	591/590		22.8/21.2		95/94
	JD	-13/23	1/37	549/536		18.9/ 6.2		83/68
	fast MM	35/63	15/43	504/539		24.0/20.7		92/93
	AK	62/123	57/118	538/600		21.8/18.8		97/96
	RB	-56/93	-120/29	570/514		20.4/24.4		88/90
exp 5	JL	23/371	-11/337	632/655		9.8/ 9.4		100/100
	WS	31/94	28/91	606/637		26.4/18.9		95/94
	slow JG	37/674	28/664	858/895		12.2/10.9		100/100
	PA	43/146	59/162	518/561		13.3/15.7		99/99
	TJ	181/423	140/384	1054/1231		17.1/17.8		99/96
exp 7	JD	61/175	59/173	643/704		32.8/33.9		97/97
	ET	-51/482	-90/443	818/767		28.3/25.8		99/99
	JC	48/161	106/219	644/692		27.5/29.0		95/97
	MM	-56/202	-171/85	741/685		28.6/28.8		93/91

Table of the relationship between easy and difficult bias and easy and difficult trials for several experiments.

Columns as follows:

- (1) b = mean latency of difficult bias - that of easy bias
i.e. effect of bias. ON EASY TRIALS
t = mean latency of difficult trials - that of easy ones.
i.e. effect of trials. IN EASY BIAS.
- (2) ditto above for DIFFICULT TRIALS AND BIAS.
- (3) Mean latencies for easy trials
e in easy bias
d in difficult bias.
- (4) Difference in error rate between easy and difficult trials.
e in easy bias d in difficult bias
- (5) Percentage correct in easy (e) and difficult (d) bias.

Table 7 - 4.



Sample Size Analyses for Experiment 7. Windows 1 to 5
Figures 7-1 and 7-2.

thick lines — easy bias
thin lines — difficult bias

Figure 7-3 Subject JD.

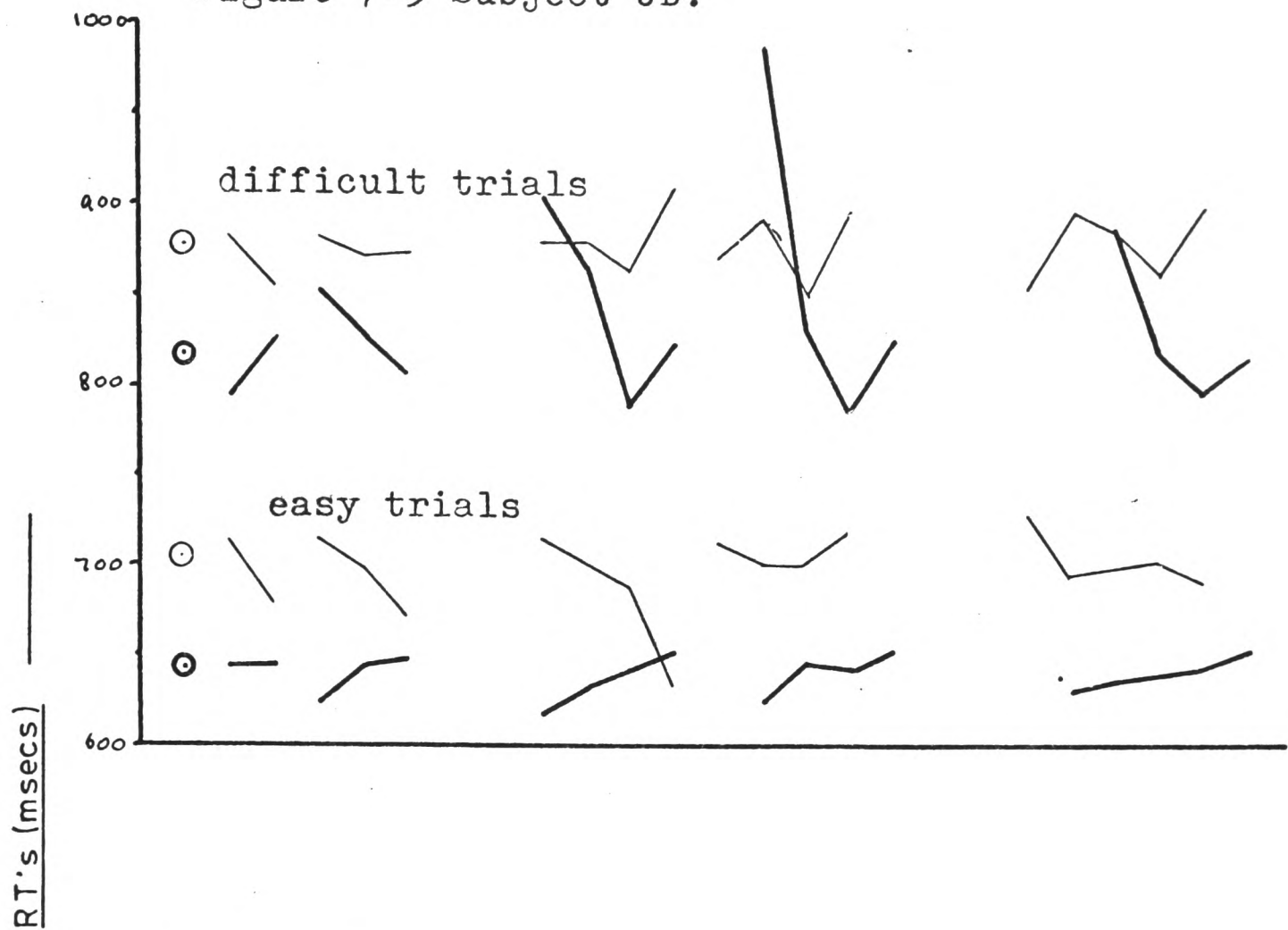
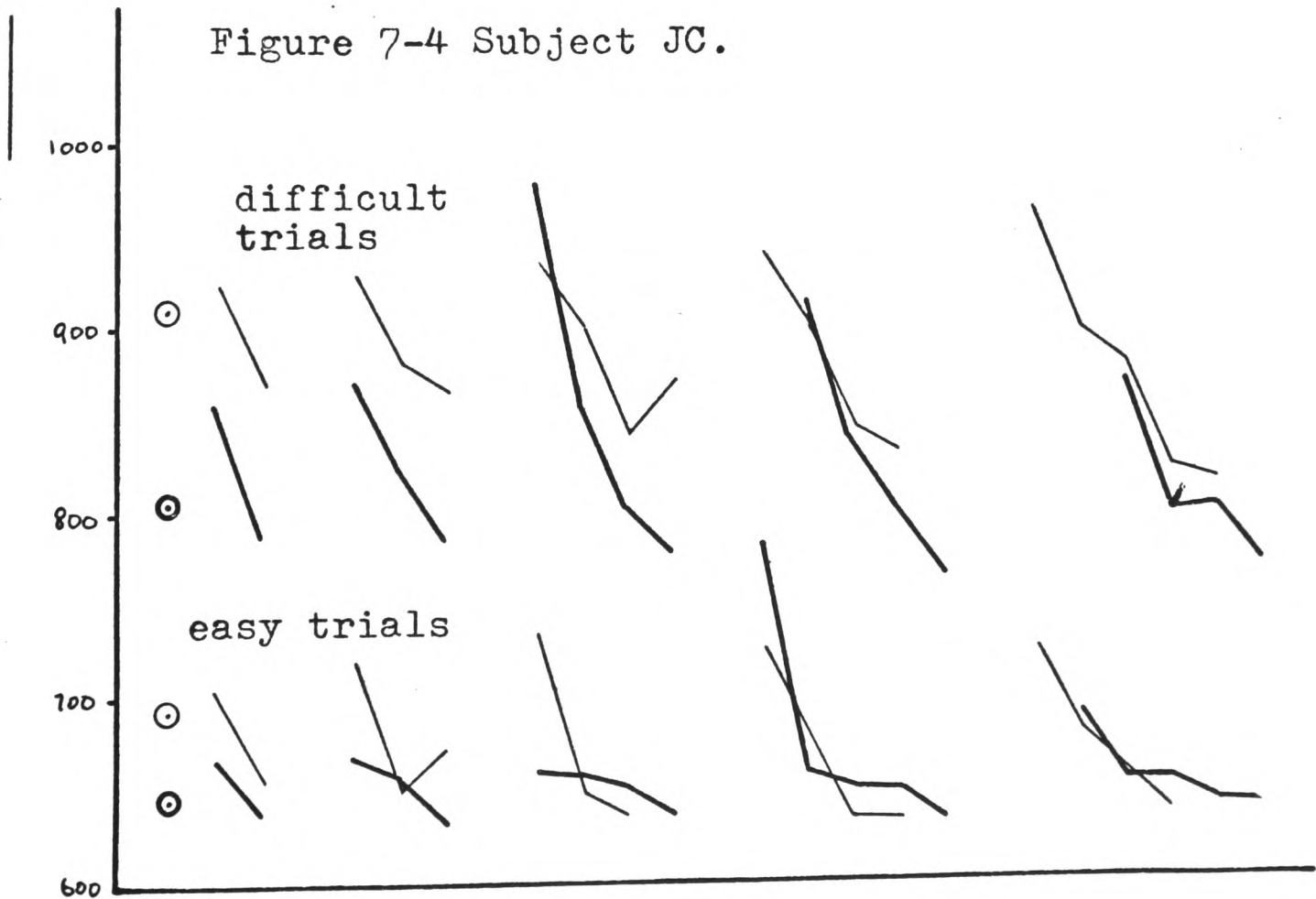


Figure 7-4 Subject JC.



Sample Size Analyses for Experiment 7. Windows 1 to 5.
Figures 7-3 and 7-4.

CHAPTER 8

THE INTERACTION BETWEEN RSI AND DIFFICULTY

EXPERIMENT 8

In all the experiments so far described, when any effect of the level of difficulty of preceding trials upon the latency of the present trial, n , was observed, the level of difficulty of trial $n-2$ had a greater effect on the latency of trial n , than did the level of difficulty of either trial $n-1$ or $n-3$. This is curious, since intuitively one might suppose that the level of difficulty on any one trial would have its maximum effect on the latency of the immediately following trial (i.e. that trial $n-1$ would maximally affect the latency of trial n).

As previously mentioned, Drazin (1961), and several others since, have shown that when foreperiods are repeated, the trial that mostly influences the reaction time to the present trial is trial $n-1$. Drazin found some, but very little effect of trial $n-2$. John (1967) in a simple reaction time experiment and Holland and Lockhead (1968) - as qualified by Jestadt et al (1977) - have shown that the amplitude of trial $n-1$ influences the latency to the present trial. If sequential effects are searched for, the assumption is that the effect will be maximal at trial $n-1$, and will reduce as the distance between events increases.

It was suggested earlier that the effect may be due to the relatively short RSI that was being used throughout the experiments of 200 msec. It seems unlikely that this RSI

alone would account for the effect as Rabbitt (1969) found that trials immediately following errors were slower than average when the RSI being used was as short as 20 msec, and also when RSI's were 120 and 220 msec. This would imply that the subject is capable of adjusting his criteria rapidly in the event of error.

One possibility is that when the discriminations between stimuli are relatively difficult, as they are in these experiments, subjects take longer to become aware that an adjustment is needed. This could be because it takes longer for them to notice that the level of difficulty of trials has changed. One difficulty which these experiments raise for all models is that we must not only indicate how subjects find some discriminations more difficult than others, but must further consider how it is that they become aware of the relative difficulty of the various decisions they make and adjust their speed accordingly. For instance, with an Accumulator Model we might suggest that subjects slow down their performance when they notice that the values in the two accumulators are similar, as this might imply to the subject that the decisions are difficult. This kind of decision may take the subject some time to make.

In order to see if something of this nature is occurring, the RSI's were varied. At longer RSI's the subjects would have plenty of time to consider whether they need to move their criteria.

Also, since the effects that have been noted have only occurred slowly - or at least are only seen in analysis when several previous trials are considered - it may be that the

subject is aware of local context by his memory of how difficult the preceding trials were. This could mean that he is aware of the preceding sequence. If this were the case, then lengthening the RSI should reduce the number that the subject can remember, and either remove the effect or make it more immediate.

Another possibility is that the subject only remembers the criterion level itself and that this gets altered by a small amount on each trial, but that our post hoc analysis only picks up the effect after several trials because of sampling error. If this is the case, then the effect of increasing the RSI should be minimal.

METHOD

Subjects:

There were 10 subjects aged 20 - 30 years. None had taken part in any other of the experiments in this report.

Apparatus and Stimuli - Subjects were run using the same testing room, conditions and stimuli as were described under Standard Experimental Setup in Experiment 2. Both levels of display difficulty occurred equally often. There were two differences from the Standard Setup:

1. The Response-Stimulus Interval was varied between runs. It was always the same value during a run. The three values used were 500 msec, 1500 msec and 4000 msec.
2. There was no time limit of four seconds imposed on the response. Timing was accurate to 10 msec and the subject had to respond within 40 seconds.

Procedure - Each subject attended for three sessions of about one hour duration. During each session they were presented with three runs of 200 stimuli. Each run was at a different RSI and the order in which these were presented was counterbalanced across subjects, as shown in Table 8-1.

Instructions - "Please press with your index finger the key to the side of the longer line. The display will remain until you respond. Do not take too long over each response, but please try to be accurate."

Results and Discussion:

In all the following, subjects' results from all three sessions were used. An analysis of variance across all 10 subjects was performed, examining the effect of the difficulty level of the previous two trials on the present one. See Appendix: AN8-1.

There were three significant results:

1. Current trial level of difficulty ($F 42.976$ df 1:9)
2. RSI ($F 3.759$ df 2:18) See 3.
3. An interaction between present trial difficulties and RSI ($F 11.115$ df 2:18). This indicated that as RSI increased, so the RT to both easy and difficult trials increased, though the easy trials were affected less than difficult ones. This general finding is compatible with earlier findings that as overall RT increased so RT's to difficult displays increased by more than RT's to easy displays.

We might expect that as R-S interval increases, subjects become less able to precisely predict the moment of onset of each sequent display. Subjects have difficulty in estimating

elapsed time as interval durations get longer. This alone should lead to greater variability for the signals preceded by longer RSI's and to longer response times, but this effect should be the same for difficult and easy trials. We have seen that the effect was greater for difficult trials. This might mean that the criterion was altered to a more cautious level by the presence of a longer RSI.

There were no significant effects of earlier trials. If the effects of earlier trials were examined, then those which come closest to significance were ACE : this trial E/D x N-2 E/D x RSI ($F 1.91$ df 2:18), and BC : N-1 E/D x N-2 E/D ($F 2.95$ df 1:9). Once again the only effect showing any influence on current RT was trial N-2.

The analyses were repeated separately for the different RSI's with much the same results. See Appendix: AN8-2, -3, -4. In all cases, the effect of current trial difficulty was significant. The other values were:

1. For short RSI: The interaction between the difficulty of trial N-1 and the difficulty of trial N-2 had an F value of 2.92 df 1:9. All others < 1 .
2. Medium RSI: The interaction of current trial difficulty and the difficulty of trial (N-2) $F 2.597$ df 1:9. All others < 1 .
3. Long RSI: This current trial difficulty interaction with the difficulty of trial n-1 $F 1.68$ df 1:9. Others < 1 .

Thus, very tentatively, if there was any trend at all it was that trial N-2 had some small influence on current RT for the shorter RSI's, but for the RSI of four seconds, trial (N-1) had some influence. These values, significant only around 20% level, can only be used as pointers, but the influence of trial N-2 has been 'pointed to' now in Experiments 2, 3, 6 and 7. It is suggested therefore that the difficulty of trial N-2 in these experiments had an influence on the RT of trial n that was rather small and so below the level required for the error terms being used.

Sample Size Analysis

If there was some influence of the difficulty of earlier trials on the latency of later trials, this would show up in a Sample Size analysis. The latencies of trials were averaged according to the number of easy or difficult trials that preceded them within various window sizes. The analysis was performed separately for window sizes of one to six trials, with the data being averaged across all subjects. The results of the analysis can be seen in Figures 8-1, 8-2 and 8-3.

500 msec RSI

For window sizes of one to four, difficult trials became slightly faster as the number of easy trials in the previous group increased. Above a window size of 4, difficult trial RT's were slower after higher proportions of easy trials. Easy trial RT's were relatively unaffected over window sizes of one to three, but when window sizes of four or more were considered, easy trial RT's became faster as the number of easy trials in the window increased. However, both easy and difficult trial RT's were relatively faster after a pure run of three or more difficult trials.

1500 msecs RSI

Both easy and difficult trial RT's were strongly affected by the difficulty of the preceding trials. As the proportion of easy trials in the preceding sequence increased, so both easy and difficult trial latencies became faster. Again, difficult trials were relatively fast after a pure sequence of three or more difficult trials.

4000 msecs RSI

Difficult trial latencies were faster after sequences with high proportions of easy trials. Easy trial RT's for window sizes of one to three were slower after sequences with high proportions of easy trials, but for larger window sizes showed little effect of previous difficulty until, at window size six, easy trial latencies became faster as the proportion of easy trials increased.

This experiment illustrates that criteria were still being influenced by level of earlier trial difficulty when long intervals elapsed between trials.

The simplest hypothesis would be that a single criterion had a value that was being adjusted from trial to trial according to the level of difficulty of the preceding trials. As a higher proportion of difficult trials were presented, so the criterion shifted towards that appropriate for more difficult discriminations. If the time between trials was increased, then the process should have been relatively unaffected, except that the criterion itself may have become more variable because it might drift during the period between trials. This would seem to be supported by the findings of this experiment, with the additional caveat that as

the time between trials increased so the criterion appeared to drift towards greater caution. This was supported both by the increased RT and by the concomitant increase in accuracy after the longer RSI's.

In the Sample Size analysis for an RSI of 500 msec, the latencies for difficult trials were seen to get longer as the proportion of easy trials increased. Conversely, easy trial latencies were shorter after high proportions of easy trials. This result is similar to those in Experiment 1 and also in Experiment 6 when the subjects were being cautious. It was considered to indicate that the subjects were using two criteria, and that the criterion not being used at any one time would drift towards a level of greater caution. This hypothesis of the drift of the criterion in the direction of caution receives support from this experiment since, as we have seen, as the RSI increased so subjects responded more cautiously (i.e. more slowly and accurately).

The increase in latencies for trials after long RSI's was not simply due to a greater uncertainty as to when the display would appear, as this factor should have affected easy and difficult displays similarly. In this experiment, difficult trial latencies were affected more than easy trial latencies and, as the latencies increased, so the error rates dropped for both easy and difficult displays. Both these facts were consistent with a change in criterion towards a more cautious level.

Also, there is the suggestion that for the very long RSI's - those of four seconds - the level of difficulty of trial $n-1$ began to have more effect on the latency of trial n than did the level of difficulty of trial $n-2$.

This experiment has demonstrated that the local bias effects of difficulty that have been noted throughout the experiments in this report are still present with response-stimulus intervals of one and a half and four seconds. In fact, the effect appears to be as strong at the longest RSI as was first noticed at RSI's of only 200 msec. Again, the effect seems to build up as one takes larger sample sizes. This, together with the fact that it is present over long RSI's, supports the suggestion that it is a small cumulative effect which gradually builds up over a number of trials.

The question was raised earlier as to whether subjects monitor their performance by remembering the level of difficulty of the preceding trial. Since the effect appears to be similar over such a wide range of R-S intervals, it seems unlikely that this is the case, as we would expect the level of difficulty of the preceding trials to be increasingly forgotten as the interval between successive trials increased.

The slight support - and it must be emphasised that there is no real evidence for this - from the analysis of variance for a change of influence from trial $(n-2)$ to $(n-1)$ when the RSI was four seconds would imply that subjects did need some considerable time, per se, to establish that a task was more or less difficult; or at least that they required time for such a recognition to be reflected in their criterial movement.

Conclusion

In this experiment, the response-stimulus intervals were varied over quite a large range and local effects of bias for display difficulty were found even at the longest RS's of four seconds. This was taken to imply that subjects do not adjust their performance in the light of a specific memory trace of the preceding sequence. Of course this is only rendered unlikely, and not impossible, because it is based on the untested assumption that such memory traces would become weaker as RSI's become longer. The hypothesis which was preferred was that subjects maintain their criteria at particular values which are adjusted according to recent trials, in particular trial (n-2). The fact that trial (n-2) was involved in marginally higher F-ratios in the short and medium RSI runs, whereas trial (n-1) was marginally higher in the long RSI suggests that subjects do take a long time to adjust criteria when the displays are difficult.

Finally, some aspects of the data were explained by the assumption that a criterion not being used in an actual test of a display will drift towards a more cautious level over time, so that RT's for both easy and difficult displays increase as RSI's become longer.

Subject	Session -		
	1	2	3
JB (S1)	S M L	L S M	M L S
SE	L S M	M L S	S M L
SB (s5)	M L S	S M L	L S M
TC (S6)	S L M	M S L	L M S
JGS (S7)	L M S	S L M	M S L
PP (S8)	M S L	L M S	S L M
DJ	S M L	M L S	L S M
GR	S L M	L M S	M S L
JT	M S L	S L M	L M S
DG	L M S	M S L	S L M

Experiment 8 Counterbalancing of order of RSI's.

S = 500 msecs RSI.

M = 1500msecs RSI.

L = 4000msecs RSI.

Table 8.1

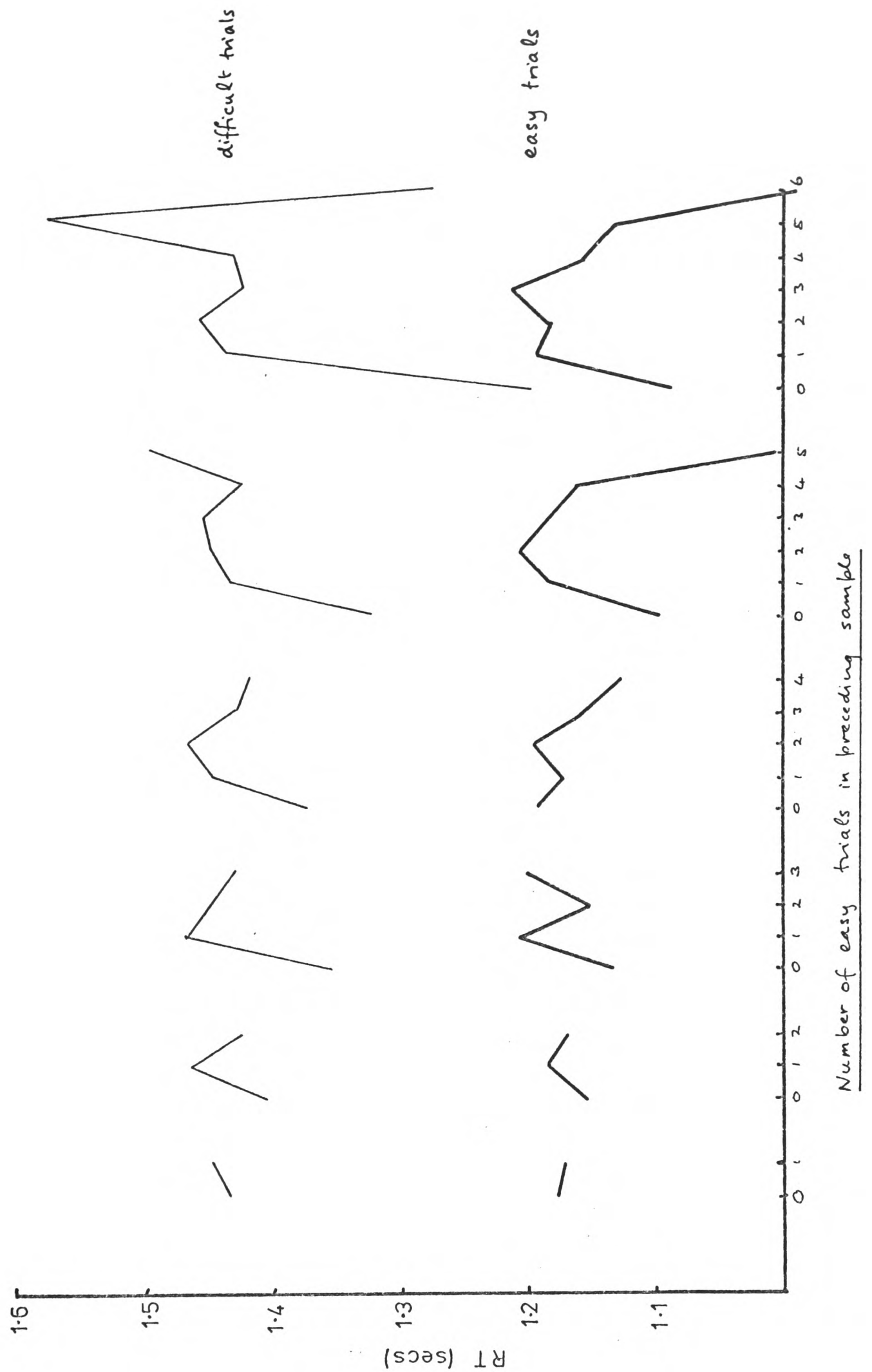
RSI	EASY TRIALS	DIFFICULT TRIALS
500	1177	1438
1500	1310	1613
4000	1386	1797

Mean RT's for experiment 8 for the three RSI's.

