Aspects of preparation in choice reaction time

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ABSTRACT

Aspects of Preparation in Choice Reaction Time

A.P. White

A comprehensive review of the principal empirical findings in the study of choice reaction time is provided, with particular emphasis on effects due to types of preparation bias by the subject and their relation to errors. This is followed by a brief review of the chief types of model which have been proposed for choice reaction processes. The fast-guess model is focused on in detail, including a full explanation of the process of parameter estimation. Some experiments are reported which utilise the fast-guess model in an attempt to establish the locus of certain well-known findings. Finally, a scheme for a comprehensive stage model of choice reaction processes is suggested, which is developed with the aid of Sternberg's additive factor method. The scheme incorporates bypass features not normally included in such models, in an attempt to account for certain preparation effects described earlier.

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ASPECTS OF PREPARATION IN CHOICE REACTION TIME

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This thesis is based on work conducted in the Department of Psychology at Durham University. It is submitted for the degree of Doctor of Philosophy from the University of Durham, 1981.

DECLARATION

I hereby declare that none of the material contained in this thesis has ever been submitted for a degree at any university.

A.P. White December 1981

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NOTES TO THE READER

Section Cross Referencing

All sections are cross-referenced by the section number without its chapter prefix, accompanied by an indication of the chapter concerned if it is in a chapter other than that in which the reference is made. Thus, if a reference to Section 5.4 is made anywhere in Chapter 3, then the full section number is 3.5.4. However, if the reference had been to Section 5.4 in Chapter 6, then the full section number would have been 6.5.4.

Significance Levels in Tables

Three significance levels have been employed for statistical tests throughout this thesis. In tables, they are indicated by asterisks, as follows:

* $p < 0.05$

** $p < 0.01$

*** $p < 0.001$

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CHAPTER 1: TASK-DETERMINED VARIABLES IN CHOICE REACTION TIME

1.1 INTRODUCTION

The phenomenon of preparation in reaction time studies has two aspects. One is best termed the temporal aspect of preparation and the other the selective aspect. The former concerns the tendency for responses to be faster if the stimulus arrives when the subject is "ready" for it. The latter aspect is concerned with the tendency for responses to be faster when a particular member of the stimulus set occurs that the subject is expecting. Now, the nature of preparation itself is somewhat elusive. However, Naatanen & Merisalo (1977) have suggested that the nature of preparation lies in "....performing in advance what can be performed in advance of a response." This rather broad definition gives great scope for further refinement but seems to capture the essence of the concept.

This thesis is concerned with examining both aspects of preparation and attempting to explain them in the context of a general information processing scheme. In order to do this, an extensive review of choice reaction time research is required that examines various well-known empirical effects in the field and attempts (a) to relate them to each other; (b) to break them down into their constituent parts; and (c) to relate them to the appropriate aspect of preparation.

The beginnings of the scientific study of CRT (choice reaction time) are usually thought of as being in the latter half of the last century, with the work of Donders (1868) and Merkel (1885). However, most of the work in this area that is relevant to present day research has been carried out within the last thirty years. Well over five hundred papers on CRT have appeared in the scientific literature during that period, which makes a really comprehensive review a rather daunting task. It is therefore helpful to subdivide the topic and consider it in a piecemeal fashion (at least initially). One of the best ways of doing this is to classify research according to the various independent variables used.

There are many variables which affect RT (reaction time) but they can be broadly split into two classes - task-determined variables and subject-determined ones. The former are dependent on the apparatus and the way it has been programmed to operate. These include number of choices, stimulus and response probabilities, S-R (stimulus-response) mappings and sequential dependencies in the stimulus sequence. The second class of subject-determined variables can be further subdivided into two categories. The first category is concerned with those variables which have no intrinsic connection with the task (e.g. age). This category of variables is not dealt with in this thesis. The second category consists of variables that are connected with the subject's strategy for dealing with the task, e.g. error rates, stimulus prediction, etc.

This first chapter is concerned principally with empirical findings involving task-determined variables. However, topics such as the refractory period, sense-modality differences, intersensory facilitation, effects due to stimulus intensity and responses to multidimensional stimuli are not discussed in detail because of their peripheral relevance. For similar reasons, dual and multiple component tasks are not mentioned at all.

The next chapter deals with those variables concerned with the subject's strategy, including expectancy, prediction effects, errors and speed-accuracy tradeoff. Foreperiod effects are also covered there. (This is strictly a deviation from the classification just outlined because foreperiod variables are obviously task-determined. However, foreperiod effects are intimately connected with the temporal aspects of preparation which are also discusssed in the second chapter). The third chapter deals with theories and their adequacy in dealing with the effects described in the first two chapters.

1.2 EMPIRICAL FINDINGS

1.2.1 Number Of Choices

Many CRT experiments have been run with a 1:1 mapping between stimuli and responses, where N (the number of equiprobable stimuli) is changed either between groups of subjects or between blocks of trials. Probably the first such experiment was performed by Merkel (1885), who used tachistoscopically presented digits as stimuli. He used key-release rather than key-press as the form of the response. Merkel observed that the form of the relationship between RT (reaction time) and N was a smooth curve, with RT an increasing but negatively accelerated function of N. However, he did not propose any quantitative theory or mathematical relationship.

After a lapse of many years, Hick (1952) investigated the same effect using lights as stimuli and key-presses as responses. He found that RT appeared to be a logarithmic function of N and proposed the equation

$$RT = b.\log(N+1)$$

to represent the relationship. He obtained a good fit over the entire range used for N (from one to ten alternatives) and found a slope of about 200 ms per bit. This became known as Hick's Law. Similar results with lights and keys were obtained by others (Bernstein & Reese, 1965; Brainard, Irby, Fitts & Alluisi, 1962; Hyman, 1953; Kaufman & Levy, 1966; Lamb & Kaufman, 1965).

It is interesting to note that Hyman (and indeed most other workers in the field, except Hick) fitted the following equation:

$$RT = a + b.\log(N)$$

This formula seems intuitively more appropriate than that proposed by Hick, who justified his equation on the grounds that at any instant prior to the presentation of a stimulus, the subject is in a state of uncertainty concerning <u>precisely</u> <u>when</u> the stimulus will occur, as well as being uncertain <u>which</u> stimulus is going to occur. Although Hick obtained a reasonable straight-line fit with his equation, it seems most implausible on a priori grounds that the temporal uncertainty will be <u>always</u> (irrespective of foreperiod parameters) <u>exactly</u> equal to that obtained by increasing the number of equiprobable alternatives by one. (Also, this quantity itself decreases as N increases, whereas temporal uncertainty might justifiably be expected to be independent of the other sources of entropy in the stimulus sequence). The second alternative seems far more appropriate. Here, temporal uncertainty and all the various time lags associated with visual perception and motor response may be accommodated by the intercept in the equation, leaving a straightforward linear relation between RT and stimulus information.

Not surprisingly, Hick's Law was also found to hold when visually presented digits were used as stimuli (Brainard et al, 1962; Hale, 1968, 1969a) and also when lights were used as stimuli with vocal responses (Brainard et al, 1962; Hyman, 1953). Some studies which employed only two different values of N provide a degree of confirmatory evidence (Alegria & Bertelson, 1970; Broadbent & Gregory, 1965; Costa, Horwitz & Vaughan, 1966; Palef, 1973) although the logarithmic nature of the relationship between RT and N is obviously not put to the test in these cases.

However, there are disconfirming instances. Some tasks involving visually presented digits or letters as stimuli, paired with vocal responses, seem to show either no significant relationship between RT and N (Alluisi, 1965; Brainard et al, 1962; Mowbray, 1960) or else a rather small one (Burns & Moskowitz, 1972; Fitts & Switzer, 1962). However, Alluisi, Strain & Thurmond (1964) found that when the S-R (stimulus-response) compatibility of such a number-naming task was reduced by pairing the stimulus numerals to other number-names, the expected relationship between RT and N reappeared. Similarly, in an experiment requiring names as responses to five different classes of stimuli, Morin, Konick, Troxell & McPherson (1965) found the slope of the line relating RT to log(N) to be very much less for letters than for faces, drawings, colours and geometric symbols. These results suggest that for very compatible tasks (like conventional number/letter naming) RT is essentially independent of N. This idea is supported by Leonard (1959) who used vibrotactile stimuli and key-press responses. He found that RT increased when N was increased from one to two, but did not increase further as N was increased from two to four to eight alternatives. Such a task is normally regarded as being a highly compatible one. Similarly, Hellyer (1963) used three tasks - light-naming, reading names from slides and number-naming. He found that the slopes of the information functions for the reading and number-naming tasks were considerably less than for the light-naming one.

Mowbray & Rhoades (1959) conducted a long experiment (45000 RTs) with one subject (using lights as stimuli and keys as responses) under two conditions - either two or four alternative stimuli. They did find that after 42000 responses that there was no difference in the mean RT between the two conditions. However, it should be noted that the difference after 3000 responses was only 10 ms, which is very much less than the figure of around 100 ms usually found in similar studies (Teichner & Krebs, 1974). The magnitude of this discrepancy is such as to place other findings of this experiment in some doubt.

Typically, only small values are chosen for the number of alternatives in experiments of this sort. Few experiments have been run with more than ten alternatives. Of those that have been, some suggest that the linear relation between RT and the logarithm of N is maintained but others do not. Among the former, Pollack (1963) found a linear relationship with a word-naming task, up to about a thousand alternatives. On the other hand, Seibel (1963) used a task that required a chord key-press response to lights and found little or no increase in RT as N was increased from 32 to over a thousand.

To summarise, it seems reasonable to say that, with the exception of highly compatible or well-practised tasks, RT is always found to be a monotonically increasing function of N, at least for small values of N. This is one of the most consistent findings in CRT research. This conclusion suggests that the less compatible the task, the steeper the slope of the function relating RT to log(N). This idea was tested by Hawkins & Underhill (1971) who ran an experiment with two, four and eight choices and visually presented letters as stimuli. They used two types of response (key-press and naming) in order to vary S-R compatibility and found the expected interaction between N and S-R compatibility.

The selectivity of preparation is of possible relevance to this effect for the following reason. On any given trial, if the subject has pre-selected a given response in advance of the stimulus and is holding it ready for execution then, if the stimulus corresponding to the favoured response occurs, the appropriate response can be executed with a saving of time. Now, if the subject does this on every trial, it is clear that the probability of pre-selecting the correct response decreases as the number of possible responses increases. Furthermore, the less compatible the S-R relation is, the greater the time saving that is made by pre-selecting the required response and hence the steeper the gradient of the function relating RT to N. It is just possible that such a mechanism is the sole cause for these effects but such an explanation seems rather unlikely because it predicts that RT will be a negatively accelerated function of N, rather than a logarithmic one as seems to be the case. However, any such response pre-selection will certainly modify the functional relationship between RT and N and should therefore be taken into account.

Another aspect of this phenomenon is that it could be due to number of stimuli, or number of responses, or both. Of course, where the S-R mappings are 1:1, then it is impossible to distinguish between these possibilities. However, some investigators have used many:1 mappings of stimuli on to responses in an attempt to resolve the issue. Rabbitt (1959) used a card-sorting task in which the variables were number of sorting categories (responses) and number of symbols per response category. He found that RT increased with each of these variables and also that RT increased most when the number of symbols per category was increased from one to two. The variables interacted so that the effect of further increasing the number of symbols per response category was greater the larger the number of categories. Broadbent & Gregory (1962a) also used a card-sorting task to demonstrate that the effect on RT of increasing the number of symbols per category from one to two disappeared when the stimulus pairs used were such that they required the same response in everyday life, thereby suggesting the operation of a compatibility effect here, too.

Other studies have used 1:many mappings of stimuli on to responses to study the same question. These mappings give the subject a random choice of response that he can make to one or more of the stimuli. Using this technique with geometric shapes as stimuli and mens' names as responses, Morin and Forrin (1963) found that both number of stimuli and number of responses per stimulus influenced RT. They also found an interaction such that the effect of either variable was increased by increasing the other. Schlesinger (1964) carried out a similar study using key-press responses to light stimuli. He used a 2 x 2 design (one or two stimuli mapped on to one or two responses). He found that both number of stimuli and number of responses affected RT and also that these variables interacted in the same manner as those in the study just described. However, a later experiment of similar type by Bernstein, Schurman & Forester (1967) found that when the number of responses was increased <u>beyond</u> two (with the number of stimuli held constant) there was little effect on RT.

This suggests the existence of some mechanism which is influenced by whether response selection requires choice. Now the mechanism for response pre-selection which was described earlier can accommodate such a finding if an elaboration is allowed. This modification concerns the process by which the pre-selected response is validated. In the case of there being only one possible response which is required on every trial, such validation is clearly unnecessary. When a choice is required, however, it is necessary to check whether the pre-selected response is the appropriate one. It could well be the case that this check is stimulus-specific and that a saving of time is made only if the expected stimulus occurs. Thus, for two or more responses, the response pre-selection mechanism would be affected only by the number of stimuli.

Thus it seems justified to draw the tentative conclusion that the effect due to number of alternative choices has two components: one due to number of stimuli and the other due to number of responses. Also, it seems reasonable to bear in mind that response pre-selection might be responsible for all or part of the effect.

1.2.2 Stimulus Probability

Another consistent finding in this area is that in tasks with unequal stimulus frequencies (and 1:1 S-R mappings), response time to the various stimuli is inversely related to stimulus frequency. This was probably first shown by Hyman (1953) using vocal responses to stimulus lights. He used various conditions, differing in number of stimuli and their probabilities of occurrence. Fitts, Peterson & Wolpe (1963) obtained similar results using a vocal response to nine visually-presented numerical stimuli and also with manual responses paired to nine stimulus lights. In this case, the manual response involved moving a finger from a "home" key to one of nine response buttons. Other investigators have used similar systems for just two stimuli and have obtained similar findings (Kaufman & Levy, 1966; Lamb & Kaufman, 1965). The same effect has been observed with direct manual responses and two stimulus lights (Kanarick, 1966; Remington, 1969) and also with manual responses to just two visually presented numerals (Bertelson & Barzeele, 1965) and similarly with five numerical stimuli (Leonard, Newman & Carpenter, 1966). Leontjev & Krinchik (1964) found that, in an experiment which used a two-choice task with verbal responses to light stimuli, the effect of probability on RT appeared to be satisfactorily fitted by a straight line. The stimulus probabilities ranged from 0.07 to 0.93.

It is worth observing that, irrespective of the number of stimuli used, none of the foregoing studies used more than two different values of stimulus probability in the same experimental condition. However, a fuller test of this effect is provided by Falmagne (1965) who used manual responses to six stimuli, each having different probabilities of occurrence. He found a perfect inverse relationship between stimulus probability and RT.

A small number of studies have been carried out on the possible interaction between stimulus probability and S-R compatibility. The paper by Fitts et al (1963), which was described at the beginning of this section, found that the task with the vocal response showed a larger stimulus frequency effect than the task with the more compatible motor response. However, the experiment by Hawkins & Underhill (1971), which was mentioned in the previous section, also included three other conditions (at both levels of compatibility) in which the stimuli were not equiprobable. No significant interactions between compatibility and stimulus frequency were reported. Another experiment by Hawkins & Friedin (1972), using a condensation task, also failed to find an interaction. Yet an experiment by Blackman (1975), with a three choice number-naming task found the elusive interaction. He used a stimulus frequency ratio of 4.7:1 and manipulated compatibility by getting subjects in the low-compatibility condition to name a number one larger than the stimulus.

The existence of the stimulus probability effect prompts the question of whether it is this which is responsible for the effect described in the previous section. After all, if the number of equiprobable stimuli present in a set is increased, then all the stimulus probabilities are necessarily reduced. Broadbent & Gregory (1965) varied number of alternatives, stimulus probability and S-R compatibility in a task that required manual responses to vibrotactile stimuli. Two levels of compatibility were employed: either responding with the finger stimulated or with the corresponding finger of the other hand. Stimulus arrangements used either two or four alternatives and stimulus probabilities were such that one member of the set had a probability of 0.75 (with the remainder being equiprobable in the four-choice case). With both levels of compatibility, they found that responses to the high-probability signal were significantly faster when it was one of two possibilities than when it was one of four. The stimulus probability effect was apparent only in the conditions using four stimuli, where it was found to interact with compatibility, (i.e. the effect of unequal stimulus probabilities was greater in the incompatible condition).

Further work in this area showed similar results. Mowbray (1964) ran an experiment in which the subjects made vocal responses to visually presented numerals. Number of S-R pairs and probability of a key stimulus were both varied in a 2 x 2 design. Thus the task involved either four or ten choices and the probability of a key stimulus was either 0.1 or 0.25. The results showed the expected probability effect but no effect of number of choices. However, there was a significant interaction between the two independent variables, with the probability effect being larger for ten alternatives than for four. Another experiment on similar lines by Krinchik (1969) used four levels of choice and three levels of probability in a task that required key-press responses to tachistoscopically presented geometrical shapes. The results revealed significant effects for both the number of choices and their probabilities. The interaction was also significant and in the same direction as in Mowbray's study. Yet another experiment of this type was run by Kornblum (1975). He used two tasks - one with lights as stimuli and one with visually presented digits. Both tasks had key-press responses. Four levels of choice were used, with N ranging from two to five. The probability of the "critical" stimulus remained fixed at 0.5 and the RSI was 50 ms. Each task had two variants. Both used 1:1 S-R mappings but in one type, the response to the critical stimulus was made with a different hand from the other responses. In the other version, all the responses were made with the same hand. The results showed that RT to the critical stimulus increased as N increased, under all conditions. However, the rate of increase was less rapid than for the other responses.

It is also worthwhile speculating whether the pre-selection mechanism described in the previous section could account for this effect, too. If it is supposed that responses are pre-selected with a frequency proportional to their frequency of use, it follows that the most frequent responses will, on average, be made with the shortest RTs. Just as with the number of alternatives, it is also possible to account for the interaction with compatibility.

Thus it seems that there are two separate effects: that due to number of S-R pairs and that due to their various probabilities of occurrence. As before, it should be remembered that response pre-selection might be involved here, too.

1.2.3 Stimulus Versus Response Probabilities

Just as the effect due to alternatives was investigated to determine whether stimulus or response components were important, so condensation tasks (i.e. those involving many:1 S-R mappings) have been used to examine the stimulus and response components of the probability effect. LaBerge & Tweedy (1964) used a simple condensation task with key-press responses to colours. Red and blue were mapped to one response and green (which had a fixed probability of 0.4) was mapped to the other. They varied the relative frequencies of occurrence of red and blue and produced a concomitant change in RT, thereby demonstrating the existence of a stimulus probability component (response probability being held constant at 0.6).

Other studies were designed to investigate both possible effects. Bertelson & Tisseyre (1966) used key-press responses to visually presented numerals in a task with a similar mapping arrangement to that used in the experiment just described. The probabilities were arranged so that the two stimuli mapped to the same response had different probabilities and so that one of those stimuli had the same probability as the remaining stimulus. Thus the effect of stimulus probability could be assessed with response probability held constant and vice versa. They found a stimulus probability effect only. Similar results were obtained by Orenstein (1970), using a similar design (with lamps as stimuli and a manual response) and by Hawkins & Friedin (using vocal responses to visually presented letters or digits).

There seem to be only two studies using many:1 mappings that failed to find a stimulus probability effect where it might have been expected. One was carried out by Hawkins and Hosking (1969) who used key-press responses to letter stimuli. Four stimuli were mapped to one of the responses and the remaining stimulus to the other. Although a stimulus probability ratio of 8:1 was employed, no stimulus probability effect occurred. The suggested explanation was that the subjects probably regarded the first four stimuli merely as members of a negative memory set and failed to distinguish between them. The other study was a series of experiments reported by Dillon (1966). The experiments were somewhat unusual in that a conditional response technique was used, i.e. not all trials required a response. Those that did were marked by an auditory response demand signal, which followed the main stimulus after a delay. The stimuli were eight visually presented letters, matched in pairs for stimulus frequency and response demand probability. One member of each pair was mapped to a unique response and the other to a common response. The sequential dependencies of the stimulus sequence were constrained so that no first-order repetitions occurred and no stimulus was immediately followed by its matched-frequency twin. In the first experiment, the response demand probability was fixed at 0.5 for each stimulus. The stimulus probability ratio was 4:1 and the response probability ratio was 10:1. RT to the common response was found to be independent of stimulus probability, i.e. there was no stimulus probability effect. However, the unique responses showed a probability effect and each was slower than its common counterpart, indicating the existence of a response probability effect. In another experiment in the series, the response demand probabilities were chosen so as to keep the probabilities of the unique responses equal. The response demand signal followed the main stimulus after a delay of 500 ms and the stimulus probability ratio was 3:1. No RT differences were found among the unique responses, indicating the absence of a stimulus probability effect. The final experiment of the series used the conditional response technique to make response probability vary, while keeping stimulus probability constant. The response probability ratio was 3.6:1 and the stimuli were equiprobable. The unique responses showed the relative frequency effect and were found to be slower than their common counterparts, indicating the existence of a response probability effect.

Returning to the issue of the response probability effect, perhaps it is worth noting that, of those experiments just mentioned which failed to find an effect, none used response probability ratios greater than 5:1. Further work carried out with larger response probability ratios tends to show the existence of a response probability effect. Biederman & Zachary (1970) employed key-press responses to shapes and colours. They used five different conditions, each with the same sort of mapping arrangement as Bertelson & Tisseyre (1966). The conditions differed in the probabilities used. The stimulus probability effect was clearly evident but the response probability effect only appeared when the response probability ratio was as high as 9:1. Likewise, Hawkins, Thomas & Drury (1970) used visually presented digits as stimuli in a similar type of experiment and found a stimulus probability effect wherever there was a stimulus bias but only found a response probability effect where the response probability ratio was highest (11:1). Blackman (1972a) used a different type of experimental design which required making a hand movement response from a "home" key to one of two response keys. The stimuli were letters in one condition and circles in another. His design was such that he could only observe the effect of a stimulus frequency bias while response frequency was either controlled or allowed to change with stimulus frequency. He found a stimulus probability effect on decision time for both types of stimuli and he also found that this effect was enhanced by the presence of a response frequency ratio of 9:1. A later experiment of similar type (Blackman, 1972b) showed RT to be inversely related to stimulus probability as this was varied from 0.05 to 0.5, with response probability held constant at 0.5. Finally, it is worth noting that Spector & Lyons (1976) used a condensation task to isolate the stimulus frequency effect and showed that it interacted with compatibility. (The stimuli were colours in the low compatibility condition and visually displayed letters in the high compatibility condition. Responses for both conditions were letter names).

Thus far, it seems that the effect of response probability only appears when the response probability ratio is high. However, some other studies destroy the neatness of this conclusion. The work by Dillon (1966), which was described earlier in this section, obtained response probability effects with quite modest response probability ratios. LaBerge, Legrand and Hobbie (1969) used four conditions, each with similar stimuli, responses and mapping arrangements to those described for LaBerge & Tweedy (1964). They found the usual stimulus probability effect and also found an effect due to response frequency bias, even when the response probability ratio was as low as 2:1. Similarly, Hawkins, MacKay, Holley, Friedin & Cohen (1973) and Hawkins, Snippel, Presson, MacKay and Todd (1974) used vocal responses to letter stimuli and found a response probability effect with a response probability ratio of 3:1. It is worth noting that these studies also manipulated S-R compatibility and they showed that both stimulus- and response-probability effects were enhanced by a reduction in compatibility.

To summarise, it seems likely that both stimulus and response probability effects exist but more research needs to be done to find out why one or other of these components disappears under certain experimental conditions.

1.2.4 General Sequential Effects

This section deals with those sequential effects that occur in sets of trials where no sequential dependencies exist. The next section is concerned with the effects of manipulating transition probabilities. Some of the work described in this and the following sections is derived from an extensive and thorough review by Kornblum (1973b) which deals with both these aspects of the topic and also with interactions between sequential and other effects.

Regarding the occurrence of sequential effects when no sequential dependencies are present in the stimulus sequence, many investigators have reported what has become known as the "repetition effect", in which repeated signals are responded to more rapidly than non-repeated ones. This was first mentioned briefly by Hansen (1922) and later by Hyman (1953) and was investigated more fully by Bertelson (1961) who used key-press responses to stimulus lights in a two-choice task. He found a significant RE (repetition effect) when an RSI (response-stimulus interval) of 50 ms was used but not when a longer RSI of 500 ms was employed.

Bertelson's work prompted further research into both repetition effects in general and the effect of length of RSI on RE in particular. A number of later studies also used two-choice experiments with key-press responses. Bertelson & Renkin (1966) used symbolic stimuli and obtained a positive RE which decreased in magnitude as RSI increased in stages from 50 ms to 1 s. Hale (1967a) used visually-presented numerals as stimuli and, at an RSI of 100 ms, found a positive RE which decreased to a small negative value at an RSI of 2 s. In a second experiment (Hale, 1967b) with longer RSIs (incorporating a foreperiod), he found nothing but small, negative values for the RE. Williams (1966) also obtained a small negative RE with an RSI of about 12 s in a two-choice task which employed coloured lights as stimuli and a three-position switch for the responses. Kirby (1972) reported an experiment that used lights as stimuli and obtained negative REs at RSIs ranging from 2 s to 8 s. He found no systematic effect of RSI on RE within this range. In a later experiment (Kirby, 1976b) he found a positive RE at an RSI of 50 ms and obtained negative values at the longer RSIs of 500 ms and 2 s. Schvaneveldt & Chase (1969) employed lighted buttons as combined stimuli and response keys and obtained small negative REs for RSIs varying from 100 ms to 8.5 s. They obtained similar results with a four-choice experiment, except that some of the REs had small positive values. Similarly, Keele & Boies (1973) failed to find a significant RE in a series of four-choice tasks in which the RSI was varied between zero and 500 ms. Keele (1969) used a six-choice task with lights as stimuli and an indirect S-R mapping and obtained quite a large RE of 120 ms with an RSI of 2 s and an even larger one with an RSI of 4 s. At the longest RSI of 8 s, the RE was 60 ms. However, this effect vanished when he used a compatible S-R mapping. Kirby (1975) found a positive RE with an eight-choice task (also using lights as stimuli) at RSIs of both 1 ms and 2 s, but there was no significant difference in the magnitude of the effect at the two different RSIs.

To summarise these results, there does seem to be a general tendency for the RE to become smaller as RSI is increased. With an RSI of more than about 2 s, the RE frequently vanishes altogether or takes on a small negative value which changes little as the RSI is further increased. Kornblum (1973b) is less sanguine concerning the drawing of conclusions from the earlier work in this area but appears to have overlooked the importance of the discrepant findings. The clue to these appears to lie in the levels of compatibility to be found in the S-R codes of the various experiments. It is noteworthy that Schvaneveldt and Chase (1969) did not obtain the usual sequential effect and that they used lighted buttons for both their stimuli and responses - a highly compatible arrangement. With less compatible tasks (reported in the same paper) larger REs were found. However, these did not seem to depend on RSI. Conversely, Keele (1969) found a particularly large RE at quite long RSIs when using an incompatible S-R mapping, but not when using a compatible arrangement. Thus it appears that incompatible S-R codes facilitate the repetition effect and also that they prolong the decay time of the effect. Further support for this idea comes from an experiment reported by M.C. Smith (1968). This used a four-choice task involving stimuli with two dimensions of variation, i.e. two different digits and two colours of background. Key-press responses were used and the S-R relationship was necessarily arbitrary. A large RE of 120 ms was found at an RSI of 2 s. REs of 93 and 65 ms were obtained for RSIs of 4 s and 8 s respectively.

Before leaving the topic of RE decay, it is worth noting that Kornblum (1973b) points out that stating the effect of increasing RSI on the RE is really not sufficient. It is also desirable to know the locus of this effect. For example, if the RE decreases with increasing RSI (as it often seems to do), is this effect due to non-repetitions becoming faster or to repetitions becoming slower? The answer to this question is not as obvious from an inspection of the data as are the changes previously mentioned. This fact itself suggests that the RE may involve a preparation tradeoff between different classes of stimuli or response.

Returning to the effect of S-R compatibility on the repetition effect, not many experiments have been directed towards this particular issue. However, a few studies have been reported which have dealt with the matter. Bertelson (1963) conducted two experiments. The first of these used a two-choice task with key-press responses to light stimuli and three different levels of compatibility. The RSI was about 100 ms. This experiment yielded a clear positive RE which increased as compatibility decreased. The second experiment used a four-choice task with visually presented numerals as stimuli. Two different S-R mappings were employed and the RE was substantially larger in the less compatible one. Kirby (1976a) used an eight-choice task with light stimuli, key-press responses and an RSI of 500 ms. He looked at two types of non-repetition - those involving the same hand as the preceding response and those requiring the other hand. By comparing both these separately with performance on repetitions, he found that reducing S-R compatibility led to an increase in both types of RE. Schvaneveldt & Chase (1969) also reported a positive RE for both two- and four-choice tasks using incompatible S-R codes. (This should be contrasted with their other results, obtained with highly compatible tasks, reported earlier).

Thus it seems reasonably conclusive that reducing S-R compatibility does facilitate the repetition effect. Bertelson (1963) points out that with a difficult S-R mapping, the strategy of repeating the previous response if the stimulus is repeated can be clearly faster than following the decoding process through again. Conversely, with a highly compatible S-R mapping, the gains are minimal. This idea is supported by the results of a series of two- and ~~four~~ three-choice experiments carried out by Shaffer (1965, 1966, 1967) in which the stimuli were either two horizontally arranged lights, or three lights at the vertices of a triangle. He used key-press responses. The tasks were arranged with a variable S-R mapping and each trial was accompanied by a signal specifying the particular mapping to be used on that trial. The results showed that the fastest RT was obtained when both the mapping rule and the stimulus were repeated. The slowest RT occurred when the stimulus was repeated but the mapping rule was changed.

It is instructive to see how well these observations concerning the repetition effect fit in with the notions of selective preparation, mentioned earlier. The mechanism of response pre-selection is clearly similar to the ideas proposed by Bertelson (1963). The findings concerning RE decay are also interesting in that they suggest that it is not possible to hold a response in the pre-selected state for long.

Turning to the effect of stimulus probabilities on the repetition effect, Falmagne (1965) used key-press responses to visually-presented numerals in a six-choice task with an RSI of approximately 750 ms. The usual probability effect was observed and the expected RE was found but there was no interaction between the two. Remington (1969) employed a two-choice task with lights as stimuli and key-press responses. He used two conditions, one with equiprobable stimuli and one with a 7:3 ratio of stimulus probabilities. The foreperiod was 1 s and the total RSI unspecified. In the equiprobable condition, he obtained the expected RE. In addition, he looked at higher order sequential effects and presented the results in the form of a branching diagram which clearly indicated that the mean RT for repetitions was smaller the longer the run of repetitions that preceded it. Also, the breakdown showed that the RT for non-repetitions was greater for a long preceding run of repetitions than for a short one. For the condition with stimulus probability bias, Remington obtained qualitatively similar results for the two stimuli considered separately. However, the facilitating effect of repetition run length on repetitions was found to be greater for the low-probability stimulus than for the high-probability one. On the other hand, the inhibiting effect of repetition run length on non-repetitions was greater for the high-probability stimulus than for the low-probability one. The first-order RE was found to be larger for the high-probability stimulus than for the low-probability one. However, in a later study (Remington, 1971) which used a four-choice task with a probability ratio of 2:1:1:1, there was no difference in the magnitude of the RE between the high-probability stimulus and the others.

Another study (Krinchik, 1969) varied both the number of stimuli and the stimulus probabilities in a fully crossed experimental design. Three different probabilities of a critical stimulus were employed (0.067, 0.5 and 0.933) and three levels of choice were used (two, four and eight alternatives). The RSI was 7 s. No systematic effect of critical stimulus probability on the RE was observed, but number of alternatives was found to have a pronounced effect, with the RE increasing with number of alternatives. Thus there is little evidence that stimulus probability per se affects the repetition effect but some evidence that number of S-R pairs might do so. Kornblum (1973b) points out that indirect evidence from two different experiments by Bertelson (1963) supports the idea that the repetition effect becomes larger as the number of alternatives is increased.

A few other studies have examined this effect. Hale (1969a) used key-press responses to equiprobable, visually presented numerals. He found a greater RE with eight choices than with either two or four. Examining repetitions and non-repetitions separately, it was found that RT for both repetitions and non-repetitions increased as N increased, but the increase for non-repetitions as N increased from four to eight was much greater than the corresponding increase for repetitions. Remington conducted a two-choice experiment (Remington, 1969) and a four-choice one (Remington, 1971) that, taken together, also suggest that RE increases as N increases. Both tasks used key-press responses to light stimuli. Again, it was found that RT for non-repetitions increased rather more than that for repetitions as N was increased from two alternatives to four. Schvaneveldt and Chase (1969) also found some evidence for a similar effect, at least with one of their less compatible tasks.

Thus it seems reasonable to conclude that the effect is a real one and also that it appears to be due to a larger increase of RT in non-repetitions than in repetitions as N is increased. This, in turn, suggests that there is some connection between the two phenomena. From comments made earlier, it seems that subjects are better prepared for repetitions than for non-repetitions and the fact that the effect due to number of alternatives is less apparent with repetitions (i.e. with higher levels of preparation) suggests two things. Firstly, the fact that some form of preparation can attenuate the effect due to number of alternatives suggests that higher levels of preparation do indeed involve a bypass mechanism such as response pre-selection (mentioned earlier). Secondly, the fact that the effect is present in the partitioned data (i.e. the repetitions and non-repetitions considered separately) is indicative of there being a genuine effect of number of alternatives, quite independent of any by-product of preparation mechanisms.

As with the effects previously discussed, sequential effects can be examined from the viewpoint of whether they are located on stimuli or responses. If a condensation task is employed in a CRT experiment, each trial can be categorised according to its relationship to the preceding trial. Trials involving stimulus repetitions are termed "identical" (I), while those requiring the same response to a different stimulus are called "equivalent" (E). Trials termed "different" (D) are those which require a different response from the preceding trial. Bertelson (1965) used a condensation task which mapped two visually presented stimuli to each of two key-press responses. The RSI was approximately 50 ms. D responses had RTs 96 ms longer than E responses, which were in turn 19 ms longer than the I responses. Thus the major part of the RE was located on the (D-E) difference, i.e. was due to a response effect, which is where it might be expected to be found if it were largely concerned with the facilitation of response selection.

Rabbitt (1968b) reported three experiments on similar lines. Two of them used eight stimuli; one of these tasks having two responses and the other, four. The remaining task had four stimuli mapped to two responses. All three experiments produced differences (between the various types of response) in the same direction as in Bertelson's study (just mentioned) but the relative magnitudes of these differences did not form any consistent pattern. However, it is worth reporting that Peeke & Stone (1972) found D responses faster than E responses in tasks which mapped two stimuli to each of two responses. One of the tasks concerned used colours as stimuli and the other employed geometric shapes. There was nothing obviously different about the tasks that might have accounted for the anomalous findings.

1.2.5 Effect Of Manipulating Transition Probabilities

Bertelson (1961) manipulated both RSI and transition probability in a two-choice task which required key-press responses to light stimuli. Two different RSIs were used - 50 ms and 500 ms. The stimuli were equiprobable but occurred with different transition probabilities in different conditions. One condition (RAND) had no sequential dependencies, i.e. the probability of a repetition was 0.5. Another condition (REP) favoured repetitions with a probability of 0.75. The third condition (ALT) favoured alternations with a probability of 0.75. As might have been expected, RT to repetitions increased as repetition probability was reduced. The opposite happened with alternations. Thus the RE decreased, actually becoming negative in the ALT condition. However, in the condition with the shorter RSI, the magnitude of the increase in RT was greater for repetitions than the magnitude of the decrease for non-repetitions. Kirby (1976b) used a similar task with RSIs of 1 ms and 2 s. The RAND sequence was similar to that used in Bertelson's study but the REP and ALT sequences had slightly less extreme repetition probabilities - namely 0.7 and 0.3. The results were qualitatively similar to those just described for Bertelson's study. Moss, Engel & Faberman (1967) also used a similar task, but with a single RSI of about 12 s. They obtained rather different findings. Firstly, RT's to both repetitions and alternations were faster when the probability of repetitions was 0.5 than in any other condition. Secondly, except when repetitions predominated, the RE was negative - presumably because of the long RSI. (However, the RE did increase monotonically with increasing repetition probability). Thirdly, repetition RT was no faster under the condition favouring repetitions than the one favouring alternations. Alternations, on the other hand, were rather faster in the latter condition than in the former.