RSI	EASY TRIALS	DIFFICULT TRIALS
500	93.1	70.6
1500	94.3	72.0
4000	95.7	73.1

Percent Correct for experiment 8.

Table 8 - 2.

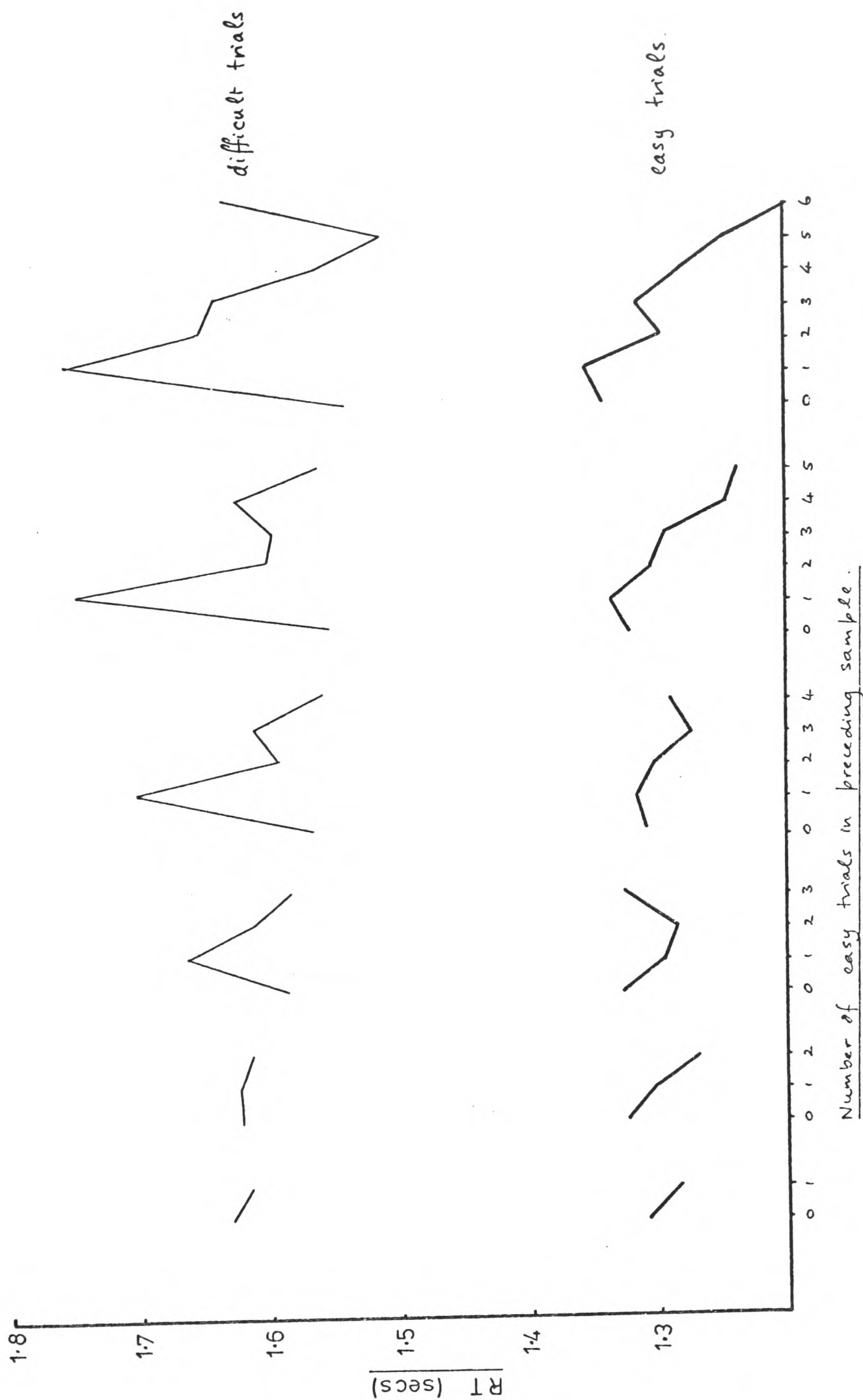


Sample Size analysis for Window sizes 1 to 6.

Experiment 8

RSI = 500 msecs.

Figure 8 - 1

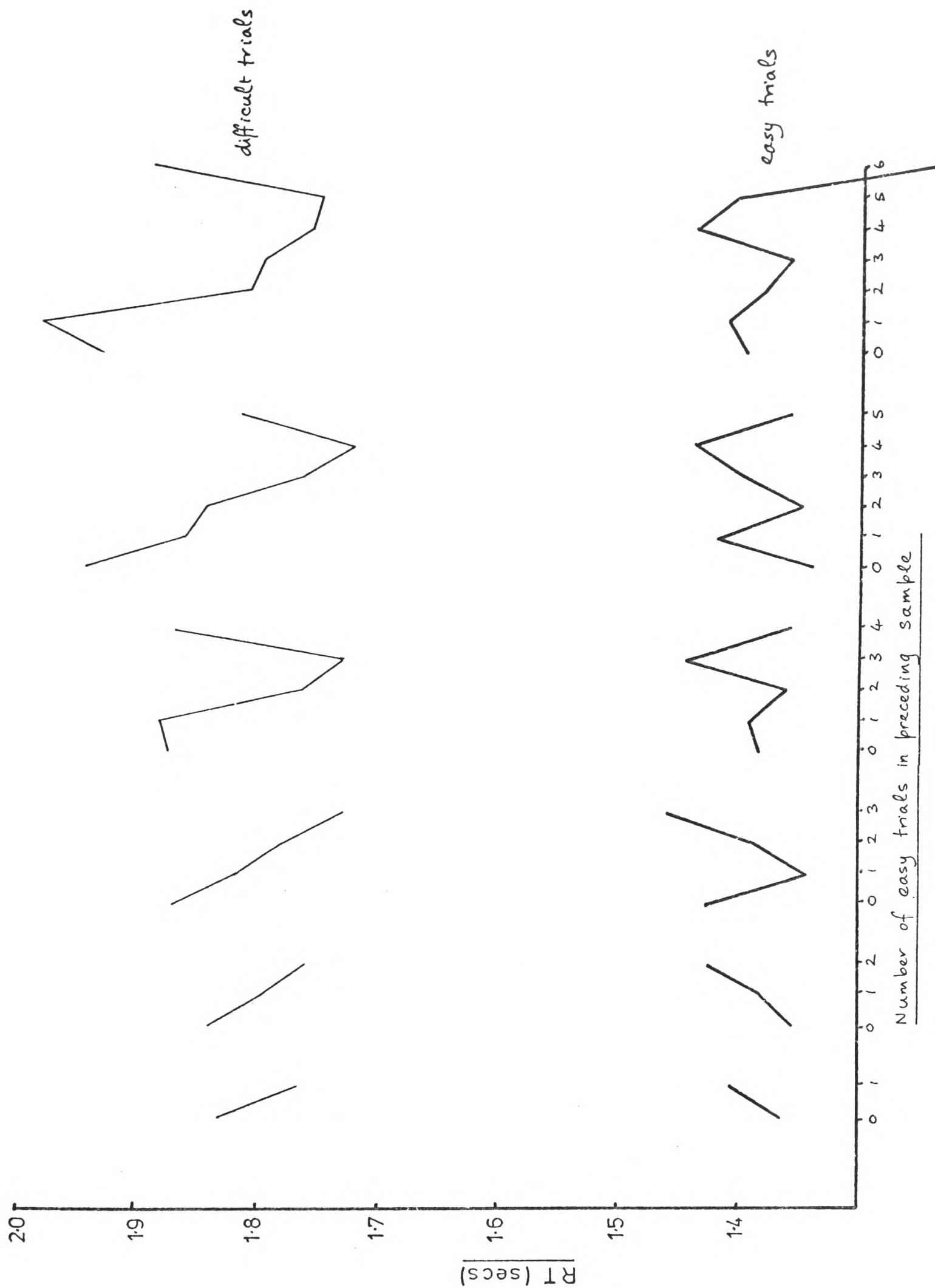


Sample Size analysis for Window sizes 1 to 6

Experiment 8

RSI = 1500 msecs.

Figure 8 - 2



Sample Size analysis for Window sizes 1 to 6

Experiment 8

RSI = 4 seconds

Figure 8 - 3

CHAPTER 9

THE ROLE OF ERRORS IN THE ADJUSTMENT OF CRITERIA

We shall now return to a problem briefly mentioned earlier. How do subjects actually regulate their performance - do they need to be aware of their errors, or can they adjust their criteria according to their perception of the level difficulty of the trials even when no errors occur?

Some authors, such as Rabbitt and Vyas (1970), have suggested that in order for subjects to maintain fast and accurate performance they must detect the errors which they commit and avoid the (faster) RT bands at which errors are more probable. On this model a subject may become aware of the average level of difficulty of a run of trials simply by monitoring the frequency of his errors. The data we have discussed suggest an alternative possibility. It seems that, independently of their detection of errors, subjects may be directly sensitive to the level of difficulty of particular trials and to the mean level of difficulty of runs of trials. They may thus adjust their criteria accordingly to their perception of trial difficulty, independently of whether they make errors or detect the errors which they make.

There are three ways in which we shall explore this problem, First, some of the previous experiments will be examined for the direct effect of errors. Second, some of the data will be observed in detail to see if there are correlations between successive trial latencies. It could be considered that if subjects are adjusting their criteria over a period, then RT's for successive trials should be correlated if the same criteria

have been used during a run. Third, some of the Sample Size analyses will be re-computed with all errors omitted from the calculations to see what happens when only correct trials are considered.

The Effect of Errors

It has been noted in several experiments that following a series of difficult trials the latencies of both easy and difficult trials become longer. Such adjustments in RT are typically observed after errors. Rabbitt (1966b) reported that in a task in which subjects were required to correct their errors, the response following the correction response was slowed, though latencies quickly returned to normal over the next few responses. The nature of Rabbitt's equipment, however, forced feedback on the subjects, ^{the stimuli remained on} as, until the subject had made the correct response. Kinchla (1966) and MacDonald (1974) have shown that feedback is an important influence in sequential effects.

Rabbitt (1966a) demonstrated that subjects could detect most of their errors without feedback from the experimenter in a task with highly discriminable signals. Rabbitt (1969) went on to show that RT's to trials following errors were still slower than average RT's when feedback was not provided by the experimenter, and when subjects were not required to correct their errors. Thus there is good evidence that, when signals are highly discriminable, subjects are aware of their errors, and that they regulate their performance after making errors.

The "learning theorists" - such as Kac (1969), Thomas (1973) and Ambler (1976) place a strong emphasis on criterial movement contingent upon the detection of errors. They make the assumption

that subjects will be aware of their errors - usually through feedback from the experimenter - and will adjust criteria (of the kinds postulated by Signal Detectability Theory) towards more caution after making an error, and towards less caution after making a correct response.

Rabbitt and Vyas (1970) have suggested that there are two main classes of error - Perceptual and Motor. Perceptual errors are incorrect identifications of the stimuli, which are probably due to the inherent difficulty of the task. It is unlikely, they suggest, that subjects can identify such errors and correct them. Motor errors may occur after a correct identification of the stimulus, and may be due to an incorrect response being chosen through response bias, motor programming failure, or motor confusion. It is very probable that most of these errors can be detected and corrected. These errors are most likely to occur when subjects respond rapidly and these response errors are probably faster than perceptual errors (Rabbitt and Vyas 1970).

In the tasks described in this report, with the exception of the easy trials of Experiments of 5 and 7, the discriminations were relatively difficult. Also, subjects responded fairly slowly - in Experiment 2, for example, mean RT's ranged from 900 to 1600 msecs. Under these conditions we might suggest that few motor errors occurred and that most errors must have been due to perceptual misidentifications. This is probable because, even when subjects in Experiments 4 and 5 were asked to be especially careful, errors continued to occur. If, in all these experiments with difficult displays, most errors were in fact due to perceptual confusions, and subjects were consequently unaware of them, the occurrence of errors might be expected to have

relatively little influence on their performance. Data from three experiments were examined to see if errors, when they occurred, were followed by slowing of sequent responses, as reported for experiments with very discriminable stimuli by Rabbitt (1966, 1969).

In Experiment 2, both easy and difficult displays were difficult to discriminate. In consequence it was felt that most errors would be perceptual and so would not affect the data very much. RT's for error responses were in general slower than those for correct responses - see Table 9-1. However, when considering easy displays, error responses were faster than correct responses by two of the subjects, one of whom showed the same trend in responding to difficult displays. RT's for trials immediately following an error to an easy display were in general faster than the average RT for trials following a correct response to an easy display; for one subject only, the converse was true.

In contrast, most subjects did not alter speed on trials immediately following an error to a difficult display. One subject slowed down considerably (200 msec) on the second trial following an error to a difficult display.

Thus it can be seen that there was some disruption of reaction times after errors to easy displays, but relatively little disruption after errors to difficult displays. This is consistent with the hypothesis that subjects are much less aware of making errors to difficult displays than to easy ones. This would fit Rabbitt's hypothesis that motor errors are usually detectable, but that perceptual errors are not. However, it is unlikely that many of the errors were motor errors, as their latencies were on average longer than the overall mean latencies. One possibility

is that the subjects were aware of some of their perceptual errors. One objection to such an hypothesis is that subjects did not slow down after their errors. Instead, the RT's to the trials immediately following an error were rather faster than RT's to trials immediately following a correct response.

A possible explanation might be that as the errors to easy displays were slower than correct responses to easy displays, and if it was not realised that they were errors, subjects would have sped up subsequent responses because they considered that they were responding too slowly. However, this could not be so, because one would also expect responses following difficult trials to be faster for the same reason. In addition since errors to difficult displays were slower than correct responses to difficult displays, these also should have affected the data, but it appears that they did not do so.

One or two things need mentioning at this point. The apparent speeding up of responses after errors may be due to the comparisons made within the data. RT's for response to trials E+1 and E+2 were compared with RT's to trials following correct responses. This could well bias the data, as if errors occur more often when subjects are being fast, then E+1 and E+2 would occur within a run of fast responses so that their latencies might reflect the average latencies for such fast runs. That this may not be the case is suggested by the fact that the errors were, on the whole, slower than the overall mean RT's for correct responses. It would have been reasonable to have supposed that if it had been the case, then the errors would themselves also have been fast. Certainly E+1 and E+2 latencies were on average much faster than the errors that preceded them. In the case of the two subjects whose errors

to easy displays were faster than their correct responses, responses to trials E+1 and E+2 were similar in latency to their errors, and faster than their correct responses. Second, if their E+1 and E+2 responses were fast because they occurred in sequences of fast responses, then, if we suppose that difficult trials are affected in the same way as easy ones by their context, trials following errors to difficult displays should be similarly biased. We have already seen that they were not. Third, no allowance was made for the nature of the display for E+1 when mean E+2 RT's were computed. It was hoped that the different difficulty levels would average out, but if this was not so, the data might reflect this random factor.

It had been thought possible that a greater effect would be observed where the easy displays were much easier, as in Experiment 7. A summary of the transitions involving errors is shown in Table 9-2. The effect appears smaller. RT's for errors were not on the whole significantly different from correct RT's. One problem with the analysis is that relatively few errors were made to easy displays. Errors to difficult displays again appear to have little effect on the RT's of trials following them.

Finally, the effect of errors on sequent display RT's was examined in Experiment 5. The mean latencies for trials up to eight places after an error were calculated. This was done separately for easy and difficult errors and an error of the group being counted was not allowed to be included in the sequence twice. That is, trials were labelled by the number of trials since the last error in the group being examined. The calculation was continued for up to eight trials from the error. Errors made to displays of the other difficulty level were ignored.

The results for separate subjects up to four trials after an error to an easy display are shown in Table 9-7 and the mean result, together with regression lines, plotted in Figures 9-4 and 9-5. The regression lines were calculated as an integral part of the analysis and did not include the original error. It was arbitrarily decided to calculate the regression lines over all eight trials to see whether there were any trends in RT for responses following errors. If correction of the sort referred to by Rabbitt is occurring then it could show up as a negative slope after the error, with the left end of the line pointing above the error latency. It perhaps should only be calculated for a reduced number of trials as it was noticed by Rabbitt that performance returns fairly quickly to normal but it was felt that the analysis actually performed would probably show up any trends which might occur in responses immediately following errors.

If Figures 9-4 and 9-5 are examined, then certain features appear to be present:

1. Errors made to easy displays were faster than the average RT's to easy displays, except for the "slow" group in the difficult bias condition.
2. Errors to difficult displays were of the same or slower latency than average RT's to difficult displays.

It could be proposed from the taxonomy of Rabbitt and Vyas (1970) that the faster errors were probably mostly motor errors, and so were more likely to be detected. Most of the difficult errors were probably perceptual misidentifications and so they were much less likely to be detected. If this was

the case, then it may be that trends can be seen which show variations in RT following (detected) "easy" errors, but not after (non detected) "difficult" errors.

If the data are again examined, there is such a trend for responses following errors to easy displays. Apart from "slow" subjects in the difficult bias condition, the regression lines point above the RT level of the preceding error, suggesting that the subjects tended to slow down immediately after errors to easy displays, and then gradually speed up again over the subsequent trials. Both regression lines for latencies to easy displays in the easy bias condition ("fast" group and "slow" group) are significantly different from zero. It is harder to see any such effect on RT's to difficult displays. There are no real trends observable and difficult trials immediately after easy errors are not slower than average. This, however, is a difficult point to assess, since, although such responses are fairly fast, they might be occurring within a fast run, and so may have been slowed down relative to the other difficult trials in the run.

Errors to difficult displays, on the other hand, did not appear to greatly influence RT's on the trials that followed them. At only one point ("fast" easy bias, difficult trials) were RT's on trials after errors slightly slower than the RT's for the errors themselves. For the rest of the time subjects did not slow down after errors to difficult displays.

In conclusion, we have seen that when displays were highly discriminable, subjects could adjust their performance after errors in the same way as was described by Rabbitt (1969). The error latencies were faster than average latencies and sequent response

latencies were slower. When the displays were a little more difficult, the occurrence of an error affected sequent RT's, but not in the same way. The errors were slower than the average RT's, and the sequent responses rather faster than average. When displays were difficult to discriminate, the occurrence of an error did not affect RT's to sequent displays.

These points imply that the maintenance of performance when the displays were difficult to discriminate was probably not controlled by detection of errors and consequent adjustments of criterion. If errors could not be detected when they occurred, subjects evidently could not use them to control their RT level, and must have used some other means to adjust their criteria during an experimental run.

Correlation Analysis from Trial to Trial

One possible factor that could be related to the above findings might be the level of confidence which a subject has about the accuracy of his successive decisions. Henmon (1911) showed that when discriminability was held constant, confidence rose as RT's became shorter. Volkman (1934) and Johnson (1939) have both shown that confidence rises as mean discriminability increases. Whether this is simply due to an overall decrease in RT, or to a combination of this factor with some awareness of average stimulus difficulty is not known.

It is possible that subjects respond faster after a run of easy trials either because they become more confident, and so relax their criteria, or because, besides becoming more confident, they also adapt to an overall response rate. This would imply that the level of confidence, or criteria, were related to the speed at which the subject responded over particular sequences

of displays. In this case we would expect that latencies of strings of successive responses should correlate highly with one another. In order to test this the data for Experiment 2 were once again analysed to obtain correlations between the latencies of responses on successive trials.

The data were partitioned to separate transitions between correct and incorrect responses on the one hand, and easy and difficult trials on the other. Thus for each calculation there were 16 categories. The correlations calculated were the Pearson Product Moment correlation coefficients between the latencies to successive trials, and were achieved by a programme written by the author. The original data were accessed by computer from magtape. Though the process was computerised, a considerable amount of work was involved. Because of this the following strategy was used. The correlations were first calculated across all sessions, and then selections were made, firstly of particular sessions, and then of particular runs within sessions in which high correlations were seen.

First, correlations were calculated for each subject across all four sessions. The results of these computations are shown in their original form in Appendix C. They can be interpreted as follows:

Group 1 are easy trials. Group 2 are difficult trials.

C refers to correct responses.

E refers to error responses.

Thus the first entry concerning Groups 11 and CC refers to correct easy trials following correct easy trials. A later entry referring to Groups 21 and EC refers to correct responses to easy trials following error responses to difficult trials.

The first entries after these identifications are N (the number in the sample) which is repeated on the next line, and the means of trials $n-1$ and n (the two groups being correlated).

The next line gives the correlation and t value, together with degrees of freedom. The final line is the best fit regression line.

Arrows indicate negative values.

Many of these correlations were significant. Next, correlations were calculated separately for the four sessions experienced by subject one. The significant correlations which involved the highest degrees of freedom (i.e. that affect most of the data in a session) occurred in session four. It would seem reasonable for the final session of subjects to contain the highest correlations, since subjects' performance should have settled and should become less variable than in earlier sessions. In view of this, the correlations were next calculated for the fourth session of each subject.

The highest proportion of significant correlations was seen in session four of subject three. The five runs within the session were then studied separately. In the first run, 17 transitions were involved in significant correlations (in which errors to difficult displays follow correct responses to easy displays $r = 0.54$; $t = 2.49$). The same story repeats itself over the other runs, with 11 of the remaining 64 comparisons producing significant correlations at the level of 5% or greater. Thus, when the runs are considered separately, the number of trials involved in significant correlations are only 157 out

of the 1000 trials of the session. In the original calculations of correlation across sessions half the trials had been correlated with one another significantly - mostly at 1% level.

The analyses of data from the other subjects produced a few significant correlations when these were calculated over their last sessions. However, no session was as prolific of significant correlations as that by subject three discussed above. Thus, when the data are examined in fine enough detail, there seems to be very little correlation between latencies on successive trials.

At first sight this is rather embarrassing for the notion that criteria are slowly changed from one trial to the next. It had been felt that this hypothesis should lead to much higher correlations between latencies for successive trials.

On the other hand, if the stimuli were being sampled in the manner suggested by several of the counting theories, there is less reason to suppose that successive latencies would be correlated. If the criteria used remained fairly constant during a run - as our other analyses have shown - then there is no reason why a slow response to a display should be followed by another slow response. The theories predict that, if the same criterion is used, RT's for responses to successive displays of similar difficulty should be distributed about the same mean interval, but they do not predict that when one response is faster than this mean the next response should also be faster than this mean. Correlations between RT's for successive responses would only be seen if the criteria changed quite considerably during a run. They would then reflect the overall changes in mean, rather than any actual correlation between latencies for immediately successive responses.

It is considered that this is the nature of the significant correlations that were seen when the correlation calculations were computed across all the sessions.

A different mechanism, which could lead to low correlations, would be for subjects to change their criteria by a large amount from trial to trial. This would lead to low correlations as a particular criterion would not remain at any one value long enough for its effect to register. This would appear to be less plausible than the first alternative (of small criterial movements) since gross and rapid criterion shifts would make it much more difficult for a subject to maintain a steady performance within, and especially across, experimental runs.

It appears that subjects did not regulate their performance by the speed of the preceding responses. It also does not appear that subjects made strings of fast responses or strings of slow responses. The correlations observed are consistent with the idea that subjects use a decision process in which criteria are adjusted by relatively small amounts from trial to trial.

Sample Size analysis without errors

When the displays used were fairly difficult, as in Experiment 2, the occurrence of errors has been shown to have relatively little effect on the RT's to sequent trials. Nevertheless, we did see some disturbance to the RT's for displays following errors to easy displays. It is also possible that the relative slowing of latencies following difficult displays could be due to the errors made to difficult displays. Sequences of trials which

a proportion of these errors, then this could lead them to slow later responses. In view of these possibilities it was decided to re-run the Sample Size analysis of Experiment 2, omitting all sequences which included errors.

The Sample Size analysis involved setting up a window that included from one to eight trials. The window of a particular size was moved along the trials of a run, and the trial immediately following the window was categorised by the number of easy trials which occurred within the window. In this particular analysis RT's to trials which followed a window in which an error occurred were not included. It was hoped that any influence that the occurrence of errors might have on the RT's to later trials would be removed by this technique.

The results of this Sample Size analysis on the data of Experiment 2 can be seen in Figure 9-1. There is still a very strong influence of display difficulty. As the number of easy displays in a particular sequence increases, so RT's to the sequent trials become faster. This clearly shows that the effect of difficulty on latencies is not caused by the influence of errors.

It was thought that this local difficulty bias effect might have been an artifact of the way in which the data had been analysed. As the size of window being used for the analysis became larger, so the required number of responses that had to be correct in sequence increased. It is possible that there may be a different tradeoff between speed and accuracy for fast and slow subjects. If the tradeoff was such that fast subjects were more highly represented when the proportion of trials that have to be correct were easy ones than when they were difficult, then they would contribute more to the means at the right hand end of the

curves, and so lead to the observed speeding of responses after high proportions of easy trials.

This was checked in two ways. First, the proportions of each subject's contribution to each point of the window size that covered eight trials were calculated. The overall proportions at each point were found and then each subject's proportion was compared with these using a one sample Kolmogorov-Smirnov test (Siegel 1956). See Table 9-3. None of the points exceeded a probability level of 0.1. Only three points exceeded a probability of 0.2, and these represented fairly central values in the windows. This indicated that each subject contributed similarly to each mean in the window size eight analysis. It was assumed that this window size was the most likely to represent the limits of variability in the subject's speed.

Second, the analyses were again calculated for each subject in turn, averaging across all four sessions. The plots for window size eight are shown in Figure 9-2. It would appear that for most subjects there was a greater effect on the RT's to difficult displays than on the RT's to easy displays. As the proportion of easy displays preceding a trial increased, so the RT's to that trial became faster for four subjects, slower for one subject, and showed very little effect for the last subject. In general the data for individual subjects did not contradict the averaged data. These subjects all clearly show the effect on latency of the difficulty level of the preceding trials.

These analyses also show that detection of errors is not the only factor leading to adjustment of criteria from trial to trial. It is unlikely that the RT's to displays which followed a high proportion of difficult displays were slower than average simply

because the preceding RT's were themselves slow (because they were responses to difficult displays). We have seen that this is not the case, because, if it were, then there would have been more significant correlations between successive trial latencies in the preceding correlation analyses.

One more point about the Sample Size analysis for Experiment 2, which we have just discussed, needs further comment. If Figure 9-1 is examined, it can be seen that, as the window size that is studied increases from one to eight, so the mean RT for the group of responses being examined also increases. This can be related to the fact that, as the window size becomes larger, so the analysis will selectively remove responses of error-prone subjects. An examination of the data shows that the subjects who made the most errors were also the fastest subjects, and so the gradual elimination of their responses from the analysis can account for the gradual rise in mean RT. We have already seen that this did not unduly bias the data in the analyses in which the local bias effect was examined, but this remains a methodological difficulty for all analyses of this kind.

Conclusion

In this chapter we have seen that when displays were easily discriminable subjects were able to adjust their performance after they made errors. It was noted that subjects immediately slowed up after fast errors to easy displays and that they then gradually resumed a faster speed. This corresponds well with the proposal by Rabbitt and Vyas (1970) that subjects can "track" their speed-error tradeoff limit by recognising the errors they make.

On the other hand, errors made to more difficult displays had very little effect on subsequent performance. It would seem that subjects were often unaware that these errors occurred.

The difference between the changes in RT's following these two types of errors might be expected if errors to easy displays were mainly motor errors, and errors to the more difficult displays were mainly perceptual errors, as suggested by Rabbitt and Vyas (op.cit.).

Errors were not the only source of feedback as to the average difficulty level of performance since when displays were more difficult subjects could adapt their performance to the level of difficulty of the preceding trials even when errors did not occur. This can be seen from the Sample Size analyses which were performed on sequences in which errors did not occur.

EASY to EASY				DIFFICULT to EASY			
C+1	ERR	E+1	E+2	C+1	E+1	E+2	
1277	2214	1136	1599	1190	1185	1224	
1318	1849	1378	1561	1288	1308	1316	
970	938	934	898	955	982	990	
1135	1040	1087	1032	1156	1152	1170	
1397	1591	1367	1322	1432	1413	1435	
1106	1239	997	1036	1011	1095	1120	
mean	1200.5	1478.5	1150	1241	1187	1189	1209

EASY to DIFFICULT			DIFFICULT to DIFFICULT				
C+1	E+1	E+2	C+1	ERR	E+1	E+2	
1733	1569	2148	1734	2159	1732	1974	
1502	1767	1639	1550	1718	1577	1468	
976	939	934	985	1001	1013	977	
1276	1126	1203	1255	1250	1276	1259	
1579	1510	1514	1607	1625	1618	1597	
1221	1215	1198	1260	1283	1260	1231	
mean	1388	1354	1439	1398.5	1506	1413	1418

Table of comparisons of mean latencies of trials immediately following correct (C+1) or error (E+1) trials with two after errors (E+2) and the errors themselves (ERR). Data given for each subject and for transitions between difficulties.

Experiment 2.

Table 9 - 1.

EASY to EASY					DIFFICULT to EASY		
	C+1	ERR	E+1	E+2	C+1	E+1	E+2
easy bias:							
ET	808	848	1086	956	878	766	729
JD	641	655	715	688	630	666	751
MM	731	772	700	600	773	779	631
JC	634	604	700	601	661	677	666
mean	703.5	720	800	711	736	721	694
difficult bias:							
ET	719	573	-	658	774	796	739
JD	677	608	731	665	708	723	697
MM	690	798	634	758	684	686	697
JC	659	585	651	547	687	738	698
mean:	686	680	-	684	713	736	708
(of 3)	(675)	(664)	(672)	(657)			

EASY to DIFFICULT				DIFFICULT to DIFFICULT			
	C+1	E+1	E+2	C+1	ERR	E+1	E+2
easy bias:							
ET	1351	1318	2043	1145	1373	1145	1341
JD	825	783	794	799	972	814	953
MM	959	874	699	987	872	785	858
JC	786	754	654	858	819	859	828
mean:	980	932	1047.5	947	1009	900	995
difficult bias:							
ET	1192	1550	790	1214	1347	1218	1220
JD	852	690	718	896	772	761	758
MM	786	1087	1054	775	885	864	884
JC	876	735	690	937	922	895	896
mean:	926.5	1015.5	813	955.5	981.5	934.5	939.5

Table of comparisons of mean latencies of trials immediately following correct (C+1), or error (E+1) trials with two after errors (E+2) and the errors themselves (ERR). Data given for each subject and for transitions between difficulties.

Experiment 7.

Table 9 - 2.

subjects -							
1	2	3	4	5	6	total	prob
1	2	0	2	2	1	8	.0037
15	4	0	1	5	4	29	.0134
54	17	2	11	14	36	134	.0621
147	70	4	23	36	81	361	.1671
196	87	19	51	75	140	568	.2628
188	121	20	38	64	145	576	.2665
114	80	9	24	26	100	353	.1634
36	16	4	8	16	30	110	.0509
3	4	1	0	9	5	22	.0102

754 401 59 158 247 542 2161

now for above as cumulative probabilities

.0013	.0050	.0	.0127	.0081	.0018	.0037
.0212	.0150	.0	.0190	.0283	.0092	.0134
.0928	.0574	.0339	.0886	.0850	.0756	.0791
.2878	.2319	.1017	.2342	.2308	.2251	.2462
.5477	.4489	.4237	.5570	.5344	.4834	.5090
.7971	.7506	.7627	.7975	.7935	.7507	.7756
.9483	.9501	.9153	.9494	.8988	.9354	.9389
.9960	.9900	.9831	1.0	.9636	.9908	.9898
1.0	1.0	1.0	1.0	1.0	1.0	1.0

.0416	.0601	.1445	.0480	.0401	.0256	D max
Values for significance:						
.0390	.0534	.1393	.0851	.0681	.0459	20%
.0415	.0569	.1484	.0907			15%
	.0609					10%

Computation of Kolmogorov-Smirnov statistic for the relative contributions of subjects to window size 8 sample size analysis on experiment 2 .

Table 9 - 3.

"FAST" easy bias						mean
	ERR	E+1	E+2	E+3	E+4	RT
RB	478	527	513	497	482	570
MM	586	558	525	567	514	504
SJ	612	701	610	566	582	591
AK	569	696	641	577	495	538
JD	501	515	539	536	565	549
mean	549	599	566	549	528	550

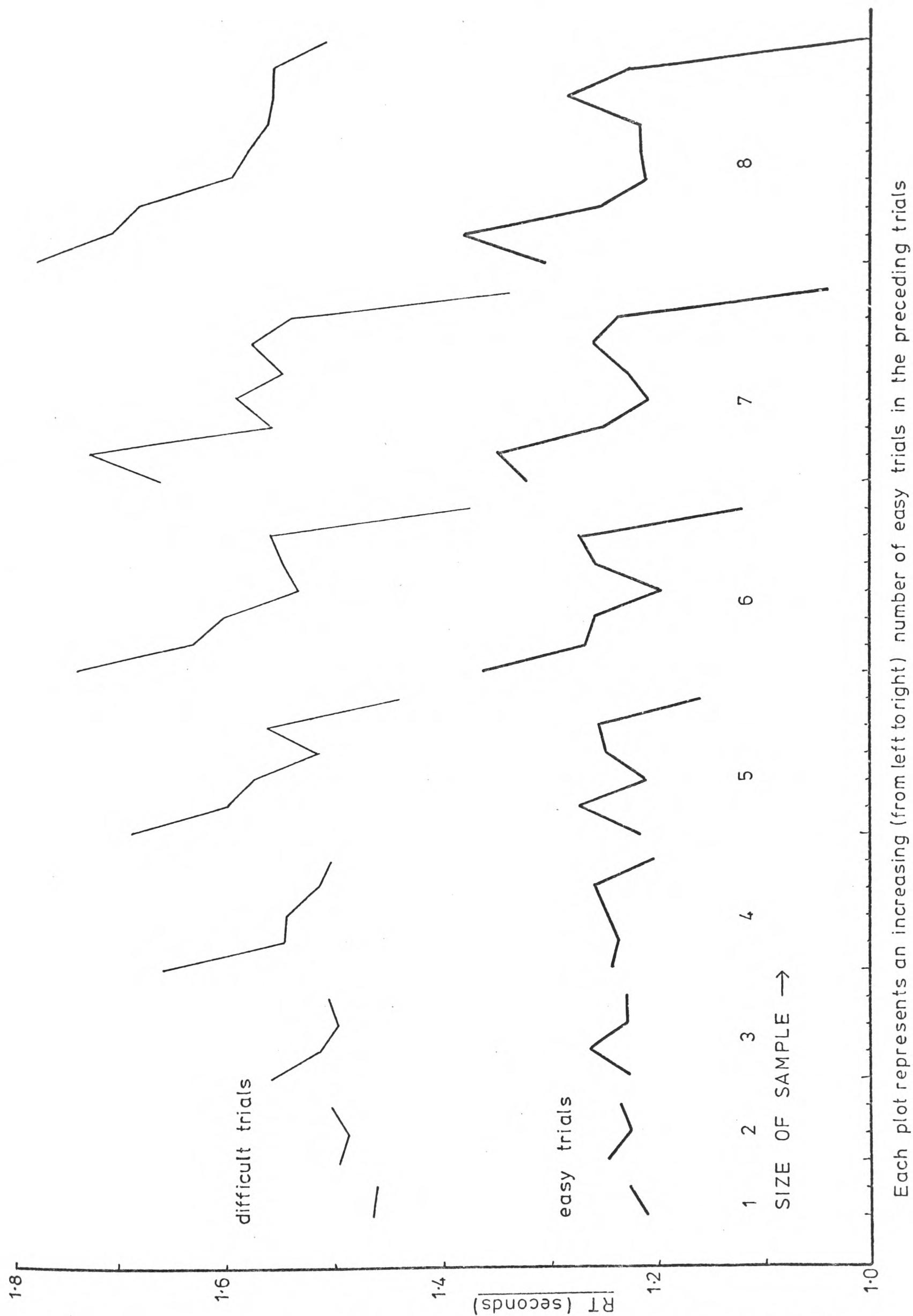
"FAST" difficult bias						
RB	421	435	440	375	499	514
MM	555	629	482	604	492	539
SJ	675	676	663	518	540	590
AK	554	-	627	566	595	600
JD	501	506	546	544	547	536
mean	541	562	552	521	535	556

"SLOW" easy bias						
JL	-					
TJ	1013	1269	1199	1076	774	1050
JG	-					
WS	696	748	649	770	637	606
PA	334	825	538	504	468	518
mean	681	947	795	783	626	725

"SLOW" difficult bias						
JL	-					
TJ	1037	1243	1231	784	1009	1231
JG	-					
WS	955	785	551	962	1052	637
PA	488	-	-	-	590	561
mean	996	1014	891	873	1031	934
not PA						

Table of mean latencies following errors (E+1) (E+2) etc and mean latencies of errors themselves (ERR) for Easy trials after Easy errors. Experiment 5.

Table 9 - 8.



Sample Size analysis with no errors in the preceding trials.

Experiment 2

Figure 9 - 1

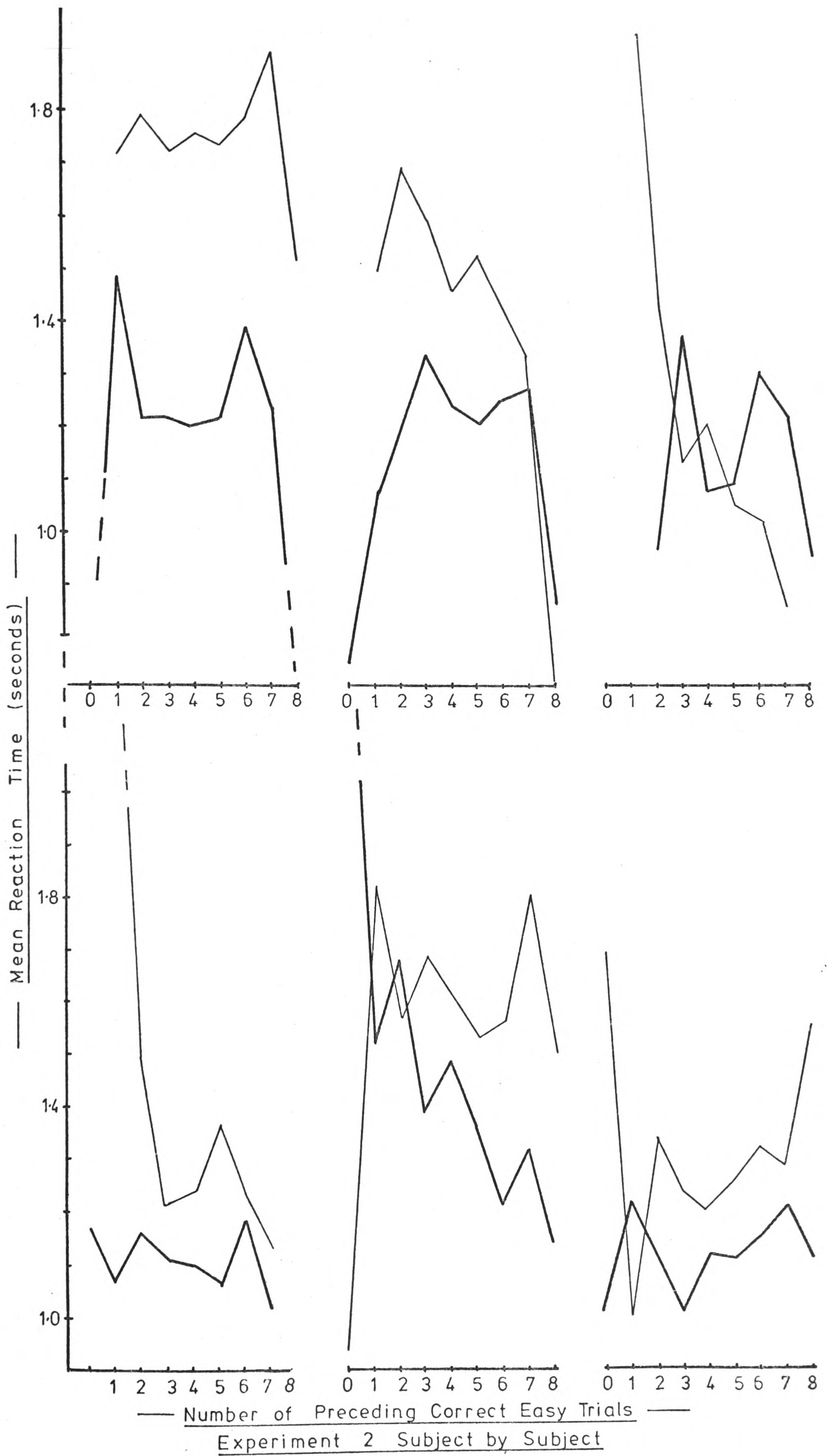
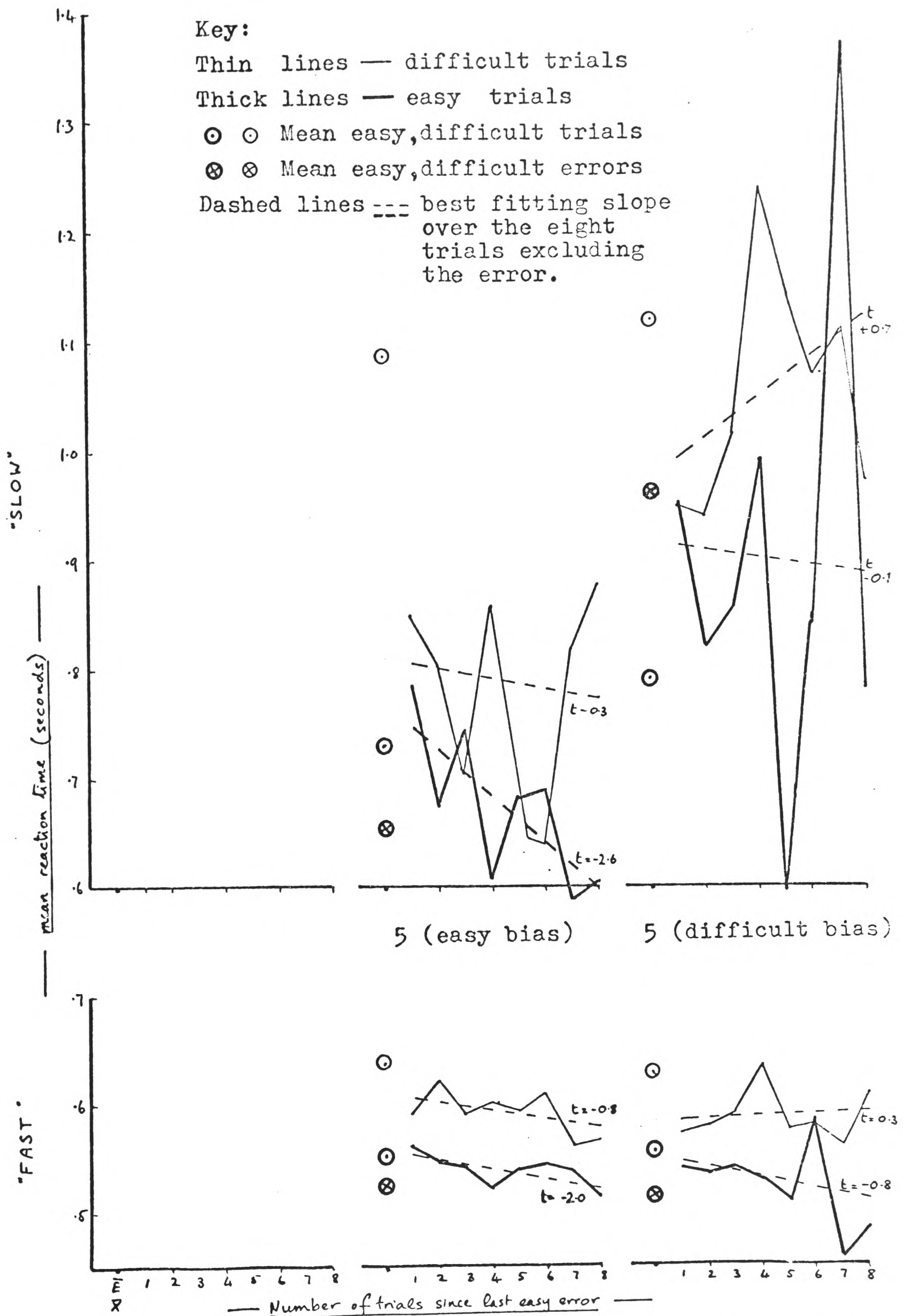
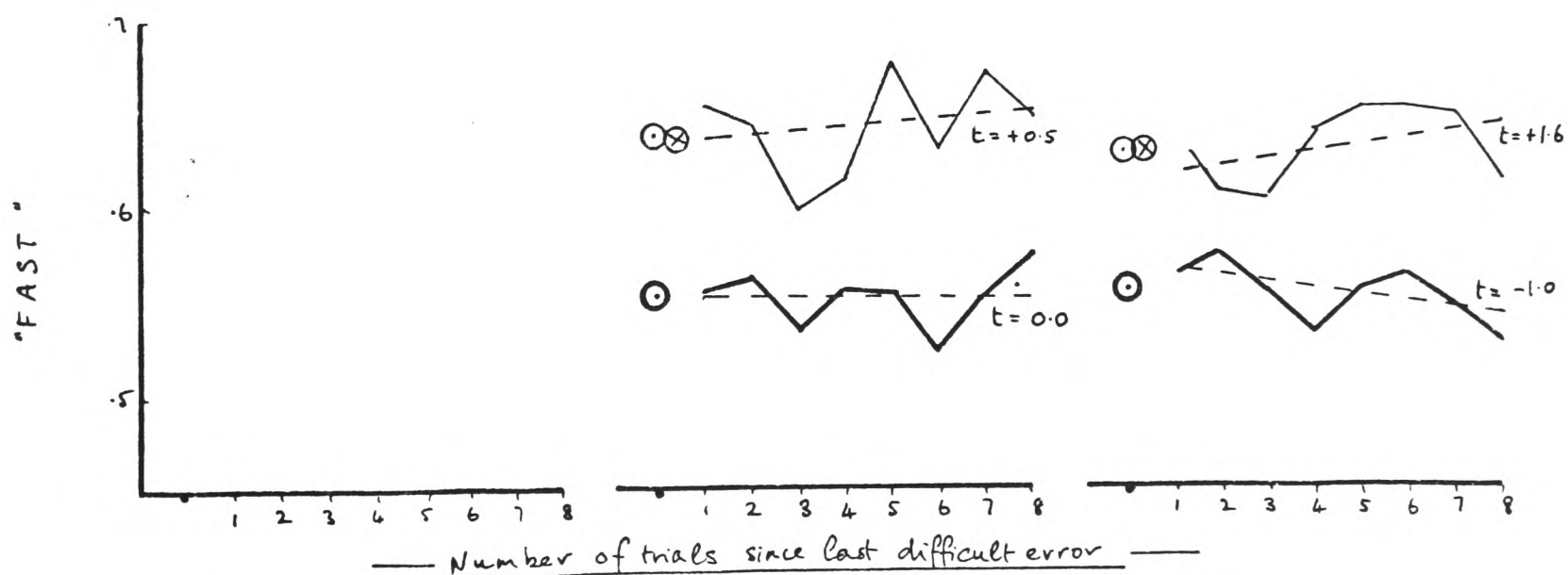
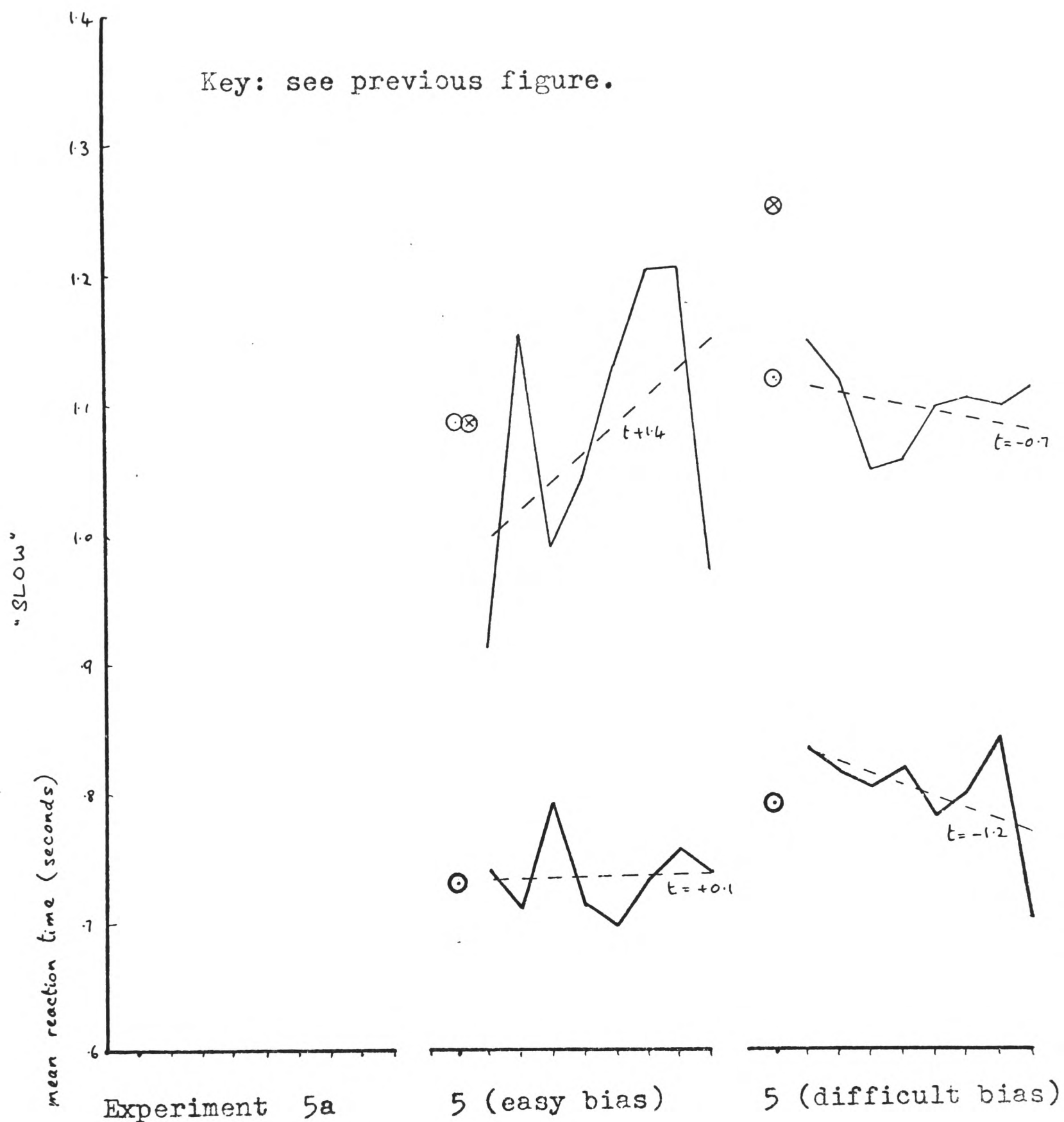


Figure 9-2.