Turning to larger numbers of alternatives, Kornblum (1973b) reported the results of regression analyses carried out on data from an earlier experiment (Kornblum, 1967). The experiment used a wide range of transition probabilities in equiprobable tasks with two, four and eight choices. The RSI was 137 ms and key-press responses were made to light stimuli. For each task, a straight line fit was performed separately for repetitions and non-repetitions versus the transition probability of the partition used. The intercept for repetitions was found to be smaller than that for non-repetitions. The difference was negligible for the two-choice task, but increased as the number of choices increased. In each case, the magnitude of the gradient was greater for repetitions than for non-repetitions but the difference was much greater in the two-choice task than in the others. In a later experiment, Kornblum (1969b) used two four-choice tasks. The serial task was similar to those just described, with an RSI of 140 ms. The other task was a discrete one, using key-press responses to visually presented numerals. The RSI included a foreperiod and varied between 2.9 and 3.4 s. For each task, the same sort of regression analysis was performed as that described above. Again, the intercept for non-repetitions was significantly larger than that for repetitions for each task. In neither case was the difference between gradients significant. The intercepts in the discrete task were larger than for the serial one. It is worth noting that a further analysis of the same data in Kornblum (1973b) showed that the facilitating effect of increasing repetition probability on repetition RT was present for first-order repetitions considered alone. This effectively forestalls the possible criticism that the earlier analyses were not allowing for the increased frequency of higher-order repetitions, which many researchers (e.g. Remington, 1969) have found to be faster than first-order repetitions.

Another experiment shows findings compatible with this picture. Umilta, Snyder & Snyder (1972) reported results from a four-choice task requiring key-press responses to visually presented numerals. The experimental design was a factorial one and had three different RSI's ranging from 250 ms to 3.75 s and two different transition probability conditions. In one of these, repetitions had a high probability (0.82) and the alternations were equiprobable. In the other condition, one of the possible alternations after each stimulus had a probability of 0.82 and the remaining transitions were equiprobable. High-probability repetitions from the first condition were found to be faster than high-probability alternations from the second, although this difference declined as RSI increased and was absent altogether at the longest RSI. Another point worth mentioning in passing is that, for the two shorter RSI's, second-order low-probability repetitions were found to be considerably faster than first-order ones.

Returning to the issue of stimulus versus response aspects of sequential effects, further light is cast on the matter by Kornblum (1973b). Kornblum conducted an experiment on the same lines as that reported by Bertelson (1965), described earlier. The RSI was 100 ms, but the main difference between the tasks was that Kornblum manipulated the transition probabilities in his experiment. He used three different conditions in which the probability of a response repetition took different values - namely 0.3, 0.5 and 0.7. Within each of these conditions, five different sub-conditions were arranged. In one of these, the probability of a response repetition was made equal to the probability of an I transition and in another it was made equal to the probability of an E transition. For the remaining three sub-conditions, the relative weighting of I and E transitions was varied symmetrically between these two extremes. A graph of RT versus the probability of the transition showed very neatly that I responses were faster than E responses and that these in turn were faster than D responses. The graph also showed that, for all types of transition, increasing the probability from 0.5 to 0.7 produced a much greater decrease in RT than an increase in probability from 0.3 to 0.5. There was no interaction between type of transition and probability of transition - indeed the lines on the graph were almost exactly parallel. The (I-E) difference was virtually the same size as the (E-D) difference, suggesting that, if the various types of transition are normalised (with respect to their probability of occurrence) before comparison, then both stimulus and response repetition effects become apparent.

When transition probabilities are manipulated, the information content is reduced, i.e. the stimulus sequence becomes more predictable. Thus, this type of manipulation is similar to that employed in altering the stimulus frequencies themselves. For this reason, the same remarks that were made in Section 2.2 concerning the preparation mechanism of response pre-selection are applicable here, too.

1.2.6 Sequential Effects Confounded With Others

In previous sections, the problem of the confounding of different effects was mentioned. For example, the issue of the possible confounding of the effects of equiprobable number of choices and the probability of stimuli was dealt with by examining the results of experiments designed in such a manner as to separate these factors properly. This section deals with two more such problems. Firstly, the question of whether the relative frequency effect is due to stimulus or response probabilities is examined again - this time from the standpoint of possible confounding ~~of~~ with sequential effects. Secondly, the effect of number of choices is also examined again (from a similar perspective).

Dealing with the first of these topics, a tentative observation was made in Section 2.4 to the effect that there seemed to be a tendency for response probability effects to become apparent only when the response probability ratio is large. There is a possible reason for this, located in the fact that repetitions tend to be faster than non-repetitions. Kornblum (1973b) explains why this is so, but his explanation is not given in terms of conditional transition probabilities and thus is not really satisfactory. A more appropriate explanation runs as follows. It depends on the experiment using a condensation task of the sort employed by Bertelson & Tisseyre (1966). Suppose that two stimuli (A and B) are mapped to one response and a third stimulus (C) is mapped to a second. Let the probability of occurrence of stimulus A be p(A) and similarly for B and C. Furthermore, let the probabilities p(B) and p(C) be equal. Now a response probability effect is assessed by comparing responses to stimuli B and C. Given that stimulus B has occurred, the conditional probability of it producing a response repetition is p(A)+p(B) and the conditional probability of it producing a response non-repetition is p(C). Given that stimulus C has occurred, the two conditional probabilities are respectively p(C) and p(A)+p(B). Putting p(C) equal to p(B), the conditional probability ratios of response repetitions to non-repetitions are thus (p(A)+p(B))/p(B) for stimulus B and p(B)/(p(A)+p(B)) for stimulus C. Now the response probability ratio is also (p(A)+p(B))/p(B) and so, as p(A) is increased, both the response probability ratio and the ratio of response repetitions to response non-repetitions for the shared response are increased. There is thus a clear confounding between the response repetition effect and the response probability effect. For the sake of completeness, it should be pointed out that with a task of this type, a similar problem occurs with the stimulus probability effect being confounded with the stimulus repetition effect.

Turning to the second problem, attention was first drawn to the matter by Kornblum (1967) where he pointed out that, for equiprobable stimuli with no sequential dependencies, the number of choices is confounded with the probability of non-repetition transitions. In a later paper (Kornblum, 1975), he reported an experiment designed to disentangle the confounded effects. The basic experiment was described in Section 2.2. However, a sequential analysis was also performed after the data had been partitioned into repetitions and non-repetitions. The results suggested that, for both mapping arrangements, repetitions of the critical stimulus did not show an increase in RT as N was increased. This in turn suggests that, with stimulus probability held constant, the site for the action of increasing the number of choices is located on the non-repetitions. This does not completely solve the original problem, because although the overall probability of the critical stimulus occurring as a non-repetition is independent of N, the probability of it occurring after any <u>particular</u> non-critical stimulus declines as N is increased. Thus the final verdict on this issue must await an experiment which takes this aspect of the problem into account, too.

However, it is obvious from what has been said that sequential effects are quite pervasive and could be at least partially responsible for some of the other effects described. This is important because of the links previously mentioned between sequential effects and preparation. The point is that shortly after having made a particular response, the subject is ready to make that response again - it does not have to be "reloaded". Therefore, any effect which is confounded with the general RE could possibly be partially (or even completely) due to this preparation effect.

1.3 <u>CONCLUSIONS</u>

The findings related in this chapter constitute a morass of interrelated effects. However, three points can be re-emphasised in conclusion. Firstly, the general repetition effect does appear to work through a fading trace of each response remaining available for a short time. This facilititates repetitions of that response and also, by virtue of the confounding of the RE with other effects, can contribute to these other effects. Secondly, quite apart from any connection with the RE, it seems quite plausible that many effects are partially due to the fact that pre-selected responses can lead to shorter RTs in certain experimental conditions. Thirdly, the interaction of most, or all of the effects described in this chapter with S-R compatibility suggests that they are at least partially located at the stage of response selection or concerned somehow with this process.

CHAPTER 2: PREPARATION, PREDICTION, EXPECTANCY AND ERRORS

2.1 INTRODUCTION

This chapter deals firstly with observed foreperiod effects and with the related theoretical notions of expectancy and preparation. The discussion then focuses on the stimulus- or response-specific aspects of preparation and subjects' prediction effects. Thirdly, the commission of errors is covered, including the connections between errors and other phenomena.

2.2 EXPECTANCY AND PREPARATION

2.2.1 Foreperiod Effects

Much CRT research has been concerned with various foreperiod effects. Foreperiods may be manipulated in two ways. They can be changed either between blocks of trials or within blocks of trials. A foreperiod or PI (preparatory interval) which remains the same for the duration of a block of trials is usually known as a "fixed", "regular" or "constant" foreperiod. Where the foreperiod is changed within a block of trials, it is generally drawn from some distribution of possible foreperiods which may be either discrete or continuous. In the latter case, the continuity is often only apparent, with the distribution actually being composed of a large number of discrete values.

Dealing first with fixed foreperiod effects, it has been found that increasing the PI typically leads to an increase in RT. This was apparently first noted by Woodrow (1914). Karlin (1959) confirmed the finding with an SRT (simple reaction time) experiment. Both stimulus and warning signal were auditory and the response was key-release, rather than key-press. However, Woodrow used foreperiods ranging from 1 s to 24 s and found that the minimum RT occurred with a foreperiod of 2 s. In contrast, Karlin used foreperiods ranging from 500 ms to 3.5 s and found the minimum RT at the shortest foreperiod. Returning to visual tasks, Aiken & Lichtenstein (1964a) did not use a warning signal but used regular ISIs (inter-stimulus intervals) between 1 s and 10 s in an SRT experiment. Again, RT was fastest at the shortest interval. In a second experiment (Aiken & Lichtenstein, 1964b), the subjects were made to react either to every fourth, every second or every stimulus in a regularly-spaced series. RT was found to depend on ISI rather than on inter-response interval, thereby suggesting that time estimation was the underlying factor for the foreperiod effect. Further evidence for this comes from a study by Foley & Dewis (1960), who compared three methods of varying the RSI (response-stimulus interval) and found that keeping the foreperiod constant was the only technique which did not cause RT to increase with increasing RSI. (The other techniques involved either no warning signal at all, or a constant "afterperiod" and an increasing foreperiod). It is also worth noting that an earlier study by Foley (1959) failed to show that the duration of the warning signal had any effect on RT, suggesting that the foreperiod itself was the only important factor as far as time estimation was concerned.

Bertelson & Tisseyre (1969) investigated the foreperiod effect with a visual two-choice task, using key-press responses. Both auditory and visual warning signals were tested and the foreperiod range was -20 ms to 700 ms. Some control trials were also run, in which there was no warning signal at all. The results showed that the optimum foreperiod was 200 ms with the visual warning signal and between 70 and 120 ms with the auditory one. (This difference is, of course, in the expected direction, as auditory RTs are faster than visual RTs because of the different nature of the sense receptors). Another visual two-choice experiment with an auditory warning signal was described by Bertelson (1967). This used a range of regular foreperiods ranging from zero to 300 ms and produced the fastest RTs with foreperiods of 100 and 150 ms.

More recently Naatanen, Muranen & Merisalo (1974) ran an experiment employing two tasks to test the time estimation hypothesis. One task used visual SRT, with regular foreperiods from 250 ms to 4 s. The other task involved getting the subject to attempt to synchronise his key-press with the stimulus, following a warning signal. The same range of foreperiods was used in both tasks. The results showed that the minimum RT was obtained with a foreperiod of 500 ms, rather than the shortest foreperiod. For the synchronisation task, a similar picture was obtained, with the minimum absolute anticipation error also occurring with the 500 ms foreperiod. As time estimation is clearly involved in a synchronisation task, the similarity of the results does indeed suggest that time estimation error is responsible for the foreperiod effect. The only difficulty with this explanation is that it does not account for the lengthening of RT as the foreperiod is reduced below 500 ms, or for the similar increase in anticipation error. The explanation suggested by Naatanen et al for the latter phenomenon was that, for the shortest foreperiod of 250 ms, the subject adopted a different strategy because of the difficulty of preparing a synchronisation movement with such a short warning interval. The suggested strategy was that the subject was actually treating the warning signal as an imperative stimulus and making an RT response to it. Naatanen et al further suggested that the long response time in the genuine RT task at the shortest foreperiod was due to the psychological refractory period. To deal in detail with this much researched topic would go rather beyond the scope of this thesis. Suffice it to say that when two stimuli are presented in close succession, response to the second is delayed by the presence of the first, even when no response to it is required. Thus, Naatanen et al suggested that the increase in both RT and absolute anticipation error as foreperiod length increases is due to an increasingly inaccurate (in absolute terms) time estimation process, whereas the tendency for both these dependent variables to increase as the foreperiod becomes <u>very</u> short is due to <u>different</u> processes in the two tasks.

Turning to irregular foreperiod effects, many more studies have been conducted in this area. Klemmer (1956) found that with irregular foreperiods (presumably drawn from uniform distributions), RT increased with foreperiod range and with foreperiod mean. The most striking finding was that the important determining factor of RT was not the immediate foreperiod, but rather the distribution of foreperiods from which it had been drawn. Thus Zahn & Rosenthal (1966) showed that, with an auditory SRT task, the more frequent a particular foreperiod in a discrete distribution of foreperiods, the faster was RT. However, both Baumeister & Joubert (1969) and Karlin (1966) did show that, with a variety of distributions, the PI itself did have a significant effect on RT. Elliott (1973) also showed that the interactions between the PI itself and both the mean and range of the distribution from which it had been drawn were also significant. More precisely, the PI effect increased as the range increased and as the mean decreased. Similarly, Fishburne & Waag (1973) used a four-choice task with lights as stimuli and key-press responses. Three different ISIs of 2, 3 and 4 s were employed in three conditions. Each of the latter used a different schedule of presentation. They found the fixed interval schedule to be faster than the one based on the patterned intervals, which was in turn faster than the random schedule, for all intervals. For the fixed interval schedule, there was a clear indication that ISI itself affected RT in the expected direction. The importance of the foreperiod distribution was demonstrated by Rothstein (1973), who used three overlapping foreperiod ranges with a visual SRT task. These foreperiod ranges all included a common value of 2.5 s. The results showed that RT was slowest when the common foreperiod was the lower limit of the range and fastest when it was at the upper limit.

Under a time uncertainty explanation of foreperiod effects, it would be expected that regular foreperiods would produce shorter RTs than irregular ones of the same length. This is usually the case. However, Bertelson & Tisseyre (1968) failed to find any difference between these conditions in an SRT task with very short foreperiods (300 ms and below), using either auditory or visual warning signals. The refractory effect could well have been responsible for this.

However, Botwinick & Brinley (1962) demonstrated that, in both visual and auditory tasks, SRT was slower for a given foreperiod when this foreperiod was one of an irregular series than when it was incorporated in a regularly varying one. It is, perhaps, worth noting in passing that, in this study, the warning signal for the auditory task was a visual one and vice versa. This somewhat unusual arrangement was presumably employed in order to rule out any possible peripheral (i.e. sense-specific) aspects of preparation.

2.2.2 Time Uncertainty

As indicated in the previous section, it seems that many aspects of foreperiod effects are mediated by a time estimation process whose level of absolute accuracy decreases as the interval to be estimated (in this case, the foreperiod) increases in length. The implications that this has for the preparation process will be discussed in the following section. However, the phenomenon of temporal uncertainty in CRT tasks has been investigated empirically. Klemmer (1957) used an information-theoretic approach applied to a visual SRT task with an auditory warning signal and a key-press response. He used both regular and irregular series of foreperiods and manipulated time uncertainty by varying foreperiod length in the regular series and both mean length and range in the irregular series. He estimated the variance in RT due to the subject's imperfect time keeping ability by the use of synchronisation tests. For the series with irregular foreperiods, he combined this with the variance of the foreperiod distribution itself and thus arrived at a single variance measure which he then converted into an informational one (relative to a fixed foreperiod of 1 s). For each subject considered separately, he obtained a good straight line fit between RT and temporal information, with the slopes of the functions varying between 12 s and 24 ms per bit. Klemmer himself was rather dismissive about this result, because of the small value for the gradient (just quoted), compared with that usually obtained in studies where the entropy is manipulated by altering some aspect of the stimulus probabilities.

Snodgrass, Luce and Galanter (1967) found that the coefficient of variation for a time estimation task was of the order of 0.1, for time intervals ranging from 600 ms to 5 s. They made the observation that this was considerably larger than the figure obtained in RT tasks (measuring the interval from the warning signal) and concluded that processes other than simple time estimation were at work in RT tasks with warning signals. In a subsequent monograph, Snodgrass (1969) reported the results of an experiment which attempted to determine the location of a subject's "true" SRT distribution by rewarding him for responding consistently. Narrow payoff bands were used, whose positions were systematically varied along the time axis. The "true" SRT distribution was presumed to be the one with the smallest variability. The location of this "true" SRT distribution was not significantly affected by increasing the foreperiod range from zero to 300 ms, providing evidence that the locus of the foreperiod effect is not on the "actual" RTs but rather on the time estimation process. Snodgrass explained the results in terms of an anticipation model for foreperiod effects. This model consisted of an underlying RT distribution of low variability and a more variable distribution of time estimations, affected by foreperiod variability and payoff.

Another approach to the investigation of time uncertainty in RT was described by Gottsdanker (1970a). Gottsdanker was much concerned to examine the influence of time uncertainty on RT in a more carefully controlled fashion than is usually possible with conventional methods. He devised a technique which he called the "transit-signal" method. This was a combination of the "transit" method and the more orthodox "signal" approach. The transit method typically requires the subject to respond when a moving target of some sort crosses a reference line. The obvious drawback with this approach is that it gives the subject sufficient information to make a very accurate synchronisation response. However, Gottsdanker's combination technique utilised a dark rotating disc with a white sector on it. The signal was a lamp which might be illuminated at any time that the sector was crossing the reference line. The chief advantage of using this technique (compared with the conventional approach with an irregular foreperiod) is that both the foreperiod range and the temporal progress through it are apparent to the subject on every trial. This avoids the problem of objective time uncertainty being confounded with the subject's time keeping inaccuracy.

A later paper (Gottsdanker, 1970b) reported an experiment which investigated the effect of time uncertainty on SRT, using both the traditional method and the transit-signal method (just described). Two different foreperiod ranges were used - namely 1 s and 3 s. For the former, the foreperiod ranged from 2.5 s to 3.5 s and for the latter, the range was 1.5 s to 4.5 s. For all conditions, the distribution of foreperiods was uniform. The results showed that RTs obtained using the transit-signal technique were always faster than those obtained with the conventional method. For all conditions (except that using the short foreperiod range with the conventional signal method) RT was found to be a decreasing function of foreperiod length (within the range used). For the transit signal method, the slope of the function was approximately linear and appeared to have a steeper slope for the shorter foreperiod range than for the longer one. These findings suggest that the gradient is actually a result of the subject's increasing level of preparation as foreperiod length increases. The gradient is steeper for the transit signal method because each subject has a more accurate indication of the passage of time within the foreperiod range than is available from his own time keeping ability. The significance of this is discussed in the next sections.

2.2.3 Expectancy And The Time-Course Of Preparation

One of the earlier attempts to examine the phenomenon of preparation is reported in a monograph by Mowrer (1940), where he describes a number of methods of measuring preparatory set. Among these methods is one which looks at preparation in the context of SRT. Mowrer used a task which required the subject to make a manual key-release response to a tone presented with an ISI of 12 s. This background task was used to establish an expectancy peak 12 s after each stimulus. However, interspersed with these signals were occasional test stimuli at other intervals, ranging from 3 s to 24 s, in increments of 3 s. (This was really an example of the so-called "probe" technique, because the test stimuli were used to probe the level of expectancy at various intervals before and after the peak level). The results showed that RT had a maximum value at the 3 s interval and decreased to a minimum value at 12 s. The graph of RT against interval then increased again, up to 24 s, though with a shallower slope. The interpretation put on this picture was that the mean RT to test stimuli at a given ISI interval reflected the level of preparedness to respond.

Karlin (1966) used the same sort of probe technique with an auditory SRT experiment in which he obtained RTs using foreperiods drawn from a variety of unimodal and bimodal distributions. Minimum RTs occurred with foreperiods at or near the modes of the distributions used, even when these were as short as 150 ms.

As with many other aspects of CRT, sequential phenomena play a part in the preparation process. It has been found that SRTs tend to be shortest when the foreperiod is the same as on the immediately preceding trial (Schupp & Schlier, 1972; Possamai, Granjon, Requin & Reynard (1973). Similar effects have been observed for two-choice tasks (Alegria, 1975a, 1975b). Also, RTs on trials involving foreperiod repetitions were found to be faster when foreperiod repetitions were frequent than when they were not (Alegria, 1975b; Granjon & Reynard, 1977). The former study revealed that multiple repetitions of the same foreperiod only led to a further reduction in RT when foreperiod repetitions were frequent. Possamai, Granjon, Reynard & Requin (1975) used an auditory SRT task with countdown information. The foreperiod distribution consisted of just two values. The chief finding was that there was a marked first-order repetition effect present for the shorter foreperiod. It appeared that the repetition effect was partially cancelling out the usual lack of preparation for short foreperiods. Gosling & Jenness (1973) looked at the so-called "surprise" effect in a visual SRT task and found that RT increased as the difference between the current foreperiod and its predecessor increased (subtracting the former from the latter). A similar effect can be observed in the data from an experiment reported by Alegria & Delhaye-Rembaux (1975) which employed the same type of moving spot signal display as that used by Alegria (1974). It seems reasonable to claim that this effect is due to the subject basing his preparation partly on the time course of the preceding trial and not being ready for the next stimulus when it arrived "early". However, sequential foreperiod effects cannot always be found. Buckoltz & Wilberg (1975) found no interaction between previous foreperiod and current foreperiod in a visual SRT task. It seems that more work needs to be done to find out under precisely which circumstances sequential foreperiod effects appear.

It has been frequently postulated that, in tasks with an irregular rectangular foreperiod distribution, the level of preparation for the longer foreperiods should be higher than that for the shorter ones by virtue of the fact that the conditional probability of stimulus occurrence (i.e. the probability of a stimulus occurring, given that it has not already occurred) increases during the preparation interval. If this is so, then it should be reflected in a decreasing RT with increasing foreperiod length. This was found to be the case with auditory SRT in six studies (Karlin, 1966; Elliott, 1973; Granjon & Reynard, 1977; Possamai et al, 1973; Possamai et al, 1975; Zahn & Rosenthal, 1966) and also with visual SRT (Baumeister & Joubert, 1969). However, Botwinick & Brinley (1962) found the opposite tendency with both visual and auditory SRT tasks. The reason for this discrepancy is not clear.

A difficulty with investigating preparatory processes in this type of experiment is that conditional probability of stimulus occurrence is confounded with subjective time uncertainty. A number of techniques have been developed to cope with this problem. One solution involves using a "countdown" procedure to control for time uncertainty. This involves presenting the subject with a periodic signal to mark the passage of time during the foreperiod. Requin & Granjon (1969) used this technique with an auditory SRT task with irregular foreperiods from a discrete rectangular distribution having values at 8, 12, 16, 20, 24 and 28 s. One condition involved marking the passage of time through the foreperiod with a click at intervals of 4 s. The other condition was a control and did not have this feature. The results showed a marked conditional probability effect in the experimental condition but hardly any such tendency in the control. Obviously, the absence of the countdown information resulted in the conditional probability effect being obliterated by high subjective time uncertainty. More surprising, however, was the finding that RTs in the experimental condition were slower than in the control condition. The explanation offered was that the experimental condition could be construed as a different type of task altogether, in which the timing click could be considered as a warning signal which might be followed by a stimulus 4 s later. The probability of stimulus occurrence is actually known to affect RT in such cases (which are discussed later), with RT decreasing as stimulus probability increases. A later experiment of the same type (Requin, Granjon, Durup & Reynard, 1973) produced similar results. Stilitz (1972) ran an auditory SRT experiment with irregular foreperiods of 1, 3 and 5 s. Two different conditions were used, both with unequal probabilities for the different foreperiods. The results suggested that RT was an increasing linear function of reciprocal conditional probability.

Another means of dealing with the confounding of subjective time uncertainty and the conditional probability effect uses the complementary approach. Rather than controlling for time uncertainty and manipulating conditional probability as just described, it is possible to hold conditional probability constant and let subjective time uncertainty vary with foreperiod length. This technique involves the so-called "non-ageing" foreperiod. It can be used with either a discrete range of foreperiods, or with a continuous range. In the former case the foreperiod is geometrically distributed, whereas the latter requires that the probability density conforms to an exponential decay function. Most experiments that have employed the technique have used a set of discrete values for the possible foreperiods. These are separated by equal time intervals, or epochs. Briefly, the method works as follows. Suppose the shortest foreperiod in the range has a probability 'p' of occurring on a given trial. This means that the next possible foreperiod must have a probability of p(1-p) in order for the conditional probability to remain constant with a value of 'p'. In general, the nth. shortest foreperiod will have a probability of $p(1-p)^{n-1}$. (Theoretically, this leads to an infinite maximum value for the foreperiod but practical constraints necessitate the truncation of the process at some stage). The theory is explained more fully by Nickerson (1967).

A plausible theoretical application of these ideas is to operationally define the conditional probability of stimulus occurrence as expectancy itself. There are possible objections to this, on the grounds that, properly speaking, expectancy is a subjective state and hence cannot be adequately represented by an expression derived solely from parameters of the experiment. Other objections have been raised by Naatanen and Merisalo (1977). As mentioned earlier, genuine expectancy seems to be influenced by sequential foreperiod effects, which conditional probability is not. Also, if a subject is suffering from fatigue, he might not be capable of attaining a high level of expectancy even when the stimulus is very likely to occur. Nevertheless, it would be surprising if true (i.e. subjective) expectancy (i.e. readiness to respond) were not closely related to expectancy as thus defined. Nickerson & Burnham (1969) ran a visual SRT experiment which employed non-ageing foreperiods. The warning signal was auditory and eight different values of 'p' were used to give eight different non-ageing foreperiod ranges, with expected values ranging from 250 ms to 32 s in equal logarithmic steps with a factor of two. All epochs had the same duration of 25 ms. RT was found to increase with increasing expected foreperiod length. In fact, a good linear fit was obtained using the logarithm of the expected foreperiod length. Bearing in mind that the expected foreperiod length varies inversely with the reciprocal of 'p' (which may be operationally defined as expectancy) the result was expressed as follows (using logarithms to base 2):

$$RT = 150 - 30\log(p)$$

Nickerson & Burnham drew attention to the fact that this equation is obviously structurally similar to that used in the information-theoretic approach to the relationship between RT and stimulus probability. However, they also made the observation that the parameter values also depend on epoch duration. (In fact, the slope would be expected to vary inversely with epoch duration).

Naatanen (1970) compared the effect of ordinary rectangular foreperiod distributions with that of non-ageing foreperiods in an auditory SRT task. Three different foreperiod ranges were used and there were three possible foreperiods in each range. A value of 0.333 was used for the 'p' parameter in the generating process for the non-ageing foreperiods and a proportion of catch trials was included so that the longest foreperiod had the appropriate probability of occurrence. As expected, the results for the conventional conditions showed RT decreasing with increasing foreperiod length. The opposite tendency was observed with the non-ageing foreperiods. This was obviously due to the effect of increasing subjective time uncertainty becoming apparent when conditional probability was held constant. It was also observed that RTs were slower for all foreperiods in the non-ageing condition than for the corresponding foreperiods in the conventional condition. This was presumably due to the fact that, for all the foreperiods except the shortest, expectancy (as operationally defined) is lower for the non-ageing condition than for the conventional one. A later experiment by Naatanen (1971) also used audititory SRT. However, the task did not include a warning signal per se - each signal served as a warning for the next. Naatanen used four different series of ISIs. All were non-ageing, with epochs of 1 s and had expected values of 5, 10, 20 and 40 s. The results showed that mean RT increased between the series as 'p' decreased in value. Unlike the previous experiment, there was no clear tendency for RT to increase with increasing foreperiod length within a series (i.e. with 'p' held constant).

Some experiments have combined the countdown technique and the non-ageing foreperiod. Granjon, Requin, Durup & Reynard (1973) compared the effects of changing ISI duration under two different conditions in an auditory SRT task. The experiment was similar to those reported by Requin & Granjon (1969) and Requin et al (1973), described earlier in this section, except that three distributions of ISIs were used. Each of these consisted of nine values arranged so that the six shortest were conditionally equiprobable, with conditional probabilities of 0.333. The distributions differed in step size (and hence also in range). In general, the slight tendency for RT to increase with ISI when the time marker was not present disappeared, or changed to a slightly decreasing tendency when the time marker was used. The results are thus largely explicable in terms of the concepts already used - namely expectancy and subjective time uncertainty.

Some studies have been designed to focus specifically on the time course of preparation. Alegria (1974) employed another technique to control for subjective time uncertainty in two such experiments. Like Gottsdanker's transit-signal method, this provided continuous information on the flow of time. It employed a spot traversing an oscilloscope screen for this purpose. It was used in an auditory two-choice task with key-press responses. In the first experiment, the stimulus could occur at either of two points indicated on a scale superimposed on the oscilloscope screen. The temporal separation between these points was varied between blocks of trials in five steps, ranging from 150 ms to 900 ms. The experiment had two different conditions. In the control condition, the experimenter informed the subject on each trial at which of the two possible instants the signal would occur. In the experimental condition, the stimulus arrived in an unpredictable manner at either interval with a constant conditional probability of 0.5. (This produced catch trials with a probability of 0.25). In the control condition, RT did not depend on the magnitude of the time interval, presumably because subjective time uncertainty was rendered negligible by the oscilloscope display. In the experimental condition, on the other hand, RT to the signals occurring at the second possible instant showed a significant quadratic trend, reaching a maximum when the size of the interval was between 250 and 400 ms. In addition, the RTs tended to be somewhat slower than those made in response to the signals occurring at the first instant and also slower than those made in the control conditions. Alegria attributed these findings to a decrement in preparation after a peak at the first possible instant of stimulus occurrence, followed by a recovery. Thus he concluded that, at least for the parameters used in his experiment, preparation takes at least 250 ms to dissipate and a rather longer time (at least 500 ms) to recover after this.

The second experiment was carried out to test whether these findings were dependent upon the predictability of the interval separating the possible instants at which the signal could occur. There were four of these, ranging between 300 ms and 700 ms after the start of the trial. The probability of the stimulus arriving at the first of these instants was 0.7 and the remaining probabilities were all 0.1. The same type of control condition was used as in the first experiment. The results showed that RT again reached a maximum 250 ms after the first unused peak of preparation and grew shorter as the stimulus occurred later, thus confirming the results of the first experiment.

Gottsdanker (1975) also conducted experiments on the time course of preparation, using his transit signal method (described earlier). In one experiment (specifically concerned with the attaining of preparation), he used radial transit _lines_ (rather than sectors) on the disc. These were arranged so that one passed the reference point every 2 s. On a proportion of these transits (occurring with a probability of 0.071) an auditory stimulus occurred which required a response. On another 1/8 of the transits, a warning light flashed in advance, informing subjects that the probability of a stimulus occurring on _that_ transit would be 0.5. The lead of the light was varied in five steps from 200 ms to 1200 ms. The results showed that the presence of the light cue resulted in shorter RTs at all lead times. However, a lead time of approximately 300 ms appeared to be necessary to obtain maximum benefit from the warning light.

A second experiment, reported in the same paper, was concerned with the _maintenance_ of preparation, rather than its development. For this experiment, Gottsdanker used two transit lines on the disc. The auditory signal occurred at the first transit with a probability of 0.5. If the signal did not occur at this instant, it occurred at the second transit with a probability of 0.5. On another 0.125 of the trials, the signal was given as a probe mid-way between the transits. There were six inter-transit intervals, ranging from 200 ms to 3200 ms. RTs to signals occurring at the transits were approximately 150 ms. RTs to the probe signals, on the other hand, showed an increase as the inter-transit interval was increased. However, this increase was only really apparent for inter-transit intervals of 800 ms and over. In other words, it appeared that preparation was maintained for some 300 ms after the first transit before it began to decline. Another experiment used the transit-signal analogue of the non-ageing foreperiod in an attempt to examine whether the subject could maintain preparation for longer than this if there were some utility in doing so. Trials began with a probability of 0.5 that the signal would occur when the lead line made its transit. If the auditory signal did not occur at this point, the probability was again 0.5 that the signal would occur on the next transit, 400 ms later. This process continued until finally the signal did occur, ending the trial. Another condition used the same parameters but no transit lines (except the lead line). The resulting RTs tended to be slightly slower without the transit lines (presumably because of increased subjective time uncertainty). There was a significant tendency for RT to increase with lateness of signal, although this increase was small in magnitude for both conditions (between 12 ms and 36 ms for the two longest delays of 2.4 s and 2.88 s). Thus it seems as if subjects have some ability to sustain preparation (if there is a need to do so) but there does seem to be a tendency for it to "leak away" with the passage of time. However, Gottsdanker did state in his report that his average figures hid the fact that some of his subjects showed no increase in RT over the time interval studied. It is interesting to speculate what increases might have been obtained had the parameters been changed to lengthen the course of the trials.

Another method of manipulating expectancy in SRT tasks is to vary the probability of stimulus occurrence by using catch trials. Drazin (1961) used a visual SRT task, with an auditory warning signal. For three different foreperiod ranges, RT was found to increase as stimulus probability was reduced from 1.0 to 0.5. Naatanen (1972) varied both regular foreperiod and stimulus probability in an auditory SRT task and found that, although the expected foreperiod and stimulus probability effects were present, there was no interaction between the two. Buckoltz & Wilberg (1975) obtained similar results with a visual SRT task. Other studies have examined the nature of the function relating SRT to imperative stimulus probability. Gordon (1967) varied stimulus probability in nine equal steps from 0.1 to 0.9 in an auditory SRT task with a visual warning signal. The foreperiod was a constant 750 ms. He obtained a good fit between RT and stimulus probability using an power ~~exponential decay~~ curve. (It should be noted, however, that the function suffers from the rather disturbing theoretical disadvantage that RT tends to infinity as stimulus probability tends to zero!). Naatanen & Koskinen (1975) looked at the effect of very low imperative stimulus probabilities in a similar task with a visual warning signal. They used probabilities as low as 0.004 and the foreperiod was a constant 1 s. Unlike Gordon, they found that a quadratic function fitted better than the power ~~exponential~~ type, although the fit appeared to be rather poor for the lower probability values. Exactly which function should be used to describe the relationship between RT and stimulus probability is a difficult point. If we accept the idea that imperative stimulus probability can act in this type of task in much the same way as conditional probability in a task with an irregular foreperiod, then it seems reasonable to use it as an operational definition of expectancy. However, it is questionable whether _very_ low imperative stimulus probabilities mirror the _real_ level of expectancy, or whether the subject may be rather more ready for the signal than the probability figure would lead us to believe. In everyday life, _totally_ unexpected events can sometimes produce very long RTs of several seconds but it is difficult to arrive at reasonable instantaneous probability figures for these rare events.

Similar results with catch trials have also been obtained by including them in choice reaction tasks. Alegria (1978) used a visual two-choice task with two conditions, differing in catch trial probability. As before, RT was inversely related to catch trial probability. Also, the study revealed certain sequential effects involving catch trials. RTs were longer following catch trials than following correct responses and errors were more likely. Furthermore, the effect was shown to depend on the nature of more than just the immediately preceding stimulus. RTs tended to be very much slower when preceded by a string of catch trials and to be as fast as in a control task, when preceded by a string of conventional stimuli.

Occurrence uncertainty can also be manipulated in CRT tasks by using selective response techniques (i.e. requiring the subject to respond only to certain specified stimuli). Brebner & Gordon (1962) used a task that required a vocal response to just one of a set of equiprobable visually presented numerals. They found that RT increased as ensemble size increased. In later experiments (Brebner & Gordon, 1964a, 1964b) they varied independently ensemble size, relevant stimulus probability and signal rate. The results showed that RT increased as ensemble size increased (with positive stimulus probability held constant) and decreased as relevant stimulus probability increased (with signal rate held constant). On the other hand, two experiments reported by Gordon (1970) presented a rather different picture. In both experiments, the task involved making a key-press response to just one of a number of coloured lights used as stimuli. There was an auditory warning signal of 600 ms. The results showed that increasing ensemble size while holding relevant stimulus probability constant did <u>not</u> lead to an increase in RT. Also, the experiments showed that if the trials on which a response was not required were presented as catch trials (i.e. with a warning signal but no stimulus at all, rather than an irrelevant one) then this led to a reduction in overall RT. Gordon did not put forward any explanation for the disparity between these results and those from his earlier work with Brebner but it seems likely that the latter finding was due to the subject being able to set a lower decision threshold for making a positive response when the possibility of irrelevant stimuli was absent.

To summarise, it seems that high levels of preparation are aversive (Gottsdanker, 1975) and that they are avoided whenever possible. According to Gottsdanker (1975), about 300 ms is needed to build up preparation to a maximum level. Results obtained by Alegria (1974) suggested that it took rather longer (about 500 ms) to attain a second peak of preparation if the first were unused. However, it is not clear whether the fact that Alegria used a two-choice task rather than one requiring SRT was in any way responsible for the difference. Gottsdanker's results also suggested that high levels of preparation appear to be maintained for a short time (about 400 ms) and showed a tendency to decline gradually after this, in spite of the subject's efforts to prevent this from happening. Naatanen & Merisalo (1977) conjecture that preparation involves an increased readiness to respond, held in check by enhanced inhibitory tendencies. If this motor readiness is increased to too high a level, it breaks through into overt motor action.