Plots of mean RT at successive trials after an easy error without another intervening easy error. Experiment 5

Figure 9-4.



Plots of mean RT at successive trials after a difficult error without another intervening difficult error.

Experiment 5

Figure 9-5.

Key:

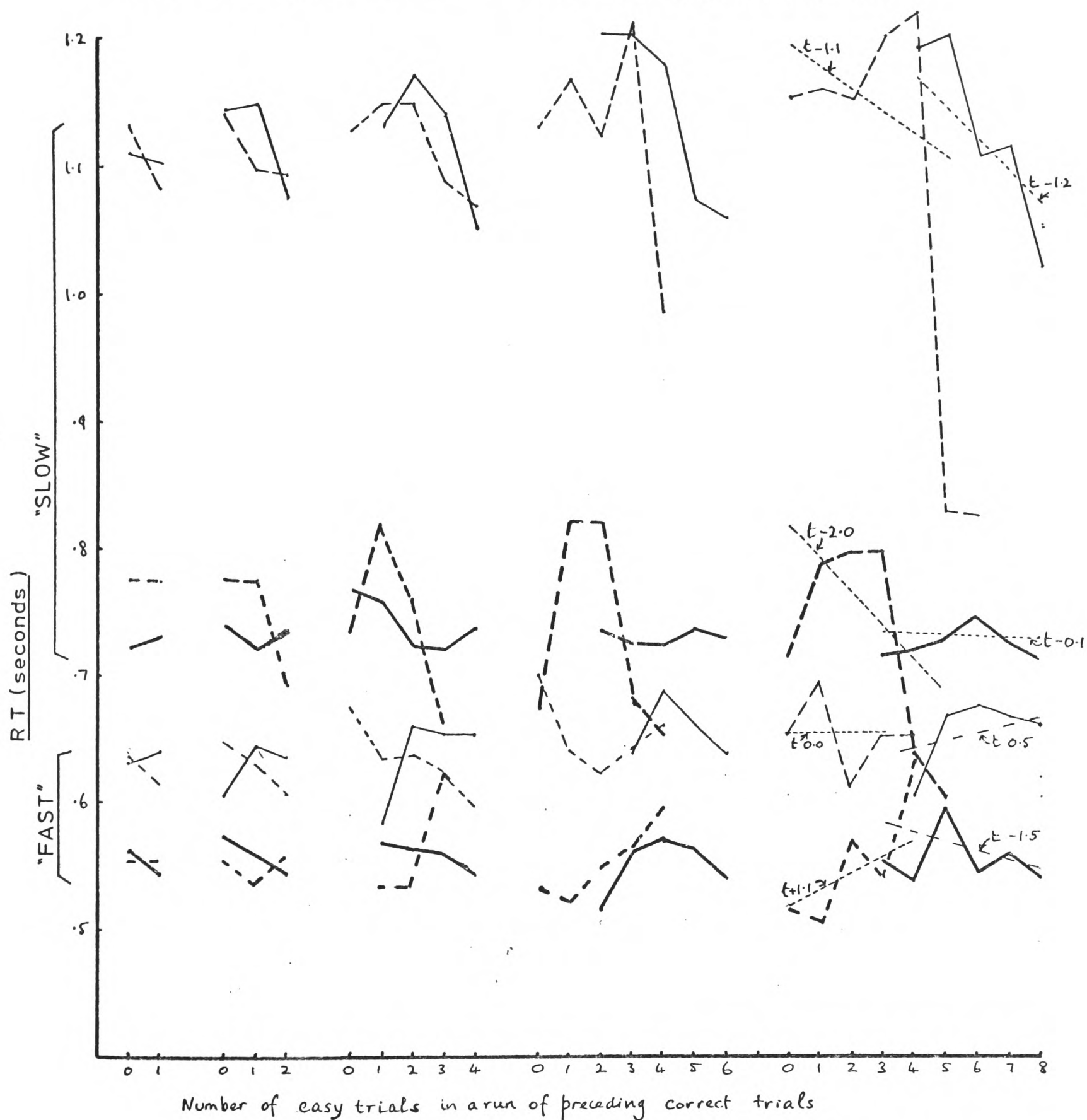
Dashed lines = difficult bias

Solid lines = easy bias

Thin lines — difficult trials

Thick lines — easy trials

Dotted straight lines best fit regression line



Sample Size analysis for window sizes 1,2,4,6 and 8.

Experiment 5. No errors in preceding sequence.

Figure 9 - 6

CHAPTER 10

FINAL DISCUSSION

It has been assumed by several writers that observers in serial choice response tasks regulate their speed of performance by detecting the errors which they make. In this report experiments have been described which show that subjects are also capable of such regulation without using feedback from errors, and instead show themselves able to adapt to fluctuations in the level of difficulty of the task from trial to trial.

Theorists have rarely considered how subjects can actually set and maintain the criteria that are proposed in the models they put forward. The Response Orientated Theorists (see Chapter 2, section 2) are only concerned with the tradeoff of one response against the others, and in predicting response bias. The few theorists who do consider some adjustment in relation to a SDT type mechanism on the part of the subjects (e.g. Schoeffler 1965, Kac 1969 and others) ignore the possibility of adjustment to difficulty alone, and predict that subjects will only alter the position of their criteria after errors.

Rabbitt and Vyas (1970) and Lappin and Disch (1972) have suggested that subjects track their speed-error tradeoff limits by recognising errors - that is, that subjects will gradually speed up until they make an error and then slow down again. Rabbitt (1966, 1969) has published evidence that subjects can do this. It has similarly been shown in this report that when displays were easy, as they were in Experiment 5, subjects were able to adjust their performance by slowing down after errors.

When the displays were more difficult, as with the difficult displays of Experiment 5, or with both the easy and the difficult displays of the other experiments, no adjustments to RT's on trials following errors were observed.

Errors made to difficult displays had very little effect on later RT's, and their latencies were characterised by being similar to or longer than the latencies of correct responses. They therefore may have corresponded to the perceptual errors in the classification outlined by Rabbitt and Vyas (1970).

The question of the effect of errors on the data was carefully considered in Chapter 9 and it was shown that subjects can adjust their performance according to the level of difficulty of preceding trials, independently of their detection of any errors which they may make.

One of the main findings of this research was that the difficulty of preceding trials influences the latency of later trials; this was demonstrated using a Sample Size analysis. To carry out this analysis we used a sample window incorporating several trials which immediately preceded a current trial. This window could be varied to inspect a sample of from one to eight trials wide, and each trial considered might be classified according to the number of easy trials which occurred in the preceding window. In general, as the number of easy trials in the window increased, so the (mean) latency of the trial following the window reduced. This was called a "positive difficulty bias effect" and was also considered to be a "local effect". If RT's to both easy and difficult trials were faster after a high proportion of easy displays than after a high proportion of difficult displays (or indeed if both were slower - so that both were affected in the same way), this was thought to be consistent with the

hypothesis that the subjects were using one criterion for both levels of difficulty.

Two separate kinds of difficulty bias were observed. The type described above, which was generally only noticeable on a Sample Size analysis when window sizes larger than three were examined, was described as a "local effect". There was also an overall difficulty bias effect which was related to the average difficulty of the whole run. The direction of influence of these two effects was not necessarily the same. Even within the results of individual subjects it was possible to see a positive local effect, in which the RT's to displays following a high proportion of easy displays were faster than average while, at the same time, the mean RT's in a run biased towards difficult displays were faster than those in a run biased towards easy displays to give an apparent negative overall bias. In other words, if the trend within a run biased towards easy displays was towards faster RT's when the preceding trials had been easy, this did not necessarily cause the mean overall RT of the run to be faster than the mean overall RT for a run biased towards difficult displays. The suggestion that subjects can be influenced at two different levels has been confirmed in other experiments. Annet (1969) has shown that subjects who are run on a mixed schedule of ISI's are influenced both by the durations of the preceding ISI's and also by the maximum ISI that is possible during the run. Granjon and Reynard (1977) have shown that the effect on reaction times of repetitions of intertrial intervals depended not only on the number of repetitions of an interval, but also on the number of possible repetitions of particular intervals in a run.

Leonard, Newman and Carpenter (1966) have shown that subjects are similarly affected at different levels by response bias. They showed that when the stimuli in a self-paced task were of different probabilities, subjects were affected in the short term by response repetitions, but that they were also influenced by the overall probabilities of the stimuli used.

We have seen that subjects behaved in a way which suggests that they were separately aware of overall and local biases for the different levels of display difficulty. However, it has been demonstrated by Parducci and Sandusky (1970) that when stimuli are difficult to discriminate from each other, subjects are not aware of changes in the relative probabilities of different stimuli. The two ideas appear to conflict. On the one hand there is evidence from these experiments that subjects do respond to changes in the proportion of moderately difficult stimuli and yet Parducci and Sandusky have shown that subjects cannot discriminate bias towards one or other stimulus. It is felt that this difference is indicative of two alternative approaches. The study by Parducci and Sandusky is concerned with response bias, whereas the present studies are dealing with difficulty bias; changes in difficulty bias during a run will not necessarily influence response bias and vice versa. Both response bias and difficulty bias may be influenced by common factors, such as discriminability, but the two can remain relatively independent. MacDonald (1974, 1976) confirmed this by demonstrating empirically that measures of response bias were affected by preceding sequences of responses but not by previous levels of accuracy. Conversely, measures of sensitivity were not influenced by response sequences, but were influenced by the correctness of the preceding responses.

The fact that subjects behave in such a way that the effects of two separate levels of bias can be distinguished in their performance strongly implies that they actively adjust their performance during a run. It is possible to predict changes in performance from a fairly passive system such as Adaptation Level Theory as proposed by Helson (1948, 1964). In particular Hébert (1972) has suggested a model which combines ALT and SDT. He proposed that the criteria which subjects use (of the kind proposed by SDT) are related to an internal scale which is considered to be the subject's internal representation of the external world. This scale is in turn related to an anchor or adaptation level (AL). As the values in the external world change, so the AL changes, drawing with it the scale and so shifting the criteria. This model could only be applied to the data of these experiments if it were to be assumed that subjects utilise the absolute difference between the two lines on each display to make their decisions. It was shown in Chapter 4 that the predictions made from the model based on this assumption were in the opposite direction to those observed in most of our experiments, so that this theory has not been considered further. It should be emphasised that this does not throw doubts on the theory in general, since it is strictly relevant only to absolute judgements, rather than to simultaneous relative judgements unless this additional, apparently invalid, assumption is made.

If we consider the models which utilise counters (as outlined in Chapter 1) the changes in latency which have been seen to follow changes in difficulty would be consistent with the idea that subjects reduce the size of the counter (or in other words, reduce the amount of evidence which they require to make

a decision) when a run of successive trials are easy. This kind of adjustment by subjects does not appear to have been considered by the theorists.

In view of these observations there seems to be a need for some modification to current theories of discrimination to describe how criteria are set and maintained according to task difficulty. This author has no specific suggestions as to how they should be modified, and acknowledges that, since the level of correction to RT's as a result of difficulty bias has been seen to be small, the quantitative error in prediction due to ignoring such adjustments may be minimal. However these adjustments are evidently made, and models which do not explain how this is done cannot be taken seriously.

The small size of the effect is emphasised by the fact that the changes in performance between different levels of difficulty bias could be observed as changes in latency, but not as changes in error rate. This was considered reasonable because subjects generally attempt to maintain a fairly steady level of performance and any gross changes in error rate would be linked with very large changes in latency, as indicated by an inspection of any of the various Speed-Accuracy tradeoff functions presented in this report.

The data for most subjects was consistent with the hypothesis that they used the same, single criterion for both easy and difficult discriminations. This was inferred from the way in which plots of a Sample Size analysis showed similar trends for the RT's to both easy and difficult displays. In general, RT's to both easy and difficult displays became faster as the proportion of easy trials in the preceding sequence increased. There were

exceptions to this: the subjects of Experiment 1, both subjects in Experiment 6 (when being careful) and one subject in Experiment 7 seemed to use two criteria - one for each level of difficulty. This was evident because the latencies for the two levels of difficulty were differently affected by the difficulty of the preceding trials - RT's to easy displays became faster and RT's to difficult displays became slower as the proportion of easy trials increased. Several possible effects which might be observed if two criteria were being used were described at the beginning of Chapter 3. The trends actually observed were consistent with the idea that criteria drift towards greater caution as unfilled time between successive displays elapses.

Perhaps here we could consider two other hypotheses. First, there may be a case for considering a process analagous to the exchange function described by Audley et al (1975), who state that the "speeding up of one response led to a slowing of the other response". One likely explanation for this was thought to be the relative preparedness of responses, possibly in the way described by Falmagne (1965). In the data we have discussed, the speeding up or slowing down of responses would be due to the relative preparedness for using a particular criterion. If response bias can be attributed to preparedness, then, since the repetition effect reduces as the interval between responses increases (Bertelson 1961, Hale 1967, etc), so preparedness may also be seen as a time dependent process.

Second, it could be considered that the speeding up of responses is due to the facilitation provided by re-using neural pathways and linkages. This process would also be time dependent as facilitation would dissipate with lapse of time since a pathway was last activated.

There is some evidence that criterion changes are not time dependent: It is logical to assume that the reduction in RT's for responses to easy displays as the proportion of preceding easy displays increased was caused by the same mechanisms in all the many experiments in which it was observed, in particular in Experiments 1 and 8. In Experiment 8 the Sample Size analyses indicated that when RSI was increased to four seconds, sequential effects of difficulty did not reduce, and seemed as much in evidence as they did in the experiments containing shorter RSI's. Thus it would seem that neither the concept of preparedness, nor that of the facilitation of neural pathways, is a sufficient explanation of the changes observed in RT after varying difficulty levels.

It was stated above that when subjects were considered to be using two criteria, the criterion not being used appeared to drift towards greater caution. This hypothesis was supported by the data from Experiment 8, in which, as the interval between responses increased, so there was also a drift towards greater caution. Three related observations suggested this. First, as the RSI's increased, so RT's to both easy and difficult displays also increased (as subjects become more cautious, so they will respond more slowly). Second, when RSI's increased, RT's to difficult displays increased faster than RT's to easy displays (models of discrimination predict that movements of criteria will have more influence on RT's to difficult discriminations than on RT's to easy discriminations). Third, as RSI's increased, there was a slight increase in accuracy (as subjects become more cautious, so they should make fewer errors).

These three facts, when considered together, rule out the possibility that longer RT's following long RSI's were only due to greater uncertainty as to when the display would appear. Such temporal uncertainty should produce the same increase in RT to both easy and difficult displays.

Thus there is quite strong evidence that when a criterion is not being employed, either during the interval between decisions or because another criterion is being used, it will drift towards a value that will give rise to more cautious decisions.

We saw in Experiments 5 and 6 that subjects who were responding quickly showed a much smaller overall difference between RT's to easy bias runs (runs in which there was a high proportion of easy displays) and the RT's to difficult bias runs (runs in which there was a high proportion of difficult displays). At first it was thought that this was related to the fact that when subjects responded quickly the difference between RT's to easy and difficult displays was much smaller. It could have been that, as the difference in RT between easy and difficult displays increased, so subjects became more aware of the difference in difficulty between them. This in turn could have led to a greater difference between RT's during easy bias and difficult bias runs. In this case there should have been a correlation between, on the one hand, the difference in RT to easy and difficult displays and, on the other hand, the difference in RT to easy bias and difficult bias runs. Correlations calculated within the various experiments in which differing proportions of easy and difficult displays were used showed that this was only true in Experiment 3. It was felt that this reflected the dichotomy between subjects who responded

quickly (and showed little effect of difficulty bias) and subjects who responded more slowly (and did show an effect of bias). For subjects who did not respond quickly there was no correlation between the effect of difficulty bias on RT's and their degree of caution (as measured by the overall effect of bias on RT).

The absence of a correlation in most of the experiments led to the conclusion that it was not decision time that was the important cue which led subjects to alter their criteria. One hypothesis was that subjects could become aware of the difficulty of the displays to which they responded independently of their knowledge of the time that it took them to make the necessary decisions. This idea was supported to some extent by Experiment 7 in which the levels of difficulty of easy and difficult displays were more widely separated than in earlier experiments. The absolute difference between the RT's to easy bias and difficult bias runs was greater in this than in the other experiments in which bias occurred. However the difference between RT's to easy and difficult displays was not greater than, for example, the difference between the RT's to easy and difficult displays seen for the "slow" subjects in Experiment 5. The implication is that subjects do not need to know how long discriminations take in order to judge how difficult they are.

We have seen that when subjects are presented with displays which are easy to discriminate, they are able to maintain their performance by adjusting their speed after errors. More importantly though, whether the displays that were used were easy or difficult, subjects were able to adjust their performance to the average difficulty level of the task. In conclusion, we have seen that it is possible to describe subjects' adjustments

in performance in terms of changes in the values of criteria, and that these changes in turn reflect the fluctuations in difficulty level of the task. Subjects seem to have direct cognizance of the levels of difficulty of discriminations, and do not simply infer these from variations in their own decision latencies.

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APPENDICES

- A Analyses of Variance for the various experiments
- B Regression Analyses for experiments 1 and 2.
- C Correlation calculations for chapter 9.

```

NWAY?5  A 2  B 2  C 2  D 2  E 10 NCELL?1
S?E
N W/IN? 4 :A:B:C:D
FUNC:0
DATA:0

```

E	+4033050.000	9	+448117.000	
WITHIN				
A	+2309.230	1	+2309.230	+0.273
AE	+76044.500	9	+8449.390	
B	+3632620.000	1	+3632620.000	+19.870
BE	+1645350.000	9	+182817.000	
C	+5921.290	1	+5921.290	+1.757
CE	+30339.500	9	+3371.050	
D	+17267.500	1	+17267.500	+2.675
DE	+58096.000	9	+6455.110	
AB	+12260.000	1	+12260.000	+2.117
ABE	+52127.000	9	+5791.890	
AC	+395.511	1	+395.511	+0.060
ACE	+59353.500	9	+6594.830	
AD	+641.875	1	+641.875	+0.128
ADE	+45144.500	9	+5016.050	
BC	+1987.210	1	+1987.210	+0.717
BCE	+24941.500	9	+2771.280	
BD	+3410.480	1	+3410.480	+1.415
BDE	+21697.000	9	+2410.780	
CD	+7722.310	1	+7722.310	+1.520
CDE	+45718.000	9	+5079.780	
ABC	+8005.210	1	+8005.210	+3.862
ABCE	+18655.500	9	+2072.830	
ABD	+3143.480	1	+3143.480	+0.519
ABDE	+54497.000	9	+6055.220	
ACD	+709.242	1	+709.242	+0.109
ACDE	+58618.000	9	+6513.110	
BCD	+1755.690	1	+1755.690	+0.401
BCDE	+39426.500	9	+4380.720	
ABCD	+14579.200	1	+14579.200	+5.284
ABCDE	+24830.500	9	+2758.940	.
TOTAL	+10000600.000	159		

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FACTOR:

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Analysis of Variance for Experiment 1 RT's

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AN. 1-1

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A trial n Left / Right
B " " easy / difficult
C trial n-1 Left / Right
D " " easy / difficult
E subjects

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```

NWAY?5C A 2 B 2 C 2 D 2 E 10 NCELL?1
S?E
N W/IN? 4 :A:B:C:D
FUNC:0
DATA:3
M=      +83.901
E      +7060.960      9      +784.551

```

WITHIN				
A	+154.175	1	+154.175	+0.911
AE	+1523.610	9	+169.291	
B	+23459.900	1	+23459.900	+137.991
BE	+1530.090	9	+170.010	
C	+2.458	1	+2.458	+0.055
CE	+401.465	9	+44.607	
D	+78.614	1	+78.614	+1.005
DE	+704.201	9	+78.245	
AB	+340.512	1	+340.512	+3.290
ABE	+931.372	9	+103.486	
AC	+0.681	1	+0.681	+0.002
ACE	+3361.790	9	+373.532	
AD	+15.885	1	+15.885	+0.179
ADE	+799.942	9	+88.882	
BC	+18.967	1	+18.967	+0.525
BCE	+325.421	9	+36.158	
BD	+134.171	1	+134.171	+2.074
BDE	+582.347	9	+64.705	
CD	+67.880	1	+67.880	+2.811
CDE	+217.360	9	+24.151	
ABC	+213.999	1	+213.999	+1.548
ABCE	+1244.560	9	+138.285	
ABD	+8.628	1	+8.628	+0.100
ABDE	+778.819	9	+86.535	
ACD	+454.968	1	+454.968	+3.244
ACDE	+1262.350	9	+140.262	
BCD	+30.458	1	+30.458	+1.685
BCDE	+162.665	9	+18.074	
ABCD	+671.931	1	+671.931	+3.896
ABCDE	+1552.330	9	+172.482	
TOTAL	+48092.600	159		