2.2.4 Selective And Temporal Aspects Of Preparation

Earlier parts of this thesis have dealt with two different aspects of preparation - temporal and selective. To what extent are these related? Holender & Bertelson (1975) reported some experiments on this topic. They all used two-choice tasks (key-press responses to "Nixie" lights) with two constant foreperiods of 500 ms and 5s. The first employed equiprobable stimuli and an incentive technique in which the subject was rewarded more for fast correct responses to one stimulus than to the other. Both the expected incentive and foreperiod effects were present in the data but there was no interaction. The second experiment required the subject to predict the stimulus about to occur. Both foreperiod and prediction effects were obtained and the prediction effect was larger at the shorter foreperiod, though not significantly so. The final experiment used unequal stimulus frequencies. (A frequency ratio of 3:1 was actually employed). As expected, the stimulus frequency effect was present and so was the foreperiod effect but there was no interaction. The overall conclusion was that temporal and selective preparation involve separate processes.

On the other hand, an earlier experiment by Bertelson & Barzeele (1965) which was of similar type to the final one just described (except that the stimulus frequency ratio was 4:1) did find the relative frequency effect to be larger for the shorter foreperiod. Holender & Bertelson were aware of this discrepancy between this finding and their own results but were unable to account for it.

A related topic concerns the extent to which temporal aspects of preparation influence the effect due to number of choices. Bertelson & Boons (1960) investigated this matter with an experiment which used both simple and two-choice tasks. Both tasks used visually presented numerical stimuli ("Nixie" lights) and both were used with two different conditions - one with a short, constant foreperiod of 500 ms and the other with an irregular foreperiod, drawn from a uniform distribution ranging from 250 ms to 5.5 s. As expected, the irregular foreperiod produced longer RTs for both tasks than the regular foreperiod. It was claimed that this effect was larger for the SRT task than for the other. However, examination of the data does not support this observation. On the other hand, Simon & Slaviero (1975) ran a similar experiment which used countdown information provided by a series of six lights. The foreperiod was 2 s and constant. The presence of countdown information was found to improve RT more for the two-choice task than for the simple one.

Other experiments were conducted along similar lines. Broadbent & Gregory (1965) used two- and four- choice tasks (requiring key-press responses to vibrotactile stimuli) with and without an oral warning signal, given by the experimenter. They found no interaction between the presence of the warning signal and the number of choices. Similarly, Alegria & Bertelson (1970) failed to find any interaction between number of choices (which was varied from two to eight) and foreperiod length.

On balance, the results of these experiments seem to suggest that selective and temporal preparation involves separate processes. However, the lack of agreement is puzzling.

2.3 PREDICTION EFFECTS

2.3.1 Introduction

Another aspect of preparation is concerned with the selectivity of preparation. If the subject can direct his preparation towards the stimulus that he is expecting to occur or towards the response that he is expecting will be required and if, as the result of this selective preparation, some of his RTs are faster than they would otherwise have been, then this has implications for the way in which the effects described in Chapter 1 are interpreted as well as for theories of CRT. A number of studies have required subjects in CRT experiments to actually make a verbal prediction (recorded by the experimenter) of the identity of each stimulus just prior to its occurrence.

2.3.2 Number Of Choices

The well-attested finding that RT increases as the number of equiprobable alternatives is increased could well be partially due to the fact that, as N is increased, the likelihood of the subject being well-prepared for the stimulus that occurs on any given trial is decreased. (It is unlikely that this mechanism could be solely responsible for the entire effect because the function relating RT to N would be the wrong shape. However, it is possible that such a mechanism could be an important factor). Bernstein & Reese (1965) ran an experiment in which the number of equiprobable alternatives was varied using respectively one, two, four and eight conditions. The task used key-press responses to light stimuli. The results revealed the expected linear relationship between RT and N for the overall data and also for the incorrectly predicted stimuli considered separately. On the other hand, the correctly predicted stimuli displayed a quite different picture, showing an increase in RT as the number of choices was increased from one to two but no further increase with further increases in N.

The most promising explanation appears to be to appeal to some sort of preparation theory, in which the subject is ready for a particular stimulus to occur and has pre-selected the corresponding response, ready to make it if that stimulus does indeed occur. Thus a response can be made more quickly to this stimulus than to any of the others. Such a hypothesis would also account for the lack of dependence of RT on N when predictions are correct. That RT for the correctly predicted stimuli increases as N is increased from one to two is possibly due to the fact that, when N is greater than one, the subject has an extra decision to make - namely whether or not the stimulus that has just occurred was the predicted one. A later experiment (Bernstein, Schurman & Forester, 1967) used a number of tasks with a variety of S-R mappings. Each involved key-press responses to light stimuli. One, two, four or eight equiprobable stimuli were mapped to one, two, four or eight equiprobable responses, with the constraint that there were never more responses than stimuli. Unfortunately, the actual RTs for correctly and incorrectly predicted stimuli were not quoted - instead the authors presented the results on prediction outcome as the <u>facilitation</u> (in ms) obtained, for each condition, by having a correct prediction outcome. However, the general result seemed to be that the magnitude of this facilitation was independent of the number of stimuli (when the number of responses was held constant) but increased with the number of responses (when the number of stimuli was held constant).

2.3.3 <u>Stimulus And Response Probability</u>

A number of studies have looked at prediction effects in CRT tasks with unequal probabilities. Hinrichs (1970) used a two-choice task with visually presented digits as stimuli and key-press responses. The probability ratio was 2:1. As expected, RTs to correctly predicted stimuli were faster than those to incorrectly predicted ones. More important, however, was the fact that the stimulus probability effect (which was present in the unpartitioned data) was virtually absent in the data for correct and incorrect partitions considered separately. A later experiment by Hinrichs & Craft (1971a) used a similar task, but with five different conditions in which the probability ratio for the stimuli ranged from equiprobable to 9:1. Each of these conditions was run in two modes - one requiring prediction by the subject and one not. RTs to both low- and high-frequency stimuli were found to be substantially slower when prediction was required. (Possibly, the process of verbal prediction added an information processing load which retarded the following RT. However, Geller (1975) found that the prediction process facilitated RT). As usual, the stimulus probability effect was found in the condition not requiring prediction and also in the unpartitioned data from the prediction condition. In the latter condition, prediction correctness showed the expected effect for all stimulus frequencies. The probability effect was greater for incorrectly predicted stimuli, although not entirely absent for the correctly predicted ones (particularly when the larger probability ratios were considered).

Thus there are grounds for thinking that the mechanism that was tentatively proposed in the previous section is applicable here, too. As the probability of occurrence of a stimulus is increased, so the subject is more likely to predict that it will occur and that prediction becomes increasingly likely to be fulfilled. Rather puzzling, though, is the fact that the study by Hinrichs & Craft revealed a tendency for the probability effect to manifest itself in the data from the <u>correct</u> predictions, particularly when the probability ratio was high. A similar tendency was observed in a two-choice experiment reported by Geller, Whitman & Post (1973) which used a probability ratio of 7:3. However, de Klerk & Eerland (1973) did not obtain this effect in a similar task with a probability ratio of 4:1. When this tendency does occur, it could be due to the pre-selection effect not operating with perfect efficiency, or it could be due to the subject not preparing for the predicted stimulus on some of the trials.

Some studies have used simple condensation tasks with three stimuli mapped to two responses in an attempt to establish whether the facilitating effects of correct prediction are stimulus- or response-based. Hinrichs & Krainz (1970) used equiprobable stimuli - two mapped to one of the responses and the remaining one to the other. (Illuminated digits were used as the stimuli and key-press responses were required). The results clearly indicated that prediction was stimulus-based. Although the usual facilitation was present when the stimulus was correctly predicted, no such effect was observed when the stimulus that was predicted did not occur but shared the same response as the one that did. A later experiment of similar type (Hinrichs & Craft, 1971b) used a variety of different stimulus probabilities and obtained essentially the same result with each arrangement. Another experiment by Whitman & Geller (1972a) used illuminated symbols as stimuli, mapped to two equiprobable manual responses. The two stimuli that shared one of the responses each had probabilities of occurrence of 0.25. As in the other experiments, a stimulus effect was clearly evident. There was no indication of a response effect, except under one particular set of conditions - namely, when a stimulus had been incorrectly predicted but the other stimulus mapped to the same response had occurred <u>and</u> this state of affairs <u>had also occurred</u> on the preceding trial.

Similar results are apparent in an experiment reported by LaBerge, Tweedy & Ricker (1967) in which incentive, rather than stimulus frequency inbalance, was used to influence the subject's preparation strategy. They used a condensation task with three coloured lights mapped to two key-press responses. The green light (which occurred with a probability of 0.5) was assigned to one of the responses, while the other two lights (respectively red and blue in colour) each occurred with a probability of 0.25 and were both assigned to the other response. An incentive points system was used to encourage some subjects to respond rapidly to the red light and others to the blue. The results showed that the emphasised colour was responded to considerably faster than the other one sharing the same response, indicating that the selectivity was operating at a perceptual level. Another method of directing the subject's preparation is to employ a cueing technique, in which the stimulus is preceded by a signal giving information about its probable identity. LaBerge, Van Gelder and Yellott (1970) ran an experiment which utilised this technique. Four conditions were used, differing in predictability value of the cue. The results indicated that, for those tasks on which the cue was correct, RT was an inverse function of the predictability value of the cue.

A more complicated experiment was reported by Hacker & Hinrichs (1974). It used four equiprobable visually-presented alphabetic stimuli mapped to two equiprobable key-press responses. Two conditions were employed. One required the usual prediction concerning the stimulus that was expected to occur next, while the other condition required <u>two</u> such predictions - for the most likely and second most likely stimuli. As before, the results showed no response effect. The stimulus effect was present, with single, first and second correct predictions being faster than incorrect predictions. ~~incorrect=predictions.~~ Also, first correct predictions were found to be faster than second correct predictions. The authors interpreted the results as evidence for a serial, self-terminating memory scanning model of CRT. Certainly, these results suggest that response selection may be more than a two-state process and hence cast some doubt on the pre-selection model of response facilitation. However, the pre-selection model could still account for the results if the notion of a dynamic equilibrium between different states is allowed, i.e. if the subject's expected stimulus does not actually remain fixed during the interval between prediction and the occurrence of the stimulus event but oscillates between two or more different stimuli. Presumably, the longer the interval between prediction and stimulus, the more likely this is to occur and the less likely it is that the stimulus actually prepared for is the one that was predicted. This idea is supported by one of the experiments reported by Holender & Bertelson (1975) who found that the prediction effect decreased as the interval between prediction and stimulus increased.

Finally, it is worth considering an experiment reported by Hannes (1971). This used a two-choice task and required manual responses to light stimuli. Repetition probability was manipulated between experimental conditions and it was found that, for both correct and incorrect guesses considered separately, RT for both repetitions and alternations decreased with increasing probability of occurrence for the partition concerned. However, this effect was more pronounced for the repetition data than for the alternations.

The effect of transition probability on response latency is rather difficult to explain under the pre-selection model. However, it could be assumed that the values of the transition probabilities would have an effect on the <u>confidence</u> of predictions and it is just feasible that this intervening variable could have an effect on the probability that the predicted stimulus would, in fact, be the one prepared for. The fact that repetitions were faster than alternations when frequent, but slower when infrequent, does not seem to be as easily accounted for.

2.3.4 S-R Compatibility And Prediction

Keele (1969) ran a four-choice experiment (using lights and keys) in which the subjects were required to predict what the next stimulus would be. Two levels of compatibility were used - one involving a direct spatial correspondence between stimuli and responses and the other using a less compatible arrangement. The usual facilitating effect of correct predictions was found but this effect was greater for the low compatibility condition. This, of course, is what would be expected if the pre-selection theory were true. Under conditions of low compatibility, selecting the appropriate response for a given stimulus takes longer than when the S-R correspondence is direct. If a given stimulus is predicted and the corresponding response pre-selected, it is obvious that the processing time saved will be greater in the former case.

Other experiments have produced essentially the same result. Whitman & Geller (1971b) ran a two-choice experiment with unequal stimulus probabilities, which required manual responses to symbolic stimuli. Two levels of compatibility were used - one with a direct spatial correspondence between stimuli and responses and the other with a transposed arrangement (i.e. the LH stimulus mapped to the RH response and vice versa). As before, the facilitating effects of correct prediction were greater for the less compatible condition. A two-choice experiment by Craft & Hinrichs (1975) of similar type (except that the stimuli were equiprobable) did not show a significant interaction between compatibility and prediction outcome but there was a non-significant tendency for the results to show the same pattern, with prediction facilitation greater for the less compatible condition.

It is worth noting, however, that all these results still showed a substantial compatibility effect for the correctly predicted stimuli considered alone. A possible explanation for this was suggested in the previous section - namely that the pre-selection effect may not have been operating with perfect efficiency.

An earlier study by Broadbent & Gregory (1962b) provides some further evidence for response pre-selection. This involved making comparisons between choice and selection tasks (i.e. Donders' 'b' and 'c' reactions). Two types of task were used - one using manual responses to vibrotactile stimuli and the other requiring verbal responses to aurally presented words. Both types of task had two stimuli. It was found that 'c' reactions were faster than 'b' reactions only when the S-R mapping was incompatible. This seems to be because response pre-selection confers a greater advantage in the case of the 'c' reaction than it does with the 'b' reaction. This is due to the fact that, in the former case, the pre-selected response is never wrong; if it is not required, it is merely withheld. With the 'b' reaction, on the other hand, if the wrong response is pre-selected, it is not unreasonable to suppose that this would lead to a longer RT than if no pre-selection had taken place. However, this fact only confers a tangible advantage when the response selection time is long, which is only the case when the S-R mapping is incompatible.

2.3.5 Sequential Effects Of Prediction Outcome

Some work has been done on sequential effects of prediction outcome. Whitman & Geller (1971a) ran a two-choice experiment with unequal stimulus probabilities, using manual responses to symbolic stimuli. The results showed that RTs were significantly faster when the prediction outcome of the preceding trial was correct than when it was incorrect. Another experiment of similar type (Whitman & Geller, 1971b) found a similar effect - but only when an S-R mapping of low compatibility was used. A direct mapping produced no such effects. (The previous experiment had used a mapping of intermediate difficulty).

A more complicated experiment was conducted by Whitman & Geller (1972b), using a two-choice task which required manual responses to illuminated symbolic stimuli. The S-R mapping was of intermediate difficulty and there was a frequency inbalance in the stimuli such that the high-frequency one occurred with a probability of 0.7. The usual stimulus probability and prediction effects were present and also, there was an interaction between preceding prediction outcome and current prediction outcome such that when the latter was correct, a correct outcome on the former had a facilitating effect. It seems that subjects are more confident in their predictions if the preceding prediction was correct. This idea is explored more fully in the next section.

2.3.6 Other Aspects Of Prediction Outcome

Two studies have been reported which have actually manipulated the probability of the subject's predictions being correct by making the stimulus partially dependent upon the preceding prediction. Whitman & Geller (1973) utilised this technique in a two-choice task with equiprobable symbolic stimuli and manual responses. Five different levels of prediction correctness were used (with probabilities ranging from 0.1 to 0.9). The results indicated that RT to correctly predicted stimuli was a monotonic decreasing function of the probability of a correct prediction, whereas RT to incorrectly predicted stimuli was not consistently influenced by this parameter. A later experiment of similar type (Geller, 1974) used just three probabilities of prediction correctness (0.3, 0.5 and 0.7) and an S-R mapping of intermediate difficulty and obtained essentially the same result. In addition, the RTs to correct predictions on the current trial were found to be faster when the preceding prediction outcome was also correct, similar to the experiment reported by Whitman & Geller (1972b) that was described in the previous section.

Geller (1974) pointed out that an expectancy model which is compatible with these findings seems to require two processes - one to account for variations in RT facilitation following correct predictions and another to explain the inhibition of RT following incorrect predictions. The pre-selection model is still a possible candidate if the modification tentatively proposed in Section 3.3 is borne in mind. Increasing the success of predictions could well increase their stability. One way of indirectly testing this idea is to examine the level of confidence that subjects have in their predictions and to see how this affects RT. This was done by Geller & Whitman (1973) with a two-choice task in which the subjects were required to make a stimulus prediction and a confidence judgement on each trial. The results showed RT to correctly predicted stimuli to be an inverse function of the level of confidence in the prediction, thus providing some support for the notion of differential stability of pre-selected responses.

2.4 ERRORS AND SPEED-ACCURACY TRADEOFF

2.4.1 The Nature Of Errors

The study of errors in CRT tasks is of value because it offers the possibility of yielding some degree of insight into the workings of the choice reaction process. Rabbitt (1966a) reported two experiments which used manual responses to light stimuli. The first experiment used two conditions - one with four equiprobable alternatives and the other with ten. The stimulus sequences were programmed to exclude repetitions and the RSI was 20 ms or less. An unusual feature was that an incorrect response caused the stimulus presentation to halt until the correct response had been made, whereupon the presentation was resumed. The results showed that errors were faster than correct responses. Rabbitt also reported that error-correction responses were faster than correct ones. The second experiment used a similar ten-choice task to examine more closely the types of errors made. Rabbitt distinguished two types of error: adjacent errors (in which the subject had pressed that part of the response grid immediately adjacent to the correct part) and non-adjacent errors. The former were attributed to aiming errors whereas the latter appeared to be due to various types of double response, where a second response was recorded by the apparatus, on either the same part of the response grid as the first response, or else on the part adjacent to it. A later experiment (Rabbitt, 1968a) used a task with eight stimuli mapped to four responses that did not signal in any way to the subject when he had made an error. The subject was nevertheless required to correct his errors by making the response that he should have made, followed by a pause of a few seconds. The results showed that subjects were quite able to carry out this task, i.e. even in the absence of any external indication they knew when they had committed errors and what the responses should have been. Another study (Rabbitt, 1966b) showed that the latency of error correction was independent of the nature of the following stimulus, giving further support to the idea that error correction responses are not governed by external stimuli.

Two further experiments by Rabbitt (1967) were concerned with error detection. The first used a condensation task in which the visually displayed digits 1-8 were mapped onto four manual responses. As before, the RSI was very short (i.e. between 15 ms and 20 ms). Subjects were required to signal the detection of errors that they had just committed by pressing a further pair of response keys. Error detection responses were timed from the preceding (incorrect) response and were found to be significantly faster than correct responses. The second experiment employed six conditions. Two of them used eight neon lights as stimuli, mapped to two manual responses. (One used a compatible mapping and the other used an incompatible one). The remaining four conditions used visually displayed digits as stimuli and manual responses. (Three of these conditions had eight stimuli mapped to two, four and eight stimuli, respectively. The remaining condition used four stimuli and four responses, with a 1:1 mapping). As far as the correct responses were concerned, the usual compatibility effect was found. Also, RT was found to increase as the number of responses was increased, with the number of stimuli held constant at eight. In addition, RT increased when the number of stimuli was increased, with the number of responses held constant at four. The results for the error-detection times for the various conditions were puzzling. Early in the experiment, the effects were similar to those for the correct RTs. After more practice, however, the compatibility effect appeared to reverse and the other effects which had been present earlier, disappeared. Rabbitt was unable to draw any firm conclusions about error-detection from these results.

A later experiment by Rabbitt (1968a) compared various types of error-detection response. A condensation task was used in which eight equiprobable, visually presented digits were mapped to two equiprobable manual responses. Three groups of subjects were each assigned to a different condition. Members of the first group were required to respond to their errors by making an error-correction response. Those in the second group signalled recognition of errors by pressing a third key, which was not one of the standard response set. Members of the third group were required to respond to their errors by pressing a specified key which was also one of the two standard responses. Subjects in all three groups were instructed to pause for a few seconds after making an error-detection or error-correction response. The results showed that error-correcting responses were significantly faster than those error-detection responses that used a separate response key. These, in turn, were faster than error-detection responses made by the third group. (In this latter category, error-detection responses that were the equivalent of error-correction responses were faster than those that were not). Only the genuine error-correction RTs were faster than the correct RTs. Rabbitt considered many possible explanations of the data but the most promising seems to be the idea that the subject continues processing perceptual evidence after the moment at which a response is launched. If the response is incorrect, the subject is likely to realise the fact shortly afterwards and hence is able to make an error-correction response very soon after. Neutral error-detection responses take longer, because the subject is having to switch to performing another type of task. Error-detection responses which require a standard response are presumably inhibited by confusion with the standard task.

Returning to the topic of error-correction, Rabbitt & Phillips (1967) reported an experiment which used a condensation task with ten equiprobable, visually displayed numerals mapped to two equiprobable manual responses. Two conditions were used - a compatible one with a straightforward S-R mapping and a less compatible one with a crossover mapping. The results showed that, as before, error-correction responses were faster than their correct counterparts. Also, although the compatibility effect was larger for the error-detection responses than for the correct ones at the beginning of the experiment, the position had reversed after practice. Once again, the results defied adequate explanation.

It appears that the tendency to correct errors is an important feature of conventional tasks (i.e. those where the subject is not supposed to make error-correction responses). Hale (1968) ran an experiment in which key-press responses were required to visually presented numerals. The RSI was 100 ms. Three different conditions were employed, using two-, four- and eight-choice tasks respectively. Error RTs were found to be faster than corresponding correct RTs and varied in the same way as the correct RTs with different numbers of alternatives and degrees of practice. A large proportion of the errors which immediately followed other errors were attempts at "illegal" error-correction. Burns (1971) reported results from an eight-choice task which used key-press responses to light stimuli. A mixture of two different RSIs was employed (150 ms and 820 ms) and two levels of S-R compatibility were used - namely a spatially direct correspondence and the mirror image of this. An auditory signal was used to provide error feedback information to the subject. The results indicated that post-error responses showed a clear difference in RT between the two different RSIs, with the shorter RSI producing the longer RT. This difference was larger for the less compatible S-R mapping. Burns attributed the effect to an error-contingent extended psychological refractory period. However, such an explanation does not seem to account for the illegal error-corrections found by Hale.

A later experiment, reported by Rabbitt & Rodgers (1977) examined the "illegal" error-correction process in greater detail, by focusing on post-error behaviour. The experiment was similar to that reported by Hale (1968), except that a mixture of two different RSIs (20 ms and 200 ms) was employed. In all conditions, errors were found to follow one another more frequently than expected by chance. Also more involuntary error-correction responses appeared than expected by chance. When an error was immediately followed by a repetition of the same signal, subjects were able to respond more accurately and quickly than to any other signal. It seems to be the case that when subjects make errors, they are predisposed to make error-correction responses and hence have this response pre-selected. If it is the one required, then it can be made quickly; if not, other types of response are delayed because, in order to produce them, subjects have first to suppress a tendency to make an error-correction response.

Turning to the nature of errors themselves, there are very few reports of the effect of task variables on error latency. However, Laming (1968) manipulated relative stimulus frequency in a two-choice task and found the usual effect on the latency of correct responses. Error responses showed a similar pattern, but were faster than correct responses by between 50 ms and 75 ms throughout the probability range used. Similarly, Egeth & Smith (1967) found that results for a character recognition task were much the same as regards "yes/no" differences and practice effects for errors as for correct responses, with the exception of being about 50 ms faster. They concluded that errors were generated by incomplete versions of the same processes as those that led to correct responses.

On the other hand, when Remington (1973) re-examined the data from his earlier experiments on sequential effects (Remington, 1969, 1971) he found a complete absence of repetition effects in the error data. These findings suggest a rather different mechanism for error production. Taken together, these differing results are indicative of the heterogeneous nature of errors. They can be produced by more than one mechanism. It is not unreasonable to suppose that any of the constituent stages or processes involved in choice reaction behaviour may go wrong and lead to an erroneous response and also that errors made at different stages may well have different properties. Thus Briggs & Shinar (1972) found that in the Sternberg task (Sternberg, 1966) visual noise level interacted with accuracy. They suggested that, for this experiment at least, S-A tradeoff was located at the stimulus encoding stage of processing. However, the higher error rates typically found in tasks of low S-R compatibility (e.g. Hawkins & Underhill, 1971) suggests that, in these cases, errors are occurring at the response selection stage.

Another important finding concerning errors is the fact that errors are more likely to occur when an unlikely stimulus is presented. Laming (1968) found this to be the case when he manipulated stimulus probability in a two-choice task and Kornblum (1969a) reported similar results from the manipulations of sequential dependencies in a four-choice task. This is presumably indicative of some form of preparatory bias in favour of the more likely stimulus or response or S-R link at the expense of an increased likelihood of committing an error if the less probable stimulus should occur.

2.4.2 Approaches To Speed-Accuracy Tradeoff

The fact that a subject can trade off accuracy against speed in a wide range of information processing tasks has been recognised for a long time, both at an everyday level and at a scientific one. Reports of experiments on the topic even date back to the last century with the work of Woodworth (1899) on S-A tradeoff in movement responses. However, as far as CRT research is concerned, it did not really feature in experiments until after 1960, except for the use of a speeded RT task by Hick (1952) to examine the effect of speed on transmitted information.

It seems that the more recent interest in S-A tradeoff in CRT tasks began with Howell & Kreidler (1963) who used a ten-choice task with lights as stimuli and key-press responses. The manipulation of interest was the instructional set. Four types of instruction were used. The first of these emphasised speed at the expense of accuracy and the second emphasised accuracy at the expense of speed. The third set emphasised both speed and accuracy and the final set required subjects to respond so as to attempt to maximise transmitted information. Behaviour under conflicting instructions (i.e. the third and fourth set) was found to correspond more closely to that for accuracy than for speed, with an error rate of about 4% (as opposed to 2% for the accuracy condition). Under instructions emphasising speed, the error rate was between 10% and 13%. RT was, of course, shortest in this condition (about 540 ms) and longest in the accuracy condition (approximately 585 ms). A somewhat similar 15-choice task was used by Fitts (1966) in a broadly similar experiment. Different payoff schedules were used in order to put the emphasis on speed or accuracy. The results supported the idea of an S-A tradeoff and confirmed that subjects could be induced, by appropriate payoff schemes, to focus on speed at the expense of accuracy, or vice versa.

Hale (1969b) reported two experiments which used a serial three-choice task (involving manual responses to visually-presented stimuli) in which the RSI was zero. Under speed instructions, both correct and incorrect RTs were found to be faster than when under instructions emphasising accuracy. In addition, more errors were committed under the speed instructions, as expected.

The relationship between speed and accuracy for a particular CRT task may be empirically derived by employing a number of conditions with different degrees of emphasis on speed and accuracy and plotting accuracy (expressed in terms of the percentage of correct responses) against RT. The resulting curve is normally found to be S-shaped, broadly speaking. However, the central and lower parts are usually essentially linear, with the upper part being a monotonically increasing, but negatively accelerated curve. At the extreme lower end of the curve, it flattens out rather abruptly and becomes horizontal at chance levels of accuracy, for the obvious reason that any further increase in speed will not result in any further decrement in accuracy.

Pachella (1974) made a number of points about this type of S-A function. Firstly, errorless performance is rarely, if ever, achieved. Pachella attributed this to the fact that subjects are always under <u>some</u> degree of speed stress (otherwise the very measure of RT is meaningless) and consequently respond rather faster than they should to achieve perfect accuracy. (Another possibility is that perfect accuracy may be an asymptote to the S-A curve, i.e. the curve may approach it but never reach it, no matter how long the RT). Pachella's second point is that, while it is not a matter of much importance that subjects do not operate at a level of perfect accuracy, it is of great concern that differences in the S-A criterion may be correlated with experimental conditions and hence may contribute to false findings. Thirdly, because of the shape of the S-A curve at high levels of accuracy, small differences in error rate can correspond to large differences in RT. The second and third points, taken in combination, mean that small differences in error rates between conditions, due to nothing more than small shifts in the subject's performance criterion on the S-small curve, could be correlated with quite substantial differences in correct RTs which could, in turn, be misconstrued as being due to manipulations of the independent variable.

Statistical techniques for dealing with the problem of differential error rates were also discussed by Pachella. Firstly, it is possible to apply analysis of covariance to the RT data by using error rate as the covariate. Unfortunately, one of the assumptions on which this technique is based is that of a linear relation between the dependent variable and the covariate. As explained earlier, this is conspicuously absent at high levels of accuracy on the S-A curve. However, it is possible to apply a suitable transformation to the data which makes the S-A relation more nearly linear. One possible candidate for such a procedure is the so-called "log-odds" transformation, i.e. the logarithm of the accuracy odds:

log(p(correct)/p(error)).

Another possible statistical solution to the problem involves the use of multivariate analysis of variance, treating RT and the accuracy measure as a bivariate dependent variable. Actually, this technique is also based on an assumption of a linear relation between RT and accuracy, because the bivariate population from which the sample is assumed to have been drawn has the Pearson product moment correlation coefficient as a parameter. Of course, it would be possible to apply the "log-odds" transformation prior to using the technique in the same way as just described.

A different approach to the matter is to use experimental techniques to deal with the problem and possibly to use other measures which attempt to tap some invariant aspect of the S-A curve, so that the values of these new parameters from different experimental conditions may be compared. The general experimental procedure involves inducing subjects to work at different points of the S-A continuum for each of the experimental conditions, so that what Wood & Jennings (1976) describe as a SATF (speed-accuracy tradeoff function) can be obtained for each condition.

Both Pachella (1974) and Wickelgren (1977) have outlined various ways of doing this. The simplest way (although probably the least effective as far as getting a good "spread" of performance is concerned) is simply to use different sets of instructions, emphasising speed and accuracy to different extents. Such a method has been used by Hale (1969b), Hick (1952) and Howell & Kreidler (1963). A more effective method is to provide the subject with a deadline on each trial and to instruct him to respond as accurately as possible whilst ensuring that his responses beat the deadline. By providing feedback and varying the deadline between blocks of trials the desired spread of speed (and hence accuracy) can be achieved. The technique has been used for absolute judgement tasks by Pachella & Fisher (1969, 1972) and Pachella, Fisher & Karsh (1968). It has also been used to examine the effect of alcohol on CRT (Jennings, Wood & Lawrence, 1976). However, whether subjects are as accurate as they should be is a matter of some doubt, because there is little incentive (other than the instructional set) to respond accurately. A more sophisticated adaptation of these methods uses a time band rather than a deadline, thus imposing both lower and upper limits on RT. This method has not actually been used to generate a SATF but has been used by Snodgrass, Luce & Galanter (1967) for other purposes.

An approach which appears to be more popular with experimenters is to use some sort of payoff technique in which the subject is rewarded for speed and penalised for errors. By varying the parameters of the payoff function, the differential emphasis on speed or accuracy can be changed. There are two variants of this technique, one using a continuous cost for RT and the other using a payoff matrix in which responses beating some deadline are rewarded for speed and penalised for inaccuracy. The former method has been used for a visual discrimination task (Swensson, 1972a) and also for CRT tasks (Swensson, 1972b, Swensson & Edwards, 1971). The latter method has been used both for stimulus classification tasks (Lyons & Briggs, 1971; Swanson & Briggs, 1969) and for CRT experiments (Fitts, 1966; Ollman, 1966; Pachella & Pew, 1968; Yellott, 1971).

Another technique (known as the forced RT method) is rather different from any of the foregoing. The essence of the technique is that, at some interval after the stimulus has been presented, the subject is given another signal. The subject is instructed to make his response coincide with this signal. By varying the time interval between the main stimulus and this response-synchronising signal, the experimenter is able to generate a SATF. This method was used by Schouten & Bekker (1967) with a two-choice visual CRT task. The auxiliary signal was auditory and actually consisted of three tone pips, each of 20 ms duration, with a delay of 75 ms between them. Subjects were instructed to respond in synchrony with the third pip. This method gives a tight control over RT but appears to suffer from the same deficiency as the pure deadline method (and also the time band technique) in that it has no direct influence over accuracy at all.

A quite different approach involves inducing the subject to work somewhere near the centre of the accuracy range of the S-A continuum (e.g. at an error rate of about 25% for a two-choice task) and then partitioning the resulting responses into groups according to the magnitude of the RT. The error rate for each group is then calculated and the required tradeoff function derived. Wood and Jennings (1976) call this type of tradeoff function the CAF (conditional accuracy function) because it is formally defined as the conditional probability of a response being correct, given that its latency has a particular value. (In practice, the CAF is computed as described above, for obvious reasons). In contrast to the SATF, a CAF need not be derived from several conditions differing in speed and error emphasis but can be computed from the data drawn from a single condition. This technique has been used to investigate the effects of a number of variables on the relationship between speed and accuracy, e.g. stimulus probability (Lappin & Disch, 1972a), stimulus intensity (Lappin & Disch, 1972b), temporal uncertainty (Lappin & Disch, 1973) and both stimulus probability and S-R compatibility (Harm & Lappin, 1973). It has also been used by Rabbitt & Vyas (1970) to examine the nature of errors in CRT and by Schouten & Bekker (1967) with the forced RT method to investigate the behaviour of the CAF itself at different RTs.

Although the SATF and CAF are both empirically derived S-A tradeoff functions, the difference in the way that they are derived is reflected in an important theoretical distinction made by Pachella (1974) concerning the difference between two types of S-A tradeoff - macro-tradeoff and micro-tradeoff. Macro-tradeoff is due to changes in the subject's criterion, i.e. his position on the S-A continuum. Micro-tradeoff, on the other hand, is concerned with small, perhaps random, changes in RT (and associated error rates) for any fixed criterion value. It is the former that is clearly responsible for the gross increases in error rates observed when subjects are forced to reduce their RTs by one of the deadline methods described earlier in this section. However, it is the latter to which CRT theorists are referring when they talk about the relative speed of correct and incorrect responses and the implications that this has for models of CRT. More will be said about this latter aspect of the matter later in the chapter.

Now, it is more than likely that the experimental manipulations used in producing the SATF will actually be successful in altering the subject's S-A criterion and hence that the SATF will, in fact, correspond to the theoretical macro-tradeoff. The CAF, on the other hand, is more problematic. Even if it is derived from a single experimental condition, the experimenter can by no means be sure that no criterion changes have taken place and hence cannot be sure that the CAF will correspond to the micro-tradeoff.

Wood & Jennings (1976) deal with this matter at some length and also with the associated issue of whether a single S-A condition is used or whether multiple conditions are employed. (The latter approach is necessary for obtaining a SATF but either type of experimental design can be used with the CAF, which can be applied to each condition of a multi-condition experiment). The multi-condition design is intended to induce systematic variations in the subject's S-A criterion by experimental manipulations (and also enables the experimenter to check whether these manipulations have had the desired effects on RT and accuracy), while the single condition design has no control over this parameter. Because of this, it seems more likely that the multi-condition design will produce interactions between S-A criterion variations and other aspects of performance than the single condition design.

As regards the two types of tradeoff function, the SATF is more suitable to use in many experiments because, unlike the CAF, the validity of its use does not depend on stringent assumptions. For the SATF it is not necessary to assume that a single criterion is employed within a given S-A condition. It is not even necessary to assume that the within-condition criterion variability is small. However, because the CAF partitions data into categories based on _obtained_ RT, instead of according to some other basis, this makes it dependent upon some much more specific assumptions concerning variation in the subject's S-A criteria. The CAF implicitly assumes that either the CAF is invariant across changes in S-A criteria or that the S-A criterion is constant for all the data comprising the CAF. Now, it is most unlikely that the latter condition is ever satisfied but the former one can be tested simply by comparing the various CAFs derived from a multi-condition experiment.

This was done by Schouten & Bekker (1967) using the forced RT method (described earlier). The authors claimed that the CAFs obtained were well fitted by a single standard curve, indicating an invariance of the CAF over S-A criterion changes. However, as the CAF curves did not overlap and as no attempt was made to produce a _linear_ function by transformation, this claim is difficult to verify. Another test for CAF invariance was made by Jennings, Wood & Lawrence (1976). (The experiment was not actually designed with this application in mind as it was concerned with the effect of graded doses of alcohol on S-A tradeoff in CRT. However, the data from the last practice session for each subject - prior to the first alcohol treatment - served admirably for the purpose). The deadline method (described earlier in this section) was used to manipulate the subject's S-A criteria. Five different deadlines were used (175, 225, 275, 325, 375 ms) and presented in a separate block of trials. The two-choice task used auditory stimuli and manual responses. The deadline was marked by a visual signal. Separate CAFs were computed for each deadline condition for each subject, by dividing the ranked

RTs into five equal-N categories. The results showed clearly that the CAF invariance was not present. For example, for RTs in the range 225 ms to 275 ms, the error rate (averaged across all subjects) ranged from 33% for the condition with the shortest deadline to 6% in the condition with the longest. Thus it seems reasonable to conclude that the CAF is not invariant over changes in the S-A criterion and hence that it does not represent the micro-tradeoff.

2.4.3 Findings Involving Speed And Accuracy

This section ~~is concerned~~ deals with experimental findings concerning the nature of the S-A tradeoff and its interaction with the effects of various independent variables. Dealing first with the nature of the S-A tradeoff in CRT, Swensson & Edwards (1971) used a payoff technique with a continuous cost for time in a visual two-choice task with manual responses. The foreperiod was an irregular one, drawn from a continuous uniform distribution between 1 s and 3 s. The results indicated that subjects tended to either respond accurately or make a detection response to the stimulus. This finding provided some support for the fast guess model, described in detail in Chapter 5. Some later experiments by Swensson (1972a) used a two-choice discrimination task with a similar payoff scheme. Detection responses were again apparent and Swensson attempted to remove these from the data by inspection, before applying various transformations to yield a number of linear tradeoff functions. The results suggested that the slopes of these functions tended to be shallower for more difficult discriminations, indicating a lower rate of information processing. The intercepts of the functions revealed substantial "dead times" before discrimination responses exceeded chance levels of accuracy. At the point where this occurred, the discrimination responses were some 80-100 ms slower than detection responses. Discrimination error RTs tended to be faster than their correct counterparts when time pressure was heavy. When high levels of accuracy were required, however, error RTs tended to be <u>slower</u> than correct RTs, particularly for difficult discriminations. This latter result was also obtained by Wilding (1971a) with a difficult ten-choice discrimination task. Further analysis by Wilding (1971b) showed this effect to be greater for stimulus repetitions and response repetitions than for other types of trial.

Turning to the effects of independent variables on S-A parameters, Lappin and his colleagues have done work in this area by calculating the slope and intercept of the CAF function derived by plotting d' (as a measure of accuracy) against RT. Applying their technique to stimulus probability, Lappin & Disch (1972a) found no difference in either parameter between the two-choice visual CRT tasks - one with equiprobable stimuli and the other with a 7:3 probability ratio. This demonstrated that, although the usual stimulus probability effect was present in the data, this was not due to any difference in rate of information processing between the two tasks. A later experiment by Harm & Lappin (1973) used the same type of task but at two levels of S-R compatibility. The low-compatibility condition used a simple crossover S-R mapping. The results again showed that stimulus probability had no effect on information processing speed (at either level of compatibility). However, S-R compatibility itself affected the slope (but not the intercept) of the LOC (latency operating characteristic). This was due to the fact that, at chance levels of accuracy, RTs were much the same at either level of compatibility; whereas at higher levels of accuracy, the low-compatibility task was slower. The slope was steeper for the more compatible condition, indicating a higher rate of processing, presumably because of the lower response selection times for the more compatible task.

Another visual two-choice experiment (Lappin & Disch, 1972b) dealt with stimulus intensity. Three intensities were used and they differed by equal intervals on a logarithmic scale. Both the slope and intercept of the tradeoff function were found to vary with stimulus intensity. As intensity increased, the slope increased and the intercept decreased. The greatest differences were between the two lower intensities. The increase in slope again indicated a higher rate of processing for the more intense stimuli, whereas the larger intercept for the less intense stimuli presumably indicated the necessity for waiting longer before detection responses could be made. A fourth experiment (Lappin & Disch, 1973) looked at the effect on the tradeoff function of manipulating temporal uncertainty. Again, they used a two-choice task. This was conducted with two groups of subjects using three different foreperiod conditions. For each group, one of the conditions used a fixed foreperiod of 1 s. The other condition employed an irregular foreperiod with two equiprobable values. For one group of subjects, the values were 975 ms and 1.025 s and for the other they were 750 ms and 1.25 s. For each group, mean RT increased with temporal uncertainty. As far as the tradeoff functions were concerned, the slope was found to be greatest for the fixed foreperiod condition and shallowest for the longer of the irregular foreperiods. There was no consistent or meaningful variation between the intercepts. The results are not easily explained, as it might have been expected that high levels of temporal uncertainty should have had a greater effect at low levels of accuracy, leading to a larger intercept but a shallower slope than low levels of temporal uncertainty.

Other experiments have looked at the relationship between sequential effects and tradeoff parameters. Swensson (1972b) used a two-choice task with visual stimuli and key-press responses. He used the same type of payoff technique to manipulate performance as that used in earlier experiments (Swensson & Edwards, 1971; Swensson, 1971a). The task was unusual in that the stimuli were presented serially in groups of five. Two conditions were employed. In one, the RSI for a sequence of trials was 1 s, whereas in the other, each response triggered the next stimulus presentation immediately. For each condition, the trials were partitioned into response repetitions and response alternations and each of these data sets was partitioned again into trials following correct responses and trials following errors. Unfortunately, although Swensson applied the "log-odds" transformation to his data, he did not fit linear tradeoff functions. However, he claimed that his data showed that, for the serial task, the repetition effect was due to a tradeoff bias, rather than an efficiency effect, i.e. that response repetitions were faster only because they were less accurate. Inspection of the partitioned data for the serial task did indeed reveal a pronounced repetition bias following correct responses (and also an alternation bias following error responses). The latter effect was attributed to the presence of error-correction responses.

2.4.4 The Treatment Of Errors In CRT Experiments

Whenever subjects perform a CRT experiment, they will commit errors on a proportion of the trials. Exactly how large the proportion is depends on various parameters of the experiment, including discrimination difficulty, S-R compatibility, practice and number of S-R pairs. It also depends on the relative emphasis of speed versus accuracy in the experimental instructions. Nevertheless, if the RTs are to be regarded as meaningful measures of information processing speed, it seems unavoidable that some errors will be committed.

The fact that errors are committed leads immediately to a problem for the investigator in deciding how to deal with the errors when analysing the data. The most common approach appears to be to omit errors from the data (and sometimes post-error responses as well) and report RTs on the correct trials only. Occasionally, error RTs are given too (Egeth & Smith, 1970). Another approach is to include both errors and correct responses in the quoted RTs. A significant problem is the fact that experimenters frequently do not report which of these approaches they have adopted in calculating RTs. Added to this is the fact that some investigators do not quote error rates at all and, of those that do, not all give separate error rates for each experimental condition. As far as speed-accuracy tradeoff considerations are concerned, the crux of the matter is that when two conditions from a conventional experiment differ in mean RT, it is only possible to say with certainty that the condition with the longer RT is more difficult if it is also true that it has a higher error rate (or at least one of the same magnitude). On the other hand, if the error rate corresponding to the longer RT is actually lower, then this could well be due to the subject having adopted a speed-accuracy criterion weighted more towards accuracy than in the other conditions.

For those few experiments which are reported with a full complement of error rates for each of the conditions, it is often found to be the case that if all the conditions are ordered according to increasing RT, they are not in order of increasing error rate. This, in turn, means that if the RT results could somehow have been adjusted for criterion differences between conditions, then quite different pictures might have emerged.

An example of such a case is the study by Bertelson (1967). The results showed an inverse relationship between RT and error rate over the foreperiod range used (zero to 300 ms) for both constant and variable foreperiods. This meant that no valid inferences could be drawn concerning the effect of foreperiod length on the efficiency of the reaction process. A rather similar picture was obtained in a later experiment by Bertelson & Tisseyre (1968). Some experiments on foreperiod effects reported by Holender & Bertelson (1975) also failed to show the ideal relationship between RT and error rate for the various conditions.

This point is important and will be taken up again later. Essentially, what it suggests (at least for experiments concerned with the temporal aspects of preparation) is that strategy effects frequently occur and are confounded with the experimental manipulation of interest.

2.5 CONCLUSIONS

It seems reasonable to draw the following conclusions. Firstly, as the term implies, the temporal aspect of preparation appears to be concerned with time uncertainty. The less certain the subject is concerning when the stimulus is going to arrive, the less well prepared he tends to be when the stimulus does occur. Gottsdanker's work suggests that this is because maintaining high levels of preparation is aversive.

Secondly, the relationship between the selective and temporal aspects of preparation is contentious. Section 2.4 gave instances of various studies in which both selective and temporal aspects of preparation were manipulated and some yielded interactions, whereas others did not. This matter will be investigated further by experiments in Chapter 4.

Thirdly, it appears to be the case that for experiments in which foreperiod variables are manipulated, there is a tendency for criterion shifts to occur, with the result that any foreperiod effects are confounded with strategy effects. In order to control for this, it appears to be necessary to employ some technique to separate task effects from strategy effects, i.e. to control for criterion shifts.

CHAPTER 3: THEORIES OF CHOICE REACTION TIME

3.1 INTRODUCTION

Theories which attempt to predict or explain aspects of CRT are numerous. Some more modest endeavours are directed towards particular features of CRT performance, while others are concerned with the total picture. This chapter is a brief review of those theories which claim to be of the global variety. Firstly, the various classes of model will be outlined and secondly, they will be assessed as regards their adequacy in explaining the phenomena described earlier in this thesis. Before dealing with the models themselves, the notion of processing stages will first be examined because of its relevance in describing and classifying the various models.

3.2 PROCESSING STAGES IN CRT

The roots of this approach go back to Donders (1868), who distinguished between three types of CRT task: 'a', 'b' and 'c' reactions. The first of these was what we now call SRT. The 'b' task involved two stimuli and two responses with a 1:1 S-R mapping between them, i.e. the "conventional" type of task. The third type of task (the 'c' reaction) used two stimuli but only one response. In this case, only one of the stimuli required a response. Donders assumed that the time elapsing between stimulus and response in the 'b' and 'c' reactions is taken up by the operation of a number of independent, non-overlapping processing stages. In the 'b' reaction, these stages included stimulus categorisation and response selection, while only the former was required for the 'c' reaction. Donders assumed that, by applying simple subtraction logic, he could arrive at estimates of the time taken by each of these stages, viz. that (c-a) would yield the stimulus categorisation time and (b-c) would give the time taken for response selection.

The application of Donder's ideas towards the end of the last century met with criticism from the introspective school on the grounds that it might be difficult to devise experimental tasks that would add or delete processing stages without also altering some of the other stages present. Interestingly, according to D.A. Taylor (1976), the method has never been properly discredited by experimental investigation. One of the few recent experiments which used the technique was reported by D.H. Taylor (1966). The stimuli were two coloured discs and the responses were made by pressing microswitch pushbuttons. An auditory warning signal was used, with an irregular foreperiod. In addition to the usual 'b' and 'c' conditions, two others were used - b' and c'. In the first of these, both responses were required, but only one of the coloured discs was used as a stimulus - the other stimulus being marked by a null stimulus event. The c' condition was similar but required only one response, which was made to the coloured disc. Taylor claimed that the b' and c' conditions did not require true stimulus discrimination and that the stage durations for stimulus discrimination and response choice could each be obtained by subtracting the mean RT for the c' condition from the mean RTs for the c and b' conditions, respectively. Furthermore, he claimed that the difference (b-c') would give the sum of these two stage durations and that, if the additivity hypothesis were true, this would equal the sum of (c-c') and (b'-c'). This is equivalent to testing the hypothesis:

$$b-c-b'+c'=0$$

The obtained value for the LHS of this expression was 20 ms. This was not sufficient to reject the null hypothesis of additivity because the confidence interval was so large. It seems reasonable to concur with Sternberg (1969) that Taylor's experiment was insufficiently precise.

More recently, Sternberg (1969) revived interest in processing stages of CRT with his additive factor method. The essence of this approach is that, if a processing model involving additive components is assumed, then a statistical interaction between a number of factors (i.e. independent variables) in a multifactor experiment indicates that each of the particular factors is operating on the same component (i.e. stage). From the pattern of interactions obtained (possibly from a number of experiments) it is possible to infer the existence of a number of processing stages.

Sternberg was careful to point out that, although the assumption of additivity is required by the method, the assumption of stochastic independence of stage duration is not. Sternberg gives some examples of circumstances in which stage durations might be correlated, although still additive. For example, it is conceivable that if the subject is prepared for the stimulus that appears, then this will shorten more than one processing stage. In such a case, the stage durations concerned would be positively correlated. To take another example, suppose that the duration of a stage is inversely related to the quality of its input. If it is also true that the longer the duration of the processing stage, the higher the quality of its output, then the durations of the two stages will be negatively correlated.

Of course, failure to find an interaction between two factors does not necessarily mean that they do not influence the same processing stage. It is possible that they do so but that the effects are additive, in which case an interaction will not be produced. It is also important to realise that some circumstances can give rise to an interaction between two or more factors, without these factors actually influencing the same process. For example, if two independent processes occur in parallel and both must be completed before the next stage can begin, then two factors (one influencing each process) can be expected to interact "negatively", as Sternberg puts it (i.e. in such a manner that increasing the processing load on both factors results in a faster RT than would be expected under a hypothesis of additivity of effects). Sternberg also points out that processing capacity that is shareable between serial processes can produce an interaction of the "positive" type, (i.e. in which increasing the processing load on two factors produces a longer RT than would be expected if the effects were additive). However, what Sternberg means here is difficult to understand. How can serial processes share processing capacity? In any case, even if they could, it is not clear how or why this could lead to an interaction of any type.

3.3 A META-CLASSIFICATION

In order to provide a high-level categorisation of the various theories to be described, a classification scheme based on processing stages is employed here. The system bears a strong resemblance to that employed by E.E. Smith (1968) in his well-known review paper. The scheme postulates four stages, as follows:

1. Stimulus encoding. This involves forming an internal representation of the stimulus which can then be employed by the following stage.

2. Stimulus identification. Here the stimulus is categorised as one of the possible members of the stimulus set.

3. Response selection. The appropriate response is chosen for the stimulus just identified.

4. Response execution.

Only the first three of these stages are relevant here, because the last stage would appear to involve nothing more than motor activity.

The following section describes the chief types of CRT model and subsequent sections evaluate them in terms of their ability to account for the phenomena described earlier. The coverage takes the form of verbal outline rather than mathematical detail.

3.4 TYPES OF THEORY

3.4.1 Models Based On Information Theory

Although information theory has historical pride of place among CRT theories, (e.g. Hick, 1952; Hyman 1953) it has fallen out of favour in recent years for a variety of reasons, which will be explained later. However, it has formed a part of a more recent theory described by Briggs and his colleagues, in various papers, for describing the process of memory search proposed by Sternberg (1966). More precisely, Briggs has used it to model the stimulus identification stage of the task (Briggs & Swanson, 1970).

3.4.2 The Fixed Sample Model

The fixed sample model was first described by Stone (1960). It employs the idea that stimulus identification is based on a process of sampling discrete quanta of information arising from the encoded stimulus. The quanta of information are such that they arrive regularly spaced in time and conveying imperfect information concerning the identity of the stimulus. Furthermore, the model assumes that the sampling is conducted for a predetermined interval of time (and hence results in a fixed number of quanta). (This sampling is assumed to be fixed for the duration of a block of trials run under a particular condition but is regarded as being free to vary with experimental conditions between blocks of trials).

On the basis of the sample of evidence collected, the subject then decides which stimulus has been presented. In Stone's model, this means deciding in favour of that signal with the maximum likelihood, i.e. the one that yields the greatest value for the conditional probability of the evidence received, given the signal.

3.4.3 Optional Stopping Models

The largest class of model is concerned with a development of the fixed sample model in which, rather than sampling evidence for a fixed period of time, the subject continues to collect evidence until some particular criterion is reached, allowing the subject to decide in favour of one of the signals. There ~~are~~ is obviously a large number of ways in which such a process could operate. However, a clear distinction can be drawn between those models which use absolute criteria and those that use relative ones.

Dealing firstly with the former category, Broadbent (1971) has produced a classification, based on earlier work by Audley & Pike (1965). The idea is that, associated with each stimulus is an accumulator. Accumulator theories seem to be preferred by those attempting to model discrimination tasks (Audley & Pike, 1965; LaBerge, 1962; Pike, 1966; Vickers, 1970). Each piece of evidence is assumed to provide clear (but imperfect) evidence for one of the stimuli and increases the count of the corresponding accumulator by one unit. The possible categorisations concern the way in which the accumulators work (and hence the way in which the decision is made). The simplest idea is to conceive of the accumulators as keeping a straightforward count of the evidence in favour of each of the alternatives. When a predetermined critical value for one of these is reached, the decision is made in its favour. Another possibility (sometimes called the "runs" model) is that the decision is based on attaining a consecutive sequence of elements which indicate a particular stimulus. If the sequence is broken before the criterion is reached, the counting procedure is aborted and started again.

The second approach (i.e. that of relative criteria) seems more popular with CRT theorists. In this type of model, each piece of evidence counts in favour of one particular alternative and also, unlike the type just described, counts *against* the other(s). For the two-choice case, this is best conceived of as a random walk in one dimension, with the two boundaries representing choice in favour of one or other of the alternatives. The two-choice model has been dealt with by a number of theorists (Carterette, 1966; Fitts, 1966; Kintsch, 1963; Laming, 1968; Link, 1975; Stone, 1960; Swensson & Green, 1977) including an interesting version by Edwards (1965) which is formulated in terms of a *continuous* flow of information, rather than a discrete one. For tasks with more than two alternatives, the mathematics becomes more difficult. With three stimuli, the random walk ~~is also one dimensional but~~ occurs in two-dimensional space. Thus the random walk takes place within a regular triangle, rather than on a line. Similarly, for four choices, the walk is three-dimensional and can be thought of as occurring within a regular tetrahedron. In general, for 'm' alternatives, the walk ~~is one dimensional~~ occurs within a space of m-1 dimensions.

3.4.4 *Preparation Models*

The best-known preparation model is that of Falmagne (1965), which posits all-or-none preparation states in which the subject is either fully prepared, or not prepared at all, for the stimulus which occurs. The RT for a given trial is drawn from one of two distributions (differing in mean latency) according to whether the subject was prepared, or not. Thus the model is a two-state one. The adequacy of such a model for dealing with two-choice tasks was examined by Theios & Smith (1972). A later paper by Falmagne & Theios (1969) considered a three-state model and these were compared with four-state models by Lupker & Theios (1975, 1977). The tentative conclusion from examining empirical evidence was that a two-choice model was adequate and there was no justification in postulating more complicated models. Hence only the original two-state model is considered here. Markov elaborations on this framework specify the manner in which the levels of preparation change from trial to trial.

Falmagne himself is vague concerning the locus of preparation in his model. E.E. Smith (1968) regards it as a model of response selection. However, it seems most unlikely that the model is a suitable representation of response selection because the preparation levels do not sum to a constant (e.g. it allows the subject to be *fully* prepared for more than one alternative). It is difficult to accept this as a true state of affairs.

3.4.5 The Fast-Guess Model

The fast-guess model was first put forward by Ollman (1966). Further work was done with it by Yellott (1967, 1971). It assumes that S/A (speed-accuracy) tradeoff is achieved by varying the relative proportions of two types of response: SCRs (stimulus controlled responses) and FGs (fast-guesses). The former are largely correct responses, produced after normal processing of the stimulus information. The latter are faster detection responses, accurate at chance level only. Thus the model is concerned with the stimulus identification stage, specifying that this can either be done accurately (with a corresponding time cost) or quickly (at chance levels of accuracy). Traditionally, this model has been regarded as being no more than a way of explaining S/A tradeoff. However, arguments presented later on in this chapter and in Chapter 5 show that it can be regarded as a rather more general type of theory.

3.5 ASSESSMENT OF MODELS

3.5.1 Introduction

The following sub-sections examine each of the main empirical findings in turn and assess the adequacy of the models just outlined in accounting for them. Frequently additional assumptions and subsidiary mechanisms will be found to be necessary.

3.5.2 Number Of Stimuli

The way in which information theory is used to account for the fact that RT depends on the number of stimuli depends on whether it is regarded as providing a description of the stimulus encoding stage or the stimulus identification process. Hick (1952) considered two possibilities. The first was that stimulus identification took place by simultaneous template matching, using N templates - one for each of the N possible stimuli. The templates themselves were assumed to be formed at the stimulus encoding stage, by geometric replication (which would generate the necessary logarithmic relationship between N and RT if a fixed time is allowed for each stage of the replication process). The second possibility considered by Hick was that of some sort of feature-testing process taking place at the stimulus identification stage and involving successive binary classifications.

Turning to fixed and variable sample models, it is obvious that RT will only increase as the number of stimuli is increased if the probability of individual quanta providing correct information is reduced (thereby necessitating an increased sampling time to maintain the same error rate). Stone (1960) presents an argument based on this idea which is applicable to an accumulator model and yields a good approximation to the required logarithmic relationship between RT and N.

As regards Falmagne's preparation model, the axioms are set up so as to favour stimulus repetitions, i.e. subjects are more likely to be prepared for a stimulus repetition than for a non-repetition. As N is increased, this will necessarily reduce the proportion of stimulus repetitions in the stimulus sequence and will consequently produce a lengthening of RT. However, it seems as if the function would be the wrong shape, tending to an asymptotic maximum for large values of N (where the subject is not prepared for any of the stimuli that occur and is drawing all his RTs from the distribution with the longer latency). On the other hand, there is a little evidence (Seibel, 1963) to suggest that the relationship between RT and N is not logarithmic for large values of N but does tend to flatten out. This is discussed briefly in Section 2.1 of Chapter 1.

The fast-guess model is not, at first sight, capable of producing this effect. However, it must be remembered that, if the error rate is constant over the various conditions, the proportion of fast-guesses must necessarily decrease as the number of stimuli is increased. The reasoning is in some ways analogous to the argument used in the preceding paragraph when dealing with Falmagne's preparation model, in the sense that an effect due to number of stimuli can be produced by varying the relative proportions of responses drawn from two latency distributions of different mean. Unfortunately, the function relating RT to N would tend to be the wrong shape, just as with the preparation model. However, the caveat at the end of the preceding paragraph should be born in mind.

3.5.3 Stimulus Probability

Hyman (1953) applied information theory to a task requiring verbal responses to light stimuli. He varied the entropy of the stimulus sequence in three different ways: (a) by altering the number of alternatives; (b) by changing the relative frequencies of the stimuli and (c) by altering the first order sequential dependencies between successive stimuli. For each subject considered separately, he found that the regression lines for the three different conditions virtually coincided, i.e. that a single function relating RT to stimulus information would adequately cover all three conditions. However, he also observed that, while this empirical function gave a good fit for the mean RT for stimulus sequences characterised by high levels of redundancy, it did not predict the average RT to the different events at all well. Events with low information values were slower than predicted by the function and events with high information values were much faster than predicted. Also, the RT to an event of a given information value was found to depend on other aspects of the stimulus sequence, such as number of alternatives. Similar results were obtained by Hohle & Gholson (1968) and Stone & Callaway (1964). Strangely, Lamb & Kaufman (1965) and Kaufman & Levy (1966) found the opposite tendency in experiments which used lights as stimuli and key-press responses (which required the subject to move his finger from a home key to the response button concerned). However, a later experiment by Kaufman, Lamb & Walter (1970), which used vocal responses to visual stimuli, yielded discrepancies in the same direction as in Hyman's study. All that can be said in conclusion is that although information theory appears to be a useful model for relating the overall entropy of a series of stimuli to mean RT, it is not capable of accounting for responses to individual stimuli.

Furthermore, it is obvious that a simultaneous template-matching process cannot be at work because this would not produce a stimulus probability effect at all. The feature-testing process provides a rather better model, but only if two assumptions proposed by Welford (1960) are added to the model, as described below. For the two-choice case, the subject first tests for the presence of the more probable alternative and makes a further (redundant) test for the less probable alternative if, and only if, the first test yields a negative result. These ideas were further developed by Welford (1973, 1975) but the assumptions mean that the process can no longer be regarded as conforming to an information-theoretic model, although the predicted results may be approximated by it.

The accumulator and random walk models, however, are obviously well suited to deal with the stimulus probability effect. All that is required is that subjects set the criteria for the various stimuli at levels which are inversely related to their probabilities of occurrence. For a simple accumulator model, this means that the number of quanta required to indicate the presence of a high-probability stimulus is less than the number required to decide in favour of another less frequent alternative. For the random walk model, the starting point of the process is assumed to be biased, so that the distances from the various decision boundaries reflect the corresponding stimulus probabilities, with the starting point being closest to the boundary corresponding to the stimulus of highest probability. By contrast, the fixed sample model fails completely to accommodate the stimulus probability effect. As pointed out by Broadbent (1971), because the length of the sample is fixed in advance, the RT cannot possibly depend on the stimulus which occurs.

Falmagne's preparation model is also well suited to account for the stimulus probability effect. By virtue of the fact that the axioms favour stimulus repetitions, subjects are more likely to be prepared for high-probability stimuli than for low-probability ones. Indeed, Falmagne (1965) actually made parameter estimates for his model by applying it to a six-choice task in which the stimulus probabilities varied from 0.01 to 0.56.

The fast-guess model was not intended to accommodate the stimulus probability effect. However, there is a possibility that subjects could direct their fast-guesses towards various stimuli according to their probabilities of occurrence. If this happened, then the higher the probability of a stimulus, the greater the proportion of correct responses to that stimulus would be fast-guesses. This would produce a stimulus probability effect, even in the absence of any difference in the latency of SCRs (stimulus controlled responses).

3.5.4 Sequential Effects

As mentioned in Chapter 1, sequential effects are of two types. One is simply due to the fact that repetitions tend to be faster than alternations, i.e. the so-called repetition effect. The other type is brought about by manipulating sequential dependencies in the stimulus sequence. The latter effect is clearly akin to the stimulus probability effect, in that it is based on actual probabilities present in the stimulus sequence and, with suitable elaborations, the same sort of arguments can be used in deciding how well the various models can be made to account for the findings. For example, it is obvious that Welford's additional assumptions for the information-theoretic approach concerning the possible nature of the feature-testing process could equally well be converted to apply to sequential probabilities rather than (or in addition to) the stimulus probabilities themselves.

Kornblum (1968, 1969a) drew attention to the fact that, for most experiments which tested information theory by manipulating the sequential dependencies in the stimulus sequence, stimulus information is actually confounded with the probability of non-repetitions in the stimulus sequence. (This is due to the fact that researchers have tended to reduce stimulus information by increasing the probability of stimuli repetitions rather than that of non-repetitions). Kornblum (1968) reported an experiment which attempted to distinguish between genuine information effects and those arising from the fact that repetitions are faster than non-repetitions. He made use of the fact that, for a given set of stimuli with given relative frequencies, the function relating information to p(nr) (i.e. non-repetition probability) is a parabolic one, i.e. for most values of H(s) - i.e. stimulus information - there are two possible values for p(nr). He used a four-choice task with lights as stimuli and key-press responses. Eight different conditions were employed, each of which used equiprobable sequences which differed solely in their first-order sequential dependencies. The RSI was 140 ms. Six of the conditions were equi-information pairs and one had p(nr) set at 0.75, giving the maximum possible value of 2 bits for H(s). Kornblum found that a single information function did <u>not</u> fit the results but that a good fit could be obtained by using <u>two</u> functions - one for those sequences with low values for p(nr) and one for those with high values. For those sequences with high values for p(nr) (i.e. with p(nr) greater than or equal to 0.75), RT was virtually independent of H(s). For the sequences with low values of p(nr), on the other hand, the typical linear relation between RT and H(s) was evident and had a slope of 108 ms per bit. Arguing from these findings, Kornblum claimed that "... the Information Hypothesis must be rejected as an erroneous and misleading interpretation of serial choice RT data."

Hyman & Umilta (1969) made a further investigation of the matter. They ran a four-choice experiment which required vocal responses to visually presented numerals. There was a foreperiod of

2 s and the RSI was approximately 7.5 s. Three different experimental conditions were used, each of which used the same values for p(nr) as one of the conditions in Kornblum's study. The three conditions concerned were, firstly, one with no constraints (i.e. 2 bits of information) and a pair of equi-information conditions of 1.58 bits. One of the latter had a repetition probability of zero. This condition yielded an RT of 430 ms, while its equi-information counterpart had an overall RT of 416 ms, giving a difference of 14 ms, which was considerably smaller than the difference of approximately 45 ms obtained by Kornblum. Even so, the difference was significant and the more compatible nature of the task may well have something to do with its smaller magnitude. A more interesting aspect of the paper is the suggestion that separate information functions should be obtained for the repetitions and non-repetitions, using the surprisals of the different types of stimulus event. Although there were insufficient points to test this idea adequately (because the repetition function had only two data points), the non-repetition function appeared to fit well. There was no significant difference between the slopes of the two functions but there was a 40 ms difference in the intercepts, with the repetitions being faster.

Summarising, it seems that, just as with stimulus probability, information theory cannot satisfactorily account for all the aspects of sequential effects.

In a similar manner, both the simple accumulator model and the random walk model could accommodate sequential effects if the criteria settings were allowed to reflect the appropriate sequential probabilities as well as the basic stimulus probabilities and were adjusted accordingly on a trial by trial basis. However, such complexity seems almost to require a sub-model to link the various sequential probabilities of the stimulus sequence to the criterion variations. For his version of the random walk model, Laming (1969) makes use of subjective probability as an intervening variable which reflects the more important and recent statistical features of the stimulus sequence. It is worth noting that such an intervening variable could also mediate the effects of incentive manipulations and explain why these can produce similar effects to stimulus frequency alterations, e.g. Kanarick (1966). The fixed sample model, on the other hand, fails just as before to accommodate such sequential effects.

Turning to the standard repetition effect, it is obviously possible to account for it in the same sort of way, provided that a criterion bias in favour of repetitions (which is not reflected in the <u>actual</u> sequential probabilities) is allowed. However, an additional problem is the fact that the repetition effect appears to interact with the RSI (see Section 2.4 in Chapter 1). This means that either the criterion setting mechanism must be made even more complex to account for this effect or, that some other explanation needs to be found.

Falmagne's preparation model includes a transition relation which may be summarised by four statements concerning the derivation of the preparation levels for the next trial from the current preparation levels and the identity of the stimulus on the current trial. The statements are as follows:

to be faster for shorter RSIs, thus increasing the magnitude of the repetition effect.

3.5.5 S-R Compatibility

S-R compatibility is an interesting experimental variable for two important reasons. Firstly, if looking at things from an information-theoretic point of view, decreasing S-R compatibility results in a reduction of "channel capacity". However, as channel capacity is an intended invariant of the model, this is clearly unsatisfactory and many researchers (e.g. Broadbent, 1971; Laming, 1968) have regarded this as indicative of a failure of the information model.

Secondly, there is no sensible way in which S-R compatibility can be accounted for by models which are based on the stimulus identification process. This is obviously because S-R compatibility has its effect at the stage of response selection.

Falmagne's preparation model can easily accommodate compatibility effects by letting the mean of the latency distribution for the unprepared responses reflect the difficulty of the S-R mapping. This particular approach has the virtue of making it possible to account for interactions between compatibility and certain other phenomena (e.g. the repetition and prediction effects). On the other hand, it does not give a full account of the process of response selection in the sense that it does not provide an explanation of the mechanism involved and hence does not explain why incompatible S-R codes should results in longer RTs than compatible ones.

Finally, considering the fast-guess model, it is obvious that the compatibility effect must be located on the SCRs and further, that the fast-guess model itself is not adequate to explain the effect. Thus a sub-model or a set of additional principles is necessary.

3.5.6 Reaction Time Exchange Functions

Other evidence, which has not been discussed until now, comes from the study of RT exchange functions championed by Audley, (Audley, 1973; Audley, Caudrey, Howell & Powell, 1975). The principle is more easily applied to two-choice tasks and involves the hypothesis of some form of reciprocation of preparation for the two stimuli (or responses). It was first applied by Audley (1973) to data from a number of experiments reported by Schvaneveldt & Chase (1969) and Remington (1969) in order to examine the part played by preparation factors in the production of sequential effects. The essence of the method for this particular application lies in comparing the mean RT to stimulus A with that to stimulus B, when each stimulus is preceded by the same sequence of stimuli. The empirical exchange function is derived by plotting RT(A) versus RT(B) for each such prior sequence of stimuli.

Audley (1973) found that in some cases, the resulting function had a slope of about -1, which he interpreted as being indicative of some sort of linear preparation tradeoff between the two alternatives. However, in other cases (e.g. a task using a symbolic

S-R code reported by Schvaneveldt & Chase (1969), he found that the empirical tradeoff function tended to have two linear limbs (as if it had been forced away from the origin at the centre of the line and then broken into two parts). Audley attributed this to the presence of fast-guesses in the data, which allowed responses to one stimulus to be markedly faster for one data point than ~~the other~~ another, without resulting in a corresponding lengthening of responses to the other stimulus. Audley drew the tentative conclusion that the "true" RT exchange function was linear, with a slope of -1 in the two-choice case. For tasks with more than two alternatives, similar reasoning suggests that the RT exchange function should be a hyperplane in m-space (where 'm' is the number of alternatives) although Audley thinks in terms of a linear function in 2-space (as before) with a slope of -(m-1), presumably obtained by plotting the responses to one stimulus (measured on the ordinate) against the pooled responses of the remainder. However, Audley admitted that this approach for multi-choice tasks was not very satisfactory, since the sequences of preceding stimuli are described only in terms of whether each is the same as the presented stimulus, or different.

The later paper by Audley et al (1975) deals with the derivation of RT exchange functions from equiprobable two choice tasks, in which advance information concerning the next stimulus was presented to the subject on each trial. The method employed utilised a modification of the cueing technique described by LaBerge, Van Gelder & Yellott (1970). Two experiments were conducted - one using lights as stimuli and the other using visually displayed numerals. Manual responses were used in both cases. Prior to each stimulus, a visual cue was presented to the subject indicating the most likely stimulus about to occur, accompanied by a probability figure taking one of five possible values (0.5, 0.6, 0.7, 0.8, 0.9) which gave the likelihood of the cued stimulus being the one actually presented. For both experiments, two different conditions were used - a blocked condition (in which trials with the same cue probability were presented in the same block) and a random condition.

The resulting exchange functions showed that the task with numerical stimuli had the two-limbed character mentioned previously. Because of this, the experimenters decided to attempt to remove fast-guesses from all their data by removing responses which were so fast as to be in latency ranges derived from responses which were not correct at better than chance level. When this had been done and the graphs re-plotted for the random conditions, the exchange functions showed a definite tendency to be linear, with the expected slope of -1.

Thus the experimental evidence seems to support the idea of a linear preparation tradeoff between the various alternatives, although there is nothing in this evidence to suggest whether the preparation is located on the stimuli or on the responses. How well can the various classes of theory accommodate this observation?

Looking firstly at information theory, there does not seem to be any adequate way of accounting for a linear preparation tradeoff. If preparation were expressed in terms of subjective probability and this were treated as objective probability for the purposes of calculation, then the RT exchange function would be formed from the two surprisals for the two-choice case. The function thus generated
downwards, rather than upwards,
is not linear. In fact it is convex upwards as Audley (1973) claims.
(p 524

Thus a linear preparation tradeoff is not compatible with information theory.

In order for the fixed sample model to accommodate any sort of tradeoff function, the sampling time would have to vary inversely with the level of preparation. This would lead to a concomitant change in the error rate and there is good evidence that this does occur (Audley et al, 1975). Thus, rather than regard the sampling time as absolutely fixed, it makes sense to allow it to be under the subject's control. As pointed out earlier, however, this does not mean that the sampling time could vary according to the stimulus presented.

Turning to the variable sample models, it is obvious that the subject can be differentially prepared for the two stimuli by setting different criteria for them. In fact, this was exactly the mechanism proposed to account for the stimulus probability effect. To account for the linear preparation tradeoff, however, it is necessary that the criterion settings are inversely related. Of course, such a requirement is easily met by the random walk model but an accumulator model needs additional principles to give it the appropriate behaviour. Returning to the random walk model, for a fixed error rate, the distance between the boundaries in a two-choice task is a constant. Provided the error rate is low, preparation tradeoff achieved by altering the starting point yields a linear RT exchange function (Audley, 1973).

According to Audley (1973: p 524) Falmagne's preparation model predicts a linear exchange function with a slope of -1. In fact, this does not appear to be true. Falmagne (1965: p 80) specifically did not make the assumption that the preparation levels summed to a constant. Of course, if he had done so, then his model would necessarily generate the required exchange function. However, even within the framework of Falmagne's model, particular parameter values would achieve the same result. If 'c' and c' both have the value of unity, then the model degenerates to one in which the present preparation levels have no bearing on those of the next trial. In this case, the subject's preparation behaviour is entirely controlled by the current stimulus, which he prepares for on the next trial. However, in his experimental test of the model, Falmagne obtained values of 0.328 for 'c' and 0.076 for c', which is not compatible with this possibility. Before coming to any definite conclusion on the matter, it would seem desirable to test Falmagne's model on experimental data which show a tendency to obey a linear exchange relation on the RTs.