```

FACTOR:
      A      trial n left / Right
      +84.883
      +82.920
FACTOR:B      trial n easy / difficult
      +96.010
      +71.793
FACTOR:C      trial n-1 left / Right
      +84.026
      +83.778
FACTOR:D      trial n-1 easy / difficult
      +83.201
      +84.603
FACTOR:AB
      +95.533
      +74.233
      +96.488
      +69.352
FACTOR:

```

1	TB	488	788	523	716	512	785	550	749
2	AD	684	1045	707	1104	655	1086	644	1211
3	RF	499	592	498	637	503	644	508	626
4	SF	426	513	442	551	442	520	440	535
5	AB	569	630	553	667	577	582	595	603
6	CN	509	1202	593	1212	549	1244	586	1282
7	RS	526	923	514	800	529	878	551	912
8	SV	602	1062	778	1211	689	1197	775	1210
9	NS	463	593	480	566	468	574	468	574
10	SH	541	1118	536	1067	562	991	573	928
11	FOR ANOVA 4-WAY								
12	A THIS TRIAL EASY/DIFFICULT								
13	B TRIAL N-1 EASY/DIFFICULT								
14	C TRIAL N-2 EASY/DIFFICULT								
15	D SUBJECTS (10)								
16	E12BCK.RT								

NWAY?4 A 2 B 2 C 2 D 10 NCELL?1
STD
N W/INT? 3 :A:B:C
FUNC:0
DATA:3

M=	+702.937		
D	+2020590.000	9	+224510.000
WITHIN			
A	+1806300.000	1	+1806300.000 +20.457
AD	+794661.000	9	+88295.700
B	+6390.310	1	+6390.310 +2.055
BD	+27988.200	9	+3109.800
C	+1795.510	1	+1795.510 +1.028
CD	+15725.700	9	+1747.300
AB	+1336.440	1	+1336.440 +0.979
ABD	+12285.500	9	+1365.050
AC	+154.237	1	+154.237 +0.056
ACD	+24843.700	9	+2760.410
BC	+30.011	1	+30.011 +0.023
BCD	+11948.700	9	+1327.640
ABC	+391.425	1	+391.425 +0.691
ABCD	+5101.250	9	+566.805
TOTAL	+4729550.000	79	

FACTOR:

NW 1/4 Sec 34 T2N R2E S12E
 NW 1/4 Sec 34 T2N R2E S12E
 DATA 13
 EXTRAPOLATION

M= +77.688

BETWEEN				
G	+14.444	1	+14.444	+0.001
ERROR SSQ	+62754.400	4	+15688.600	
WITHIN				
A	+195.320	1	+195.320	+0.565
AG	+45.669	1	+45.669	+0.132
ERROR SSQ	+1383.660	4	+345.915	
B	+70485.000	1	+70485.000	+185.772 **
BG	+464.134	1	+464.134	+1.223
ERROR SSQ	+1517.670	4	+379.418	
C	+28.255	1	+28.255	+0.820
CG	+12.321	1	+12.321	+0.357
ERROR SSQ	+137.894	4	+34.473	
D	+4135.860	3	+1378.620	+0.454
DG	+417.156	3	+139.052	+0.046
ERROR SSQ	+36456.700	12	+3038.060	
E	+534.116	3	+178.038	+0.270
EG	+3306.390	3	+1102.130	+1.673
ERROR SSQ	+7905.800	12	+658.816	
AB	+107.859	1	+107.859	+2.233
ABG	+150.665	1	+150.665	+3.120
ERROR SSQ	+193.186	4	+48.297	
AC	+10.286	1	+10.286	+0.087
ACG	+23.422	1	+23.422	+0.198
ERROR SSQ	+474.093	4	+118.523	
AD	+2296.410	3	+765.472	+2.441
ADG	+196.856	3	+65.619	+0.209
ERROR SSQ	+3763.560	12	+313.630	
AE	+385.412	3	+128.471	+0.970
AEG	+94.276	3	+31.425	+0.237
ERROR SSQ	+1589.290	12	+132.441	
BC	+3.917	1	+3.917	+0.044
BCG	+3.067	1	+3.067	+0.034
ERROR SSQ	+359.505	4	+89.876	
BD	+1308.900	3	+436.301	+1.110
BDG	+789.912	3	+263.304	+0.670
ERROR SSQ	+4716.790	12	+393.065	
BE	+321.931	3	+107.310	+0.523
BEG	+748.862	3	+249.621	+1.215
ERROR SSQ	+2464.380	12	+205.365	
CD	+179.276	3	+59.759	+0.806
CDG	+52.329	3	+17.443	+0.235
ERROR SSQ	+889.694	12	+74.141	
CE	+24.737	3	+8.246	+0.165
CEG	+61.726	3	+20.575	+0.412
ERROR SSQ	+599.231	12	+49.936	
DE	+5414.440	9	+601.604	+0.452
DEG	+4387.660	9	+487.517	+0.366
ERROR SSQ	+47908.400	36	+1330.790	
ABC	+192.355	1	+192.355	+5.603 NS
ABCG	+27.801	1	+27.801	+0.810
ERROR SSQ	+137.317	4	+34.329	
ABD	+66.216	3	+22.072	+0.169
ABDG	+309.756	3	+103.252	+0.792
ERROR SSQ	+1565.190	12	+130.433	
ABE	+224.806	3	+74.935	+2.782
ABEG	+123.159	3	+41.053	+1.524
ERROR SSQ	+323.204	12	+26.934	
ACD	+243.128	3	+81.043	+0.946
ACDG	+20.295	3	+6.765	+0.079
ERROR SSQ	+1028.190	12	+85.683	
ACE	+202.198	3	+67.399	+1.750
ACEG	+140.690	3	+46.897	+1.218
ERROR SSQ	+462.046	12	+38.504	
ADE	+2481.400	9	+275.711	+2.675 *
ADEG	+1179.980	9	+131.109	+1.272
ERROR SSQ	+3710.790	36	+103.077	
BCD	+82.773	3	+27.591	+0.691
BCDG	+23.470	3	+7.823	+0.196
ERROR SSQ	+479.308	12	+39.942	
BCE	+220.598	3	+73.533	+1.176
BCEG	+107.138	3	+35.713	+0.571
ERROR SSQ	+750.538	12	+62.545	
BDE	+1234.410	9	+137.157	+0.317
BDEG	+301.437	9	+33.493	+0.077
ERROR SSQ	+15571.100	36	+432.530	
CDE	+370.459	9	+41.162	+0.574
CDEG	+488.914	9	+54.324	+0.757
ERROR SSQ	+2582.880	36	+71.747	
ABCD	+104.633	3	+34.878	+0.639
ABCDG	+39.767	3	+13.256	+0.243
ERROR SSQ	+655.354	12	+54.613	
ABCE	+178.176	3	+59.392	+3.088
ABCEG	+58.529	3	+19.510	+1.014
ERROR SSQ	+230.804	12	+19.234	
ABDE	+689.209	9	+76.579	+1.270
ABDEG	+493.484	9	+54.832	+0.909
ERROR SSQ	+2171.230	36	+60.312	
ACDE	+235.334	9	+26.148	+0.475
ACDEG	+385.496	9	+42.833	+0.778
ERROR SSQ	+1981.720	36	+55.048	
BCDE	+852.626	9	+94.736	+1.279
BCDEG	+920.573	9	+102.286	+1.381
ERROR SSQ	+2666.790	36	+74.078	
ABCDE	+431.720	9	+47.969	+1.267
ABCDEG	+361.526	9	+40.170	+1.061
ERROR SSQ	+1363.010	36	+37.861	
TOTAL	+317787.000	767		

FACTOR 16

L31RR1,AN					
M= +1312.250					
BETWEEN					
G	+157706.000	1	+157706.000	+0.019	
ERROR SSQ	+33138300.000	4	+8284580.000		
WITHIN					
A	+8526510.000	1	+8526510.000	+10.698	*
AG	+377872.000	1	+377872.000	+0.474	
ERROR SSQ	+3188000.000	4	+787000.000		
B	+9482150.000	1	+9482150.000	+6.791	
BG	+792548.000	1	+792548.000	+0.569	
ERROR SSQ	+5523240.000	4	+1393310.000		
C	+5238.370	1	+5238.370	+0.050	
CG	+74073.500	1	+74073.500	+0.703	
ERROR SSQ	+421582.000	4	+105395.000		
D	+2233650.000	3	+744549.000	+3.444	
DG	+174209.000	3	+58069.700	+0.269	
ERROR SSQ	+2594280.000	12	+216190.000		
E	+3105100.000	3	+1035030.000	+4.776	*
EG	+199448.000	3	+66482.800	+0.307	
ERROR SSQ	+2600390.000	12	+216699.000		
AB	+5924.000	1	+5924.000	+0.207	
ABG	+1626.160	1	+1626.160	+0.057	
ERROR SSQ	+114590.000	4	+28647.500		
AC	+31983.600	1	+31983.600	+0.744	
ACG	+7167.780	1	+7167.780	+0.167	
ERROR SSQ	+172070.000	4	+43017.500		
AD	+76243.500	3	+25414.500	+1.541	
ADG	+113865.000	3	+37955.200	+2.301	
ERROR SSQ	+197942.000	12	+16495.200		
AE	+315602.000	3	+105201.000	+1.078	
AEG	+226984.000	3	+75661.500	+0.775	
ERROR SSQ	+1171540.000	12	+97628.500		
BC	+179.626	1	+179.626	+0.013	
BCG	+1717.780	1	+1717.780	+0.127	
ERROR SSQ	+53894.100	4	+13473.500		
BD	+46325.500	3	+15441.800	+0.648	
BDG	+28159.600	3	+9386.550	+0.394	
ERROR SSQ	+286030.000	12	+23835.800		
BE	+2310.500	3	+770.166	+0.021	
BEG	+172812.000	3	+57604.200	+1.588	
ERROR SSQ	+435334.000	12	+36277.800		
CD	+24533.500	3	+8177.830	+0.348	
CDG	+30262.600	3	+10087.500	+0.429	
ERROR SSQ	+282154.000	12	+23512.800		
CE	+40424.500	3	+13474.800	+1.451	
CEG	+82462.600	3	+27487.500	+2.960	
ERROR SSQ	+111422.000	12	+9285.180		
DE	+1157070.000	9	+128563.000	+0.720	
DEG	+418878.000	9	+46542.000	+0.261	
ERROR SSQ	+6428450.000	36	+178568.000		
ABC	+1125.630	1	+1125.630	+0.110	
ABCG	+1811.780	1	+1811.780	+0.178	
ERROR SSQ	+40798.100	4	+10199.500		
ABD	+1903.500	3	+634.500	+0.028	
ABDG	+73621.600	3	+24540.500	+1.069	
ERROR SSQ	+275414.000	12	+22951.200		
ABE	+12446.500	3	+4148.830	+0.181	
ABEG	+45640.600	3	+15213.500	+0.665	
ERROR SSQ	+274694.000	12	+22891.200		
ACD	+34735.000	3	+11578.300	+0.785	
ACDG	+74741.100	3	+24913.700	+1.689	
ERROR SSQ	+176958.000	12	+14746.500		
ACE	+14260.000	3	+4753.330	+0.235	
ACEG	+23972.100	3	+7990.720	+0.395	
ERROR SSQ	+242718.000	12	+20226.500		
ADE	+246381.000	9	+27375.700	+2.213	*
ADEG	+211185.000	9	+23465.100	+1.897	
ERROR SSQ	+445286.000	36	+12369.000		
BCD	+78497.000	3	+26165.600	+2.080	
BCDG	+45961.100	3	+15320.400	+1.218	
ERROR SSQ	+150950.000	12	+12579.200		
BCE	+93162.000	3	+31054.000	+1.318	
BCEG	+20668.100	3	+6889.380	+0.292	
ERROR SSQ	+282814.000	12	+23567.800		
BDE	+153831.000	9	+17092.400	+1.297	
BDEG	+179795.000	9	+19977.300	+1.516	
ERROR SSQ	+474518.000	36	+13181.000		
CDE	+110434.000	9	+12270.500	+0.881	
CDEG	+182883.000	9	+20320.400	+1.459	
ERROR SSQ	+501514.000	36	+13930.900		
ABCD	+16489.000	3	+5496.330	+0.284	
ABCDG	+2775.160	3	+925.052	+0.048	
ERROR SSQ	+232366.000	12	+19363.800		
ABCE	+50728.000	3	+16909.300	+1.595	
ABCEG	+16738.100	3	+5579.380	+0.526	
ERROR SSQ	+127214.000	12	+10601.200		
ABDE	+152853.000	9	+16983.700	+1.029	
ABDEG	+45103.600	9	+5011.520	+0.304	
ERROR SSQ	+594214.000	36	+16505.900		
ACDE	+73054.500	9	+8117.160	+0.830	
ACDEG	+33840.600	9	+3760.070	+0.385	
ERROR SSQ	+351942.000	36	+9776.170		
BCDE	+54516.500	9	+6057.390	+0.617	
BCDEG	+62682.600	9	+6964.740	+0.709	
ERROR SSQ	+353638.000	36	+9823.280		
ABCDE	+85030.500	9	+9447.830	+0.533	
ABCDEG	+212604.000	9	+23622.700	+1.334	
ERROR SSQ	+637566.000	36	+17710.200		
TOTAL	+92238300.000	767			

S?F
 N W/IN? 5 :A:B:C:D:EFUNC:0
 DATA:0

F	+62768.800	5	+12553.800	
WITHIN				
A	+195.320	1	+195.320	+0.683
AF	+1429.330	5	+285.866	
B	+70485.000	1	+70485.000	+177.827 **
BF	+1981.840	5	+396.369	
C	+28.255	1	+28.255	+0.940
CF	+150.226	5	+30.045	
D	+4135.860	3	+1378.620	+0.561
DF	+36873.900	15	+2458.260	
E	+534.116	3	+178.038	+0.238
EF	+11212.200	15	+747.481	
AB	+107.859	1	+107.859	+1.568
ABF	+343.875	5	+68.775	
AC	+10.286	1	+10.286	+0.103
ACF	+497.523	5	+99.505	
AD	+2296.410	3	+765.472	+2.899
ADF	+3960.440	15	+264.029	
AE	+385.412	3	+128.471	+1.145
AEF	+1683.590	15	+112.239	
BC	+3.917	1	+3.917	+0.054
BCF	+362.648	5	+72.530	
BD	+1308.900	3	+436.301	+1.188
BDF	+5506.750	15	+367.117	
BE	+321.931	3	+107.310	+0.501
BEF	+3213.280	15	+214.219	
CD	+179.276	3	+59.759	+0.952
CDF	+942.047	15	+62.803	
CE	+24.737	3	+8.246	+0.187
CEF	+660.969	15	+44.065	
DE	+5414.440	9	+601.604	+0.518
DEF	+52296.000	45	+1162.130	
ABC	+192.355	1	+192.355	+5.820 NS
ABCF	+165.242	5	+33.048	
ABD	+66.216	3	+22.072	+0.177
ABDF	+1875.060	15	+125.004	
ABE	+224.806	3	+74.935	+2.518
ABEF	+446.476	15	+29.765	
ACD	+243.128	3	+81.043	+1.159
ACDF	+1048.530	15	+69.902	
ACE	+202.198	3	+67.399	+1.677
ACEF	+602.750	15	+40.183	
ADE	+2481.400	9	+275.711	+2.537 *+
ADEF	+4890.900	45	+108.687	
BCD	+82.773	3	+27.591	+0.823
BCDF	+502.828	15	+33.522	
BCE	+220.598	3	+73.533	+1.286
BCEF	+857.812	15	+57.187	
BDE	+1234.410	9	+137.157	+0.389
BDEF	+15872.600	45	+352.724	
CDE	+370.459	9	+41.162	+0.603
CDEF	+3071.910	45	+68.265	
ABCD	+104.633	3	+34.878	+0.753
ABCDF	+695.078	15	+46.339	
ABCE	+178.176	3	+59.392	+3.080 NS
ABCEF	+289.281	15	+19.285	
ABDE	+689.209	9	+76.579	+1.293
ABDEF	+2664.620	45	+59.214	
ACDE	+235.334	9	+26.148	+0.497
ACDEF	+2367.260	45	+52.606	
BCDE	+852.626	9	+94.736	+1.188
BCDEF	+3587.260	45	+79.717	
ABCDE	+431.720	9	+47.969	+1.251
ABCDEF	+1724.900	45	+38.331	
TOTAL	+317787.000	767		


```

NWAY?7 6 A ABCDEFG
NWAY?6 A 2 B 2 C 2 D 4 E 4 F 6 NCELL?1
S?F
N W/IN? 5 :A:B:C:D:EFUNC:0
DATA:3

```

E3TRRT,AN AGAIN

```

M= +1312.260
F +33296000.000 5 +6659200.000

```

WITHIN

A	+8526510.000	1	+8526510.000	+11.956	* +
AF	+3565890.000	5	+713177.000		
B	+9462150.000	1	+9462150.000	+7.432	*
BF	+6365800.000	5	+1273160.000		
C	+5238.370	1	+5238.370	+0.053	
CF	+495656.000	5	+99131.200		
D	+2233650.000	3	+744549.000	+4.034	*
DF	+2768490.000	15	+184566.000		
E	+3105100.000	3	+1035030.000	+5.545	*
EF	+2799840.000	15	+186656.000		
AB	+5924.000	1	+5924.000	+0.255	
ABF	+116228.000	5	+23245.600		
AC	+31983.600	1	+31983.600	+0.892	
ACF	+179260.000	5	+35852.000		
AD	+76243.500	3	+25414.500	+1.223	
ADF	+311816.000	15	+20787.700		
AE	+315602.000	3	+105201.000	+1.128	
AEF	+1398550.000	15	+93236.800		
BC	+179.626	1	+179.626	+0.016	
BCF	+55628.000	5	+11125.600		
BD	+46325.500	3	+15441.800	+0.737	
BDF	+314192.000	15	+20946.100		
BE	+2310.500	3	+770.166	+0.019	
BEF	+608168.000	15	+40544.500		
CD	+24533.500	3	+8177.830	+0.393	
CDF	+312432.000	15	+20828.800		
CE	+40424.500	3	+13474.800	+1.042	
CEF	+193916.000	15	+12927.700		
DE	+1157070.000	9	+128563.000	+0.845	
DEF	+6847350.000	45	+152163.000		
ABC	+1125.630	1	+1125.630	+0.132	
ABCF	+42588.000	5	+8517.600		
ABD	+1903.500	3	+634.500	+0.027	
ABDF	+349048.000	15	+23269.900		
ABE	+12446.500	3	+4148.830	+0.194	
ABEF	+320320.000	15	+21354.600		
ACD	+34735.000	3	+11578.300	+0.690	
ACDF	+251700.000	15	+16780.000		
ACE	+14260.000	3	+4753.330	+0.267	
ACEF	+266672.000	15	+17778.100		
ADE	+246381.000	9	+27375.700	+1.877	
ADEF	+656488.000	45	+14588.600		
BCD	+78497.000	3	+26165.600	+1.993	
BCDF	+196916.000	15	+13127.700		
BCE	+93162.000	3	+31054.000	+1.535	
BCEF	+303464.000	15	+20230.900		
BDE	+153831.000	9	+17092.400	+1.175	
BDEF	+654328.000	45	+14540.600		
CDE	+110434.000	9	+12270.500	+0.807	
CDEF	+684384.000	45	+15208.500		
ABCD	+16489.000	3	+5496.330	+0.350	
ABCDF	+235252.000	15	+15683.500		
ABCE	+50728.000	3	+16909.300	+1.760	
ABCEF	+144108.000	15	+9607.200		
ABDE	+152853.000	9	+16983.700	+1.195	
ABDEF	+639336.000	45	+14207.500		
ACDE	+73054.500	9	+8117.160	+0.947	
ACDEF	+385864.000	45	+8574.750		
BCDE	+54516.500	9	+6057.390	+0.655	
BCDEF	+416408.000	45	+9253.510		
ABCDE	+85030.500	9	+9447.830	+0.500	
ABCDEF	+850032.000	45	+18889.600		
TOTAL	+92238300.000	767			

AN 2-4

N W/IN? 5 :A:B:C:D:EFUNC:0

DATA:3

E3SQRT.DA

FROM 2SEQU ABNALYSIS

M= +1275.800

F +14868300.000 5 +2973660.000

WITHIN

A	+4645580.000	1	+4645580.000	+6.770 *
AF	+3430830.000	5	+686166.000	
B	+56.617	1	+56.617	+0.001
BF	+204528.000	5	+40905.600	
C	+1507.640	1	+1507.640	+0.480
CF	+15694.000	5	+3138.800	
D	+20516.300	1	+20516.300	+4.955
DF	+20702.000	5	+4140.400	
E	+2332430.000	3	+777478.000	+1.097
EF	+10631700.000	15	+708779.000	
AB	+4700.380	1	+4700.380	+0.756
ABF	+31084.000	5	+6216.800	
AC	+38348.300	1	+38348.300	+7.561 *
ACF	+25358.000	5	+5071.600	
AD	+7389.660	1	+7389.660	+0.499
ADF	+74040.000	5	+14808.000	
AE	+32894.000	3	+10964.700	+0.822
AEF	+200078.000	15	+13338.500	
BC	+4277.430	1	+4277.430	+0.291
BCF	+73578.000	5	+14715.600	
BD	+211.605	1	+211.605	+0.035
BDF	+30000.000	5	+6000.000	
BE	+17189.500	3	+5729.830	+0.529
BEF	+162474.000	15	+10831.600	
CD	+42257.200	1	+42257.200	+17.304 **
CDF	+12210.000	5	+2442.000	
CE	+11300.500	3	+3766.830	+0.765
CEF	+73886.000	15	+4925.730	
DE	+14897.500	3	+4965.830	+0.825
DEF	+90246.000	15	+6016.400	
ABC	+8558.740	1	+8558.740	+2.293
ABCF	+18664.000	5	+3732.800	
ABD	+13.043	1	+13.043	+0.004
ABDF	+18326.000	5	+3665.200	
ABE	+5817.000	3	+1939.000	+0.423
ABEF	+68778.000	15	+4585.200	
ACD	+3889.010	1	+3889.010	+1.350
ACDF	+14400.000	5	+2880.000	
ACE	+37499.000	3	+12499.700	+0.941
ACEF	+199298.000	15	+13286.500	
ADE	+20312.500	3	+6770.830	+0.537
ADEF	+189194.000	15	+12612.900	
BCD	+25936.100	1	+25936.100	+2.999
BCDF	+43246.000	5	+8649.200	
BCE	+5501.000	3	+1833.670	+0.115
BCEF	+238158.000	15	+15877.200	
BDE	+16220.000	3	+5406.660	+1.035
BDEF	+78326.000	15	+5221.730	
CDE	+16422.000	3	+5474.000	+0.592
CDEF	+138594.000	15	+9239.600	
ABCD	+303.398	1	+303.398	+0.041
ABCDF	+37142.000	5	+7428.400	
ABCE	+19556.000	3	+6518.660	+0.572
ABCEF	+170966.000	15	+11397.700	
ABDE	+17769.500	3	+5923.160	+1.574
ABDEF	+56450.000	15	+3763.330	
ACDE	+35628.500	3	+11876.200	+1.806
ACDEF	+98626.000	15	+6575.060	
BCDE	+36883.500	3	+12294.500	+2.776
BCDEF	+66430.000	15	+4428.660	
ABCDE	+2015.500	3	+671.833	+0.141
ABCDEF	+71298.000	15	+4753.200	
TOTAL	+38878300.000	383		

W/IN? 5 :A:B:C:D:EFUNC:0
DATA:3

E3SQPC.DA FROM 2SEQU ANAL 3BACK

Y= +77.437
+29613.200 5 +5922.630

WITHIN

A	+34873.100	1	+34873.100	+174.945	>>
AF	+996.687	5	+199.337		
B	+4.671	1	+4.671	+0.632	
BF	+36.969	5	+7.394		
C	+26.268	1	+26.268	+0.602	
CF	+218.340	5	+43.668		
D	+2.548	1	+2.548	+0.149	
DF	+85.602	5	+17.120		
E	+600.807	3	+200.269	+1.214	
EF	+2474.000	15	+164.933		
AB	+0.790	1	+0.790	+0.046	
ABF	+85.664	5	+17.133		
AC	+0.505	1	+0.505	+0.009	
ACF	+271.347	5	+54.270		
AD	+23.374	1	+23.374	+0.487	
ADF	+240.012	5	+48.002		
AE	+171.701	3	+57.234	+1.458	
AEF	+588.644	15	+39.243		
BC	+0.032	1	+0.032	+0.003	
BCF	+56.070	5	+11.214		
BD	+42.259	1	+42.259	+2.018	
BDF	+104.699	5	+20.940		
BE	+16.487	3	+5.496	+0.171	
BEF	+481.027	15	+32.068		
CD	+1.025	1	+1.025	+0.040	
CDF	+128.668	5	+25.734		
CE	+50.314	3	+16.771	+0.436	
CEF	+577.531	15	+38.502		
DE	+50.969	3	+16.990	+0.643	
DEF	+396.625	15	+26.442		
ABC	+3.810	1	+3.810	+0.118	
ABCF	+160.996	5	+32.199		
ABD	+2.288	1	+2.288	+0.188	
ABDF	+60.871	5	+12.174		
ABE	+129.061	3	+43.020	+2.023	
ABEF	+318.918	15	+21.261		
ACD	+1.574	1	+1.574	+0.051	
ACDF	+154.195	5	+30.839		
ACE	+109.018	3	+36.339	+1.039	
ACEF	+524.859	15	+34.991		
ADE	+82.489	3	+27.496	+0.737	
ADEF	+559.890	15	+37.326		
BCD	+3.290	1	+3.290	+0.112	
BCDF	+146.277	5	+29.255		
BCE	+29.953	3	+9.984	+0.479	
BCEF	+312.766	15	+20.851		
BDE	+11.880	3	+3.960	+0.142	
BDEF	+419.742	15	+27.983		
CDE	+129.085	3	+43.028	+3.883	*
CDEF	+166.238	15	+11.083		
ABCD	+8.903	1	+8.903	+0.549	
ABCDF	+81.109	5	+16.222		
ABCE	+88.230	3	+29.410	+1.949	
ABCEF	+226.398	15	+15.093		
ABDE	+149.372	3	+49.791	+1.148	
ABDEF	+650.500	15	+43.367		
ACDE	+97.837	3	+32.612	+1.274	
ACDEF	+383.988	15	+25.599		
BCDE	+23.110	3	+7.703	+0.196	
BCDEF	+588.785	15	+39.252		
ABCDE	+43.267	3	+14.422	+0.593	
ABCDEF	+365.082	15	+24.339		
TOTAL	+78254.100	383			

N W/IN? 5 :A:B:C:D:EFUNC:0
DATA:3

E3TRPC.12

M= +78.211
F +50792.100 5 +10158.400

WITHIN

A	+528.679	1	+528.679	+1.200	
AF	+2194.870	5	+438.973		
B	+65066.000	1	+65066.000	+179.045	**
BF	+1817.030	5	+363.406		
C	+4.382	1	+4.382	+0.118	
CF	+185.242	5	+37.048		
D	+9233.810	3	+3077.940	+1.082	
DF	+42651.900	15	+2843.460		
E	+1464.890	3	+488.298	+0.640	
EF	+11445.000	15	+762.999		
AB	+0.156	1	+0.156	+0.002	
ABF	+370.297	5	+74.059		
AC	+14.408	1	+14.408	+0.124	
ACF	+580.609	5	+116.122		
AD	+522.457	3	+174.152	+0.349	
ADF	+7490.890	15	+499.393		
AE	+16.170	3	+5.390	+0.043	
AEF	+1866.510	15	+124.434		
BC	+251.579	1	+251.579	+5.845	
BCF	+215.227	5	+43.045		
BD	+975.951	3	+325.317	+0.729	
BDF	+6692.170	15	+446.145		
BE	+168.497	3	+56.166	+0.382	
BEF	+2203.740	15	+146.916		
CD	+126.498	3	+42.166	+0.253	
CDF	+2504.220	15	+166.948		
CE	+114.378	3	+38.126	+0.638	
CEF	+896.586	15	+59.772		
DE	+8738.000	9	+970.888	+0.742	
DEF	+58907.800	45	+1309.060		
ABC	+39.290	1	+39.290	+0.423	
ABCF	+464.008	5	+92.802		
ABD	+376.248	3	+125.416	+0.914	
ABDF	+2059.240	15	+137.283		
ABE	+289.013	3	+96.338	+0.656	
ABEF	+2203.380	15	+146.892		
ACD	+126.197	3	+42.066	+0.442	
ACDF	+1427.320	15	+95.155		
ACE	+73.927	3	+24.642	+0.589	
ACEF	+627.367	15	+41.824		
ADE	+2460.360	9	+273.373	+1.139	
ADEF	+10803.900	45	+240.087		
BCD	+5.756	3	+1.919	+0.025	
BCDF	+1167.920	15	+77.861		
BCE	+136.397	3	+45.466	+0.902	
BCEF	+756.469	15	+50.431		
BDE	+968.761	9	+107.640	+0.319	
BDEF	+15169.800	45	+337.108		
CDE	+493.659	9	+54.851	+0.558	
CDEF	+4421.020	45	+98.245		
ABCD	+66.709	3	+22.236	+0.516	
ABCDF	+646.547	15	+43.103		
ABCE	+84.334	3	+28.111	+0.312	
ABCEF	+1352.660	15	+90.177		
ABDE	+953.354	9	+105.928	+0.942	
ABDEF	+5062.400	45	+112.498		
ACDE	+380.847	9	+42.316	+0.434	
ACDEF	+4389.930	45	+97.554		
BCDE	+1865.630	9	+207.292	+3.047	**
BCDEF	+3061.210	45	+68.027		
ABCDE	+155.987	9	+17.332	+0.241	
ABCDEF	+3239.080	45	+71.980		
TOTAL	+343369.000	767			

Analysis of Var. on Percent Correct, Exper 2
using only data from the first two sessions.

NWAY?6 A 2 B 2 C 2 D 4 E 4 F 6 NCELL?1
 S?F P... e/d f... S...
 N W/IN? 5 :A:B:C:D:EFUNC:0
 DATA:3
 , E3TRRT.12

M= +1417.650
 F +54598200.000 5+10919600.000

WITHIN				
A	+2052000.000	1	+2052000.000	+0.676
AF	+15170600.000	5	+3034130.000	
B	+9934920.000	1	+9934920.000	+7.462 *
BF	+6657240.000	5	+1331450.000	
C	+63026.400	1	+63026.400	+0.302
CF	+1043470.000	5	+208694.000	
D	+2372920.000	3	+790973.000	+3.096 NS
DF	+3831870.000	15	+255458.000	
E	+2789350.000	3	+929782.000	+3.560 *
EF	+3917700.000	15	+261180.000	
AB	+57514.000	1	+57514.000	+0.752
ABF	+382632.000	5	+76526.400	
AC	+34436.000	1	+34436.000	+0.488
ACF	+352712.000	5	+70542.400	
AD	+488318.000	3	+162773.000	+3.084 NS
ADF	+791728.000	15	+52781.800	
AE	+232761.000	3	+77587.000	+0.931
AEF	+1249890.000	15	+83325.800	
BC	+2101.570	1	+2101.570	+0.111
BCF	+94904.000	5	+18980.800	
BD	+47336.500	3	+15778.800	+0.308
BDF	+768520.000	15	+51234.600	
BE	+15700.000	3	+5233.330	+0.061
BEF	+1289420.000	15	+85961.500	
CD	+3558.500	3	+1186.170	+0.037
CDF	+484112.000	15	+32274.100	
CE	+83280.500	3	+27760.100	+1.160
CEF	+359112.000	15	+23940.800	
DE	+1459180.000	9	+162130.000	+0.991
DEF	+7359560.000	45	+163546.000	
ABC	+6733.560	1	+6733.560	+0.138
ABCF	+243888.000	5	+48777.600	
ABD	+70810.500	3	+23603.500	+0.778
ABDF	+455128.000	15	+30341.800	
ABE	+347702.000	3	+115900.000	+2.257
ABEF	+770352.000	15	+51356.800	
ACD	+206063.000	3	+68687.600	+4.483 *+
ACDF	+229824.000	15	+15321.600	
ACE	+121902.000	3	+40634.200	+1.297
ACEF	+470064.000	15	+31337.600	
ADE	+174746.000	9	+19416.200	+0.419
ADEF	+2083050.000	45	+46289.900	
BCD	+37804.000	3	+12601.300	+0.307
BCDF	+615568.000	15	+41037.800	
BCE	+151225.000	3	+50408.500	+1.503
BCEF	+503096.000	15	+33539.700	
BDE	+291662.000	9	+32406.900	+0.901
BDEF	+1618620.000	45	+35969.400	
CDE	+230347.000	9	+25594.100	+0.864
CDEF	+1332540.000	45	+29612.100	
ABCD	+117226.000	3	+39075.300	+0.787
ABCDF	+744480.000	15	+49632.000	
ABCE	+101369.000	3	+33789.800	+1.815
ABCEF	+279256.000	15	+18617.100	
ABDE	+486614.000	9	+54068.200	+2.068 NS
ABDEF	+1176250.000	45	+26139.000	
ACDE	+382917.000	9	+42546.300	+1.401
ACDEF	+1366640.000	45	+30369.800	
BCDE	+173775.000	9	+19308.300	+0.722
BCDEF	+1202790.000	45	+26728.700	
ABCDE	+202667.000	9	+22518.500	+0.879
ABCDEF	+1152710.000	45	+25615.800	
TOTAL	*****	767		

ANOVA on RT's in Exper 2, Only data from
 sessions 1 and 2.

NWAYT5 A 2 B 2 C 2 D 2 E 5 NCELL?1

STE

N W/IN? 4 :A:B:C:DFUNC:0

DATA:3

E4SUM,AN

lowly

ANOVA w/out FC probably sum across sessions.

M= +745.534

E +995492.000 4 +248873.000

WITHIN

A	+33947.600	1	+33947.600	+12.660 *
AE	+10725.600	4	+2681.410	
B	+163.959	1	+163.959	+0.301
BE	+2179.500	4	+544.875	
C	+207.118	1	+207.118	+0.529
CE	+1564.870	4	+391.219	
D	+32562.800	1	+32562.800	+3.459
DE	+37650.500	4	+9412.620	
AB	+78.096	1	+78.096	+0.232
ABE	+1344.620	4	+336.156	
AC	+124.163	1	+124.163	+0.079
ACE	+6302.250	4	+1575.560	
AD	+652.640	1	+652.640	+0.267
ADE	+9787.620	4	+2446.910	
BC	+1766.540	1	+1766.540	+1.870
BCE	+3778.370	4	+944.594	
BD	+990.453	1	+990.453	+1.142
BDE	+3470.620	4	+867.656	
CD	+802.187	1	+802.187	+1.226
CDE	+2617.000	4	+654.250	
ABC	+48.197	1	+48.197	+0.114
ABCE	+1686.250	4	+421.562	
ABD	+427.961	1	+427.961	+1.180
ABDE	+1450.620	4	+362.656	
ACD	+726.562	1	+726.562	+1.247
ACDE	+2331.000	4	+582.750	
BCD	+334.453	1	+334.453	+0.480
BCDE	+2785.500	4	+696.375	
ABCD	-0.035	1	-0.035	-0.000
ABCDE	+1891.000	4	+472.750	
TOTAL	+1157890.000	79		

FACTOR:A

+724.937

+766.136

FACTOR:B

+744.105

+746.968

FACTOR:C

+747.145

+743.927

FACTOR:D

+765.712

+725.361

FACTOR:AE

+702.149

+612.972

+810.492

+627.269

+871.802

+723.143

+640.717

+855.984

+654.493

+956.343

This TRIAL e/d.

N-1

N-2

Session bias

Trial e

Bias Subj

d

ANOVA on RT's from Exp 3
omitting subject FC
AN 3-2

AN4-1

+1673.690	+2031.170	+1845.070	+1917.110	+1768.140	+1996.530	+1798.71
+1754.120	+1952.780	+1767.410	+2019.300	+1748.250	+1918.810	+1718.87
+693.176	+621.687	+654.968	+662.490	+620.562	+691.246	+686.21
+676.799	+652.731	+661.753	+651.430	+656.221	+660.666	+662.86
+1164.430	+1254.800	+1211.920	+1306.790	+1202.160	+1277.270	+1160.07
+866.302	+978.942	+921.472	+1009.510	+870.460	+1050.560	+900.41
+928.533	+1091.880	+1005.560	+1015.380	+898.483	+1035.700	+933.53
+924.689	+976.342	+953.059	+946.760	+972.941	+1061.210	+1003.30
+743.692	+815.446	+662.758	+764.536	+698.319	+784.317	+684.40
+747.065	+840.386	+739.069	+847.471	+720.647	+834.619	+733.39

NWAY75 A 2 B 2 C 2 D 2 E 5 NCELL71

STE

N W/IN? 4 :A:B:C:DFUNC:0

DATA:3 4BSUM.AN

ANOVAEXP 4bData summed across ses

M= +1075.200

E +14583600.000 4 +3645910.000

WITHIN

A	+188942.000	1	+188942.000	+6.825	10%
AE	+110736.000	4	+27684.000		
B	+1155.750	1	+1155.750	+0.756	
BE	+6114.000	4	+1528.500		
C	+33.358	1	+33.358	+0.073	
CE	+1820.000	4	+455.000		
D	+48975.000	1	+48975.000	+0.681	
DE	+287842.000	4	+71960.500		
AB	+3251.310	1	+3251.310	+2.805	
ABE	+4636.000	4	+1159.000		
AC	+2258.700	1	+2258.700	+9.372	*
ACE	+964.000	4	+241.000		
AD	+293.187	1	+293.187	+0.247	
ADE	+4748.000	4	+1187.000		
BC	+141.076	1	+141.076	+0.084	
BCE	+6696.000	4	+1674.000		
BD	+945.328	1	+945.328	+1.405	
BDE	+2692.000	4	+673.000		
CD	+2376.270	1	+2376.270	+0.673	
CDE	+14124.000	4	+3531.000		
ABC	+509.237	1	+509.237	+0.190	
ABCE	+10716.000	4	+2679.000		
ABD	+4389.500	1	+4389.500	+0.722	
ABDE	+24326.000	4	+6081.500		
ACD	+320.297	1	+320.297	+0.514	
ACDE	+2494.000	4	+623.500		
BCD	+117.484	1	+117.484	+0.141	
BCDE	+3344.000	4	+836.000		
ABCD	+171.828	1	+171.828	+0.243	
ABCDE	+2828.000	4	+707.000		
TOTAL	+15321600.000	79			

FACTOR:A

+1026.610

+1123.800

FACTOR:B

+1071.410

+1079.010

FACTOR:

ANOVA on RT's in Exp 4.

AN 4 - 1.

1.3	8.2	5.2	10.6	20.2
1.2	48.5	18.52	2.13	2.56
1.1	20.62	10.13	5.54	2.62

E52 BCK. RT 4-AUG-77