Finally, the ability of the fast-guess model to account for the linear exchange relation depends critically on whether errors are included in the data, or not. If the subject tends to bias his fast-guesses towards a favoured response and the errors are not discarded, this tends merely to produce a two-limbed RT exchange function of the type described earlier. On the other hand, if the errors are discarded from the data before the exchange relation is plotted (so that the data only include those fast-guesses that are correct), then it is obvious that a bias can be introduced, in which responses to the favoured stimulus include a greater proportion of fast-guesses than the responses to the other stimuli. In principle, such a mechanism could produce a linear exchange relation by virtue of the existence of an underlying linear tradeoff in the proportions of fast-guesses directed towards the different responses. In fact, the exchange function produced from Remington's (1969) data is based on both correct and error responses and could not, therefore be due to fast-guessing.

3.5.7 Errors And S-A Tradeoff

Audley (1973) presents certain observations concerning errors which any potential model should accommodate. Essentially, they are as follows:

1. A given response made in error is faster than the same response made correctly.

2. The latency of a given response made correctly is positively correlated with the latency of the same response made incorrectly, when the latency change is produced by some experimental manipulation.

3. For a given S-A criterion setting, in a given task, the product of the probability of a particular stimulus and the number of errors made to that stimulus is approximately constant. (For the two-choice case, this is usually expressed as an error ratio in which the ratio of the number of errors made to each stimulus is approximately equal to the reciprocal of the probability ratio. It is worth noting that the same sort of statement can be made concerning the relationship between errors and sequential dependencies in the stimulus sequence).

Turning to the models themselves, it is worth noting that one of the most marked shortcomings of information theory is its difficulty in accounting for the commission of errors. The usual attempted explanation is in terms of faster information-processing causing incomplete coding (perhaps due to curtailed feature testing) which leads to errors. Such an explanation would result in incorrectly identified stimuli being responded to faster than correctly identified ones. However, curtailment of feature-testing must result in guessing between the remaining alternatives, which would lead to the stimulus probability effect not being apparent among the less probable stimuli in a multi-choice task. This prediction is certainly not in agreement with results from multi-choice tasks (e.g. Falmagne, 1965).

As regards the fixed sample model, it seems that the only way in which errors could be faster than corresponding correct responses is by allowing the sampling time to vary (under the subject's control). Errors would then be more likely to occur with shorter sampling times (and hence faster RTs). However, even then, the model is incapable of yielding a different error rate for different stimuli for exactly the same reason as it cannot yield a different latency to different stimuli, i.e. the sampling time would have to differ according to the stimulus.

As far as variable sample models are concerned, the accumulator model predicts that errors will be slower than correct responses. While this seems to be the case for difficult discrimination tasks (e.g. Wilding, 1971a), it is not true for typical CRT tasks involving easy discriminations. The random walk model, on the other hand, predicts that a given response will have the same latency, whether it is made correctly or not. However, it is of the utmost importance to remember that both these predictions are conditional upon having fixed criteria. Bearing this in mind, it is presumptuous in the extreme to attempt to decide between contending models according to these predictions, as some reviewers have done (e.g. Broadbent, 1971: pp 295 - 296). It is entirely possible that errors are produced in a number of different ways, including the relaxing of criteria (which would account for error responses being faster than the same response made correctly). Thus, for the random walk model, if the boundaries are both moved closer to the starting point this will speed up responses at the expense of making more errors, thereby accounting for errors being faster than correct responses. This idea was proposed by Fitts (1966) and was also mentioned by Swensson & Thomas (1974) and has been developed more recently by Laming (1979).

The second observation is satisfied by the same mechanism that was proposed for explaining the stimulus probability effect and the RT exchange relation - namely that the starting point is moved closer to one boundary and farther away from the other. The third observation can be approximately accounted for by the fact that, with a fixed-boundary generation of errors, the error ratio for a two-choice task is (1-p-e)/(p-e), from Laming (1968: p 128), where 'p' is the probability of occurrence of one of the stimuli and 'e' is the overall error rate. Thus, provided that 'e' is small and the probabilities are not extreme, the error ratio is approximately (1-p)/p, as required.

Falmagne's preparation model was really not designed with errors in mind. Almost as an afterthought, they are mentioned briefly right at the end of Falmagne's major paper on his model (Falmagne, 1965). According to the model, the subject has different error rates according to whether he is prepared for the stimulus which has just occurred, or not. Presumably, if he is prepared for another stimulus, he has an increased tendency to launch the wrong response. Although such an explanation allows errors to be faster than correct responses, it does not allow the error RT to change as the stimulus probability is manipulated. Also, the error ratio derived from the model does not agree with that specified at the beginning of the sub-section.

Finally the fast-guess model obviously has no difficulty in explaining why errors are faster than correct responses. Similarly, in principle at least, there is no problem in explaining the second observation. For example, if a stimulus is made more probable, then the fast-guessing rate on the corresponding response will increase, leading to a decrease in latency for both correct and incorrect responses of that type. Unfortunately, the error ratio cannot be accounted for, just as with the preparation model.

3.5.8 Prediction Effects

Prediction effects were dealt with in Section 2.3. Basically, there are two principle findings:

1. Correctly predicted stimuli are responded to faster than incorrectly predicted ones.

2. Effects such as those due to the number of stimuli or their probabilities of occurrence interact with the effect just mentioned, so that when the data are partitioned into correct and incorrect predictions the effect either disappears altogether, or is much attenuated, particularly for the correct predictions.

In general, the first of these findings can be accounted for by the same sorts of argument as were used in dealing with the stimulus probability and sequential effects, i.e. a biasing of the criteria in the case of the sampling models and an obvious tendency for the subject to be in a prepared state for the predicted stimulus in the case of a preparation model.

The second finding requires more thought. Welford's (1960) proposal (outlined in Section 5.3) for modifying information theory is an obvious possibility and was used for this purpose by Welford (1973). For this application, the predicted alternative is tested for first and, if found to be present, the search is terminated. If the favoured alternative is not found, then an exhaustive search is made. This means that the response time to correct predictions would be independent of the number of alternatives. (The same would not also be true of incorrect predictions) ~~in two choice tasks only).~~

Turning to the various sampling models, it is not possible to account for the second prediction effect by criterion adjustments. Looking at the interaction with stimulus probability under the random walk model as an example, it would require the starting position to be more nearly central for the correct predictions than for the incorrect ones, which is clearly impossible.

Preparation models, on the other hand, seem a little more promising. Falmagne's model is clearly based on the prediction effect and will necessarily yield the first finding. As regards the second finding, however, it only achieves a partial explanation. For example, when dealing with the interaction between prediction and number of stimuli, although it successfully predicts the behaviour of the correct predictions as N is increased, it does not predict the increase in RT which is found in the incorrect predictions.

Provided incorrect responses are discarded before data analysis is undertaken, the fast-guess model can account for the basic prediction effect simply by virtue of the fact that correct predictions will contain a higher proportion of correct fast-guesses than will incorrect predictions. However, it does not seem possible for the fast-guess model to account for the fact that the prediction effect interacts with certain other effects. For example, if the number of stimuli is manipulated and the data partitioned into correct and incorrect ~~responses~~ predictions, then the effect due to the number of stimuli must necessarily disappear. In other words, the fast-guess model fails to provide a satisfactory account in exactly the same way as Falmagne's preparation model.

3.5.9 Temporal Expectancy

It is noteworthy that few theorists have made serious attempts to account for foreperiod effects such as temporal expectancy.

Notwithstanding an ingenious attempt by Klemmer (1957) to encompass foreperiod effects by information theory via the concept of temporal uncertainty (see Chapter 2), information theory cannot really be said to have explained foreperiod effects. Even if Klemmer's approach is adopted, it still leaves the mechanism unexplained.

As regards other theories, Falmagne's preparation model obviously cannot cope with temporal expectancy. At first sight, various stimulus sampling models offer some possibility of dealing with the effect. Broadbent (1971) considers the matter at some length and bases his arguments on evidence from Bertelson & Barzeele (1965) that temporal uncertainty interacts with the stimulus frequency effect. However, as was made clear in Section 3.7 of the previous chapter, the existence of such interactions is quite contentious and it seems safest not to base theorising on such slender evidence. Laming (1968) considers the possibility that stimulus sampling begins before stimulus onset, but all this does is to make the first few steps of his random walk model depend on random noise, rather than information from the stimulus. It does not increase the time taken to reach a boundary since the expected number of steps required since stimulus onset is unchanged.

By contrast, the fast-guess model is well suited to explain temporal uncertainty effects. There are two possible mechanisms, which are not mutually exclusive. The first is simply that fast-guesses could have shorter latencies when temporal uncertainty is low, because time estimation is involved and is clearly more accurate under these circumstances. For similar reasons, the subject might make more fast-guesses because he is more able to make them before the stimulus information has been processed.

3.6 CONCLUSIONS

From what has been said earlier in this chapter, it is obvious that both information theory and the fixed sample model are completely unsuitable as global theories of CRT. More extensive criticisms of information theory are given by Broadbent (1971) and Laming (1968). The latter in particular gives a number of reasons (both theoretical and empirical) why information theory must be rejected. As regards the fixed sample model, it is evident from the preceding sections that it is incapable of accounting for most of the observed effects. Furthermore, it seems a rather strange model to postulate in the first place. It makes much more sense to think in terms of the greater flexibility of the family of optional stopping models.

Such a decision leaves the optional stopping group of models, Falmagne's preparation model (or perhaps a family of similar models) and the fast-guess model. Of these, the members of the optional stopping group account for more of the findings than the others but they are conspicuously unsuccessful in dealing with foreperiod effects. Conversely, of all the types of theory considered, the fast-guess model seems to be the only one capable of dealing with these.

The reason for this is that, although these models are claimed as global theories of the choice reaction process, they are not; they are models of particular stages of the process. In Section 3, four stages of processing were proposed. Now, it is obvious that stimulus sampling models are models of the second processing stage only - that of stimulus identification. Such models should not be expected to account for effects located at other stages of processing. It was mentioned in Section 4.4 that although Falmagne did not commit himself concerning the locus of preparation in his theory, it has been construed as a model of response preparation. Thus it is able to deal with compatibility effects (albeit in a rudimentary fashion), which models of stimulus identification are not.

As mentioned above, the fast-guess model appears to be the only one capable of dealing with foreperiod effects. Unfortunately, it is incomplete in the sense that any effect which is due to differences (between conditions) in mean latency of the stimulus-controlled responses has not really been explained. (In this respect, the position of the fast-guess model is similar to that of Falmagne's preparation model when dealing with S-R compatibility). However, in spite of the fact that the fast-guess model is incomplete, it makes a fair showing of accounting for at least three effects associated with the stimulus sequence, in addition to its unique capability of dealing with effects of temporal expectancy.

The fast-guess model is really dealing with a stimulus-independent bypass mechanism which can be looked upon as an "accessory" to one or more models of other processing stages. It is interesting to compare it with a similar accessory mechanism, viz. the stimulus-dependent bypass that was tentatively proposed in Section 3 of the previous chapter for dealing with prediction effects. The difference between the two mechanisms lies in the fact that in the latter case, the bypass involves the pre-selection of a favoured response, whose actual execution is delayed until and unless the predicted signal occurs. The time that is saved is the response selection time. In the case of the fast-guess model, on the other hand, not only is the response pre-selected, it is actually made <u>before</u> the stimulus has been identified (or possibly before it has even been detected). It is not implausible that the two phenomena might share a common generating mechanism and that fast-guesses are really pre-selected responses that have exceeded the critical threshold of readiness and spilled over into overt motor action before their appropriateness can be checked against the identity of the incoming stimulus.

The fast-guess model is also of importance in accounting for the existence of errors. This is not to say that all errors need to be due to fast-guesses but it seems likely that <u>some</u> errors are, particularly in tasks with a strong emphasis on speed. Many investigators (e.g. Swensson & Edwards, 1971) are of the opinion that in easy CRT tasks, the extent to which subjects can achieve an S-A tradeoff by criterion adjustment is strictly limited (possibly because a single quantum of information is sufficient to enable a highly accurate stimulus identification to be made). Thus, any further tradeoff in the direction of increased speed can only be obtained by committing a proportion of fast-guesses.

Finally, the fast-guess model could well be of importance in helping to account for certain other effects, particularly those mentioned in Chapter 1. Although it is just about theoretically possible for the fast-guess model alone to account for these effects, the balance of the evidence suggests otherwise. Frequently, the error rate is too low for fast-guessing mechanisms to have much chance of accounting for these effects "single-handed". However, even if this is the case, it is important to examine the possibility that some or all of these effects are composite ones that might be attenuated if the effects of fast-guessing were removed. Perhaps of more importance is the possibility that if the effect of fast-guesses were actually removed from experimental data, different functional relationships between response latency and experimental parameters might be revealed. An added bonus is that the fast-guess model proposed by Yellott (1971), which is explained and extended in Chapter 5, allows for two types of error - fast-guesses and incorrect stimulus-controlled responses. The accuracy of the stimulus-controlled responses is actually a parameter of the model and can be estimated as explained in Chapter 5. This means that any variation of the conventional criterion with experimental condition can be assessed.

CHAPTER 4: THE FIRST SERIES OF EXPERIMENTS

4.1 INTRODUCTION

This series of experiments was undertaken in order to investigate the relationship between the selective and temporal aspects of preparation. As mentioned in Chapter 2, the existence of such a relationship is quite a contentious issue. Studies such as that by Bertelson & Barzeele (1965) found that the relative frequency effect was larger with a short foreperiod than with a longer one. However, Holender & Bertelson (1975) failed to find such an effect. Now, from what was said in Chapter 2, it seems that a time estimation process is at work in the temporal aspect of preparation. The problem lies in seeing how this could be of any relevance to the selective aspect.

Some pilot experiments (not reported in detail here) showed that, when error rates were allowed to rise to a level higher than is customary in CRT experiments, both the stimulus frequency effect and the error rate varied inversely with foreperiod length. This, of course, does suggest a relationship between the selective and temporal aspects of preparation. This idea was pursued in the present series of experiments, where an attempt was made to tease out the processes at work.

4.2 PLAN OF THE THREE EXPERIMENTS

The experiments were run on-line using an IBM 1130 with a WDV interface and will be referred to by the names of the computer programs used. The first experiment (CRT22) was intended to show how different inter-trial intervals affect the relative frequency effect (i.e. the difference between the low- and high-frequency RT). The second experiment (CRT24) was designed in an attempt to find whether this relationship was due to a time estimation effect or to a preparation bias decaying over time, following the preceding response. The third experiment (CRT27) was an attempt to provide a further test of the time estimation hypothesis by using both fixed (i.e. constant) and variable foreperiods.

All three experiments used a response which involved moving a finger from a home key to the response button concerned. This feature was adopted because of its effectiveness in preventing subjects from making double responses (i.e. both responses more or less simultaneously). Similar systems have been used by Lamb & Kaufman (1965) and Kaufman & Levy (1966).

Light offset was used as the stimulus event because the stimulus lights were partly responsible for illuminating the response buttons in the darkened room used. The experimental literature does not agree on whether stimulus onset is faster than stimulus offset. Rains (1961) found no difference with a visual SRT task. However, Goldstone (1968) found onset to be faster than offset for both auditory and visual SRT tasks. Spigel found a similar effect with a visual two-choice task. On the other hand, Grier (1966) obtained the opposite result with an auditory SRT task and Simon, Craft & Webster

(1971) also found offset to be faster than onset in a visual two-choice task.

4.3 EXPERIMENT CRT22

4.3.1 Method

4.3.1.1 Apparatus -

The apparatus used comprised three parts: (1) a box with stimulus lights and response buttons (termed the SR box); (2) a smaller box with a single button (called the "home key"); and (3) a wooden stand constructed to hold the two boxes in position. The SR box held eight buttons, each of which could be illuminated by a bulb with a power consumption of 0.36 W. The lights had a brightness of approximately 25 millilamberts and were driven by a Farnell stabilised power supply (type L30B), set to deliver 6.5 volts. All the buttons in the SR box had tops made of a translucent green plastic, so that they emitted a green light when illuminated. The smaller box held a single button, of the sort just described, except that it had an amber cover. For the purposes of this experiment, only four of the combination light/buttons were used in the SR box. The outside two in the top row were used as the stimuli and those directly beneath them were used as the response buttons.

4.3.1.2 Task -

The event sequence for a single trial was as follows:

1. The subject started the inter-trial interval (ITI) by pressing the home key and holding it down for the duration of the ITI.

2. As soon as the ITI had expired, the computer presented one of the two possible stimuli by switching off one of the two stimulus lights.

3. The subject then responded by releasing his finger from the home key and moving it to the appropriate response button. The time that elapsed from occurrence of the stimulus to release of the home key was recorded as the first component of the reaction time, while the time that elapsed from release of the home key to making the correct response was recorded as the second component of the reaction time. Thus the total reaction time (RT) was the sum of two components. If the subject made an incorrect response, the trial was not completed until he had made the correct response. If the subject made a premature response, (i.e. one involving release of the home key before the stimulus occurred) on any trial, the trial concerned was cancelled and started again, from the beginning of the ITI. This procedure prevented contamination of the data by premature responses.

4. As soon as the correct response had been made, both the stimulus light and the home key lit up and the subject returned his finger to the latter, thus initiating the next trial.

4.3.1.3 Design -

Trials were arranged in blocks of 60. In all blocks, stimuli occurred with a 5:1 frequency ratio. Thus 50 of the stimuli occurred at one location and the remaining ten at the other. The location of the high-frequency stimulus remained the same throughout the experiment for each subject. Another constraint on the stimulus sequence ensured that the first 12 stimuli contained exactly ten of the high-frequency signals. (The first 12 trials in each block were intended as "preparation" trials, during which the subject could become accustomed to the particular ITI in operation). Run limits were also imposed on the sequence so that low-frequency signals would not occur more than three times in succession and high-frequency signals more than 23 times in succession. The alternation run limit was five.

Five different values for the ITI were used: 250, 500, 1000, 2000 and 4000 ms. All the trials in a given block had the same ITI. Inter-block rest pauses of approximately 30 s were used. Each session comprised 15 blocks of trials. The ITI values of the blocks were presented in a different random order for each session. A constraint on the randomisation procedure ensured that each of the five values occurred once and only once in the first five blocks, the second five blocks and the third five blocks. Each subject performed nine principal sessions, each lasting approximately one hour.

4.3.1.4 Subjects -

Four subjects were employed in this experiment. All were male students (aged between 18 and 23) from Durham University. They were each paid for all the principal sessions at the rate of 20 pence per session. Two subjects (PKW and AF) were run with the high-frequency signal located on the left and the other two (PE and NB) with it located on the right.

4.3.1.5 Procedure -

The principal sessions were preceded by a short training session for each subject. At this session, a set of verbatim instructions (see Appendix 1) was read to the subject by the experimenter, who then asked if any clarification was required. If so, supplementary explanations were given, ad lib. The experimenter then ran the first block of the training session using himself as subject for demonstration purposes. The ITI for this demonstration block was 1000 ms. The experimenter then answered any further queries and the subject was given five training blocks, comprising one block at each ITI. The principal sessions followed. As far as possible, these were run at the rate of one per day for each subject.

4.3.1.6 Data Reduction -

Both components of the mean correct response times were obtained for each subject for each ITI-session combination from the last 48 trials from each of the three blocks concerned. Thus each subject produced nine sets of scores for each ITI; each set of scores being derived from a single session. Corresponding error rates for the low-frequency stimuli were also calculated. Due to a breakdown in the paper tape punch on the 1130 system, all the data from the fourth session for subject PE were lost. However, the remainder of the data were transferred to the NUMAC system for ease of data analysis.

4.3.2 Results

4.3.2.1 Observations -

Before reporting the main statistical results, two observations should be noted briefly. Firstly, subjects reported that on some occasions when the low-frequency stimulus occurred, they commenced making a high-frequency response and then changed it to the correct response during the second-component of the RT. This tended to occur particularly when many errors were being made. Secondly, correlation coefficients taken while the experiment was running revealed that there were almost invariably negative correlations between first- and second-component RTs (including errors) within blocks.

The RT and error results for all subjects (pooled) are shown graphically in Fig. 4.1.

4.3.2.2 Analysis Of Variance On RT Scores -

To begin with, two Subjects x ITI x Stimulus Frequency analyses of variance were carried out. One was performed on the first-component scores and the other on the second-component scores. In both cases, Subjects was treated as a random effect. Blocks of

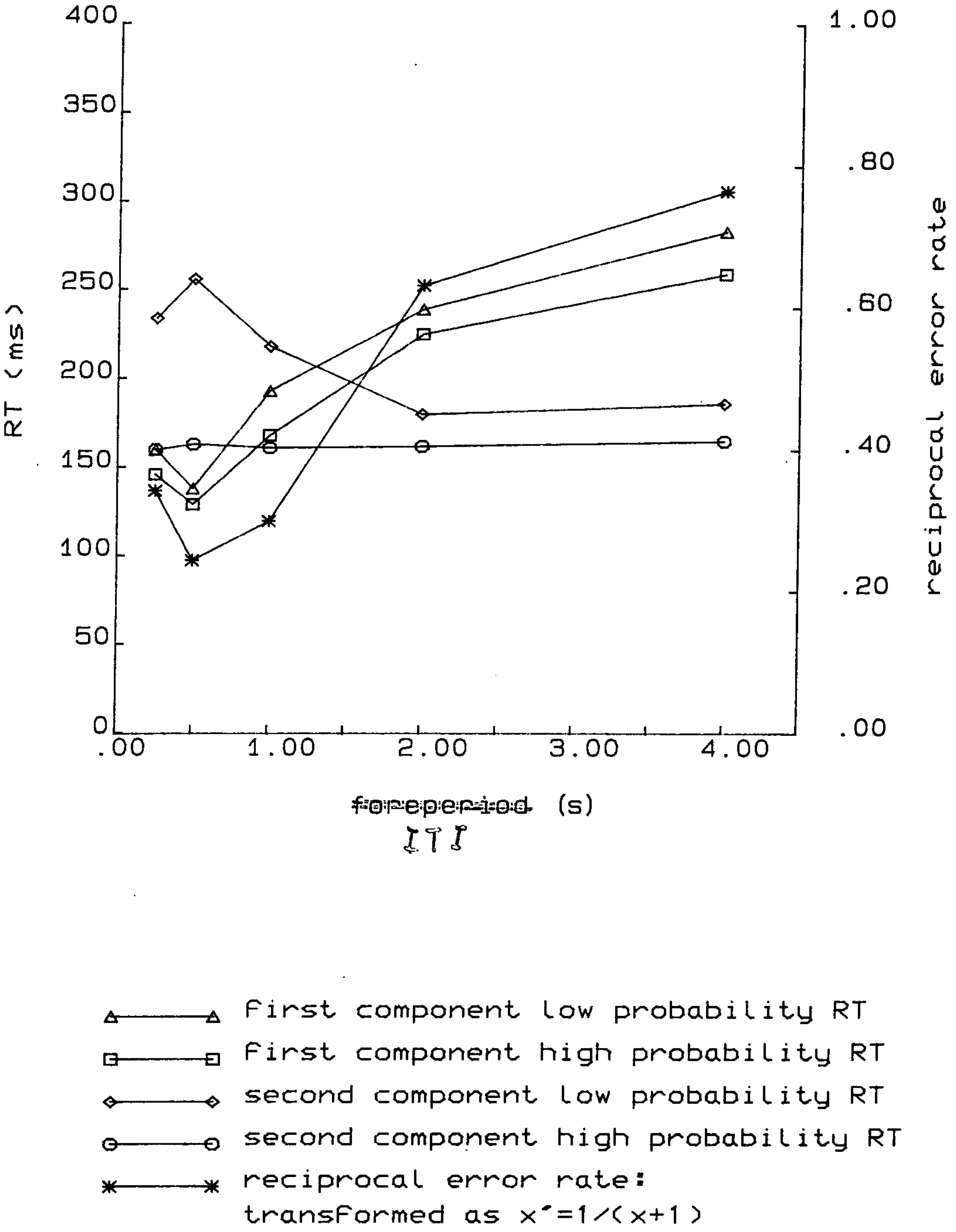


Fig. 4.1 Mean RTs and reciprocal errors for Experiment CRT22.

trials were regarded as replicates nested within Subjects x ITI, with the Stimulus Frequency factor as a repeated measure.

The first-component analysis is summarised in Table A.2.1 and the means are given in Table 4.1. The three-way interaction was not significant, but two of the two-way interactions were.

Firstly, the Subjects x ITI interaction was highly significant ($F = 3.092$; $df = 12, 155$; $p < 0.001$). For three subjects, the curves were bitonic (as defined by Ferguson, 1965) with minima at the 500 ms ITI. The fourth subject (PE) showed virtually the same overall first-component RT for both the 250 ms and the 500 ms ITI.

Secondly, the Subjects x Stimulus Frequency interaction was also highly significant ($F = 24.539$; $df = 3, 155$; $p < 0.001$). For all subjects, the low-frequency RT was longer than the high-frequency RT but the difference ranged from 9 ms for subject AF to 38 ms for subject PE.

Three main effects were also significant. The Subjects effect ($F = 41.426$; $df = 3, 155$; $p < 0.001$) was not readily interpretable, given the presence of the Subjects x ITI interaction. The ITI effect, shown graphically in Fig. 4.1, ($F = 72.306$; $df = 4, 12$; $p < 0.01$) was reasonably representative of all the subjects, in spite of the presence of the interaction. Finally, the Stimulus Frequency effect ($F = 13.028$; $df = 1, 3$; $p < 0.05$) indicated that low-frequency first-component RTs were longer than their high-frequency counterparts, as mentioned earlier.

Subject	ITI(ms)				
	250	500	1000	2000	4000
PE	166	175	212	251	266
NB	198	174	210	273	304
PKW	126	122	159	224	259
AF	149	125	192	249	300

Responses to Low Frequency Stimuli

Subject	ITI(ms)				
	250	500	1000	2000	4000
PE	147	138	166	204	228
NB	176	161	190	247	277
PKW	107	97	142	208	240
AF	153	122	174	237	286

Responses to High Frequency Stimuli

Table 4.1 Means in ms for first component times in Experiment CRT22.

The second-component analysis is summarised in Table A.2.1 and the means appear in Table 4.2. The three-way interaction was significant (F = 2.770; df = 12, 155; $p < 0.01$). Visually, the most striking features of this interaction were that subjects differed in the difference between their low- and high-frequency RTs at the 2000 and 4000 ms ITIs.

All the two-way interactions were significant. However, the Subjects x ITI interaction was not interpretable because of the presence of the three-way interaction.

The Subjects x Stimulus Frequency interaction was highly significant (F = 12.869; df = 3, 155; $p < 0.001$). This was due to greater inter-subject variation in the high-frequency RTs than the low-frequency ones.

Finally, the ITI x Stimulus Frequency interaction was significant (F = 14.215; df = 4, 12; $p < 0.05$). At this stage, it will suffice to say that this was due to greater inter-ITI variation in the low-frequency RTs than the high-frequency ones.

Two main effects were also significant. The Subjects effect was not interpretable because of the presence of the higher order interactions. The Stimulus Frequency effect (F = 40.537; df = 1, 3; $p < 0.01$) indicated that the low-frequency second-component RTs were longer than their high-frequency counterparts, as was the case with the first-component scores.

	ITI(ms)				
Subject	250	500	1000	2000	4000
PE	215	232	190	177	195
NB	223	245	240	183	202
PKW	254	262	214	169	177
AF	241	282	223	191	170

Responses to Low Frequency Stimuli

	ITI(ms)				
Subject	250	500	1000	2000	4000
PE	122	126	132	138	138
NB	159	164	160	163	174
PKW	148	155	162	164	169
AF	206	204	185	180	178

Responses to High Frequency Stimuli

Table 4.2 Means in ms for second component times in Experiment CRT22.

4.3.2.3 Trend Tests On RT Scores -

Because the principal interest in this experiment lay in the differential effect of ITI on RTs to the two different stimuli, it was decided to perform a series of one-way analyses of variance, with ITI as the independent variable, followed by trend tests. Thus the RTs for each combination of component and stimulus frequency were examined separately for each subject.

Because small changes in ITI had a greater effect on RT at short ITIs than at long ones, it was thought more appropriate to use a logarithmic time scale for the ITI values. This facilitated the trend calculations because it transformed the ITI scale to a linear one.

The F ratios and their significance levels are not given in the text in this sub-section because their number would have rendered the text far too turgid. However, the results are given in full in Tables A.2.2 to A.2.7.

The first-component high-frequency RTs displayed a high degree of consistency between subjects. All the curves were bitonic, with minima at the 500 ms ITI. In all cases, the linear, quadratic and cubic components were significant. The total high-frequency RTs gave curves of the same general shape but not as many of the components were significant.

The second-component high-frequency scores showed greater variability between subjects. Two subjects (PE and PKW) yielded significant linear components with RT increasing monotonically with increasing ITI. Subject NB did not obtain any significant trends, nor even overall significance in the analysis of variance. However, there was a general upward trend, though not a monotonic one. The remaining subject (AF) yielded a significant linear component with, curiously, a monotonically decreasing trend.

For three of the subjects, the first-component low-frequency RTs followed the same pattern as their high-frequency counterparts. The other subject (PE) showed a monotonically increasing curve, with significant linear and cubic components.

Total low-frequency RT tended to increase with increasing ITI. All subjects obtained significant linear components. However, only subject AF yielded a monotonic curve, while subject PKW obtained a significant quadratic component.

The second-component low-frequency scores displayed functions which were tritonic (as defined by Ferguson, 1965) for three of the subjects, with maxima at the 500 ms ITI and minima at the 2000 ms mark. Each of these subjects obtained significant linear and cubic components. The fourth subject (AF) obtained a bitonic curve with a maximum at the 500 ms ITI. In his case, the linear, quadratic and cubic components were all significant.

4.3.2.4 Analysis Of Reciprocal Error Scores -

The raw error scores displayed substantial heterogeneity of between-ITI variance and the standard deviations appeared to be approximately proportional to the ITI means. For these reasons, the reciprocal transformation $X' = 1/(X+1)$ was applied, as recommended by Kirk (1968). Initially, the low-frequency reciprocal error scores

Subject	ITI(ms)				
	250	500	1000	2000	4000
PE	0.153	0.227	0.231	0.688	0.938
NB	0.291	0.241	0.322	0.427	0.513
PKW	0.123	0.202	0.339	0.602	0.833
AF	0.778	0.300	0.295	0.815	0.796

Table 4.3 Mean reciprocal error rates for low frequency stimuli in Experiment CRT22.

were examined using a Subjects x ITI analysis of variance, with Subjects treated as a random effect. The summary table is presented in Table A.2.8 and the means are given in Table 4.3. The Subjects x ITI interaction was significant ($F = 4.042$; df = 12, 155; $p < 0.001$). Both main effects were also significant but were not interpretable because of the presence of the interaction.

Because of the presence of a significant Subjects x ITI interaction, separate one-way analyses of variance were also carried out on the low-frequency error rates for each subject, followed by trend tests. In order to facilitate comparisons with the RT scores, the same logarithmic time scale was used for the ITIs. The results appear in Tables A.2.9. All subjects showed significant linear components (with reciprocal errors increasing with ITI). Subject PE obtained a significant quadratic component, although the curve itself had no turning points. Conversely, subject NB produced a bitonic curve (with reciprocal errors having their minimum value at the 500 ms ITI) even though the quadratic component was not significant. Subject AF produced a curve with two turning points, the minimum reciprocal error rate being at the 1000 ms ITI and the maximum at 2000 ms. In this case, both the quadratic and cubic trend components were significant.

4.3.2.5 Partial Correlations -

Because of the similarity of shape between the reciprocal error curves (when inverted) and the low-frequency second-component RTs, it was decided to examine the partial correlations between each type of RT score and ITI, with errors partialled out. These statistics were computed separately for each subject and evaluated with two-tailed tests of significance. They are shown in Table 4.4.

With reciprocal errors partialled out, all four subjects showed substantial positive correlations between both low- and high-frequency first-component RT and ITI. The partial correlation coefficients concerned all lay between 0.661 and 0.861 and all were significant at the 0.001 level.

The partial correlations between high-frequency second-component RT and ITI were low, positive and non-significant for three subjects but somewhat larger and negative for the remaining subject (AF).

On the other hand, the partial correlations between low-frequency second-component RT and ITI were negative for all subjects and ranged from -0.249 to -0.639 and three of them were significant at, or beyond, the 0.05 level. When reciprocal errors were not partialled out, these correlations were all somewhat larger.

The partial correlations between total RT and ITI were positive for both low- and high-frequency RTs for all subjects. The values ranged from 0.211 to 0.778. Six of the eight correlations reached the 0.001 level of significance.

Three out of the four correlations between second-component low-frequency RTs and reciprocal errors (with ITI partialled out) were negative. The values ranged from 0.021 to -0.365 and two of them reached the 0.05 level of significance.

	Correlation with		Partial Correlation with	
RT Component	ITI	R	ITI/R	R/ITI
First Low	0.896***	0.644***	0.823***	-0.211
First High	0.824***	0.655***	0.661***	0.027
Second Low	-0.360*	-0.270	-0.249	0.021
Second High	0.344*	0.277	0.213	0.012
Total Low	0.533***	0.374*	0.417**	-0.084
Total High	0.742***	0.591***	0.558***	0.021

Subject PE

	Correlation with		Partial Correlation with	
RT Component	ITI	R	ITI/R	R/ITI
First Low	0.823***	0.460***	0.795***	0.322*
First High	0.812***	0.453**	0.782***	0.307*
Second Low	-0.406**	-0.427**	-0.302*	-0.332*
Second High	0.279	0.053	0.278	-0.050
Total Low	0.648***	0.201	0.630***	-0.037
Total High	0.809***	0.424**	0.778***	0.255

Subject NB

Key - R: reciprocal error rate.
Partial Correlation: variable after '/' is partialled out.

Table 4.4a Pearson product moment correlations and partial correlations for Experiment CRT22.

RT Component	Correlation with		Partial Correlation with	
	ITI	R	ITI/R	R/ITI
First Low	0.894***	0.726***	0.778***	0.260
First High	0.905***	0.792***	0.788***	0.473***
Second Low	−0.733***	−0.540***	−0.588***	−0.025
Second High	0.484***	0.518***	0.186	0.278
Total Low	0.429**	0.410**	0.211	0.161
Total High	0.882***	0.793***	0.733***	0.479***

Subject PKW

RT Component	Correlation with		Partial Correlation with	
	ITI	R	ITI/R	R/ITI
First Low	0.853***	0.459**	0.861***	0.504***
First High	0.843***	0.498***	0.860***	0.569***
Second Low	−0.663***	−0.423**	−0.639***	−0.365*
Second High	−0.560***	−0.036	−0.568***	0.121
Total Low	0.623***	0.264	0.598***	0.151
Total High	0.726***	0.534***	0.730***	0.541***

Subject AF

Key - R: reciprocal error rate.
Partial Correlation: variable after '/' is partialled out.

Table 4.4b Pearson product moment correlations and partial correlations for Experiment CRT22.

Seven out of the eight correlations between the first-component scores and reciprocal errors (with ITI partialled out) were positive. The values ranged from -0.211 to 0.569. Three of them were significant at the 0.001 level and a further two at the 0.05 level.

Partial correlations between total high-frequency RT and reciprocal errors were very similar in magnitude to the corresponding correlations obtained between first-component high-frequency RT and reciprocal errors (with ITI partialled out). None of the partial correlations between total low-frequency RT and reciprocal errors was significant.

4.3.3 Discussion

Perhaps the most important issue is why the error rate was related to the ITI. A distinct possibility is that some of the responses were initiated before the stimulus information had been processed. If the subject's ability to do this were dependent on his being able to synchronise predetermined responses with the stimulus events, then this synchronisation would be easier for short foreperiods than for longer ones. If it is assumed that, in this experiment, the ITI functioned in much the same manner as a foreperiod, then the proportion of stimuli to which these preprogrammed responses were made would decrease with increasing ITI. Of course, the preprogrammed responses would tend to be much faster than conventional responses and would usually be directed to the high-frequency response. Such responses could not be accurate beyond chance level.

This hypothesis would explain the occurrence of the "partial error" responses made to the low-frequency stimulus, which were mentioned earlier. These "partial errors" could also have been responsible for the negative intra-block correlations found between the first- and second-component RTs. Thus "partial errors" would have the short first-component RTs typical of all preprogrammed responses, whereas the second-component RTs would be unusually long. By comparison, conventional responses would have longer first-component times and shorter second-component times.

A further point can also be made with respect to the error scores. The fact that two subjects obtained lower reciprocal error scores with ITIs of 500 ms than with ITIs of 250 ms suggests the existence of some sort of refractory effect, whereby fewer preprogrammed responses were made at the shortest ITI because of the difficulty in initiating them so soon after completing the return movement of the preceding response. This, in turn, would help to explain the bitonic shape of so many of the RT curves which showed either minima or maxima at the 500 ms ITI.

In view of the similarity in shape between the reciprocal error curves and the second-component low-frequency responses, it seems likely that these two variables are causally related. If we accept the model proposed in the previous paragraph, it is not difficult to see how this could be the case. With a long motor movement, it is possible for those preprogrammed responses which start off as incorrect to be corrected during the hand movement, albeit at the cost of a lengthened movement (and consequently a longer second-component RT). It is to be expected that the rate of occurrence of these "partial errors" would be positively correlated with the actual error rate. Thus we would expect the second-component low-frequency RTs to be correlated positively with error rate, when ITI is partialled out. This is exactly what was found for each of the subjects, although the correlations were rather small.

The negative correlation between ITI and second-component low-frequency RT was somewhat attenuated by partialling out errors, although the fact of the residual correlation suggests that, with longer ITIs, more of the information processing was conducted during the first phase of the response than was the case with shorter ITIs.

The positive correlations between both low- and high-frequency first-component RTs and reciprocal errors (with ITI partialled out) would, of course, be expected under any model of speed-accuracy tradeoff. The quite substantial positive correlations obtained between both first-component scores and ITI (with reciprocal errors partialled out) suggest that the usual foreperiod effect (see Chapter 2) was operating on both low- and high-frequency first-component RTs (if the ITI is regarded as functioning as a foreperiod in the context of this experiment). However, the fact that both the first-component ITI effects were somewhat smaller when errors were partialled out suggests that there were two factors contributing to the ITI effect - one factor depending on errors and the other not. The latter factor would, presumably, correspond to the classical foreperiod effect. However, the other factor would appear to be due to the presence of preprogrammed responses in differing proportions with the different ITIs.

4.4 EXPERIMENT CRT24

4.4.1 Introduction

The previous experiment suggested that preprogrammed responses occur and that their frequency of occurrence depends on ITI. It was further suggested in the Discussion that the effect is mediated by the time estimation process. However, another logical possibility exists that the mediating process is one which depends on the time elapsed since the preceding trial. This experiment was undertaken to disentangle these two possible processes and examine their effects separately.

4.4.2 Method

4.4.2.1 Apparatus -

The apparatus used was the same as for Experiment CRT22 (see Section 4.3.1), with the addition of an extra box, containing a single amber warning light of the same type as those in the SR box.

4.4.2.2 Task -

The event sequence for a single trial was the same as for Experiment CRT22, except that an extra step - the foreperiod - was included. As soon as the ITI had expired, the computer presented a warning signal by turning on the warning light, which stayed on for the duration of the foreperiod. One of the two possible stimuli was presented at the moment when the foreperiod terminated.

4.4.2.3 Design -

The block length, stimulus frequency ratio, run limits and inter-block rest pauses were as described for the previous experiment. Each principal session consisted of 12 blocks of trials and lasted approximately one hour. The blocks were of three different types. Type I and type II had foreperiods of 500 ms, whereas type III had a longer foreperiod of 2500 ms. Types I and III had ITIs of 500 ms and type II had a longer ITI of 2500 ms. Thus types I and II had the same foreperiod but in type II blocks a longer time elapsed between terminating one response and beginning the next. This elapsed time was the same for types II and III but the latter had a longer foreperiod. The three types of block were arranged randomly within each session, with the constraint that each of the three types occurred exactly once in the first three blocks, the second three blocks and so on. All the trials in a given block were of the same type.

4.4.2.4 Subjects -

It had been intended to run four subjects for six principal sessions each. Unfortunately, one subject (MJS) was, for personal reasons, only able to complete four sessions. Because of this, it was decided to run another subject in his place, for a full six sessions. However, it was later decided to include the data from subject MJS in the statistical analysis. Thus five subjects were employed in this experiment. All were male students (aged between 19 and 21) from Durham University. They were each paid for all the principal sessions at the rate of 20 pence per session. Subjects DHR and CJS were run with the high-frequency signal located on the left and subjects MHE, MJS and PAT were run with it located on the right.

4.4.2.5 Procedure -

The principal sessions were preceded by a full training session for each subject. At this session, a set of verbatim instructions (see Appendix 1) was read to the subject by the experimenter, who then asked if any clarification was required. The experimenter then ran the first three blocks using himself as subject for demonstration purposes. The experimenter then answered any further queries and the subject was run for three blocks, under supervision. Any errors of procedure were pointed out to the subject at this stage and any remaining queries were answered. Finally, the subject was run for the remaining six blocks without supervision and the training session was ended. The principal sessions followed. As far as possible, these were run at the rate of one per day for each subject.

4.4.2.6 Data Reduction -

This was similar to the previous experiment. Thus each subject produced a set of scores for each block type, for each principal session. Each set of scores comprised the mean RT for each combination of component and stimulus frequency. The reduced data were then transferred to the NUMAC system for analysis.

4.4.3 Results

4.4.3.1 Observations -

As in Experiment CRT22, the so-called "partial errors" were reported by subjects on some occasions when the low-frequency stimulus occurred. Another similarity with the previous experiment was that the intra-block correlations between the first- and second-component RTs were negative. The average RT and error results are displayed in Fig. 4.2.

4.4.3.2 Analysis Of Variance On RT Scores -

Initially, two Subjects x Block Type x Stimulus Frequency analyses of variance were carried out. As in the previous experiment, one analysis was performed on the first-component scores and the other on the second-component scores. Both analyses utilised the same type of model as that in the previous experiment.

The first-component analysis is summarised in Table A.2.10 and the means are given in Table 4.5. The only significant interaction was Subjects x Stimulus Frequency ($F = 22.492$; $df = 4, 69$; $p < 0.001$). For all subjects, the low-frequency RT was longer than the high-frequency RT but the difference ranged from 23 ms to 56 ms.

The three main effects were also significant. The Subjects effect ($F = 37.789$; $df = 4, 69$; $p < 0.001$) gave a clear indication of overall RT differences between subjects. (In spite of the existence of the interaction just mentioned, the rank ordering of the subjects was the same for both low- and high-frequency RT).

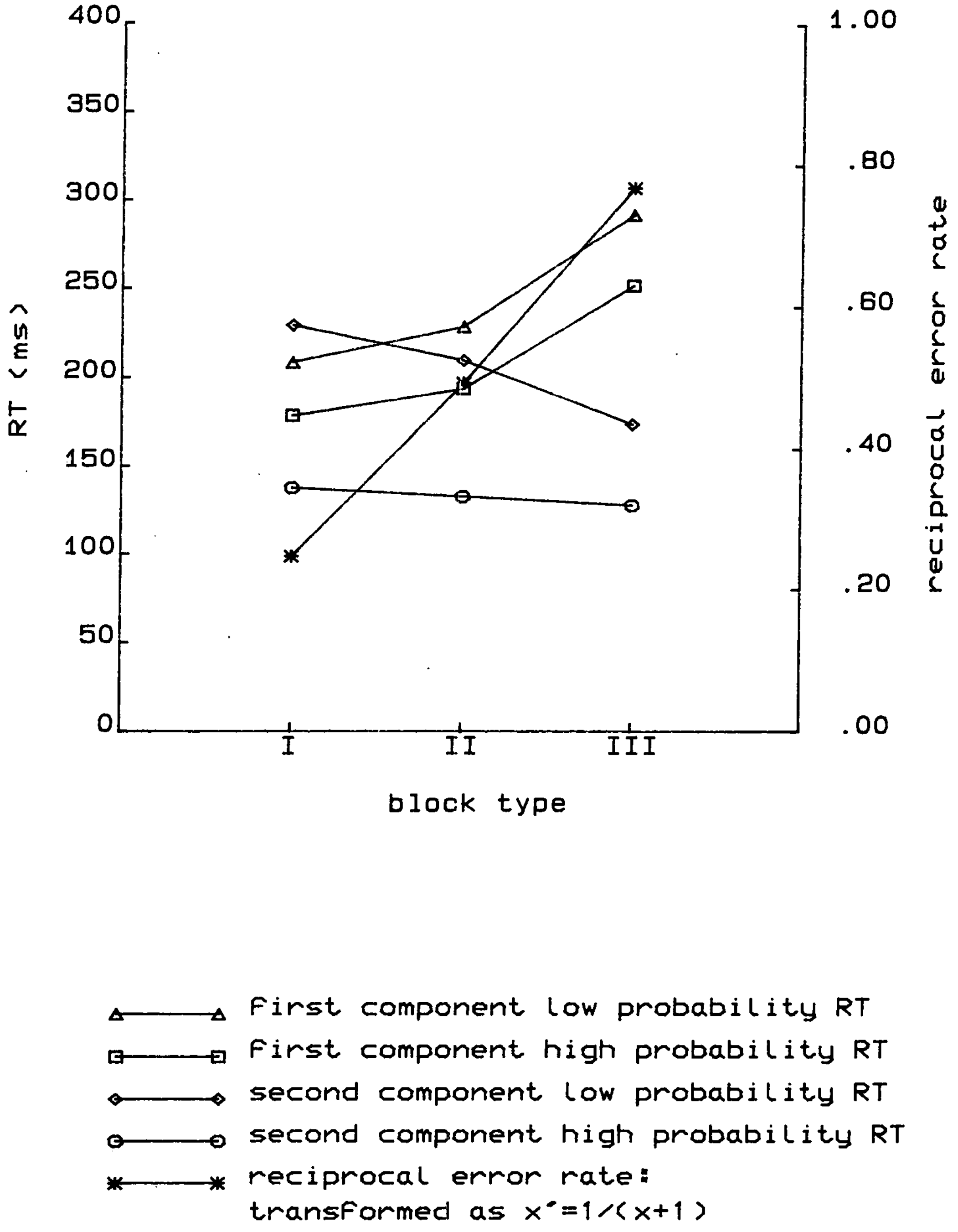


Fig. 4.2 Mean RTs and reciprocal errors
for Experiment CRT24.

The Block Type effect ($F = 34.683$; $df = 2, 8$; $p < 0.01$) was associated with means of 194, 212 and 272 ms for block types I, II and III respectively.

Finally, the Stimulus Frequency effect ($F = 20.443$; $df = 1, 4$; $p < 0.05$) indicated that the low-frequency first-component RTs were longer than the high-frequency ones.

The second-component analysis is summarised in Table A.2.10 and the means appear in Table 4.5. Two of the two-way interactions were significant.

Firstly, the Subjects x Block Type interaction ($F = 2.275$; $df = 8, 69$; $p < 0.05$) shows that subjects differed with respect to their inter-block type differences.

Secondly, the Subjects x Stimulus Frequency interaction ($F = 4.372$; $df = 4, 69$; $p < 0.01$) indicates some degree of inter-subject difference in the Stimulus Frequency effect.

The three main effects were also significant. The Subjects effect was not interpretable because of the presence of the interactions.

The Block Type effect ($F = 10.892$; $df = 2, 8$; $p < 0.05$) was associated with means of 184, 171 and 151 ms for block types I, II and III respectively. However, the general picture was that RTs tended to be shorter in type II blocks than in type I blocks and were shorter still in type III blocks.

	Low Frequency			High Frequency		
Subject	I	II	III	I	II	III
CJS	179	209	275	164	186	244
PAT	289	312	354	239	254	292
DHR	152	143	234	128	125	208
MHE	186	224	304	171	201	272
MJS	251	271	298	198	210	245

First Component Times

	Low Frequency			High Frequency		
Subject	I	II	III	I	II	III
CJS	248	197	151	136	123	119
PAT	187	167	158	131	129	124
DHR	247	256	172	151	150	145
MHE	239	211	187	125	121	120
MJS	231	221	214	151	147	136

Second Component Times

Key - I: Block Type I
II: Block Type II
III: Block Type III

Table 4.5 Means in ms for reaction times in Experiment CRT24.

Lastly, the Stimulus Frequency effect ($F = 81.691$; $df = 1, 69$; $p < 0.01$) showed the usual overall tendency for the low-frequency RTs to be longer than the high-frequency ones.

4.4.3.3 Planned Comparisons On RT Scores -

As in Experiment CRT22, it was decided to conduct a series of one-way analyses of variance. Thus the RTs for each combination of component and stimulus frequency were examined separately for each subject. Each analysis of variance was followed by two planned comparisons. The first of these examined the effect of foreperiod length (with trial separation time held constant), while the second tested for an elapsed time effect (with foreperiod held constant). The comparisons were not orthogonal but were justified on the grounds that they asked the required questions of the data and, indeed, were the only meaningful comparisons that could have been made. The results are given in Tables A.2.11 to A.2.16.

Dealing firstly with the first comparisons, all subjects showed a significant increase in first-component high-frequency RT from block type II to type III. The first comparisons for total high-frequency RT gave similar results, though that for subject MJS did not quite reach significance. On the other hand, second-component high-frequency RT showed a decrement from block type II to type III, for all subjects. However, this was only significant for subject MJS. The low-frequency first comparisons showed exactly the same tendencies as their high frequency counterparts, though fewer were significant.

As regards the second comparisons, it was generally the case that RT showed a change from block type I to type II in the same direction as that from type II to type III but of much smaller magnitude. In a few cases, the change was opposite in direction but not significant.

4.4.3.4 Analysis Of Reciprocal Error Scores -

As in the previous experiment a Subjects x Block Type analysis of variance was performed using transformed low frequency error scores. (As before, the transformation $X' = 1/(X+1)$ was applied and the Subjects factor was treated as a random effect). The summary table is presented in Table A.2.17 and the means are given in Table 4.6. The interaction was not significant but both main effects were: Subjects ($F = 14.927$; $df = 4, 69$; $p < 0.001$) and Block Type ($F = 23.151$; $df = 2, 8$; $p < 0.001$). A one-way analysis of variance was then carried out on the transformed scores, followed by two comparisons, as described in the previous section. The results are given in Table A.2.18. In this case, errors decreased from block type I to type II and also from type II to type III, both effects being significant.

4.4.4 Discussion

The increase from block type II to type III for both low- and high-frequency first-component RTs is indicative of the classical foreperiod effect. The corresponding decrease for the second-component low-frequency RTs, taken in conjunction with the

Subject	Block Type I	 II	 III
CJS	0.152	0.246	0.722
PAT	0.528	0.833	1.000
DHR	0.172	0.476	0.750
MHE	0.220	0.589	0.917
MJS	0.118	0.232	0.290

Table 4.6 Mean reciprocal error rates for low frequency stimuli in Experiment CRT24.

decrease in error rate, is attributable to a reduction in the proportion of preprogrammed responses.

Thus it seems that the time estimation hypothesis for the generation of preprogrammed responses is upheld. However, the similar changes between block types I and II suggest the possibility of a trial separation time effect also operating. However, an alternative explanation for this phenomenon could be the time estimation effect operating at a weaker level. It is possible that two short preparation periods, both of the same duration, provide a better basis for time estimation than a short period preceded by a longer one.

4.5 EXPERIMENT CRT27

4.5.1 Introduction

The purpose of this experiment was to provide a further test of the time estimation hypothesis by comparing the effects of fixed versus variable length ITIs. The principal underlying hypothesis was that, because time estimation would obviously be easier under fixed ITI conditions than variable ones, more preprogrammed responses would be made in the former condition. This, in turn, would mean that:

1. More errors would be made in the fixed ITI condition than in the variable one.

2. Second-component low-frequency RT would be longer with constant ITIs than with variable ones, because there would be more "partial errors".

4.5.2 Method

4.5.2.1 Apparatus And Task -

The apparatus and event sequence were exactly the same as those used in the first experiment.

4.5.2.2 Design -

Trials were arranged in blocks of 72. As in the two preceding experiments, stimuli occurred with a 5:1 frequency ratio. Another constraint on the stimulus sequence ensured that the first 24 stimuli in each block contained exactly 20 of the high-frequency signals. (These were intended as "preparation trials"). More of these were used than in the previous experiments in order to ensure that the subject would correctly identify, and adapt to, the blocks with variable ITIs before the 48 main trials started). Run limits were imposed on the sequence of stimuli so that low-frequency signals would not occur more than three times in succession and high-frequency signals more than 25 times in succession. The alternation run limit was five.

Three different types of block were used. Types I and II used constant ITIs of 500 and 2000 ms, respectively. The third type used a variable ITI. ITIs in this type of block were of three different lengths (500, 2000 and 3500 ms), mixed in a random order, with relative frequencies of occurrence in the ratio 2:1:1, thus giving the same conditional probability of occurrence (of 0.5) for the two shorter ITIs. In blocks of the third type, the ITIs were arranged so that they occurred with exactly the expected frequencies for each of the stimuli (assuming independence of the variables concerned). In addition, run limits were imposed on the ITI sequence in these blocks. The low-frequency ITIs had run limits of three and the high-frequency ITIs, run limits of seven. The alternation run limit was seven, as was the run limit for unspecified low-frequency ITIs. Each session comprised 16 blocks of trials, randomly arranged so that one each of types I and II and two of type III occurred in the first four blocks, the second four blocks and so on. Each session lasted approximately one hour.

It had been intended to run four subjects for seven principal sessions each. However, due to shortage of time, only one subject (NT) was run for seven sessions. The other three subjects were run for six sessions each.

4.5.2.3 Subjects -

Four male subjects (aged between 16 and 19) were employed in this experiment. They were each paid for all the principal sessions at the rate of 20 pence per session. Subjects PB and GW were run with the high-frequency signal located on the left and subjects NT and DA were run with it located on the right.

4.5.2.4 Procedure -

The principal sessions were preceded by a three-quarter length training session for each subject. This session was carried out in much the same way as the three-stage procedure described for the previous experiment, except that each stage consisted of four blocks of trials. For the training session alone, each group of four blocks was arranged in the order: type I, type III, type II, type III.

4.5.2.5 Data Reduction -

This was similar to the two previous experiments. Thus each subject produced a set of scores for each ITI type and length combination, for each principal session. Sets of scores comprised the mean RT for each component-stimulus frequency combination. As in the previous experiments, the reduced data were transferred to the NUMAC system for analysis.

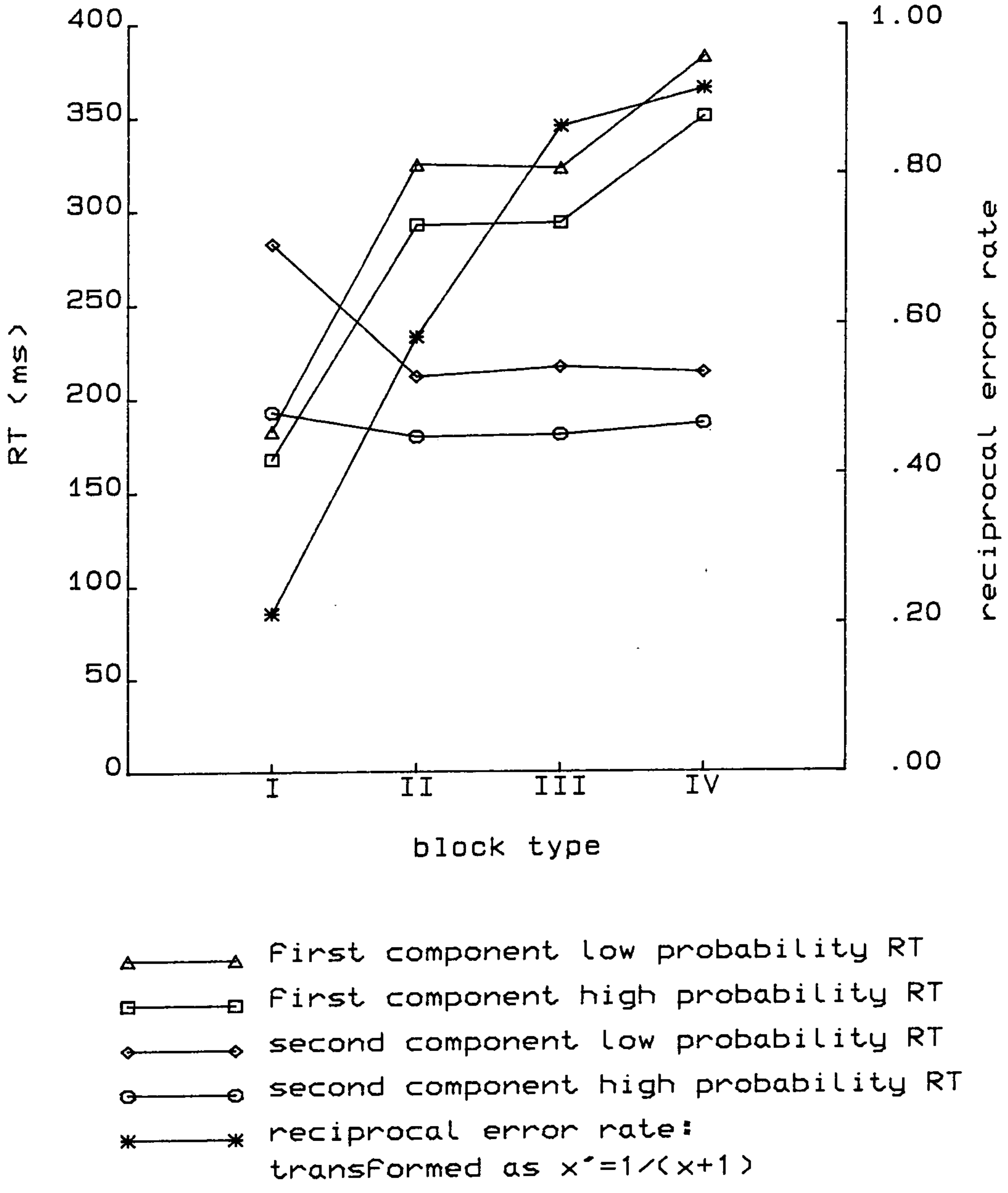
4.5.3 Results

4.5.3.1 Observations -

As in the two previous experiments, "partial errors" were reported by subjects on some occasions when the low-frequency stimulus occurred. The usual negative intra-block correlations between the first- and second-component RTs were also found. The RT and error results (averaged over all subjects) are shown in Fig. 4.3.

4.5.3.2 Analysis Of Variance On RT Scores -

Two Subjects x ITI Type x ITI Length x Stimulus Frequency analyses of variance were performed, one on the first-component RTs and one on the second. The analyses were similar to those carried out for the previous experiments, with the extra factor (ITI Length)



I constant foreperiod: 500 ms
II constant foreperiod: 2 s
III variable foreperiod: 500 ms
IV variable foreperiod: 2 s

Fig. 4.3 Mean RTs and reciprocal errors for Experiment CRT27.

regarded as a between-block factor for the purposes of analysis. However, it was really only a between-block factor in the constant ITI blocks and was varied within each variable-ITI block. (Only the two shorter ITI lengths were used from the variable-ITI blocks, for these analyses).

The summary table for the first-component analysis of variance is given in Table A.2.19 and the means are shown in Tables 4.7. The four-way interaction was not significant.

The Subjects x ITI Type x ITI Length interaction was the only significant three-way interaction ($F = 7.035$; $df = 3, 84$; $p < 0.001$). It shows that there was less inter-subject variability in the difference in RT for the different ITI lengths for the variable ITIs than for the constant ones.

Two of the two-way interactions were significant but only the Subjects x Stimulus Frequency interaction was interpretable ($F = 12.947$; $df = 3, 1$; $p < 0.001$). This interaction shows that there is a moderate degree of variability between subjects as regards the relative speed of low- and high-frequency responses.

All the main effects were significant. The Subjects effect was not meaningful because of the presence of the interactions.

However, in spite of the presence of the interactions inspection showed the remaining main effects to be representative at all levels of the other variables.

	Low Frequency				High Frequency			
Subject	I	II	III	IV	I	II	III	IV
PB	210	307	323	413	204	271	293	365
DA	188	423	373	445	156	368	328	400
GW	189	335	335	375	188	323	313	367
NT	151	250	269	310	130	220	247	283

First Component Times

	Low Frequency				High Frequency			
Subject	I	II	III	IV	I	II	III	IV
PB	229	149	138	155	164	152	146	157
DA	347	267	281	260	195	189	198	186
GW	276	186	191	188	236	195	202	210
NT	281	241	251	247	180	183	180	194

Second Component Times

Key - I: 500 ms ITI from constant ITI blocks
II: 2000 ms ITI from constant ITI blocks
III: 500 ms ITI from variable ITI blocks
IV: 2000 ms ITI from variable ITI blocks

Table 4.7 Means in ms for reaction times in Experiment CRT27.

The ITI Type effect was highly significant ($F = 674.797$; df = 1, 3; $p < 0.001$). Variable ITIs produced mean first-component RTs of 338 ms, compared with 242 ms for the constant ITIs.

The ITI Length effect was less dramatically significant ($F = 30.307$; df = 1, 3; $p < 0.001$). The short ITIs yielded first-component RTs of 242 ms, as opposed to 338 ms for the long ITIs. (That these means are exactly the same as those for the previously mentioned effect is merely a coincidence).

Finally, the Stimulus Frequency effect ($F = 15.647$; df =1, 3; $p < 0.05$) was associated with means of 304 ms for the low-frequency first-component RTs and 276 ms for the corresponding high-frequency ones.

The summary table for the second-component analysis of variance is given in Table A.2.20 and the means are given in Tables 4.7. The four-way interaction was not significant.

The ITI Type x ITI Length x Stimulus Frequency interaction was the only significant three-way interaction ($F = 20.500$; df = 3, 84; $p < 0.05$). It shows that, for the low-frequency RTs, the short, constant ITI condition produced a much greater RT than the other three conditions. This was not the case for the high-frequency second-component responses.

Four of the two-way interactions and three of the main effects were significant but, as with the first-component scores, only the Subjects x Stimulus Frequency interaction was interpretable ($F = 66.658$; df = 3, 84; $p < 0.001$). This interaction shows that there was a substantial degree of variability between subjects on the difference between the low- and high-frequency RT. Thus the two means for subject GW were virtually the same, at around 210 ms, whereas subject DA obtained a mean low-frequency RT of 289 ms and a mean high-frequency RT of 192 ms.

4.5.3.3 Analysis Of Reciprocal Error Scores -

As in the two previous experiments, the transformation X' = 1/(X+1) was applied to the error scores. (Each of the scores from the ~~long,~~ variable ITI condition was first multiplied by ~~a~~ an appropriate factor ~~of two~~ in order that the error scores for the different conditions could be regarded as being based on equal numbers of scores). A Subjects x Block Type x Block Length analysis of variance was then carried out on the transformed low frequency error scores. The summary table appears in Table A.2.21 and the means are given in Table 4.8.

The three-way interaction was not significant but ~~one~~ two of the two-way interactions ~~was~~ were. The Subjects x Block Type interaction (F = ~~2.902~~ 3.160; df = 3, 84; p < 0.05) shows that, although all subjects obtained higher scores (i.e. fewer errors) in the variable ITI condition than in the constant ITI blocks, the difference varied between subjects. The Block Type x ITI Length interaction was also significant (F = 11.434; df = 1, 3; p < 0.05) and was due to the effect of ITI length being more pronounced with the constant ITI blocks than with the variable ones.

~~All three~~ Two main effects were also significant, although the Subjects effect was not interpretable because of the presence of the interaction.

The Block Type effect was clearly significant (F = ~~38.954~~ 31.298; df = 1, 3; p < ~~0.01~~ 0.05), with a lower error rate for the variable ITI conditions than for the fixed ITI blocks.

	Block Type			
Subject	I	II	III	IV
PB	0.212	0.458	1.000	1.000
DA	0.155	0.875	~~0.806~~ 0.756	~~0.889~~ 0.771
GW	0.400	0.833	1.000	1.000
NT	0.103	0.226	~~0.680~~ 0.867	~~0.790~~ 0.759

Key - I: 500 ms ITI from constant ITI blocks
II: 2000 ms ITI from constant ITI blocks
III: 500 ms ITI from variable ITI blocks
IV: 2000 ms ITI from variable ITI blocks

Table 4.8 Mean reciprocal error rates for low frequency stimuli in Experiment CRT27.

~~Lastly, the ITI Length effect (F = 10.427; df = 1, 3; p < 0.05) indicated that longer ITIs produced lower error rates.~~

4.5.4 Discussion

The first hypothesis put forward in Section 4.5.1 was fully supported by the results. Fewer errors were made under the variable ITI condition than in the fixed ITI blocks. It should also be noted that the length of the ITI had an effect on errors that was entirely consistent with the results of the two previous experiments.

The second hypothesis proposed was only partially supported by the results. The second-component low-frequency RT was indeed longer under constant ITIs than with variable ones. However, this difference was located entirely on the short ITIs. Presumably, the error rate was too low under all conditions except the 500 ms constant ITI to produce any appreciable difference in the RTs due to partial errors.

4.6 OVERALL CONCLUSIONS

It seems likely that preprogrammed responses do occur and this lends support to the fast guess model, which was first described in Chapter 3 and is dealt with in detail in the next chapter. Thus it is assumed that fast-guesses are directed principally towards the response corresponding to the high-probability stimulus. Furthermore, it is also assumed that the launching of these responses is governed by a time estimation process that relies partly on the warning signal for information on the passage of time. The higher the error rate is allowed to rise, the greater the number of fast-guesses that will be made and the more pronounced will be the effect of foreperiod length on the relative frequency effect.

CHAPTER 5: FAST GUESS MODELS

5.1 INTRODUCTION

As explained in Chapter 3, fast guess models of CRT postulate the existence of two types of response by the subject. He can make either an SCR (stimulus-controlled response) or an FG (fast guess). The former involve actually processing the stimulus information and achieving the correct response with a reasonably high degree of accuracy. Fast guesses, on the other hand, are made independently of the identity of the stimulus and hence are no more accurate than expected by chance. Because they do not involve any of the information processing required to distinguish between stimuli, they tend to be appreciably faster than SCRs.

Fast guess models have been studied in the past (see Section 4.5 in Chapter 3) for two reasons. Firstly, they offer a simple way of accounting for the effect of S-A (speed-accuracy) tradeoff (see Chapter 2). Secondly, they provide a method for calculating "corrections for fast-guessing" in CRT research, whereby effects due to FGs can be removed from data, leaving only the effects due to SCRs. It is principally the second application that is important in this thesis. However, the emphasis given here is somewhat different in that the FGs are regarded as interesting in their own right, rather than a mere source of contamination.

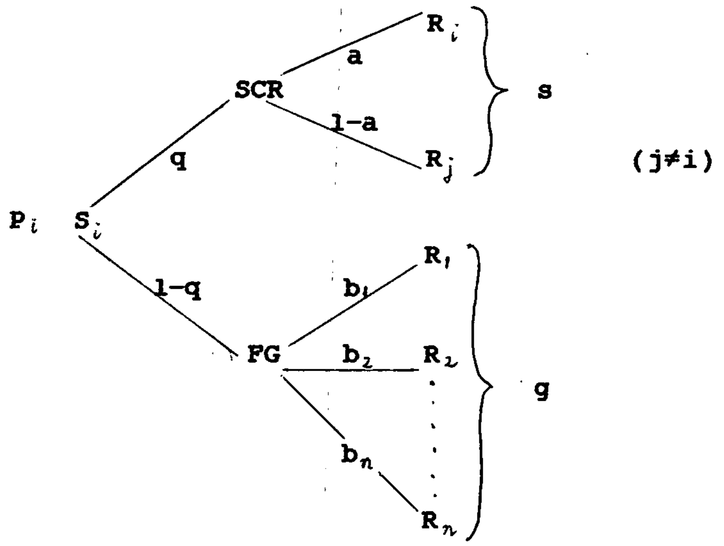
5.2 THE GENERAL FAST GUESS MODEL

5.2.1 Description

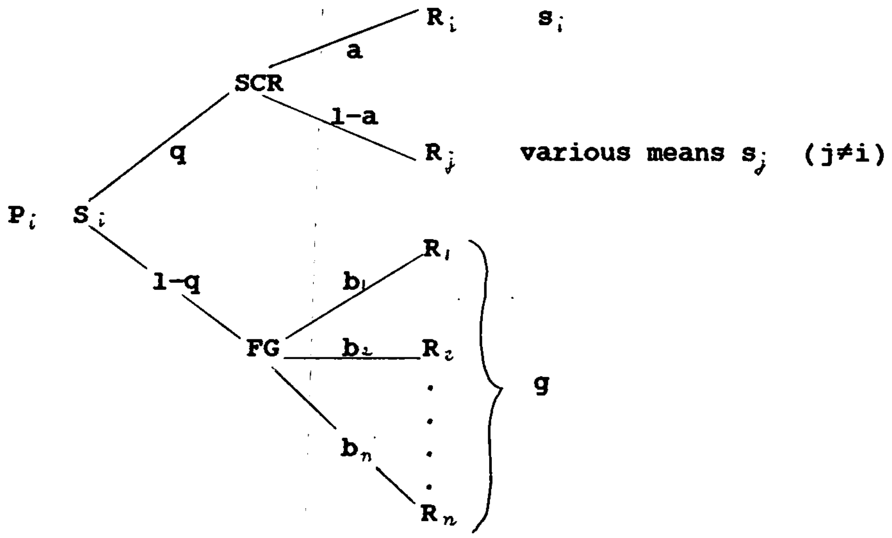
This model is applicable to any CRT experimental situation in which all the S-R mappings are 1:1. The model is essentially that provided by Yellott (1971). However, parts of the mathematics are somewhat different and the derivation goes somewhat farther in that it shows how the standard errors for the various parameters may be estimated. Fig. 5.1 gives a branching diagram for the model, which is interpreted as follows. There are 'n' stimuli (where 'n' > 1): $S_1 \ldots S_n$. Similarly, there are 'n' responses: $R_1 \ldots R_n$. The correct response to S_i is R_i. Stimulus S_i occurs with a probability of P_i. On a typical trial, the subject makes an SCR with a probability of 'q' and an FG with the complementary probability 1-q. If an SCR is made, then it is correct with a probability of 'a' and the RT is drawn from a distribution of mean 's'. Incorrect SCRs are assumed to have the same mean RT. On the other hand, if an FG is made, then the subject responds with one of the responses $R_1 \ldots R_n$. These are selected independently of the identity of the stimulus with probabilities $b_1 \ldots b_n$. Under these circumstances, response R_j is selected with probability b_j. FGs are thus only accurate at chance level. Both correct and incorrect FGs are drawn from a distribution of mean 'g'.

5.2.2 Derivation Of Equations

Using the information supplied in the previous section, some equations can be derived. In what follows, the notation p_{ij} will be



The General Fast-Guess Model



The Response Specific Fast-Guess Model

Fig. 5.1 Branching diagrams for the fast-guess models.

used to indicate the conditional probability of response 'j', given that the stimulus 'i' has already occurred. Also, M_{ij} will be used to represent the mean RT for response 'j', when made to stimulus 'i'. All summations are from 1 to 'n', unless otherwise stated.

For a correct response to the first stimulus:

$$p_{11} M_{11} = qas + b_1(1-q)g$$

Considering correct responses to all stimuli (and bearing in mind that $\Sigma b_i = 1$):

$$\Sigma(p_{ii} M_{ii}) = nqas + (1-q)g \quad [5.1]$$

For an incorrect response to the first stimulus:

$$\Sigma(p_{1j} M_{1j}) - p_{11} M_{11} = q(1-a)s + (1-b_1)(1-q)g$$

Considering incorrect responses to all stimuli:

$$\Sigma\Sigma(p_{ij} M_{ij}) - \Sigma(p_{ii} M_{ii}) = nq(1-a)s + (n-1)(1-q)g \quad [5.2]$$

Multiplying Equation 5.1 by (n-1) and subtracting Equation 5.2 gives:

$$n\Sigma(p_{ii} M_{ii}) - \Sigma\Sigma(p_{ij} M_{ij}) = nqs(na-1) \quad [5.3]$$

Considering the probability of a correct response to the first stimulus:

$$p_{11} = qa + b_1(1-q)$$

By summation:

$\Sigma p_{ii} = nqa + (1-q)$

Thus $q = (\Sigma p_{ii} - 1)/(na-1)$ [5.4]

Substituting for 'q' from Equation 5.4 into Equation 5.3:

$(n\Sigma(p_{ii} M_{ii}) - \Sigma\Sigma(p_{ij} M_{ij}))/n = (\Sigma p_{ii} - 1)s$ [5.5]

Adding Equations 5.1 and 5.2 gives:

$\Sigma\Sigma(p_{ij} M_{ij}) = nqs + n(1-q)g$ [5.6]

Substituting for 'q' from Equation 5.4 into Equation 5.6:

$\Sigma\Sigma(p_{ij} M_{ij})/n = (\Sigma p_{ii} - 1)(s-g)/(na-1) + g$ [5.7]

5.2.3 Parameter Estimation

It should be noted that the value of the expression ($\Sigma p_{ii} - 1$) ranges from zero at chance levels of accuracy (when all responses are FGs) to (n-1) at perfect accuracy. The structure of Equations 5.5 and 5.7 can be simplified by the use of new variables defined as follows:

$d = (s-g)/(na-1)$ [5.8]

$u = (n\Sigma(p_{ii} M_{ii}) - \Sigma\Sigma(p_{ij} M_{ij}))/n$ [5.9]

$v = \Sigma\Sigma(p_{ij} M_{ij})/n$ [5.10]

$x = \Sigma p_{ii} - 1$ [5.11]

Substituting from Equations 5.8, 5.9, 5.10, 5.11 into Equations 5.5 and 5.7 gives:

$$u = sx \quad [5.12]$$

and $$v = dx + g \quad [5.13]$$

Bearing in mind that 'n' is defined as the number of S-R pairs for a given experiment and that in an actual experiment there would be 'm' blocks of data, derived from the performance of a subject working at different points of the S-A continuum, then the resulting data would consist of 'm' sets of points, each consisting of three scores: u_i, v_i and x_i. From Equations 5.12 and 5.13 a single regression equation (constrained to pass through the origin) can be constructed so that the resulting coefficients provide estimates for the three parameters 'g', 'd' and 's'. Thus $\underset{\sim}{y}$ is a column vector of length 2m and $\underset{\sim}{X}$ is a matrix of shape (2m x 3):

$$\underset{\sim}{y} = \begin{pmatrix} u_1 \\ u_2 \\ \cdot \\ \cdot \\ u_m \\ \hline v_1 \\ v_2 \\ \cdot \\ \cdot \\ v_m \end{pmatrix} \text{ and } \underset{\sim}{X} = \begin{pmatrix} 0 & 0 & x_1 \\ 0 & 0 & x_2 \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ 0 & 0 & x_m \\ \hline 1 & x_1 & 0 \\ 1 & x_2 & 0 \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \\ 1 & x_m & 0 \end{pmatrix} \quad [5.14]$$

However, this procedure is only really appropriate if the two constituent regressions (represented by Equations 5.12 and 5.13) have the same error variance. Only then is it correct to make a single estimate of this error by using a single regression equation. If the error variances are obviously different, then it is necessary to carry out the regressions separately, estimating 's' from Equation

5.12 (constraining the intercept to be zero) and estimating 'd' and 'g' as the gradient and intercept, respectively, of the conventional simple regression represented by Equation 5.13.