```

NWAY?5    A 2   B 2   C 2   D 4   E
NWAY?5    A 2   B 2   C 2   D 2   E 4 NCELL?1
STE
N W/IN? 3 4 :A:B:C:DFUNC:0
DATA:3
M=            +831.593
E                +730530.000            3    +243510.000

```

WITHIN				
A	+960890.000	1	+960890.000	+12.024 5%
AE	+239745.000	3	+79914.900	
B	+16.000	1	+16.000	+0.003
BE	+17415.400	3	+5805.120	
C	+1980.250	1	+1980.250	+2.098
CE	+2832.250	3	+944.083	
D	+8055.060	1	+8055.060	+0.269
DE	+89866.900	3	+29955.600	
AB	+10712.200	1	+10712.200	+1.255
ABE	+25609.600	3	+8536.540	
AC	+4356.000	1	+4356.000	+7.762 5-10%
ACE	+1683.620	3	+561.208	
AD	+1173.060	1	+1173.060	+0.268
ADE	+13123.200	3	+4374.410	
BC	+5.062	1	+5.062	+0.003
BCE	+5035.750	3	+1678.580	
BD	+7056.000	1	+7056.000	+0.913
BDE	+23187.100	3	+7729.040	
CD	+3540.250	1	+3540.250	+5.258
CDE	+2019.870	3	+673.292	
ABC	+612.375	1	+612.375	+0.385
ABCE	+4772.870	3	+1590.960	
ABD	+5700.060	1	+5700.060	+1.160
ABDE	+14742.000	3	+4914.000	
ACD	+6560.810	1	+6560.810	+8.999 5-10%
ACDE	+2187.250	3	+729.083	
BCD	+3510.560	1	+3510.560	+1.446
BCDE	+7282.120	3	+2427.370	
ABCD	+612.062	1	+612.062	+0.265
ABCDE	+6921.500	3	+2307.170	
TOTAL	+2201740.000	63		