5.2.4 Obtaining Standard Errors

Representing the covariance matrix of these coefficients as **B**, then the latter can be defined simply as:

$$\mathbf{B} = (\mathbf{X}'\mathbf{X})^{-1}\sigma^2 \quad [5.15]$$

where σ^2 is the estimated residual error variance of **y**, (Davies & Goldsmith, 1972). Now, from Equation 5.14 by elementary matrix algebra, the following result is obtained (with summations running from 1 to 'm'):

$$\mathbf{X}'\mathbf{X} = \begin{pmatrix} m & \Sigma x & 0 \\ \Sigma x & \Sigma x^2 & 0 \\ 0 & 0 & \Sigma x^2 \end{pmatrix} \quad [5.16]$$

Thus it can be said that **X'X** has the following form, where **Z** is a general matrix and 'a', 'b' and 'c' are scalars:

$$\mathbf{Z} = \begin{pmatrix} c & b & 0 \\ b & a & 0 \\ 0 & 0 & a \end{pmatrix} \quad [5.17]$$

$$\text{Now } \mathbf{Z}^{-1} = \text{adj}(\mathbf{Z})/|\mathbf{Z}| \quad [5.18]$$

where adj(**Z**) is the classical adjoint of **Z**, i.e. the transpose of the matrix formed from the cofactors of **Z** (Lipschutz, 1974).

$$\text{Hence } \text{adj}(\mathbf{Z}) = \begin{pmatrix} a^2 & -ab & 0 \\ -ab & ac & 0 \\ 0 & 0 & ac-b^2 \end{pmatrix} \quad [5.19]$$

Thus two of the covariances will necessarily be zero. Referring back to the original problem, it can be seen that:

$$\mathrm{cov}(s,g) = \mathrm{cov}(s,d) = 0 \quad [5.20]$$

By rearranging Equation 5.8:

$$a = (s-g+d)/nd \quad [5.21]$$

For simplicity, let

$$t = (\bar{s}-\bar{g})/\bar{d} = n\bar{a}-1 \quad [5.22]$$

Consider f(u,v,w) - a general function of three variables. If the values of its partial derivatives at a single point (a,b,c) are known, then the value of the function at any other point may be derived from the multivariate form of Taylor's approximation. For present purposes, the first-degree approximation will be sufficient:

$$f(u,v,w) = f(a,b,c) + (u-a)\delta f/\delta u + (v-b)\delta f/\delta v + (w-c)\delta f/\delta w \quad [5.23]$$

The first-degree Taylor approximation about the means may thus be derived from Equation 5.21:

$$a \hat{=} (\bar{s}-\bar{g}+\bar{d})/n\bar{d} + ((s-\bar{s}) - (g-\bar{g}))/n\bar{d} - ((\bar{s}-\bar{g})(d-\bar{d}))/n\bar{d}^2 \quad [5.24]$$

Returning to the general function, consider it written in the following form, with the Greek letters representing coefficients:

$$y = \alpha u + \beta v + \gamma w + \lambda \quad [5.25]$$

The variance of y is then given by:

$$var(y) = \alpha^2 var(u) + \beta^2 var(v) + \gamma^2 var(w) + 2\alpha\beta cov(u,v) + 2\alpha\gamma cov(u,w) + 2\beta\gamma cov(v,w) \quad [5.26]$$

Applying this result to Equation 5.24:

$$var(a) \simeq var(s)/n^2\bar{d}^2 + var(g)/n^2\bar{d}^2 + (\bar{s}-\bar{g})^2 var(d)/n^2\bar{d}^4 - 2cov(s,g)/n^2\bar{d}^2 - 2(\bar{s}-\bar{g})cov(s,d)/n^2\bar{d}^3 + 2(\bar{s}-\bar{g})cov(g,d)/n^2\bar{d}^3 \quad [5.27]$$

Simplifying the RHS of Equation 5.27 by substituting from Equations 5.20 and 5.22 gives:

$$var(a) \simeq (var(s) + var(g) + t^2 var(d) + 2t cov(g,d))/n^2\bar{d}^2 \quad [5.28]$$

se(a) can then be derived simply by taking the square root of the RHS of Equation 5.28.

5.3 THE RESPONSE SPECIFIC FAST GUESS MODEL

5.3.1 Description

The general version of the fast guess model does not allow for any more than three parameter estimates, regardless of the number of stimuli. In experiments with equiprobable stimuli, this may well be acceptable but for those experiments which employ stimuli that are not equiprobable, it is obviously desirable to use a model which permits at least some conclusions to be drawn concerning the effects of the stimulus probability differences.

With this requirement in mind, it is instructive to examine the possibilities available for extending the model. Firstly, considering FGs, there is obviously no need to use separate parameters for different stimuli because these responses are stimulus-independent by definition. However, it is possible to conceive of FGs for different responses coming from distributions with different means. Unfortunately, the bias parameters, b_i, make this difficult to implement because they cannot be made to vanish as they do in the General Fast Guess Model. Secondly, with the SCRs, similar differences arise when responses to distributions stimuli are regarded as being taken from distributions of different mean. The third parameter, 'a', also suffers from difficulties of a similar nature, whether attempts are made to make its value depend on either the stimulus presented or the response chosen.

The only remaining option is to arrange the SCRs to be taken from different distributions according to the response selected. (Other possibilities for models were suggested by Yellott (1971) but these had too many parameters to allow them to be estimated from the data). The resulting model thus has 'n' SCR parameters (one for each response), in addition to 'g' and 'a'. Consequently, it is referred to as the "Response-Specific Fast Guess Model". The equations are derived in the following section.

5.3.2 Derivation Of Equations

Fig. 5.1 gives a branching diagram for the model. Similar notation is employed to that used in Section 2.

For a correct response to any specified stimulus 'i':

$$p_{ii} M_{ii} = qas_i + b_i(1-q)g \quad [5.29]$$

Considering all incorrect responses, R_j, and summing over 'j' from 1 to 'n':

$$\Sigma(p_{ji} M_{ji}) - p_{ii} M_{ii} = (n-1)q(1-a)s_i + (n-1)(1-q)b_i g \quad [5.30]$$

Multiplying Equation 5.29 by (n-1) and subtracting Equation 5.30 gives:

$$np_{ii} M_{ii} - \Sigma p(p_{ji} M_{ji}) = (n-1)q(2a-1)s_i \quad [5.31]$$

As 'i' can take any value from 1 to 'n', Equation 5.31 actually yields 'n' equations, one for each s_i. Considering all correct responses:

$$\Sigma(p_{ii} M_{ii}) = qa\Sigma s_i + (1-q)g \quad [5.32]$$

Considering all incorrect responses:

$$\Sigma\Sigma(p_{ij} M_{ij}) - \Sigma(p_{ii} M_{ii}) = (n-1)q(1-a)\Sigma s_i + (n-1)(1-q)g \quad [5.33]$$

Multiplying Equation 5.32 by (n-1) and adding Equation 5.33 gives a single equation:

$$\Sigma\Sigma(p_{ij} M_{ij}) + (n-2)\Sigma(p_{ii} M_{ii}) = (n-1)q\Sigma s_i + (n-1)(1-q)g \quad [5.34]$$

As for the general model:

$$q = (\Sigma p_{ii} - 1)/(na-1) \quad [5.4]$$

and $x = \Sigma p_{ii} - 1 \quad [5.11]$

Substituting from Equations 5.4 and 5.11 into Equations 5.31 and 5.34 and simplifying gives the following two equations:

$$np_{ii} M_{ii} - \Sigma(p_{ji} M_{ji}) = ((n-1)(2a-1)s_i x)/(na-1) \quad [5.35]$$

$$(\Sigma\Sigma(p_{ij} M_{ij})+(n-2)\Sigma(p_{ii} M_{ii}))/2 = (n-1)(\Sigma(s_i)/2-g)x/(na-1)+(n-1)g \quad [5.36]$$

The intercept of Equation 5.36 obviously yields the value of 'g'. If this value is substituted into the expression for the gradient derived from the same equation, an equation in 'a' and Σs_i is obtained. Combining this equation with the 'n' individual equations in s_i obtained from Equation 5.35 gives a set of (n+1) simultaneous equations which can be solved for the (n+1) parameters $s_1 \ldots s_n$ and 'a'.

5.3.3 Parameter Estimation

However, such a lengthy approach is not required for the most simple case where n = 2. For simplicity, let:

$$d = ((s_1 + s_2)/2 - g)/(2a-1) \quad [5.37]$$

$$u = p_{11} M_{11} - p_{21} M_{21} \quad [5.38]$$

$$v = p_{22} M_{22} - p_{12} M_{12} \quad [5.39]$$

$$w = (p_{11} M_{11} + p_{12} M_{12} + p_{21} M_{21} + p_{22} M_{22})/2 \quad [5.40]$$

Putting n = 2 into Equations 5.35 and 5.36 and substituting from Equations 5.37, 5.38, 5.39, 5.40 then gives:

$$u = s_1 x \quad [5.41]$$

$v = s_2 x$ [5.42]

$w = dx+g$ [5.43]

If, as before, an experiment is run that yields 'm' blocks of data, then the outcome would be 'm' sets of points, each consisting of four scores: u_i, v_i, w_i and x_i. From Equations 5.41, 5.42 and 5.43, a single regression equation (constrained to pass through the origin) can be constructed so that the resulting coefficients provide estimates of the four parameters 'g', 'd', s_1 and s_2. Thus **y** is a column vector of length 3m and **X** is a matrix of shape (3m x 4):

$$\mathbf{y} = \left(\begin{array}{c} u_1 \\ u_2 \\ \cdot \\ \cdot \\ u_m \\ \hline v_1 \\ v_2 \\ \cdot \\ \cdot \\ v_m \\ \hline w_1 \\ w_2 \\ \cdot \\ \cdot \\ w_m \end{array}\right) \text{ and } \mathbf{X} = \left(\begin{array}{cccc} 0 & 0 & x_1 & 0 \\ 0 & 0 & x_2 & 0 \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & x_m & 0 \\ \hline 0 & 0 & 0 & x_1 \\ 0 & 0 & 0 & x_2 \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & x_m \\ \hline 1 & x_1 & 0 & 0 \\ 1 & x_2 & 0 & 0 \\ \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot \\ 1 & x_m & 0 & 0 \end{array}\right) \quad [5.44]$$

As with the GFGM, however, if the error variances for the three parts do not appear to come from the same population, then separate regressions should be carried out.

5.3.4 Obtaining Standard Errors

Following the procedure used in Section 2.4:

$$\mathbf{X'X} = \begin{pmatrix} m & \Sigma x & 0 & 0 \\ \Sigma x & \Sigma x^2 & 0 & 0 \\ 0 & 0 & \Sigma x^2 & 0 \\ 0 & 0 & 0 & \Sigma x^2 \end{pmatrix} \quad [5.45]$$

$\mathbf{X'X}$ can be said to have the same form as $\mathbf{Z}$, where:

$$\mathbf{Z} = \begin{pmatrix} c & b & 0 & 0 \\ b & a & 0 & 0 \\ 0 & 0 & a & 0 \\ 0 & 0 & 0 & a \end{pmatrix} \quad [5.46]$$

$$\text{Hence } \operatorname{adj}(\mathbf{Z}) = \begin{pmatrix} * & * & 0 & 0 \\ * & * & 0 & 0 \\ 0 & 0 & * & 0 \\ 0 & 0 & 0 & * \end{pmatrix} \quad [5.47]$$

where "*" represents various non-zero terms. Thus all the covariances except one will necessarily be zero. Referring back to the original problem:

$$\text{cov}(s_1,s_2) = \text{cov}(s_1,g) = \text{cov}(s_2,g) = \text{cov}(s_1,d) = \text{cov}(s_2,d) = 0 \quad [5.48]$$

By rearranging Equation 5.37:

$$a = ((s_1 + s_2)/2 - g + d)/2d \quad [5.49]$$

For simplicity, let

$$t = ((\bar{s}_1 + \bar{s}_2)/2 - \bar{g})/\bar{d} = 2\bar{a}-1 \quad [5.50]$$

Consider f(u,v,w,x) - a general function of four variables. The first-degree Taylor approximation about the point (a,b,c,d) is:

$$f(u,v,w,x) = f(a,b,c,d) + (u-a)\delta f/\delta u + (v-b)\delta f/\delta v + (w-c)\delta f/\delta w + (x-d)\delta f/\delta x \quad [5.51]$$

Deriving the first-degree Taylor approximation about the means from Equation 5.49:

$$a \hat{=} ((\bar{s}_1+\bar{s}_2)/2-\bar{g}+\bar{d})/2\bar{d} + (s_1-\bar{s}_1)/4\bar{d} + (s_2-\bar{s}_2)/4\bar{d} - (g-\bar{g})/2\bar{d} - (d-\bar{d})((\bar{s}_1+\bar{s}_2)/2-\bar{g})/2\bar{d}^2 \quad [5.52]$$

Returning to the general function, consider it written in the following form, with the Greek letters representing coefficients:

$$y = \alpha u + \beta v + \gamma w + \delta x + \lambda \quad [5.53]$$

The variance of 'y' is then given by:

$$\mathrm{var}(y) = \alpha^2\mathrm{var}(u) + \beta^2\mathrm{var}(v) + \gamma^2\mathrm{var}(w) + \delta^2\mathrm{var}(x) + 2\alpha\beta\mathrm{cov}(u,v) + 2\alpha\gamma\mathrm{cov}(u,w) + 2\alpha\delta\mathrm{cov}(u,x) + 2\beta\gamma\mathrm{cov}(v,w) + 2\beta\delta\mathrm{cov}(v,x) + 2\gamma\delta\mathrm{cov}(w,x) \quad [5.54]$$

Applying this result to Equation 5.52 and simplifying the RHS by substituting from Equations 5.48 and 5.50 gives:

$$\mathrm{var}(a) \simeq (\mathrm{var}(s_1)/4+\mathrm{var}(s_2)/4+\mathrm{var}(g)+t^2\mathrm{var}(d)+2t\,\mathrm{cov}(g,d))/4\bar{d}^2 \quad [5.55]$$

As with the general model, se(a) can be derived simply by taking the square root of the RHS of this equation.

5.4 APPLICATION OF FAST GUESS MODELS TO EXPERIMENTS

5.4.1 Techniques And Experimental Designs

As indicated in Sections 2.3 and 3.3, it is necessary that the subject generate a number of blocks of data, from various parts of the S-A continuum. The experiment should be designed to focus the subject's performance at different parts of the continuum in different blocks. For an investigation into, say, the locus of the foreperiod effect, foreperiod length should be varied between blocks of trials. If the task involves equiprobable choices, then the GFGM (General Fast Guess Model) is appropriate and a regression analysis can be carried out for each value of foreperiod length used in the experiment, as specified in Section 2.4. This yields the parameter estimates 's' and 'g' directly as coefficients and an estimate for the third parameter, 'a', is easily derived by applying Equation 5.21. The standard errors of the first two parameters are obtained from the regression output and the standard error of 'a' is obtained as indicated in Equation 5.28. Thus a separate set of parameter estimates is obtained for each value of foreperiod length.

On the other hand, if the experiment were concerned with the stimulus probability effect, then the variable of interest (stimulus probability) is manipulated _within_ blocks of trials (simply by making the stimulus probabilities different) and a single regression analysis is performed, according to the specification for the RSFGM (Response-Specific Fast Guess Model) given in Section 3.4. Unfortunately, this only yields separate parameter estimates for the SCR means (i.e. high- versus low-probability responses) but this is sufficient to check whether an apparent stimulus-probability effect is located in the SCRs or not. It is interesting to note that Laming (1973: pp 194 - 195) states that the fast-guess model can be applied to separate stimuli. Of course, this is not true, as there is no way of dealing with the FG bias parameters, b_i.

Of course, it is possible to combine the two techniques. For example, if both foreperiod length and stimulus probability were being used as independent variables, then stimulus probability could be manipulated within blocks and foreperiod length varied between blocks of trials. For this type of experiment, the RSFGM should be applied separately for each value of foreperiod length, yielding four parameters each time.

Another possible technique is to split a sequence of trials from a given block into two components and allocate these to different applications of the GFGM or RSFGM. Such a technique is valid, provided that the partitioning criterion is chosen with care. The vital consideration is that the partitioning principle should _not_ be such that a fast-guessing strategy could be based on the same principle and as a result cause FGs to be more accurate than expected by chance in one sub-group of trials and less accurate than expected by chance in another.

There are obvious applications of this idea in the study of sequential effects. Unfortunately, sequences of trials should not be partitioned according to whether or not the stimulus is a repetition of the preceding one, because the subject might be using a fast-guessing strategy of making the same response again as was appropriate for the preceding trial. If this were happening, then obviously the partitioned repetitions would contain an unduly high proportion of correct FGs and the non-repetitions would contain too many incorrect FGs, which would violate the assumptions on which the fast guess analysis is based.

However, two legitimate applications of the partitioning principle can be found. The first concerns partitioning each block of trials into _response_ repetitions versus non-repetitions. A little thought will show that although the subject might use this principle as a fast guessing strategy, this would not affect the probability of FGs being correct in either sub-group of trials. The second application concerns partitioning according to the correctness or otherwise of the preceding response. Such an approach is obviously of value in studying the microstructure of S-A tradeoff.

There is an exception to the rule just described, concerning the partitioning of sequences of trials. Perusal of the derivation of Equations 5.1, 5.2 and 5.4 shows that these equations have exactly the same form for a stimulus subset of size 'k' (where 'k' > 1), provided that $\Sigma b_i = 1$ (summing from 1 to 'k'). This means that if it can be guaranteed that all the FGs are made to responses corresponding to the stimuli in the subset under consideration, then the GFGM analysis of that subset is legitimate. (Of course, the corresponding analysis of the complementary subset is not). Inspection of the derivation of Equations 5.31 and 5.34 shows that the same type of argument may be made for the RSFGM.

Now, under normal circumstances, the requirement mentioned above is not met. However, under certain special circumstances, it is. One such set of circumstances arises when the task contains catch trials. When this is the case, the null stimulus event can be regarded as an extra stimulus for which the appropriate response is a null response. Clearly, it does not make sense to think of any of the FGs as consisting of null responses and therefore all the FGs must be made to conventional responses. Thus it is legitimate to consider all the conventional stimuli separately and to apply either of the fast-guess analyses to this subset.

5.4.2 Parameter Comparison Using Analysis Of Variance

5.4.2.1 Introduction -

Knowing the mean and standard error of each parameter for each subject-condition combination and also the number of scores (i.e. blocks) on which each estimate is based gives sufficient information to perform a separate analysis of variance for each parameter in each experiment. It is not commonly realised that an analysis of variance can be performed from this cell information, obtaining the same summary table as if the original scores had been employed. The technique described below is also unconventional in another (independent) respect. It utilises an unweighted-means analysis, rather than the weighted-means (least squares) approach that is more commonly used. The two methods give identical results when all the cells in the design contain equal numbers of scores. However, when unequal cell sizes occur, the unweighted-means analysis has the advantage of maintaining the orthogonality of the design. This neatly circumvents the awkward problem of deciding on the order of extraction of the various effects which bedevils non-orthogonal analysis of variance.

Basically, two experimental designs are necessary. Where the GFGM model is being applied, the appropriate design is a two-way factorial one, in which Subjects (A) are crossed with Experimental Conditions (B), with Blocks of Trials (S) as replicates. Where the RSFGM model is used, the design is a classic split-plot with Blocks nested within Subjects but crossed with Conditions. Of course, both designs are instances of so-called "mixed-effects" models, because the Subjects effect is a random one, whereas Conditions is a fixed effect.

5.4.2.2 Unweighted-Means Analysis Of Variance -

Both Keppel (1973) and Winer (1970) discuss the unweighted-means approach for unequal cell frequencies and point out that, provided the inequality of the cell frequencies has not arisen from inequality in the sizes of the corresponding population strata, the technique is more appropriate than the weighted-means method.

Dealing firstly with the two-way factorial design, the number of levels of each of the factors A and B is represented by the same letter in lower case. The number of scores in the cell determined by level 'i' of A and level 'j' of B is represented by s_{ij}. There is a possible notational ambiguity here, in that 's' has been previously employed as the SCR parameter. However, for the remainder of this chapter, it should be apparent that when it appears with subscripts it refers instead to a cell size of the experimental design. For a typical application, the unweighted-means method utilises a quantity called the "harmonic mean" of the cell sizes. It is the reciprocal of the mean reciprocal of the individual cell sizes and is represented here by the symbol s'. Thus:

$$s' = ab/\Sigma\Sigma(1/s_{ij}) \quad [5.56]$$

(This is actually incorrect for the particular application required. However, this difficulty is resolved in the next section). Of course, where the cell sizes are equal, s' has the same value as the cell size of any of the cells. The between-cell deviations are taken about the mean of the cell means, rather than about the grand mean of all observations. This explains the term "unweighted-means" - the cells are <u>not</u> weighted according to their sizes, as they are in the weighted-means analysis. As regards the computational formulae, Keppel (1973) proceeds by first defining three quantities, obtained from the matrix of AB means and its marginal totals. Thus:

$$A'_i = \sum_{j}^{b} \overline{AB}_{ij} \quad [5.57]$$

$$B'_j = \sum_{i}^{a} \overline{AB}_{ij} \quad [5.58]$$

$$T' = \Sigma\Sigma\overline{AB}_{ij} \quad [5.59]$$

The sums of squares may then be expressed in terms of these newly-defined terms, as follows:

$$SS(A) = s'(\Sigma(A')^2/b - (T')^2/ab) \quad [5.60]$$

$$SS(B) = s'(\Sigma(B')^2/a - (T')^2/ab) \quad [5.61]$$

$$SS(AxB) = s'(\Sigma\Sigma(\overline{AB}_{ij})^2 - \Sigma(A')^2/b - \Sigma(B')^2/a + (T')^2/ab \quad [5.62]$$

The degrees of freedom for the error term in a typical application are given by:

$$df(S/AB) = \Sigma\Sigma(s_{ij} - 1) \quad [5.63]$$

(For the application required, this definition is incorrect. However, this difficulty is resolved in the next section). The sum of squares for the error term is actually the same as for the weighted-means analysis:

$$SS(S/AB) = \Sigma\Sigma\Sigma(ABS_{ijk}) - \Sigma\Sigma s_{ij}(\overline{AB}_{ij})^2 \quad [5.64]$$

One consequence of this is that, in general, the various sums of squares do not add up to the total sum of squares, as they do in the weighted-means analysis. However, this fact does not constitute a problem for this approach.

5.4.2.3 Working From Cell Parameters With The Factorial Design -

It is obvious from the equations given in the previous section that all the sums of squares except SS(S/AB) can be calculated from the AB cell means and the cell sizes, s_{ij}. However, the error sum of squares depends on the within-cell variances. Now, in the present application, the standard errors are known. For the cell determined by level 'i' of A and level 'j' of B, the standard error of the raw scores (X_{ij}) is defined by:

$$se(X_{ij}) = \sqrt{}(var(X_{ij})/\nu_{ij}) \quad [5.65]$$

By rearrangement:

$$var(X_{ij}) = \nu_{ij}\,(se(X_{ij})) \quad [5.66]$$

Hence $SS(S/AB) = \Sigma\Sigma\nu_{ij}^{2}\,(se(X_{ij}))^{2}$ [5.67]

In most cases, ν_{ij} will have the value $s_{ij}-1$. However, for this application, three degrees of freedom are used up in each regression analysis, because three parameters are estimated. Therefore:

$$\nu_{ij} = s_{ij} - 3 \quad [5.68]$$

Recognition of this fact necessitates redefining two quantities that were defined in the previous section. Now Equation 5.56 should have been written in the following more general fashion:

$$s' = ab/\Sigma\Sigma(1/(\nu_{ij} + 1)) \quad [5.69]$$

Substituting for ν_{ij} from Equation 5.68 into Equation 5.69 then gives:

$$s' = ab/\Sigma\Sigma(1/(s_{ij} - 2)) \quad [5.70]$$

Similarly, Equation 5.63 obviously has the more general form:

$$df(S/AB) = \Sigma\Sigma\nu_{ij} \quad [5.71]$$

Substituting as before:

$$df(S/AB) = \Sigma\Sigma(s_{ij} - 3) \quad [5.72]$$

However, for those cases where two <u>separate</u> regressions are used, ν_{ij} will have the value s_{ij} -1 for the analysis of the 's' parameter and s_{ij} -2 for the other two parameters. In fact, the general rule is simple. For each analysis of variance, the number of degrees of freedom lost (and hence the number that must be subtracted from each s_{ij}) is equal to the number of parameters estimated in the regression analysis which yielded the parameter concerned.

5.4.2.4 Working From Cell Parameters With The Split Plot Design -

For this design, the sums of squares for the main effects and the interaction are derived in exactly the same way as for the two-way factorial design (see Equations 5.60 to 5.62). However, the error sum of squares (defined in Equation 5.64) has to be partitioned into two components, thus:

$$SS(S/AB) = SS(S/A) + SS(BxS/A) \quad [5.73]$$

Of course, the total sum of squares can be calculated as before, but the partitioning presents something of a problem. Fortunately, however, it is possible to obtain SS(BxS/A) by another method and hence to obtain SS(S/A) by subtraction. This alternative approach depends on the fact that another analysis can be performed (using "difference" scores) which bears a simple relationship to the full split plot model. This method is described below.

The technique depends on the fact that each application of the RSFGM described in this thesis uses only two levels of B. If the individual raw scores (X_{ijk}) were available, then it would be a simple matter to form a set of difference scores (Y_{ik}) by subtracting each score at one level of B from its paired counterpart at the other level, as follows:

$$Y_{ik} = X_{i1k} - X_{i2k} \quad [5.74]$$

If a one-way analysis of variance is then performed on these difference scores, the following relationship holds where the double prime suffix indicates quantities derived from the difference scores, rather than from the original data).

$$SS(S''/A'') = 2(SS(BxS/A)) \quad [5.75]$$

This can be shown to be true by considering the sum of squares formula for the B(linear) components of the BxS/A error term. When there are only two levels of B, this is tantamount to considering the term in full. The usefulness of the exercise is merely in expressing the within-Block error sum of squares in a form which demonstrates the relationship with the error sum of squares for the difference scores.

In order to demonstrate the relationship, it is first necessary to partition the within-Block error term as follows:

$$SS(BxS/A) = SS(BxS) + SS(AxBxS) \quad [5.76]$$

Bearing in mind that the polynomial coefficients are +1 and -1, the B(linear) components of the two terms on the right are as follows:

$$SS(B(lin)xS) = \Sigma(B_1S_k - B_2S_k)^2/2a - SS(B(lin)) \quad [5.77]$$

$$\begin{aligned}SS(AxB(lin)xS) = {} & \Sigma\Sigma(A_iB_1S_k - A_iB_2S_k)^2/2 - \Sigma\Sigma((A_iB_1 - A_iB_2)^2/2s_i) - \\ & \Sigma(B_1S_k - B_2S_k)^2/2a + SS(B(lin)) \quad [5.78]\end{aligned}$$

Substituting from Equations 5.7~~6~~7 and 5.7~~7~~8 into Equation 5.7~~5~~6 gives:

$$SS(BxS/A) = \Sigma\Sigma(A_iB_1S_k - A_iB_2S_k)^2/2 - \Sigma((A_iB_1 - A_iB_2)^2/2s_i) \quad [5.79]$$

Considering a one-way analysis of variance on the difference scores, the sum of squares for the error term is given by:

$$SS(\overset{S''/A''}{\cancel{A''/S''}}) = \Sigma\Sigma(A''_iS''_k)^2 - \Sigma((A''_i)^2/s_i) \quad [5.80]$$

This error sum of squares can be expressed in terms of the cell parameters for the difference scores:

$$SS(\overset{S''/A''}{\cancel{A''/S''}}) = \Sigma\nu_i^2(se(Y_i))^2 \quad [5.81]$$

Now, $se(Y_i)$ can be obtained by applying the standard formula:

$$var(Y_i) = var(X_{i1}) + var(X_{i2}) - 2cov(X_{i1},X_{i2})$$

However, Equation 5.48 shows that, for the current application, the covariance term will be zero. Also, it is obvious from the matrix in Equation 5.47 that the variances of the two SCR parameters will be equal. Thus:

$$var(Y_i) = 2var(X_{ij}) \quad [5.82]$$

Substituting from Equation 5.83 into Equation 5.81 gives:

$$SS\overset{S''/A''}{\cancel{(A''/S'')}} = 2\Sigma\nu_i^2(se(X_i))^2 \quad [5.83]$$

Now, as b=2, it is obvious from Equations 5.67 and 5.83 that SS(A"/S") is equal to SS(S/AB). Knowing this, it is clear from Equations 5.73 and 5.75 that the within-cell sum of squares must be split into equal parts to yield the sums of squares for the two error terms:

$$SS(S/A) = SS(BxS/A) = (SS(S/AB))/2 \quad [5.84]$$

Finally, it must be remembered that, when the split plot model is applied to the RSFGM, four parameters rather than three are estimated at the regression stage and hence:

$$\nu_{ij} = s_{ij} - 4 \quad [5.85]$$

As before, when separate regressions are employed, the number of degrees of freedom will be different, according to the principle explained at the end of Section 4.2.3.

5.5 LOCI OF EFFECTS

The most common way of dealing with errors in conventional CRT experiments is to omit error trials from the data analysis. Provided that error rates from the various conditions of the experiment are neither too high nor too different from one another, many experimenters regard this approach as acceptable. However, some warnings have been sounded (see Section 4.4 in Chapter 2).

The purpose of this section is to show how effects which are apparent when errors are treated in the conventional fashion can be due to any of four fundamental mechanisms when seen from the viewpoint of the fast guess approach.

When an independent variable (e.g. foreperiod length) is manipulated by changing its value between blocks of trials and a difference in mean RT is found between the various conditions (after error trials have been discarded), then this could be due to any combination of three possible effects. Firstly, there could be an SCR effect, in which SCRs are faster under some conditions than others. Secondly, there could be an FG effect in which 'g' (the mean RT for FGs) differs between conditions. Both these effects are easily tested for by inspecting the results of a GFGM analysis. Thirdly, an S-A tradeoff effect could be present in which a larger proportion of FGs is made under some conditions than others. This effect should be detectable in the conventional analysis as a difference between error rates.

When an independent variable (e.g. stimulus probability) is manipulated by changing its value within blocks of trials and a difference in mean RT is found between the various conditions (after discarding error trials) then this could be due to either of two possible effects. As before, an SCR effect could be present (and could be tested for with the RSFGM model in this case). Secondly, a fast-guessing strategy might have been employed by the subject. (For example, in a two-choice experiment with different stimulus probabilities, the subject might have made all his FGs towards the high-probability stimulus). In this case, the appropriate RSFGM analysis would show no effect, but the conventional analysis would suggest that high-probability stimuli produced faster responses than low-probability ones, because the high-probability responses would include a larger proportion of FGs. It is worth noting in passing, that, for this type of experiment, any difference in mean RT between the two conditions could not possibly be due to a difference in mean FGs, because FGs are defined as being stimulus-independent.

CHAPTER 6: PAYOFF TECHNIQUES FOR SPEED-ACCURACY EXPERIMENTS

6.1 INTRODUCTION

As explained in Chapter 2, when conducting speed-accuracy experiments, it is usual to vary accuracy over a wide range. The necessity for doing this presents a problem as to how this variation in accuracy is to be achieved, while still encouraging the subject to work at maintain maximum performance. Before dealing with the fast-guess experiments which form the rest of the experimental work described in this thesis, the payoff system use will be described in detail because it illustrates some important principles. This chapter is devoted to that topic and a pilot fast-guess experiment is described in the next chapter.

Edwards (1961) pointed out that instructions to subjects taking part in psychology experiments are frequently ambiguous and often contradictory. This problem tends to arise when the experiment has more than one value dimension. Usually, the value dimensions are what Edwards calls <u>inconsistent</u>, i.e. actions which maximise performance on one value dimension will not maximise it on the others. Such is the case with many experiments in which the instructions emphasise <u>both</u> speed and accuracy. As these are antithetic requirements, the subject cannot maximise both simultaneously. If the instructions are vaguely expressed and merely exhort the subject to go as fast as he can and make as few errors as possible, then the subject has insufficient information on how to balance his effort. Edwards states that the way round the problem is somehow to combine the requirements into a single payoff value. This is the approach that is adopted here.

6.2 REQUIREMENTS OF A PAYOFF SYSTEM

Payoff in S-A experiments is best seen mathematically as a surface, i.e. a function of two independent variables - namely speed and accuracy. Ideally, the subject's performance should be shaped by the payoff that he receives. In the context of S-A experiments, two aims have to be born in mind simultaneously. Firstly, the subject should be rewarded for working hard (i.e. keeping to the S-A continuum). Secondly, the subject should be rewarded for working at the appropriate part of this continuum. Perhaps these two requirements need further explanation.