```

FACTOR:AC
+711.750
+706.375
+940.312
+967.937
FACTOR:ACD
+721.375
+702.125

```

ANOVA on RT's of Exp 7.

AN 5 - 1

NWAY?5 A 2 B 2 C 2 D 10 E 2 3 NCELL?1
 STD
 N W/IN? 4 :A:B:C:EFUNC:0
 DATA:3

E6A2BK.RT

M= +1453.780

D +45060800.000 9 +5006760.000

WITHIN

A	+6309600.000	1	+6309600.000	+42.976	**
AD	+1321330.000	9	+146815.000		
B	+224.267	1	+224.267	+0.023	
BD	+88608.000	9	+9845.330		
C	+11760.000	1	+11760.000	+0.530	
CD	+199752.000	9	+22194.600		
E	+3189470.000	2	+1594730.000	+3.759	*
DE	+7636190.000	18	+424233.000		
AB	+3801.730	1	+3801.730	+1.071	
ABD	+31936.000	9	+3548.440		
AC	+1961.000	1	+1961.000	+0.143	
ACD	+123448.000	9	+13716.400		
AE	+246708.000	2	+123354.000	+11.115	**
ADE	+199772.000	18	+11098.400		
BC	+14415.000	1	+14415.000	+2.949	2.22 almost 10%
BCD	+43992.000	9	+4888.000		
BE	+5720.500	2	+2860.250	+0.299	
BDE	+171996.000	18	+9555.330		
CE	+4316.000	2	+2158.000	+0.249	
CDE	+156116.000	18	+8673.110		
ABC	+7932.730	1	+7932.730	+0.606	
ABCD	+117808.000	9	+13089.800		
ABE	+22867.500	2	+11433.700	+0.856	
ABDE	+240516.000	18	+13362.000		
ACE	+41412.000	2	+20706.000	+1.915	2.6
ACDE	+194588.000	18	+10810.400		
BCE	+10506.000	2	+5253.000	+1.102	
BCDE	+85764.000	18	+4764.660		
APCE	+525.500	2	+262.750	+0.015	
ABCDE	+324044.000	18	+18002.400		
TOTAL	+65867900.000	239			

FACTOR:A **

+1291.640

+1615.920

FACTOR:B

+1454.750

+1452.820

FACTOR:C

+1460.780

+1446.780

FACTOR:E *

+1309.050

+1461.160

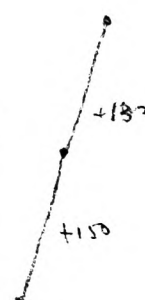
+1591.140

FACTOR:

ANOVA across RSI's in exp 8.

AN 8-1

As ISI ↑, so RT's go up (see also AE)
 more or less linearly.



1136 1620 1210 1723 1252 1488 1174 1526
1200 1297 1132 1462 918 1528 982 1148
2106 2795 2294 2489 2063 2629 1896 2378
765 1066 896 1172 824 1088 875 1211
1466 1810 1251 1799 1197 1656 1253 1773
801 917 823 954 847 914 814 980
1036 1297 1066 1221 1099 1309 1003 1236
767 771 780 839 764 828 773 774
1656 1678 1638 1888 1794 2013 1693 1799
984 1053 977 1046 982 1193 1008 1161
ANOVA EXPR 6 FOR 2 BACK 4-WAY
A THIS TRIAL E/D
B TRIAL N-1 E/D
C TRIAL N-2 E/D
D SUBJECTS (10)
E6S2BK.RT

ANOVA

EXP 6 'SHORT' ISI

NWAY?4 A 2 B 2 C 2 D 10 NCELL?1
STD
N W/IN? 3 :A:B:CFUNC:0
DATA:3

M= +1309.050
D +15758100.000 9 +1750900.000

WITHIN		S			
A	+1334890.000	1	+1334890.000	+27.525	**
AD	+436482.000	9	+48498.000		
B	+3001.250	1	+3001.250	+0.377	
BD	+71652.000	9	+7961.330		
C	+13468.000	1	+13468.000	+0.793	
CD	+152822.000	9	+16980.200		
AB	+793.500	1	+793.500	+0.114	
ABD	+62776.000	9	+6975.110		
AC	+3225.700	1	+3225.700	+0.658	
ACD	+44124.000	9	+4902.600		
BC	+23392.800	1	+23392.800	+2.942	no %
BCD	+71552.000	9	+7950.220		
ABC	+3510.700	1	+3510.700	+0.319	
ABCD	+99114.000	9	+11012.700		
TOTAL	+18078900.000	79			

FACTOR:

ANOVA on RT's exp 8, RSI = 500 msecs

AN 8-2

1037 1500 1019 1450 960 1636 1054 1434
 1020 1592 1054 1688 972 1415 1125 1385
 3094 3050 2592 3413 2713 3282 2887 3137
 801 1154 940 1175 827 1121 862 1017
 1313 2004 1278 1593 1488 1650 1348 1683
 901 1018 928 1207 954 1106 943 1150
 1211 1613 1368 1596 1344 1609 1172 1669
 875 1000 899 1051 871 1021 948 983
 1251 1708 1540 1654 1541 1632 1458 1793
 1523 1627 1342 1546 1488 1619 1439 1532
 ANOVA EXPER 6 MEDIUM (1500MSEC) ISI 4-WAY
 A THIS TRIAL E/D
 B TRIAL N-1 E/D
 C TRIAL N-2 E/D
 D SUBJECTS (10)
 E6M2BK.RT

ANOVA

EXP 6 'Medium' IS

NWAY?4 A 2 B 2 C 2 D 10 NCELL?1
 STD
 N W/IN? 3 :A:B:CFUNC:0
 DATA:3
 "

M= +1461.160
 D +25576400.000 9 +2841830.000

WITHIN		G			
A	+1840120.000	1	+1840120.000	+50.848	**
AD	+325700.000	9	+36188.900		
B	+446.515	1	+446.515	+0.043	
BD	+93720.000	9	+10413.300		
C	+1593.100	1	+1593.100	+0.316	
CD	+45420.000	9	+5046.660		
AB	+568.985	1	+568.985	+0.069	
ABD	+74336.000	9	+8259.550		
AC	+17201.400	1	+17201.400	+2.597	20%
ACD	+59616.000	9	+6624.000		
BC	+918.015	1	+918.015	+0.201	
BCD	+41068.000	9	+4563.110		
ABC	+3904.380	1	+3904.380	+0.119	
ABCD	+295288.000	9	+32809.800		
TOTAL	+28376300.000	79			

FACTOR:A
 +1309.500
 +1612.820
 FACTOR:B
 +1463.520
 +1458.800
 FACTOR:C
 +1465.620
 +1456.700
 FACTOR:AB
 +1309.200
 +1309.800
 +1617.850
 +1607.800
 FACTOR:AC
 +1299.300
 +1319.700

ANOVA on RT's Exp 8, RSI = 1500 msecs

AN 8 - 3

	$B \rightarrow C$	$C \rightarrow D$	$D \rightarrow D$
	1319.700	1631.950	1593.700

+1651.700
+1593.700
FACTOR:

PAGE 1 'E6L2BK.RT' CREATED FRIDAY AUGUST 5 1977

1357 1859 1121 1761 1232 1973 1180 1810
1784 2165 1598 2483 1504 2070 1422 2443
2098 2218 2039 2459 1691 2413 1745 2557
961 1164 995 1250 906 1256 1027 1170
1220 1854 1209 2021 1348 1946 1335 1871
1000 1382 1076 1369 1116 1432 1162 1399
1386 1783 1393 1819 1439 1680 1329 1897
879 976 880 977 964 1029 910 1049
1802 2268 1919 2276 1847 2028 1819 2012
1704 1867 1700 1716 1717 2104 1608 2063
ANOVA EXFER 6 FOR 2 BACK 4-WAY LONG ISI (4000 MSECs)
A THIS TRIAL E/D
B TRIAL N-1 E/D
C TRIAL N-2 E/D
D SUBJECTS (10)
E6L2BK.RT

NUWAY?4 A 2 B 2 C 2 D 10 NCELL?1
S?D
N M/INT? 3 :A:B:CFUNC:0
DATA:3

M= +1591.140
D +11362600.000 9 +1262510.000

WITHIN				
A	+3381300.000	1	+3381300.000	+40.097 **
AD	+758954.000	9	+84328.200	
B	+2497.610	1	+2497.610	+0.236
BD	+95276.000	9	+10586.200	
C	+1015.310	1	+1015.310	+0.058
CD	+157676.000	9	+17519.500	
AB	+25306.900	1	+25306.900	+1.682
ABD	+135446.000	9	+15049.500	
AC	+22945.700	1	+22945.700	+0.963
ACD	+214352.000	9	+23816.900	
BC	+610.514	1	+610.514	+0.319
BCD	+17250.000	9	+1916.670	
ABC	+1048.080	1	+1048.080	+0.197
ABCD	+47506.000	9	+5278.440	
TOTAL	+16223700.000	79		

FACTOR:A
+1385.550
+1796.720

FACTOR:B
+1585.550
+1596.720

FACTOR:C
+1594.700
+1587.570

FACTOR:AB
+1397.750
+1373.350
+1773.350
+1820.100

FACTOR:

ANOVA on RT's Exp 8, RSI = 4000 ms

AN 8 - 4

No of Easy Displays in Window (X)	No of Observations	Mean RT (Y)	SD	
0	98	573	166	Experiment 1 EASY display RT's $Y = 557 - 3.24 X$ $t = -1.1$ (df 3297)
1	524	553	161	
2	1072	548	177	
3	1082	548	182	
4	448	548	205	
5	75	532	179	
0	72	851	496	Experiment 1 DIFFICULT display RT's $Y = 805 + 11.57 X$ $t = +1.55$ (df 3328)
1	500	834	436	
2	1039	814	457	
3	1104	835	483	
4	509	850	460	
5	106	943	563	
0	54	1125	537	Experiment 2 EASY trial RT's $Y = 1181 - 6.83 X$ $t = -1.695$ (df 8698)
1	289	1190	575	
2	1034	1201	570	
3	1901	1140	535	
4	2303	1153	542	
5	1852	1142	535	
6	945	1151	559	
7	282	1178	560	
8	40	1014	312	
0	49	1474	808	Experiment 2 DIFFICULT trial RT's $Y = 1395 - 4.22 X$ $t = -0.8$ (df 8756)
1	326	1346	762	
2	935	1396	731	
3	1860	1387	723	
4	2321	1378	711	
5	1989	1373	696	
6	925	1355	719	
7	323	1395	765	
8	30	1325	683	

Table of the values used in the computation of Regression in Experiment 1, window size 5, and Experiment 2, window size 8. In both cases the data are summed across subjects, and are from a Sample Size analysis.

Appendix REG-1 and REG-2.

See Figures 3-5 and 3-7.

APPENDIX C

The Correlations referred to in
Chapter 9.

BN 777	100	NO OF BNS	777	12
BN 777	114	NO OF BNS	777	12
BN 777	150	NO OF BNS	777	12
BN 777	442	NO OF BNS	777	12
BN 777				
NO OF BNS	777			

Subject 1
Sessions 1 to 4GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

GROUPS 1 1

CC	942.000	1228.92		
	942.000	1292.29		
R=	0.127138	T=	3.92937	DF= 940.000
Y=	1124.70		0.136375X	

CE	13.0000	1303.85		
	13.0000	2869.92		
R=	0.169582	T=	0.57071	DF= 11.0000
Y=	2474.97		0.302916X	

EC	11.0000	2620.00		
	11.0000	1135.32		
R=	-0.088649	T=	-0.266998	DF= 9.00000
Y=	1260.25		-0.0474920X	

GROUPS 1 2

CC	759.000	1261.41		
	759.000	1870.25		
R=	0.069472	T=	1.91606	DF= 757.000
Y=	1745.62		0.093805X	

CE	190.000	1269.75		
	190.000	2130.46		
R=	0.121610	T=	1.67990	DF= 188.000
Y=	1918.94		0.166590X	

EC	9.00000	1537.33		
	9.00000	1380.00		
R=	-0.056862	T=	-0.150688	DF= 7.00000
Y=	1410.34		-0.0197374X	

EE	3.00000	1289.00		
	3.00000	2134.67		
R=	-0.69570	T=	-0.96850	DF= 1.00000
Y=	3226.54		-0.84707 X	

GROUPS 2 1

CC	764.000	1793.21		
	764.000	1210.79		
R=	0.092240	T=	2.55714	DF= 762.000
Y=	1097.47		0.063196X	

CE	6.00000	1469.17		
	6.00000	954.667		
R=	0.0419963	T=	0.034067	DF= 4.00000
Y=	927.824		0.0182708X	

EC	196.000	2151.54		
	196.000	1174.06		
R=	0.080073	T=	1.11888	DF= 194.000
Y=	1069.10		0.0487836X	

EE	1.00000	3972.00		
	1.00000	3354.00		

GROUPS 2 2

CC	586.000	1805.51		
	586.000	1743.41		
R=	0.061676	T=	1.49331	DF= 584.000
Y=	1635.21		0.059929X	

CE	148.000	1726.90		
	148.000	2261.77		
R=	-0.061054	T=	-0.73910	DF= 146.000
Y=	2366.21		-0.060479X	

EC	144.000	2051.34		
	144.000	1681.69		
R=	-0.128668	T=	-1.54611	DF= 142.000
Y=	1897.06		-0.104937X	

EE	41.0000	2247.22		
	41.0000	1906.85		
R=	-0.091098	T=	-0.57128	DF= 39.0000
Y=	2117.03		-0.093550X	

GROUPS 1 1							
CC	823.000		1269.06				
	823.000		1274.77				
R=	0.322831	T=	9.77510	DF=	821.000	*	
Y=	865.532		0.322473 X				
CE	72.0000		1398.57				
	72.0000		1908.89				
R=	0.422873	T=	3.90332	DF=	70.0000		
Y=	1156.28		0.53813 X				
EC	61.0000		1745.15				
	61.0000		1289.93				
R=	0.467675	T=	4.06412	DF=	59.0000		
Y=	731.469		0.320011 X				
EE	11.0000		1775.82				
	11.0000		1865.82				
R=	0.0309419	T=	0.092870	DF=	9.00000		
Y=	1826.01		0.0224185 X				
GROUPS 1 2							
CC	642.000		1247.99				
	642.000		1443.04				
R=	0.290731	T=	7.63702	DF=	640.000		
Y=	1010.00		0.346992 X				
CE	259.000		1259.37				
	259.000		1691.56				
R=	0.209473	T=	3.43429	DF=	257.000		
Y=	1314.83		0.299143 X				
EC	58.0000		1965.97				
	58.0000		1618.59				
R=	0.257366	T=	1.99309	DF=	56.0000		
Y=	1152.18		0.237240 X				
EE	27.0000		1851.89				
	27.0000		2084.78				
R=	0.078718	T=	0.394315	DF=	25.0000		
Y=	1934.11		0.081361 X				
GROUPS 2 1							
CC	623.000		1456.95				
	623.000		1264.27				
R=	0.234363	T=	6.00760	DF=	621.000		
Y=	963.225		0.206355 X				
CE	50.0000		1602.16				
	50.0000		1718.22				
R=	0.323525	T=	2.36384	DF=	43.0000		
Y=	1127.40		0.368764 X				
EC	291.000		1691.66				
	291.000		1254.93				
R=	0.171042	T=	2.95120	DF=	239.000		
Y=	1050.67		0.120746 X				
EE	25.0000		1951.92				
	25.0000		1923.20				
R=	0.62614	T=	3.85128	DF=	23.0000		
Y=	848.891		0.55039 X				
GROUPS 2 2							
CC	513.000		1520.01				
	513.000		1541.65				
R=	0.303403	T=	7.19779	DF=	511.000		
Y=	1076.40		0.306084 X				
CE	211.000		1450.75				
	211.000		1661.51				
R=	0.225114	T=	3.34017	DF=	209.000		
Y=	1279.70		0.263177 X				
EC	183.000		1705.56				
	183.000		1454.15				
R=	0.242013	T=	3.35571	DF=	181.000		
Y=	1118.29		0.185917 X				
EE	89.0000		1711.78				
	89.0000		1829.96				
R=	0.278579	T=	2.70551	DF=	87.0000		
Y=	1353.09		0.273573 X				

TRAJIT 0032L

BN 777

Subject 3
Sessions 1 to 4

GROUPS 1 1

CC 475.000 983.945
475.000 969.061
R= 0.470394 T= 11.5931 DF= 473.000
Y= 507.931 0.466284X

CE 188.000 906.840
188.000 925.739
R= 0.447747 T= 6.82926 DF= 186.000
Y= 548.103 0.416430X

EC 187.000 925.219
187.000 943.674
R= 0.52382 T= 8.36399 DF= 185.000
Y= 465.545 0.51677 X

EE 97.0000 874.753
97.0000 895.907
R= 0.214367 T= 2.13912 DF= 95.0000
Y= 678.971 0.247997X

GROUPS 1 2

CC 359.000 961.031
359.000 946.440
R= 0.488095 T= 10.5664 DF= 357.000
Y= 525.417 0.438095X

CE 264.000 982.049
264.000 1021.79
R= 0.61476 T= 12.6164 DF= 262.000
Y= 354.306 0.67969 X

EC 150.000 951.620
150.000 925.213
P= 0.396232 T= 5.25009 DF= 148.000
Y= 555.168 0.388858X

EE 138.000 942.341
138.000 940.326
R= 0.479157 T= 6.36630 DF= 136.000
Y= 445.221 0.52540 X

GROUPS 2 1

CC 364.000 995.588
364.000 937.393
R= 0.369861 T= 7.57420 DF= 362.000
Y= 659.754 0.279371X

CE 159.000 910.063
159.000 952.296
R= 0.485826 T= 6.96453 DF= 157.000
Y= 300.675 0.71602 X

EC 257.000 973.253
257.000 1001.85
R= 0.493086 T= 9.17258 DF= 255.000
Y= 547.513 0.466821X

EE 125.000 921.336
125.000 933.344
R= 0.53728 T= 7.06504 DF= 123.000
Y= 432.856 0.54322 X

GROUPS 2 2

CC 292.000 960.110
292.000 997.199
R= 0.55528 T= 10.7917 DF= 290.000
Y= 397.939 0.62416 X

CE 234.000 964.748
234.000 975.667
R= 0.67846 T= 14.0669 DF= 232.000
Y= 277.396 0.72379 X

EC 251.000 1012.44
251.000 998.235
R= 0.59864 T= 11.7931 DF= 249.000
Y= 369.926 0.62059 X

EE 171.000 1047.65
171.000 1023.67
R= 0.65850 T= 11.3749 DF= 169.000
Y= 371.242 0.62275 X

TRANSIT CORREL

BN 777

GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 4
Sessions 1 to 4

GROUPS 1 1

CC 593.000 1201.53
593.000 1180.85
R= 0.134895 T= 3.30961 DF= 591.000
Y= 1020.03 0.133851X

CE 133.000 1122.09
133.000 1037.80
R= 0.133850 T= 1.54590 DF= 131.000
Y= 864.031 0.154866X

EC 133.000 1057.86
133.000 1136.76
R= 0.111914 T= 1.28902 DF= 131.000
Y= 1037.01 0.094290X

EE 48.0000 881.063
48.0000 930.000
R= 0.0444501 T= 0.301774 DF= 46.0000
Y= 885.384 0.050640X

GROUPS 1 2

CC 472.000 1223.25
472.000 1365.74
R= 0.114357 T= 2.49558 DF= 470.000
Y= 1197.77 0.137309X

CE 263.000 1135.18
263.000 1312.24
R= 0.141156 T= 2.30351 DF= 261.000
Y= 1109.62 0.178492X

EC 111.000 1083.20
111.000 1171.27
R= 0.107995 T= 1.13413 DF= 109.000
Y= 1059.65 0.103047X

EE 79.0000 1012.03
79.0000 1035.06
R= 0.094001 T= 0.82853 DF= 77.0000
Y= 935.710 0.098173X

GROUPS 2 1

CC 455.000 1367.88
455.000 1222.94
R= 0.138530 T= 2.97714 DF= 453.000
Y= 1061.71 0.117867X

CE 121.000 1260.83
121.000 1069.38
R= 0.132376 T= 1.45687 DF= 119.000
Y= 901.709 0.132985X

EC 276.000 1183.29
276.000 1179.02
R= 0.145450 T= 2.43351 DF= 274.000
Y= 1025.53 0.129717X

EE 71.0000 1214.82
71.0000 1044.93
R= 0.288533 T= 2.50320 DF= 69.0000
Y= 742.290 0.249124X

GROUPS 2 2

CC 377.000 1377.96
377.000 1351.85
R= 0.070990 T= 1.37820 DF= 375.000
Y= 1258.06 0.068066X

CE 216.000 1236.93
216.000 1226.52
R= 0.074104 T= 1.08704 DF= 214.000
Y= 1133.86 0.074911X

EC 215.000 1252.76
215.000 1310.69
R= 0.222771 T= 3.33504 DF= 213.000
Y= 1039.84 0.216204X

EE 155.000 1359.52
155.000 1228.90
R= 0.264983 T= 3.39916 DF= 153.000
Y= 918.007 0.228680X

TRANSIT CORR

Subject 5
Sessions 1 to 4

GROUPS 1 1		Sessions 1 to 4	
CC	794.000 794.000	1389.64 1392.43	
R=	0.142444	T=	4.05002
Y=	1193.46		0.143182X
DF=			792.000
CE	79.0000 79.0000	1440.39 1574.47	
R=	0.105361	T=	0.92971
Y=	1418.12		0.108548X
DF=			77.0000
EC	90.0000 90.0000	1520.78 1345.37	
R=	0.0222499	T=	0.208774
Y=	1317.76		0.0181552X
DF=			88.0000
EE	14.0000 14.0000	1328.29 1504.57	
R=	-0.222901	T=	-0.79208
Y=	1845.10		-0.256364X
DF=			12.0000
GROUPS 1 2			
CC	618.000 618.000	1416.63 1568.69	
R=	0.174687	T=	4.40332
Y=	1308.02		0.184009X
DF=			616.000
CE	299.000 299.000	1349.21 1652.43	
R=	0.0380300	T=	0.65587
Y=	1592.73		0.0442503X
DF=			297.000
EC	58.0000 58.0000	1661.62 1574.41	
R=	0.214690	T=	1.64495
Y=	1225.80		0.209804X
DF=			56.0000
EE	34.0000 34.0000	1710.47 1400.91	
R=	0.150728	T=	0.86250
Y=	1194.27		0.120811X
DF=			32.0000
GROUPS 2 1			
CC	609.000 609.000	1562.06 1419.78	
R=	0.101877	T=	2.52311
Y=	1272.74		0.094129X
DF=			607.000
CE	67.0000 67.0000	1649.63 1654.97	
R=	0.198048	T=	1.62898
Y=	1309.86		0.209208X
DF=			65.0000
EC	298.000 298.000	1631.48 1391.32	
R=	-0.00382404	T=	-0.065792
Y=	1397.32		-0.00368126X
DF=			296.000
EE	37.0000 37.0000	1792.49 1584.86	
R=	0.0199874	T=	0.118270
Y=	1553.57		0.0174606X
DF=			35.0000
GROUPS 2 2			
CC	433.000 433.000	1616.74 1609.92	
R=	0.058377	T=	1.21402
Y=	1512.16		0.060465X
DF=			431.000
CE	219.000 219.000	1587.08 1655.36	
R=	0.157714	T=	2.35271
Y=	1401.65		0.159354X
DF=			217.000
EC	221.000 221.000	1576.93 1637.36	
R=	0.121978	T=	1.81870
Y=	1441.97		0.123906X
DF=			219.000
EE	83.0000 83.0000	1588.72 1566.90	
R=	0.135300	T=	1.22900
Y=	1390.62		0.110957X
DF=			81.0000
TRANSIT CORREL			
BN 777			

GROUPS 1 1
CC 893.000 1100.23
893.000 1097.15
R= 0.285471 T= 8.89119 DF= 891.000
Y= 785.669 0.283104X

CE 44.0000 1105.98
44.0000 1258.77
R= -0.075305 T= -0.489420 DF= 42.0000
Y= 1367.72 -0.093504X

EC 36.0000 1107.83
36.0000 991.722
R= -0.0301860 T= -0.176093 DF= 34.0000
Y= 1016.51 -0.0223745X

EE 1.00000 1465.00
1.00000 1178.00

GROUPS 1 2
CC 714.000 1091.20
714.000 1206.37
R= 0.228735 T= 6.26963 DF= 712.000
Y= 919.518 0.262873X

CE 254.000 1080.17
254.000 1260.62
R= 0.125467 T= 2.00760 DF= 252.000
Y= 1090.23 0.157742X

EC 25.0000 1295.68
25.0000 1220.12
R= 0.311244 T= 1.57069 DF= 23.0000
Y= 795.220 0.327936X

EE 22.0000 1342.45
22.0000 1208.27
R= 0.124929 T= 0.56311 DF= 20.0000
Y= 1102.97 0.078444X

GROUPS 2 1
CC 733.000 1232.16
733.000 1096.41
R= 0.184557 T= 5.07709 DF= 731.000
Y= 894.404 0.163943X

CE 25.0000 1188.48
25.0000 1242.32
R= 0.439479 T= 2.34641 DF= 23.0000
Y= 642.867 0.50439 X

EC 247.000 1264.22
247.000 1092.95
R= 0.258347 T= 4.18587 DF= 245.000
Y= 806.402 0.226661X

EE 14.0000 1297.79
14.0000 1127.64
R= 0.171636 T= 0.60352 DF= 12.0000
Y= 933.055 0.149938X

GROUPS 2 2
CC 513.000 1203.26
513.000 1241.30
R= 0.165529 T= 3.79418 DF= 511.000
Y= 1034.56 0.171815X

CE 192.000 1244.29
192.000 1311.06
R= 0.257108 T= 3.66727 DF= 190.000
Y= 982.787 0.263821X

EC 205.000 1311.04
205.000 1242.93
R= 0.191932 T= 2.78642 DF= 203.000
Y= 1024.64 0.166503X

EE 63.0000 1268.48
63.0000 1314.52
R= 0.114714 T= 0.90190 DF= 61.0000
Y= 1193.43 0.095467X

TRANSIT CORREL

BN ???

BN ??? 100 NO OF BNS ??? 12
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4
GROUPS 1 1

Subject 1
Session 1
All runs

CC 239.000 1362.22
239.000 1436.26
R= 0.0425131 T= 0.65507 DF= 237.000
Y= 1377.26 0.0433142X

CE 7.00000 1390.71
7.00000 2581.00
R= 0.60922 T= 1.71784 DF= 5.00000
Y= 399.669 1.56850 X

EC 5.00000 2439.80
5.00000 1291.60
R= 0.53192 T= 1.08800 DF= 3.00000
Y= 401.708 0.364740X

GROUPS 1 2
CC 186.000 1419.47
186.000 2099.13
R= 0.0181485 T= 0.246219 DF= 184.000
Y= 2062.15 0.0260496X

CE 52.0000 1456.27
52.0000 2232.63
R= 0.0376139 T= 0.256159 DF= 50.0000
Y= 2168.62 0.0439613X

EC 4.00000 2240.00
4.00000 1416.00
R= -0.176985 T= -0.254310 DF= 2.00000
Y= 1625.83 -0.093676X

EE 1.00000 880.000
1.00000 3232.00

GROUPS 2 1
CC 202.000 1909.76
202.000 1342.85
R= 0.0105631 T= 0.149394 DF= 200.000
Y= 1328.90 0.0073036X

CE 3.00000 2163.67
3.00000 1324.00
R= -0.97680 T= -4.56135 DF= 1.00000
Y= 2051.75 -0.336351X

EC 40.0000 2232.53
40.0000 1432.88
R= -0.234102 T= -1.48435 DF= 38.0000
Y= 1801.97 -0.165327X

GROUPS 2 2
CC 147.000 1969.20
147.000 1834.31
R= 0.0123597 T= 0.148842 DF= 145.000
Y= 1811.47 0.0115974X

CE 29.0000 1963.90
29.0000 2206.66
R= -0.0215602 T= -0.112056 DF= 27.0000
Y= 2240.54 -0.0172533X

EC 41.0000 1964.34
41.0000 1999.00
R= -0.136146 T= -0.85822 DF= 39.0000
Y= 2255.50 -0.130579X

EE 9.00000 2208.00
9.00000 1724.11
R= -0.064904 T= -0.172082 DF= 7.00000
Y= 1833.51 -0.0495463X

TRANSIT CORREL

BN ??? 114 NO OF BNS ??? 12
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4
GROUPS 1 1

CC 236.000 1338.58
236.000 1431.01
R= 0.115442 T= 1.77781 DF= 234.000
Y= 1255.37 0.131215X

EC 1.00000 27.0000
1.00000

Y= 1833.71 -0.0495463X
TRANSIT CORREL

BN ??? 114 NO OF BNS ??? 12
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4
GROUPS 1 1

Subject 1
Session 2
All runs

CC 236.000 1338.58
236.000 1431.01
R= 0.115442 T= 1.77781 DF= 234.000
Y= 1255.37 0.131215X

EC 1.00000 27.0000
1.00000 1924.00
GROUPS 1 2
CC 195.000 1370.93
195.000 2023.31
R= 0.0365550 T= 0.50818 DF= 193.000
Y= 1960.55 0.0457806X

CE 39.0000 1349.64
39.0000 2420.10
R= 0.079111 T= 0.482727 DF= 37.0000
Y= 2292.73 0.094372X

EC 2.00000 52.5000
2.00000 1394.00
GROUPS 2 1
CC 192.000 1884.30
192.000 1293.84
R= 0.062853 T= 0.86809 DF= 190.000
Y= 1208.07 0.0455216X

EC 44.0000 2389.09
44.0000 1270.82
R= 0.253897 T= 1.70119 DF= 42.0000
Y= 791.867 0.200474X

GROUPS 2 2
CC 158.000 2042.29
158.000 1950.52
R= -0.0473924 T= -0.59260 DF= 156.000
Y= 2045.34 -0.0464271X

CE 33.0000 2064.94
33.0000 2471.73
R= -0.217829 T= +1.24266 DF= 31.0000
Y= 2864.63 -0.190274X

EC 24.0000 2499.75
24.0000 1749.67
R= 0.174265 T= 0.83008 DF= 22.0000
Y= 1465.45 0.113697X

EE 11.0000 2298.73
11.0000 2311.09
R= -0.209410 T= -0.64247 DF= 9.00000
Y= 2897.39 -0.255054X

TRANSIT CORREL

BN ??? 130 NO OF BNS ??? 12
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG

11.0000 2298.75
 11.0000 2311.09
 R= -0.209410 T= -0.64247
 Y= 2897.39 -0.255054X
 TRANSIT CORREL

Subject 1
 Session 3
 All runs

BN 777 130 NO OF BNS 777 12
 BN 777 0 NO OF BNS 777 0
 GROUP 1 ACEG
 GROUP 2 BDFH
 GROUP 3
 GROUP 4
 GROUPS 1 1

CC 232.000 1021.59
 232.000 1115.47
 P= 0.156303 T= 2.39995 DF= 230.000 *+
 Y= 923.135 0.188273X

CE 4.00000 1195.25
 4.00000 2998.00
 R= -0.142647 T= -0.203818 DF= 2.00000
 Y= 3145.17 -0.123126X

EC 3.00000 3114.67
 3.00000 938.000
 R= 0.00272531 T= 0.00272532 DF= 1.00000
 Y= 928.772 0.00296265X

GROUPS 1 2
 CC 183.000 1209.97
 183.000 1699.14
 R= -0.058458 T= -0.78782 DF= 181.000
 Y= 1799.01 -0.082541X

CE 49.0000 1112.37
 49.0000 2037.78
 R= 0.167753 T= 1.16659 DF= 47.0000
 Y= 1730.39 0.276337X

EC 1.00000 3765.00
 1.00000 1300.00
 EE 2.00000 1493.50
 2.00000 1586.00

GROUPS 2 1
 CC 174.000 1694.03
 174.000 1095.49
 R= 0.093013 T= 1.22517 DF= 172.000
 Y= 997.476 0.057861X

CE 1.00000 931.000
 1.00000 750.000
 EC 61.0000 2175.98
 61.0000 1044.46
 R= 0.0143335 T= 0.110109 DF= 59.0000
 Y= 1028.47 0.0073461X

EE 1.00000 3972.00
 1.00000 3354.00
 GROUPS 2 2
 CC 139.000 1621.75
 139.000 1607.62
 R= 0.117291 T= 1.38239 DF= 137.000
 Y= 1415.45 0.118492X

CE 46.0000 1591.24
 46.0000 2324.54
 R= -0.0071536 T= -0.0474530 DF= 44.0000
 Y= 2337.74 -0.0082924X

EC 36.0000 2051.19
 36.0000 1486.42
 R= -0.348663 T= -2.16916 DF= 34.0000 *
 Y= 2054.03 -0.276723X

EE 9.00000 2388.22
 9.00000 2132.67
 R= -0.69659 T= -2.56878 DF= 7.00000 *
 Y= 3883.42 -0.73308 X

TRANSIT CORREL

Subject 1

Session 1+

All runs

BN ??? 442 NO OF BNS ??? 12

BN ??? 0 NO OF BNS ??? 0

GROUP 1 ACEG

GROUP 2 BDFH

GROUP 3

GROUP 4

GROUPS 1 1

CC 235.000 1187.92

235.000 1181.14

R= 0.079556 T= 1.21822 DF= 235.000

Y= 1088.27 0.078179X

CE 2.00000 1217.00

2.00000 3625.00

EC 2.00000 3625.00

2.00000 649.000

GROUPS 1 2

CC 195.000 1049.41

195.000 1659.45

R= 0.141840 T= 1.99063 DF= 193.000

Y= 1401.21 0.246079X

CE 50.0000 1167.68

50.0000 1889.12

R= 0.115899 T= 0.80842 DF= 48.0000

Y= 1674.41 0.183882X

EC 2.00000 503.000

2.00000 1334.00

GROUPS 2 1

CC 196.000 1671.91

196.000 1095.70

R= 0.146780 T= 2.06679 DF= 194.000

Y= 931.773 0.098045X

CE 2.00000 696.500

2.00000 503.000

EC 51.0000 1853.84

51.0000 1042.59

R= 0.185083 T= 1.31836 DF= 49.0000

Y= 896.064 0.079038X

GROUPS 2 2

CC 142.000 1552.49

142.000 1551.79

R= 0.0378247 T= 0.447868 DF= 140.000

Y= 1492.27 0.0383402X

CE 40.0000 1432.20

40.0000 2056.33

R= -0.171390 T= -1.07239 DF= 38.0000

Y= 2361.35 -0.212976X

EC 43.0000 1884.14

43.0000 1504.70

R= -0.157436 T= -1.02081 DF= 41.0000

Y= 1723.60 -0.116181X

EE 12.0000 2123.67

12.0000 1504.00

R= 0.444299 T= 1.56829 DF= 10.0000

Y= 710.020 0.373872X

TRANSIT CORREL

472 NO OF BNS ??? 12
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 2

Session 4

All runs

GROUPS 1 1
CC 211.000 971.502 980.640
R= 0.0237462 T= 0.343392 DF= 209.000
Y= 958.526 0.0227630X
CE 8.00000 904.375 1572.38
R= 0.118621 T= 0.292627 DF= 6.00000
Y= 1392.75 0.198619X
EC 15.0000 1284.60 916.133
R= 0.141740 T= 0.51626 DF= 13.0000
Y= 785.528 0.101670X
EE 1.00000 1063.00 1553.00
GROUPS 1 2
CC 186.000 994.758 1135.11
R= 0.100019 T= 1.36356 DF= 184.000
Y= 1027.96 0.107715X
CE 60.0000 904.000 1506.45
R= -0.108052 T= -0.82774 DF= 58.0000
Y= 1709.71 -0.224850X
EC 6.00000 1721.50 1074.67
R= -0.128132 T= -0.258394 DF= 4.00000
Y= 1248.47 -0.100958X
EE 2.00000 2003.00 1296.00
GROUPS 2 1
CC 162.000 1161.30 945.883
R= 0.132511 T= 1.69106 DF= 160.000
Y= 813.644 0.113872X
CE 10.0000 1088.60 1460.00
R= 0.481288 T= 1.55299 DF= 8.00000
Y= 639.592 0.75364 X
EC 74.0000 1405.99 1002.20
R= -0.0447372 T= -0.379988 DF= 72.0000
Y= 1041.01 -0.0275998X
EE 5.00000 1106.80 1187.00
R= -0.094280 T= -0.164028 DF= 3.00000
Y= 1255.94 -0.062288X
GROUPS 2 2
CC 130.000 1131.68 1181.55
R= 0.117268 T= 1.33595 DF= 128.000
Y= 1039.11 0.125872X
CE 61.0000 1207.89 1343.33
R= -0.056150 T= -0.431977 DF= 59.0000
Y= 1433.26 -0.074454X
EC 45.0000 1518.49 1127.62
R= -0.0301901 T= -0.198060 DF= 43.0000
Y= 1157.39 -0.0196006X
EE 15.0000 1203.07 1274.53
R= -0.178572 T= -0.65437 DF= 13.0000
Y= 1576.98 -0.251400X

522 NO OF BNS ??? 12
 BN ??? 0 NO OF BNS ??? 0
 GROUP 1 ACEG
 GROUP 2 BDFH
 GROUP 3
 GROUP 4
 GROUPS 1 1

Subject 3
 Session 4
 All runs

CC 112.000 1057.98 1003.38
 R= 0.170931 T= 1.81951 DF= 110.000
 Y= 849.051 0.145875X
 CE 58.0000 1007.29 1039.53
 R= 0.268076 T= 2.08232 DF= 56.0000 *
 Y= 812.979 0.224915X
 EC 48.0000 1024.63 1043.58
 R= 0.55716 T= 4.55055 DF= 46.0000 ***
 Y= 388.266 0.63957 X
 EE 32.0000 919.844 901.469
 R= 0.162872 T= 0.90416 DF= 30.0000
 Y= 776.347 0.136025X

GROUPS 1 2
 CC 88.0000 993.034 963.261
 R= 0.118002 T= 1.10200 DF= 86.0000
 Y= 870.304 0.093610X
 CE 64.0000 1058.72 1035.56
 R= 0.466357 T= 4.15116 DF= 62.0000 ***
 Y= 419.591 0.58181 X
 EC 40.0000 1071.33 1045.77
 R= 0.445648 T= 3.06874 DF= 38.0000 **
 Y= 747.148 0.278745X
 EE 48.0000 960.604 981.896
 R= 0.145883 T= 1.00013 DF= 46.0000
 Y= 799.954 0.189404X

GROUPS 2 1
 CC 95.0000 1070.07 998.747
 R= 0.253674 T= 2.52907 DF= 93.0000 *+
 Y= 814.182 0.172479X
 CE 35.0000 986.286 1007.17
 R= 0.191339 T= 1.11985 DF= 33.0000
 Y= 751.659 0.259066X
 EC 69.0000 1050.09 1114.99
 P= 0.148770 T= 1.23144 DF= 67.0000
 Y= 982.920 0.125767X
 EE 43.0000 993.047 962.674
 R= 0.62001 T= 5.05991 DF= 41.0000 ***
 Y= 205.219 0.76276 X

GROUPS 2 2
 CC 65.0000 957.354 985.692
 R= 0.088722 T= 0.70699 DF= 63.0000
 Y= 880.441 0.109940X
 CE 75.0000 1014.32 991.080
 R= 0.55344 T= 5.67739 DF= 73.0000 ***
 Y= 445.204 0.53817 X
 EC 76.0000 1002.08 1077.22
 R= 0.399596 T= 3.74984 DF= 74.0000 ***
 Y= 622.680 0.453601X
 EE 49.0000 1022.76 1064.96
 R= 0.231399 T= 1.63065 DF= 47.0000
 Y= 824.752 0.234862X

TRANSIT CORREL

TRANSIT CORREL

BN 777 602 NO OF BNS 777 10
BN 777 0 NO OF BNS 777 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 4

Session 4

All runs

GROUPS 1 1

CC	97.0000	1144.48	1076.33	
R=	0.118799	T=	1.16616	DF= 95.0000
Y=	958.626		0.102845X	
CE	32.0000	1209.53	1035.78	
R=	0.320502	T=	1.85323	DF= 30.0000
Y=	628.481		0.336742X	
EC	31.0000	892.548	981.323	
R=	0.111090	T=	0.60197	DF= 29.0000
Y=	894.993		0.096723X	
EE	19.0000	986.737	893.579	
R=	-0.170373	T=	-0.71289	DF= 17.0000
Y=	1011.96		-0.119975X	

GROUPS 1 2

CC	91.0000	998.802	1273.18	
R=	0.316492	T=	3.14758	DF= 89.0000 **
Y=	780.266		0.493501X	
CE	58.0000	1057.57	1406.69	
R=	-0.055476	T=	-0.415786	DF= 56.0000
Y=	1505.10		-0.093058X	
EC	27.0000	1135.04	1275.70	
R=	0.0347627	T=	0.173919	DF= 25.0000
Y=	1232.15		0.0383716X	
EE	25.0000	1019.36	1062.76	
R=	0.190320	T=	0.92973	DF= 23.0000
Y=	914.548		0.145397X	

GROUPS 2 1

CC	90.0000	1278.98	1092.23	
R=	0.388058	T=	3.94983	DF= 88.0000 ***
Y=	690.051		0.314456X	
CE	26.0000	1334.88	968.654	
R=	0.408229	T=	2.19076	DF= 24.0000 *
Y=	632.036		0.252170X	
EC	56.0000	1227.95	1126.23	
R=	0.247846	T=	1.87994	DF= 54.0000
Y=	859.294		0.217386X	
EE	27.0000	1268.81	1075.48	
R=	0.162504	T=	0.82346	DF= 25.0000
Y=	897.885		0.139971X	

GROUPS 2 2

CC	55.0000	1168.53	1323.75	
R=	0.365698	T=	2.86045	DF= 53.0000 **
Y=	860.319		0.396590X	
CE	49.0000	1331.51	1170.84	
R=	0.197694	T=	1.38261	DF= 47.0000
Y=	950.144		0.165747X	
EC	48.0000	1260.04	1180.92	
R=	0.214079	T=	1.48641	DF= 46.0000
Y=	968.212		0.168808X	
EE	43.0000	1324.77	1306.07	
R=	0.257246	T=	1.70455	DF= 41.0000
Y=	950.660		0.268281X	

TRANSIT CORREL

BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4
GROUPS 1 1

Subject 5
Session 4
All runs

CC 201.000 1118.83 1119.38
R= 0.0485530 T= 0.68573 DF= 199.000
Y= 1066.92 0.0468944X

CE 27.0000 1305.15 1243.63
R= -0.096394 T= -0.484225 DF= 25.0000
Y= 1374.54 -0.100304X

EC 21.0000 1106.33 1251.95
R= 0.0281614 T= 0.122802 DF= 19.0000
Y= 1221.04 0.0279409X

EE 7.00000 1139.14 999.572
R= -0.285893 T= -0.66712 DF= 5.00000
Y= 1160.80 -0.141533X

GROUPS 1 2
CC 150.000 1168.20 1237.34
R= 0.052302 T= 0.63715 DF= 148.000
Y= 1169.26 0.058281X

CE 75.0000 1170.80 1344.63
R= -0.208448 T= -1.82098 DF= 73.0000
Y= 1611.20 -0.227687X

EC 22.0000 1375.32 1197.14
R= -0.176091 T= -0.80000 DF= 20.0000
Y= 1400.12 -0.147588X

EE 14.0000 1323.50 1273.00
R= 0.053968 T= 0.187223 DF= 12.0000
Y= 1215.10 0.0437503X

GROUPS 2 1
CC 155.000 1294.25 1149.41
R= -0.0265719 T= -0.328792 DF= 153.000
Y= 1178.86 -0.0227488X

CE 20.0000 1265.45 1360.25
R= 0.052392 T= 0.222584 DF= 18.0000
Y= 1271.86 0.069846X

EC 75.0000 1344.15 1242.29
R= -0.0279738 T= -0.239102 DF= 73.0000
Y= 1286.45 -0.0328524X

EE 10.0000 1179.20 1221.30
R= -0.273228 T= -0.80337 DF= 8.00000
Y= 1577.81 -0.302331X

GROUPS 2 2
CC 92.0000 1216.50 1238.50
R= 0.0109021 T= 0.103433 DF= 90.0000
Y= 1225.21 0.0109260X

CE 51.0000 1256.02 1364.55
R= -0.155216 T= -1.09984 DF= 49.0000
Y= 1558.28 -0.154240X

EC 55.0000 1368.36 1398.62
R= 0.0375587 T= 0.273625 DF= 53.0000
Y= 1344.36 0.0396548X

EE 20.0000 1396.05 1427.75
R= -0.298514 T= -1.32699 DF= 18.0000
Y= 1743.30 -0.225034X

TRANSIT CORREL

BN 777 566 NO OF BNS 777 12
BN 777 0 NO OF BNS 777 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 6
Session 4
All runs

GROUPS 1 1
CC 213.000 1000.16 1016.07
R= 0.193316 T= 2.86206 DF= 211.000 **
Y= 824.200 0.191839X
CE 11.0000 1165.18 929.727
R= 0.433953 T= 1.44501 DF= 9.00000
Y= 210.896 0.61693 X
EC 7.00000 1051.71 943.000
R= -0.76571 T= -2.66201 DF= 5.00000 *
Y= 1332.16 -0.370028X

GROUPS 1 2
CC 178.000 978.191 1066.62
R= 0.077505 T= 1.03132 DF= 176.000
Y= 972.238 0.096490X
CE 61.0000 1029.11 1086.05
R= -0.232765 T= -1.83840 DF= 59.0000
Y= 1374.77 -0.280556X
EC 7.00000 1045.29 992.143
R= 0.103652 T= 0.233027 DF= 5.00000
Y= 906.941 0.081511X
EE 3.00000 673.333 1101.67
R= -0.212568 T= -0.217539 DF= 1.00000
Y= 1248.59 -0.218206X

GROUPS 2 1
CC 179.000 1112.29 982.715
R= 0.081475 T= 1.08757 DF= 177.000
Y= 907.495 0.067627X
CE 5.00000 1102.40 1194.00
R= 0.263499 T= 0.473114 DF= 3.00000
Y= 921.381 0.247296X
EC 64.0000 1151.47 996.234
R= -0.0373861 T= -0.294584 DF= 62.0000
Y= 1031.34 -0.0304888X
EE 1.00000 697.000 502.000

GROUPS 2 2
CC 138.000 1077.60 1133.92
R= -0.082526 T= -0.96571 DF= 136.000
Y= 1226.26 -0.085688X
CE 57.0000 1097.21 1168.32
R= 0.066747 T= 0.496115 DF= 55.0000
Y= 1078.61 0.081755X
EC 55.0000 1175.40 1108.04
R= -0.126487 T= -0.92829 DF= 53.0000
Y= 1233.98 -0.107152X
EE 16.0000 979.625 1190.00
R= -0.237796 T= -0.91603 DF= 14.0000
Y= 1372.32 -0.186114X

TRANSIT CORREL

BN ??? 442 NO OF BNS ??? 2
 BN ??? 0 NO OF BNS ??? 0
 GROUP 1 ACEG
 GROUP 2 BDFH
 GROUP 3
 GROUP 4
 GROUPS 1 1

Subject 1

Session 4

Run 1

CC 55.0000 1241.24
 55.0000 1241.47
 R= 0.157049 T= 1.15770 DF= 55.0000
 Y= 1052.93 0.151899X

CE 1.00000 1768.00
 1.00000 3311.00
 EC 1.00000 3311.00
 1.00000 536.000

GROUPS 1 2
 CC 42.0000 1165.74
 42.0000 1553.88
 R= 0.155977 T= 0.99871 DF= 40.0000
 Y= 1258.21 0.253631X

CE 7.00000 1171.86
 7.00000 2426.86
 R= -0.227277 T= -0.52186 DF= 5.00000
 Y= 2824.50 -0.339329X

GROUPS 2 1
 CC 40.0000 1661.80
 40.0000 1173.73
 R= 0.071857 T= 0.444106 DF= 38.0000
 Y= 1093.50 0.0482769X

EC 9.00000 1922.22
 9.00000 1249.56
 R= 0.050953 T= 0.134985 DF= 7.00000
 Y= 1195.79 0.0279704X

GROUPS 2 2
 CC 24.0000 1277.96
 24.0000 1315.04
 R= 0.57430 T= 3.29046 DF= 22.0000 *
 Y= 498.802 0.63871 X

CE 8.00000 1013.38
 8.00000 2032.88
 R= -0.155225 T= -0.384888 DF= 6.00000
 Y= 2551.84 -0.51212 X

EC 6.00000 2389.50
 6.00000 1606.67
 R= 0.163775 T= 0.332034 DF= 4.00000
 Y= 1133.66 0.197954X

EE 1.00000 3061.00
 1.00000 2440.00 TRANSIT CORREL

BN ??? 444	NO OF BNS ??? 2			
BN ??? 0	NO OF BNS ??? 0			
GROUP 1	ACEG		Subject 1	
GROUP 2	BDFH		Session 4	
GROUP 3			Run 2	
GROUP 4				
GROUPS 1 1				
CC	42.0000	1138.88		
	42.0000	1365.31		
R=	-0.062639	T=	-0.396944	DF= 40.0000
Y=	1462.03		-0.084924X	
GROUPS 1 2				
CC	40.0000	1053.70		
	40.0000	1496.95		
R=	0.112958	T=	0.70080	DF= 38.0000
Y=	1336.74		0.152048X	
CE	9.00000	1201.00		
	9.00000	1646.44		
R=	0.58150	T=	1.89110	DF= 7.00000
Y=	969.711		0.56347 X	
EC	1.00000	503.000		
	1.00000	1828.00		
GROUPS 2 1				
CC	38.0000	1836.63		
	38.0000	913.079		
R=	0.231816	T=	1.42985	DF= 36.0000
Y=	752.310		0.087535X	
CE	1.00000	658.000	503.000	
EC	12.0000	2024.92	795.583	
R=	0.52175	T=	1.93404	DF= 10.0000
Y=	477.036		0.157314X	
GROUPS 2 2				
CC	26.0000	1514.42	1806.50	
R=	-0.0244429	T=	-0.119781	DF= 24.0000
Y=	1848.03		-0.0274202X	
CE	13.0000	1503.54	2081.69	
R=	-0.141548	T=	-0.474236	DF= 11.0000
Y=	2361.62		-0.186178X	
EC	12.0000	1539.42	1939.83	
R=	-0.115724	T=	-0.368428	DF= 10.0000
Y=	2107.66		-0.109022X	
EE	3.00000	2339.67	2174.67	
R=	0.99417	T=	9.22404	DF= 1.00000
Y=	-864.739		1.29908 X	
TRANSIT CORREL				
BN ??? 472				

GROUP 1 ALGG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 3
Session 4
Run 1

GROUPS 1 1
CC 24.0000 1064.54 1036.46
R= 0.240083 T= 1.16002 DF= 22.0000
Y= 805.860 0.216618X
CE 9.00000 934.333 1107.11
R= -0.476153 T= -1.43261 DF= 7.00000
Y= 2614.10 -1.61291 X
EC 6.00000 1216.50 888.500
R= -0.51390 T= -1.19811 DF= 4.00000
Y= 1178.60 -0.238470X
EE 4.00000 746.250 1175.00
R= -0.0218657 T= -0.0309302 DF= 2.00000
Y= 1187.05 -0.0161485X

GROUPS 1 2
CC 17.0000 1078.47 1058.00
R= 0.225463 T= 0.89629 DF= 15.0000
Y= 802.648 0.236773X
CE 17.0000 1104.53 1029.41
R= 0.54090 T= 2.49070 DF= 15.0000
Y= 406.795 0.56369 X
EC 9.00000 972.222 1109.33
R= 0.463526 T= 1.38404 DF= 7.00000
Y= 609.343 0.51428 X
EE 10.0000 1178.80 1150.50
R= 0.306017 T= 0.90916 DF= 8.00000
Y= 944.869 0.174442X

GROUPS 2 1
CC 21.0000 1227.38 1037.33
R= -0.078288 T= -0.342299 DF= 19.0000
Y= 1123.82 -0.070464X
CE 9.00000 1109.67 982.111
R= -0.129198 T= -0.344714 DF= 7.00000
Y= 1082.38 -0.090358X
EC 16.0000 1053.75 1174.56
R= 0.233043 T= 0.89665 DF= 14.0000
Y= 876.174 0.283168X
EE 7.00000 1104.00 1045.57
R= 0.171972 T= 0.390357 DF= 5.00000
Y= 737.190 0.279331X

GROUPS 2 2
CC 13.0000 959.539 1150.46
R= -0.0226377 T= -0.075100 DF= 11.0000
Y= 1180.78 -0.0315994X
CE 11.0000 1083.55 1046.45
R= 0.57341 T= 2.09974 DF= 9.00000
Y= 494.116 0.50975 X
EC 15.0000 1140.27 1148.60
R= 0.142755 T= 0.52004 DF= 13.0000
Y= 992.515 0.136884X
EE 12.0000 1045.25 1143.25
R= -0.0104699 T= -0.0331104 DF= 10.0000
Y= 1158.61 -0.0146959X

TRANSIT CORREL

GROUP 4

Subject 3

Session 4

Run 2

GROUPS 1 1

CC	25.0000	1007.00	1014.20	
R=	0.282650	T=	1.41338	DF= 23.0000
Y=	802.402		0.210326X	

CE	13.0000	1027.69	1082.54	
R=	0.147069	T=	0.493134	DF= 11.0000
Y=	920.308		0.157860X	

EC	11.0000	990.818	922.545	
R=	0.74181	T=	3.31847	DF= 9.00000 * *
Y=	181.722		0.74769 X	

EE	9.00000	1067.33	948.444	
R=	0.208388	T=	0.56372	DF= 7.00000
Y=	797.036		0.141857X	

GROUPS 1 2

CC	18.0000	1015.28	1023.83	
R=	-0.320051	T=	-1.35128	DF= 16.0000
Y=	1278.03		-0.250375X	

CE	10.0000	1205.30	1012.10	
R=	0.51582	T=	1.70299	DF= 8.00000
Y=	780.841		0.191868X	

EC	7.00000	1172.29	1096.71	
R=	0.219139	T=	0.50222	DF= 5.00000
Y=	968.769		0.109142X	

EE	11.0000	966.818	949.636	
R=	0.57578	T=	2.11267	DF= 9.00000
Y=	296.126		0.67594 X	

GROUPS 2 1

CC	18.0000	999.444	1007.61	
R=	0.212276	T=	0.86891	DF= 16.0000
Y=	846.448		0.161253X	

CE	8.00000	1081.63	1009.50	
R=	0.290800	T=	0.74449	DF= 6.00000
Y=	798.409		0.195161X	

EC	13.0000	1145.23	1251.54	
R=	-0.189138	T=	-0.63883	DF= 11.0000
Y=	1601.84		-0.305875X	

EE	8.00000	935.375	1082.63	
R=	0.80016	T=	3.26786	DF= 6.00000 *
Y=	-170.683		1.33990 X	

GROUPS 2 2

CC	11.0000	1017.18	1063.45	
R=	-0.109687	T=	-0.331058	DF= 9.00000
Y=	1241.29		-0.174830X	

CE	16.0000	1162.88	1214.19	
R=	0.158692	T=	0.60139	DF= 14.0000
Y=	1061.45		0.131346X	

EC	17.0000	1105.41	1096.12	
R=	0.57619	T=	2.73036	DF= 15.0000 *
Y=	425.659		0.60652 X	

EE	4.00000	1027.50	942.500	
R=	-0.69495	T=	-1.36679	DF= 2.00000
Y=	1152.59		-0.204471X	

TRANSIT CORREL

TRANSIT CORREL

BN ??? 526 NO OF BNS ??? 2
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 3
Session 4
Run 3

GROUPS 1 1

CC 27.0000 1157.44 1039.11
R= -0.080055 T= -0.401563 DF= 25.0000
Y= 1118.95 -0.068980X

CE 11.0000 846.182 1054.09
R= -0.173530 T= -0.52861 DF= 9.00000
Y= 1239.40 -0.218991X

EC 9.00000 1079.89 929.444
R= 0.346854 T= 0.97843 DF= 7.00000
Y= 525.477 0.374082X

EE 5.00000 1126.20 911.600
R= -0.148695 T= -0.260442 DF= 3.00000
Y= 1080.89 -0.150322X

GROUPS 1 2

CC 19.0000 1022.68 976.421
R= -0.083922 T= -0.347246 DF= 17.0000
Y= 1060.72 -0.082424X

CE 10.0000 893.900 907.200
R= 0.80859 T= 3.88700 DF= 8.00000 *
Y= 372.160 0.59855 X

EC 9.00000 1001.44 1084.67
R= 0.51187 T= 1.57645 DF= 7.00000
Y= 747.721 0.336460X

EE 9.00000 923.000 932.778
R= 0.57492 T= 1.85904 DF= 7.00000
Y= -231.494 1.26140 X

GROUPS 2 1

CC 12.0000 1039.83 1045.67
R= 0.453043 T= 1.60703 DF= 10.0000
Y= 603.791 0.424949X

CE 8.00000 921.125 1066.13
R= 0.0416131 T= 0.102019 DF= 6.00000
Y= 1022.07 0.0478288X

EC 19.0000 837.158 1050.53
R= 0.420861 T= 1.91292 DF= 17.0000
Y= 478.675 0.68309 X

EE 9.00000 1169.22 1049.44
R= 0.68159 T= 2.46441 DF= 7.00000
Y= 347.372 0.60046 X

GROUPS 2 2

CC 11.0000 872.545 912.364
R= 0.312369 T= 0.98647 DF= 9.00000
Y= 627.462 0.326517X

CE 19.0000 958.737 937.000
R= 0.52391 T= 2.53607 DF= 17.0000
Y= 347.316 0.61506 X

EC 10.0000 890.200 834.900
R= 0.281757 T= 0.83058 DF= 8.00000
Y= 648.045 0.209902X

EE 12.0000 1000.08 1005.17
R= 0.220576 T= 0.71514 DF= 10.0000
Y= 777.932 0.227216X

TRANSIT CORREL

Y= 777.952
TRANSIT CORREL

Subject 3
Session 4
Run 4

BN 777 530 NO OF BNS 777 2
BN 777 0 NO OF BNS 777 0
GROUP 1 ACEG
GROUP 2 BDFHH
GROUP 3
GROUP 4

GROUPS 1 1
C 20.0000 974.200 931.550
R= 0.245038 T= 1.07230 DF= 18.0000
Y= 682.583 0.255561X
E 16.0000 1065.13 1002.94
R= 0.57105 T= 2.60279 DF= 14.0000 *
Y= 620.111 0.359419X
C 11.0000 1077.82 1211.73
R= 0.82532 T= 4.38482 DF= 9.00000 **
Y= 346.991 0.80230 X
EE 6.00000 829.333 802.333
R= 0.78791 T= 2.55899 DF= 4.00000
Y= 242.524 0.67501 X

GROUPS 1 2
CC 15.0000 929.400 875.867
R= 0.238935 T= 0.88719 DF= 13.0000
Y= 747.782 0.137815X
CE 12.0000 1212.33 1404.17
R= 0.452915 T= 1.60646 DF= 10.0000
Y= 611.410 0.65391 X
EC 6.00000 1528.67 1150.33
R= 0.77450 T= 2.44876 DF= 4.00000
Y= 754.138 0.259177X
EE 13.0000 811.846 1015.69
R= -0.356498 T= -1.26552 DF= 11.0000
Y= 1515.38 -0.61550 X

GROUPS 2 1
CC 19.0000 1203.32 1041.00
R= 0.297078 T= 1.28280 DF= 17.0000
Y= 834.654 0.171481X
CE 4.00000 833.500 1263.25
R= 0.422983 T= 0.66015 DF= 2.00000
Y= -217.055 1.77601 X
EC 13.0000 1339.31 1021.23
R= 0.373712 T= 1.33628 DF= 11.0000
Y= 819.860 0.150354X
EE 9.00000 931.333 821.556
R= 0.90523 T= 5.63649 DF= 7.00000 *
Y= -90.0686 0.97884 X

GROUPS 2 2
CC 13.0000 951.846 918.154
R= 0.183604 T= 0.61948 DF= 11.0000
Y= 720.550 0.207601X
CE 15.0000 1012.93 919.867
R= 0.78667 T= 4.59425 DF= 13.0000
Y= 217.549 0.69335 X
EC 18.0000 993.611 1238.39
R= 0.55812 T= 2.69049 DF= 16.0000
Y= 530.280 0.71266 X
EE 10.0000 1219.70 1202.30
R= 0.289612 T= 0.85582 DF= 8.00000
Y= 860.683 0.280082X

TRANSIT CORREL

BN ??? 532 NO OF BNS ??? 2
BN ??? 0 NO OF BNS ??? 0
GROUP 1 ACEG
GROUP 2 BDFH
GROUP 3
GROUP 4

Subject 3
Session 4
Run 5

GROUPS 1 1

CC	16.0000	1064.69	966.375	
R=	0.106536	T=	0.400901	DF= 14.0000
Y=	881.328		0.079880X	
CE	9.00000	1144.89	957.111	
R=	0.63093	T=	2.15157	DF= 7.00000
Y=	465.045		0.429794X	
EC	11.0000	855.364	1174.45	
R=	0.53548	T=	1.90212	DF= 9.00000
Y=	284.111		1.04039 X	
EE	8.00000	779.625	779.875	
R=	0.000123252	T=	0.000301905	DF= 6.0000
Y=	779.749		0.000161927X	

GROUPS 1 2

CC	19.0000	916.105	876.947	
R=	0.398806	T=	1.79308	DF= 17.0000
Y=	691.118		0.202847X	
CE	15.0000	896.067	848.867	
R=	-0.074231	T=	-0.268384	DF= 13.0000
Y=	934.304		-0.095347X	
EC	9.00000	856.889	834.000	
R=	0.063112	T=	0.167312	DF= 7.00000
Y=	769.110		0.075728X	
EE	5.00000	965.000	716.200	
R=	0.391136	T=	0.73611	DF= 3.00000
Y=	371.339		0.357369X	

GROUPS 2 1

CC	25.0000	902.040	905.320	
R=	0.251582	T=	1.24664	DF= 23.0000
Y=	735.793		0.187938X	
CE	6.00000	862.833	792.333	
R=	0.88274	T=	3.75747	DF= 4.00000 *
Y=	76.9021		0.82917 X	
EC	8.00000	923.875	1079.38	
R=	-0.233898	T=	-0.58928	DF= 6.00000
Y=	1299.30		-0.238051X	
EE	10.0000	858.500	857.600	
R=	0.211769	T=	0.61287	DF= 8.00000
Y=	598.590		0.301700X	

GROUPS 2 2

CC	17.0000	976.059	908.471	
R=	0.201774	T=	0.79788	DF= 15.0000
Y=	727.388		0.185524X	
CE	14.0000	867.072	842.286	
R=	0.413521	T=	1.57330	DF= 12.0000
Y=	489.796		0.406529X	
EC	16.0000	842.188	960.375	
R=	0.0073708	T=	0.0275798	DF= 14.0000
Y=	953.850		0.0077475X	
EE	11.0000	842.182	964.455	
R=	0.190423	T=	0.58192	DF= 9.00000
Y=	784.394		0.213803X	

TRANSIT CORREL

BN ???