If S-A tradeoff is regarded as being linear throughout most of its range, then it can be portrayed as in Fig. 6.1. The diagram is intended to represent all combinations of speed and accuracy, with the broken line signifying the S-A continuum (i.e. the line of maximum effort). The area below and to the right of this line represents performance that the subject cannot achieve, i.e. speed (represented in reverse on the y-axis) is too high for the level of accuracy represented on the x-axis. The area above and to the left of the S-A continuum represents sub-maximal performance by the subject. Here, the subject could either be working faster at the same level of accuracy or more accurately at the same speed (or, in fact, could improve his performance by any intermediate combination

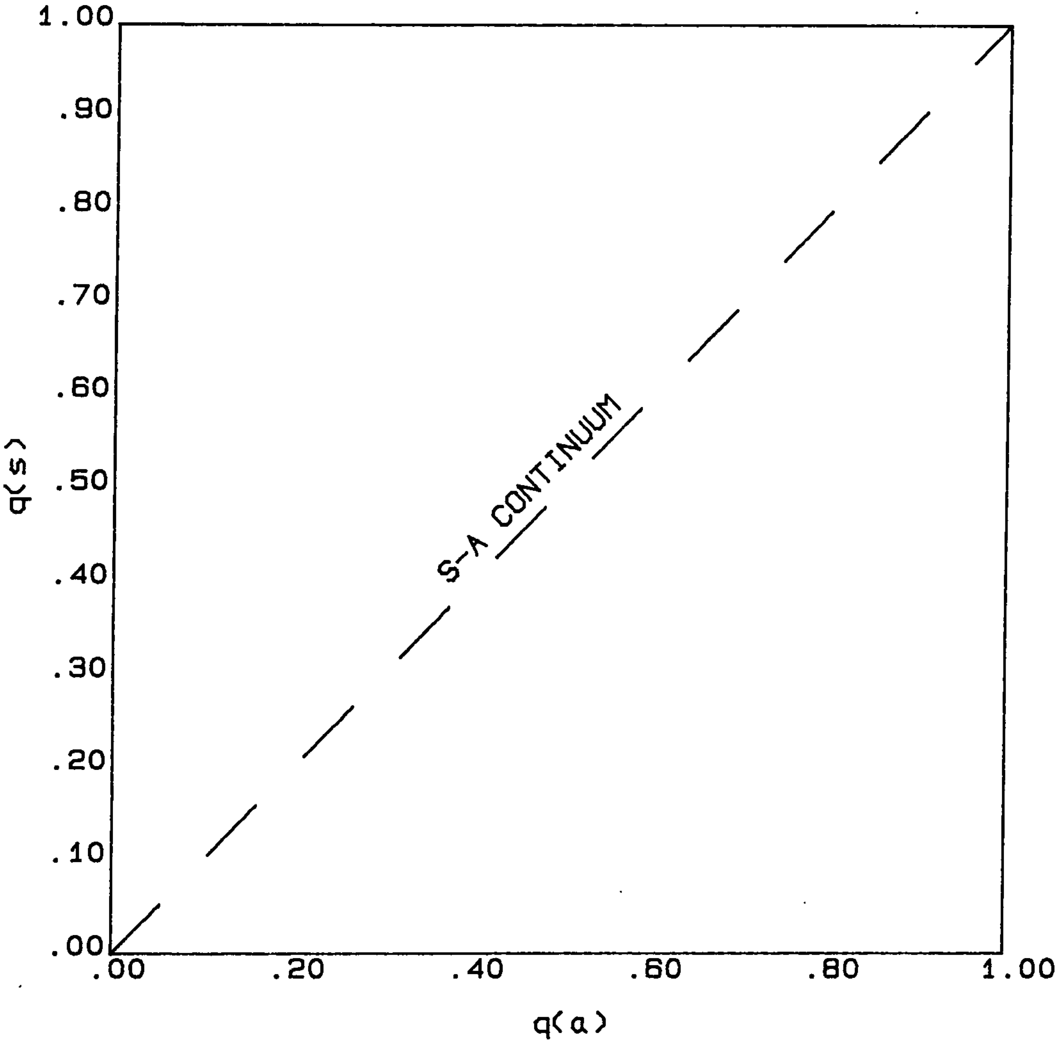


Fig. 6.1 Linear S-A tradeoff.

of these strategies until his performance reached the broken line at some point).

Thus the first requirement of the payoff system can be satisfied by ensuring that the payoff function has two properties, as follows. Firstly, for speed held constant at any level, payoff must be a monotonically increasing function of accuracy. Secondly, for accuracy held constant at any value, payoff must be a monotonically increasing function of speed.

Turning to the second requirement, it was stated in Section 4.1 of the previous chapter that, for the adequate application of fast-guess models, different blocks of trials should be drawn from performance at different parts of the S-A continuum. In order to induce the subject to concentrate his performance at different parts of the continuum, the payoff function must possess two further properties. When performance is restricted to the S-A continuum, speed and accuracy are perfectly inversely correlated. Under these circumstances, payoff can be regarded as a function of a single variable. The two further properties just mentioned refer to the shape of this restricted function. Firstly, in order to be able to focus performance at any particular point on the continuum, this function must be curvilinear, with a single maximum and no other turning point. Secondly, the position of this maximum should be determined by a parameter of the payoff function, so that the experimenter has control over the position on the tradeoff line at which maximum payoff will be available.

6.3 EXAMINATION OF THE PAYOFF FUNCTION

If the simplifying assumption is made that all errors are due to FGs, then the accuracy coordinate of the graph in Fig. 6.1 is synonymous with an estimate for 'q', the probability of making an SCR in a fast guess model. Let this estimate be represented by q(a), i.e. an estimate for 'q' derived from the accuracy of the subject's performance. Similarly, let q(s) be another estimate for 'q' derived from the speed of the subject's performance. Thus the longer the RTs, the larger the value of q(s). Let Q be that value of 'q' which the experimenter has decided will be the optimum proportion of SCRs for the block of trials under consideration. Thus Q is a parameter which has its value changed between blocks of trials. Finally, let M be the value of the payoff function corresponding to optimal performance by the subject and R be a range constant determining the difference in the value of the function between optimal performance and some other specified point on the payoff surface. Normally, both M and R would be held constant across both subjects and conditions for the duration of an experiment.

The following mathematics deals with a payoff function which has the required properties. It is not claimed that it is the only such function, nor even that it is necessarily the simplest. The function is:

$$y = \exp(z) \quad [6.1]$$

where $z = c(w(a)\ln(1+q(a)) + w(s)\ln(2-q(s))) + \ln(M)/R \quad [6.2]$

In this equation, 'c' is a constant and $w(a)$ and $w(s)$ are weighting factors as follows:

$c = \ln(M)(R-1)/R$ [6.3]

$w(a) = (1+Q)/((1+Q)\ln(1+Q) + (2-Q)\ln(2-Q))$ [6.4]

$w(s) = (2-Q)/((1+Q)\ln(1+Q) + (2-Q)\ln(2-Q))$ [6.5]

That the first two properties are satisfied is easily shown by taking the partial derivatives of 'z' with respect to $q(a)$ and $q(s)$:

$\delta z/\delta q(a) = cw(a)/(1+q(a))$ [6.6]

and $\delta z/\delta q(s) = -cw(s)/(2-q(s))$ [6.7]

As $q(a)$ and $q(s)$ are constrained to the interval (0,1) by virtue of being probabilities, this means that:

$\delta z/\delta q(a) > 0$ [6.8]

and $\delta z/\delta q(s) < 0$ [6.9]

Thus 'z' increases as speed is increased (i.e. $q(s)$ reduced) with accuracy held constant. As 'y' is simply $\exp(z)$, it is obvious that the same reasoning applies to 'y'. Inspection of Equations [6.2], [6.4] and [6.5] also shows that there is a symmetry concerning the way in which speed and accuracy are manipulated. For example, when $Q = 0.5$, the weighting factors $w(s)$ and $w(a)$ are equal. Similarly, when $Q = 0.1$, $w(a)$ has the same value as $w(s)$ does when $Q = 0.9$. Also, when $q(a) = 0.1$, the expression $(1+q(a))$ has the same value as $(2-q(s))$ does when $q(s) = 0.9$.

Turning to the second requirement, it is necessary to deal with the payoff function for the S-A line. On this line, it is true that:

$$q = q(a) = q(s) \quad [6.10]$$

Using the following equation to represent the restricted function:

$$y' = \exp(z') \quad [6.11]$$

where z' is derived by substituting from Equation [6.10] into Equation [6.2] yielding an equation in terms of 'q':

$$z' = c(w(a)\ln(1+q) + w(s)\ln(2-q)) + \ln(M)/R \quad [6.12]$$

It is necessary to get the first derivative of the restricted payoff function in order to find any stationary values and:

$$dy'/dq' = (dy'/dz')(dz'/dq') \quad [6.13]$$

Now $dy'/dz' = \exp(z') \quad [6.14]$

and $dz'/dq' = c(w(a)/(1+q) - w(s)/(2-q)) \quad [6.15]$

From Equations [6.13] to [6.15] it is clear that dy'/dq' is only zero when:

$$w(a)/(1+q) = w(s)/(2-q) \quad [6.16]$$

Substituting from Equations [6.4] and [6.5] into Equation [6.16] gives:

$$(1+Q)/(1+q) = (2-Q)/(2-q) \quad [6.17]$$

By rearrangement this gives:

$$q = Q \quad [6.18]$$

Thus it has been shown that the restricted payoff function has a single stationary value at the point $q = Q$. It remains to be shown that this is a maximum, rather than a minimum or point of inflection. From Equations [6.13] and [6.14], it is clear that as $\exp(z')$ must necessarily be positive, the sign of dy'/dq' is determined by that of dz'/dq'. Considering the RHS of Equation [6.15], it is obvious that, provided $\ln(M)$ is positive (i.e. $M > 1$), 'c' will be positive (from Equation [6.3]) and the sign of dz'/dq' will be determined by the remaining factor of the RHS. Let this factor be called 'h'.

Thus $h = w(a)/(1+q) - w(s)/(2-q)$ [6.19]

For simplicity, let

$$d = 1/((1+Q)\ln(1+Q) + (2-Q)\ln(2-Q)) \quad [6.20]$$

Substituting for $w(a)$ and $w(s)$ from Equations [6.4] and [6.5] into [6.19] and simplifying from Equation [6.20] gives:

$$h = d((1+Q)/(1+q) - (2-Q)/(2-q)) \quad [6.21]$$

As Q is a probability and thus constrained to lie in the interval (0,1), 'd' is necessarily positive. Thus the sign of dy'/dq' is ultimately determined by the sign of 'k', where:

$$k = (1+Q)/(1+q) - (2-Q)/(2-q) \quad [6.22]$$

Now let ϵ be some suitably small positive number. Putting $q = Q-\epsilon$ and substituting into Equation [6.22] gives:

$$k = (1+Q)/(1+Q-\epsilon) - (2-Q)/(2-Q+\epsilon)$$

Here it is clear that $k > 0$. Putting $q = Q+\epsilon$ and repeating the process gives:

$$k = (1+Q)/(1+Q+\epsilon) - (2-Q)/(2-Q-\epsilon)$$

which makes $k < 0$. Therefore, just before the point where $q = Q$, ~~'y'~~ y' is an increasing function of 'q' and just after the same point it is a decreasing function of 'q'. Thus from elementary calculus, we know that the point $q = Q$ is a maximum, rather than any other type of stationary value.

Before leaving the restricted payoff function, it is instructive to show that the maximum value is M, as required. Putting $Q = q$ in Equation [6.12] and substituting for w(a) and w(s) from Equations [6.4] and [6.5] and simplifying gives:

$$z' = c + \ln(M)/R \quad [6.23]$$

Substituting for 'c' from Equation [6.3] into Equation [6.2~~0~~3] gives simply:

$$z' = \ln(M) \quad [6.24]$$

The proof is completed by substituting for z' from Equation [6.24] into Equation [6.11], giving y' = M.

Graphs of the restricted payoff function are displayed in Figs. 6.2 to 6.4, each graph having a different value of Q. Note how the graph in Fig. 6.3 (Q = 0.5) is symmetrical and how the graphs in Figs. 6.2 and 6.4 (Q = 0.1 and Q = 0.9, respectively) differ from it. Graphs of the whole payoff surface are shown in Figs. 6.5 to 6.7. Values of 450 for M and 1.15 for R were used throughout.

6.4 APPLICATION OF THE PAYOFF FUNCTION

The application of the payoff function just described to a CRT experiment requires that the experimenter decides on suitable values for M (payoff at optimal performance) and R (range of payoff). These parameters remain fixed over the entire experiment. Each subject is then run for a number of sessions, each session consisting of a number of blocks of trials. The Q parameter (position of optimum performance on the S-A line) is best varied between sessions. This should ensure that the subject has a chance to get used to working at or around a certain point on the S-A line. It should also ensure that the experimenter is able to get an even spread of performance over most of the S-A continuum.

As regards the variables q(a) and q(s), values for these are, of course, determined by the subject's performance. The payoff for a given block of trials is determined by the values of these variables, given the parameters described earlier. The first of these variables, q(a), is easily derived from the proportion of errors made during the block of trials. The second variable presents more of a problem. Essentially, there are two possible approaches. Either a deadline technique can be used (in which the subject is rewarded only for RTs which are faster than a specified criterion time) or a method of continuous costing for time can be applied. Both methods have been used in the past, in combination with a penalty for errors. Yellott (1971) used the deadline technique in his fast-guess experiments, while Swensson and Edwards (1971) used the continuous approach in their investigation of S-A tradeoff.

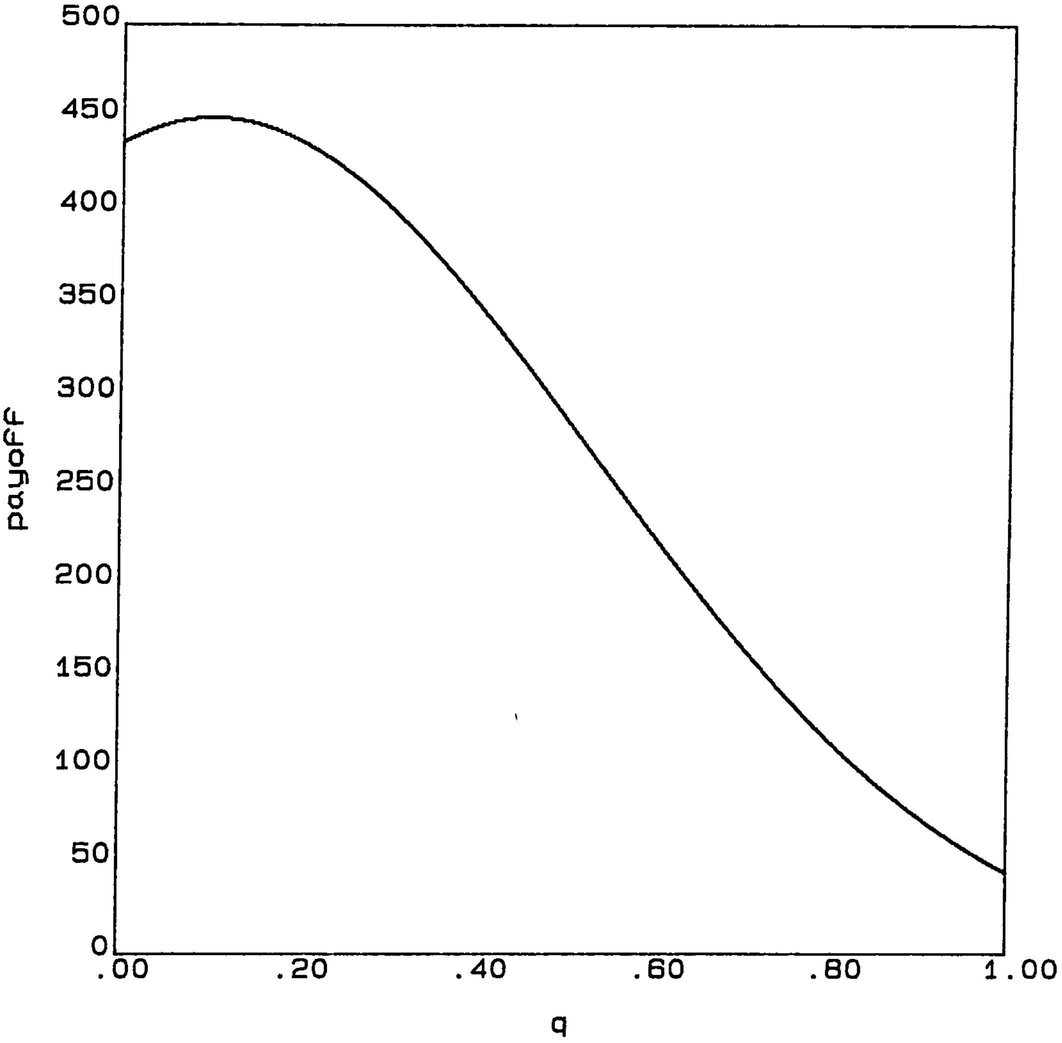


Fig. 6.2 Restricted payoff function for Q=0.1.

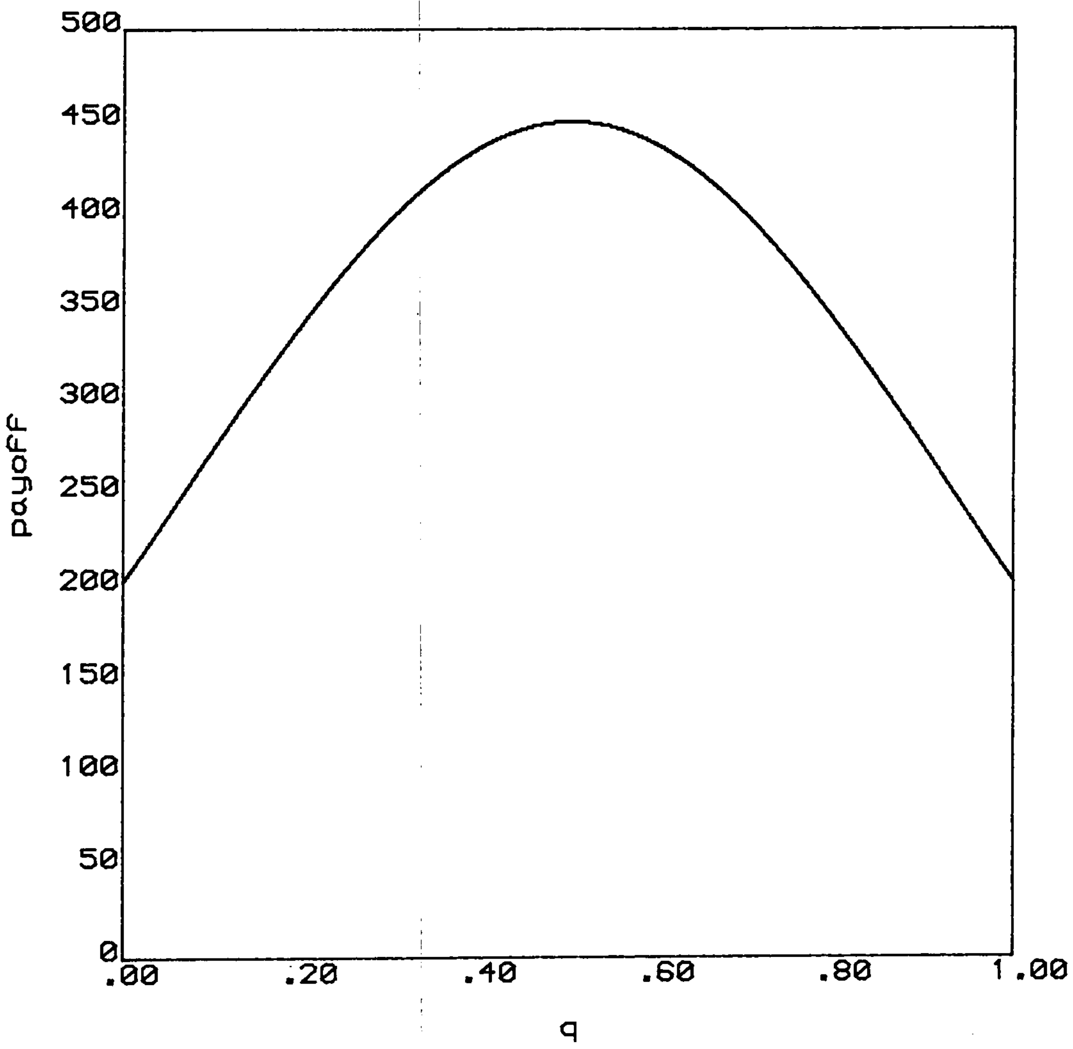


Fig. 6.3 Restricted payoff function for Q=0.5.

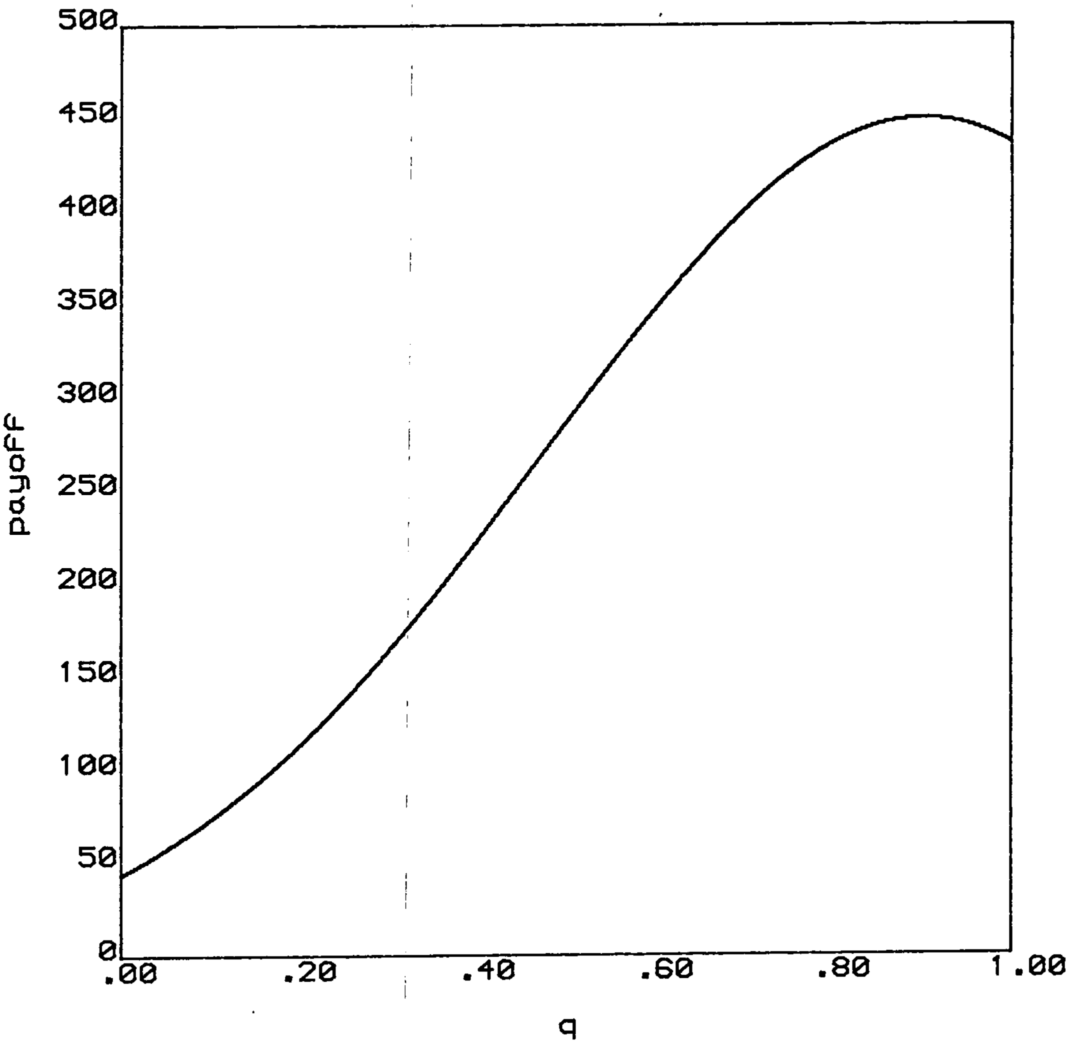


Fig. 6.4 Restricted payoff function for Q=0.9.

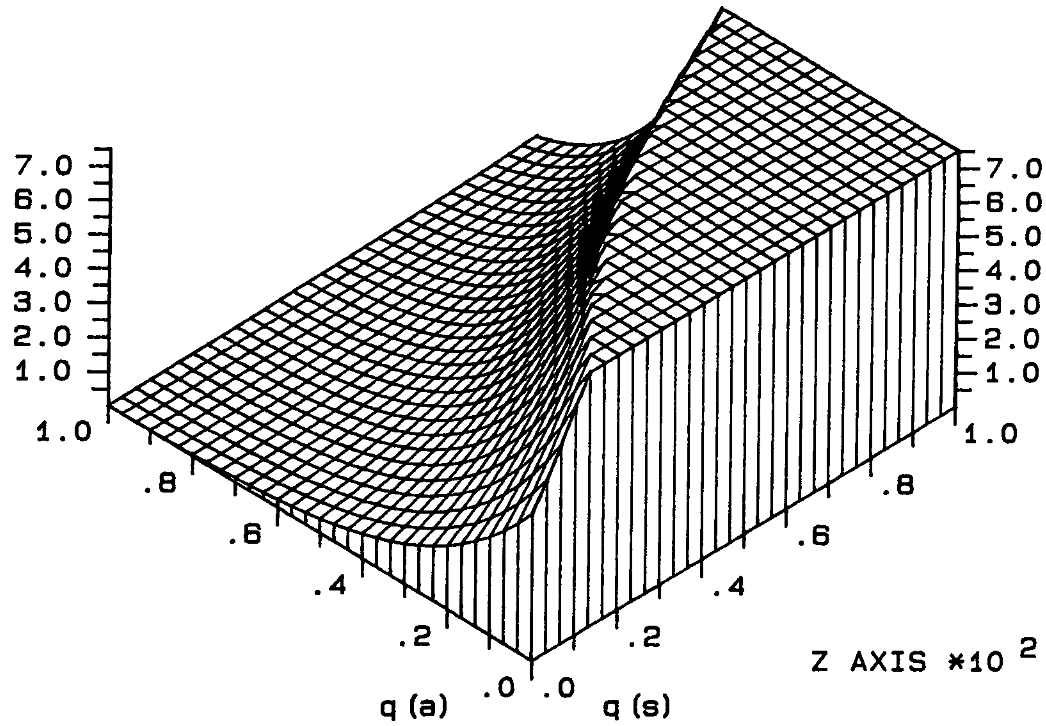


Fig. 6.5 Payoff surface for Q=0.1.

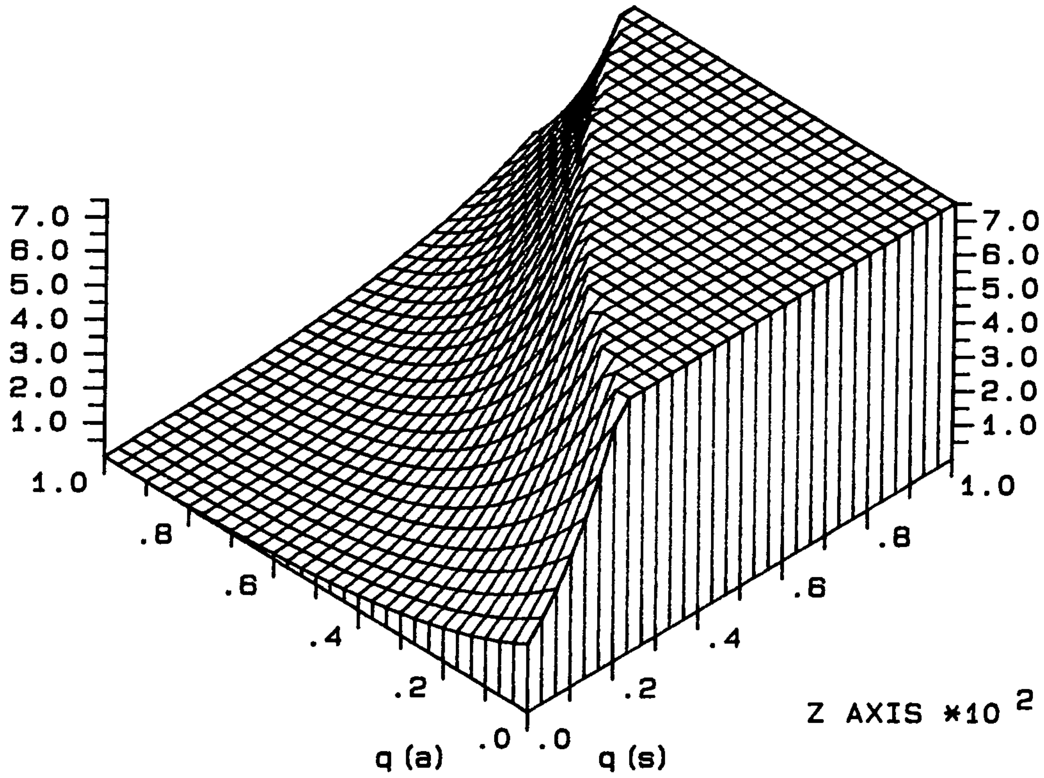


Fig. 6.6 Payoff surface for Q=0.5.

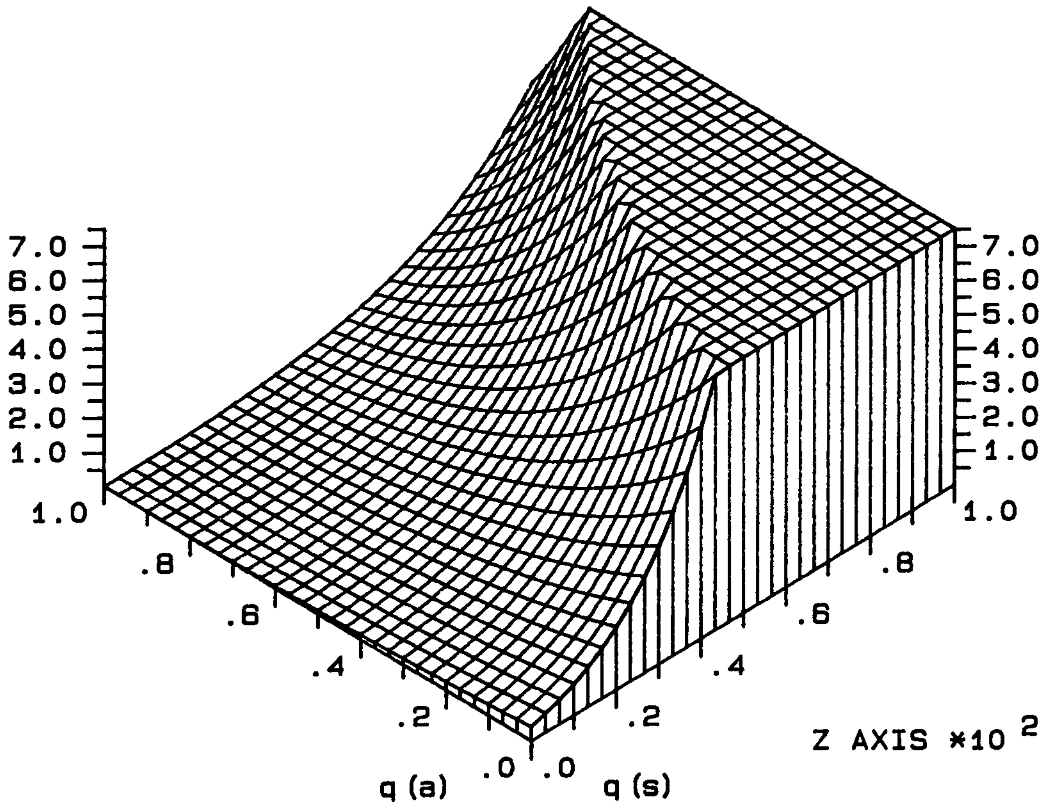


Fig. 6.7 Payoff surface for Q=0.9.

CHAPTER 7: A PILOT FAST-GUESS EXPERIMENT

7.1 INTRODUCTION

The term "fast-guess model" can be used in two senses. In the narrower sense of the term, it refers to models such as the GFGM and RSFGM which were described in Chapter 5. However, the term can also be used in a broader sense to refer to any model which uses fast-guesses to explain S-A tradeoff. Now, the reasoning presented at the end of Chapter 4 comes close to a theoretical approach known as the "deadline" model. This was first expounded by both Kornblum (1973a) and Ollman & Billington (1972) in order to account for foreperiod effects in SRT. However, it is quite reasonable to apply the same principles to a CRT task. If this is done, the result is a the fast-guess model (in the wider sense of the term) which has been enhanced by a mechanism which explains the differential genesis of SCRs and FGs.

The deadline model assumes that, on each trial, the subject sets a deadline (which is usually regarded as a random variable with a symmetric distribution). If the stimulus arrives before the deadline, the stimulus information is processed in the usual way. On the other hand, if the deadline expires first, then an FG is made.

Now, if the subject is using deadlines, it seems quite reasonable for the time-estimation process to be closely connected with them. More precisely, it is to be expected that the variance of the deadline distribution will reflect the accuracy of time-estimation. Thus, when time-estimations are precise, the variance would be small and when time-estimations are less precise (as they would tend to be if the foreperiod were long or variable) the variance would be larger. This effect would lead to the further one of a positive correlation between the mean and variance of the deadline distribution, in order that the proportion of premature responses should remain constant. Thus, with these assumptions, the model predicts that deadline-controlled RT (i.e. FG latency) increases with temporal uncertainty.

The experiment described in this chapter was undertaken in order to investigate the feasibility of examining the effect of manipulating time-uncertainty under the fast-guess model, by comparing the results obtained using fixed and irregular foreperiods. This manipulation was thought worthy of attention for the following reason. It is a common practice in CRT research to run experiments with error rates as high as ten per cent and then to discard the error responses and analyse the correct ones. This procedure carries with it the tacit assumption that the data thus obtained are much the same as if no errors had been committed by the subject. However, if we make the reasonable assumption that the higher error rates actually include preprogrammed responses (or fast guesses), as seems likely from the experiments reported in Chapter 4, then this assumption is clearly at fault. The data obtained in the manner described will still contain the remaining (correct) FGs. This could be important for two reasons. Firstly, the FGs obtained in some experimental conditions could be faster than those obtained in others, without there being any difference in true information processing rate between the two conditions. Secondly, different proportions of FGs could be obtained in different experimental conditions, which could also occur quite independently of any differences in SCR latency.

Now, if the model suggested in the first paragraph is appropriate, it seems quite likely that any effect of time uncertainty would be located on the FGs, because these are generated by a time estimation process. Perhaps it is worth mentioning at this point that an earlier study by the present author (White, 1976) suggested that the foreperiod effect (i.e. the tendency for RT to increase with foreperiod length, with constant foreperiods) was located entirely on the FGs. That experiment is not described here in detail because of flaws in the payoff technique used. However, the results were impressive enough to prompt the further pilot experiment that is reported here.

The deadline model also suggest another testable prediction concerning FG latency. If deadline positioning is the only factor which determines whether a given response will be an FG or an SCR, then it would be expected that FG latency would be correlated with 'q'. This is because shortening the deadline setting has the effect of reducing FG latency as well as increasing the proportion of FGs. Bearing in mind the fact that the fast-guess models assume that FG latency is invariant over changes in 'q', it is possible to decide whether the deadline model or the fast guess model (in the narrower sense of the term) provides a better fit to the data. This is done by inspecting the scatterplot of 'v' versus 'x' for non-linearity (see Equation 5.13). If the curve tends to flatten out for values of 'x' near the maximum of the range, then this suggests that the mean FG latency increases with 'q' as required by the deadline model, rather than remaining constant as in the fast-guess model.

7.2 METHOD

7.2.1 Apparatus

The apparatus consisted of four pieces of equipment, each linked to the IBM 1130 via the WDV interface. The SR box and foreperiod light were the same as those used in Experiments CRT24 and CRT27 in the previous series of experiments. In addition, a box of feedback lights was also included in the display. This was constructed on the same pattern as the SR box (described under Experiment CRT22) but with three of the green lights in the top row replaced by red, blue and amber lights of a similar type. The remaining piece of apparatus was a digital centisecond timer, employed as a counter and driven by the computer via the interface. Both the box of feedback lights and the counter were placed so that they were just to the left of the subject's centre line of vision when he was working normally. The level of illumination of all the lights used was the same as that employed for Experiment CRT22.

7.2.2 Task

As in the previous series of experiments, the task was a two-choice one, using the outer lights in the top row of the SR box as stimuli and the buttons directly underneath as response buttons. However, unlike the previous experiments, no home key was used - the subject worked with one finger from each hand resting on the response buttons. As before, light offset was used as the stimulus event. The counter-timer was used for two purposes. During each block of trials, it provided a running total of the number of errors that the subject had made in that block. At the end of each block, it displayed the payoff (in points) for that block. Anticipations and very fast responses (i.e. those faster than a specified criterion time) were recorded by a binary counter which consisted of the five green lights in the feedback box. Unlike the previous experiments, the commission of errors did not delay trial completion. However, trials on which anticipations occurred were aborted and presented again.

The subject had to press both response buttons simultaneously in order to start a block of trials. After this, the event sequence for a single trial was as follows:

1. The subject started the ITI by releasing the response button(s).

2. As soon as the ITI had expired, the warning light was illuminated for the duration of the foreperiod.

3. At the end of the foreperiod, the computer presented one of the two possible stimuli by switching off one of the stimulus lights.

4. The subject then responded by pressing a response button. The RT was recorded in the usual manner, along with the stimulus presented and the response made.

5. As soon as a response had been made, the stimulus light was re-illuminated.

7.2.3 Design

Trials were arranged in blocks of 60. In all blocks, the two stimuli were equiprobable. Two different types of block were used. One had a fixed foreperiod of 500 ms duration and the other had an irregular foreperiod drawn from a discrete uniform distribution of the same mean. (Five different values were used, with separations of 100 ms). The first ten trials in each block were purely preparation trials and, although the results from these were recorded, they did not contribute to the calculation of the payoff. The time interval between a response on one trial and the occurrence of the warning light on the next was 500 ms and an inter-block rest pause of rather more than 15 s was used. Each subject performed in five main sessions, each session comprising 24 blocks of trials. The blocks were arranged so that a different random ordering of types was produced for each session. A constraint on the randomisation procedure ensured that each of the two possible values occurred twice and only twice in the first four blocks, the second four blocks, and so on. Each session lasted rather less than an hour.

No actual frequency constraints were used in the computation of the stimulus sequences themselves. Probabilistic constraints alone were used. The decision to do this was made on the grounds that, with frequency constraints, when the end of the stimulus sequence in a block of trials is near, it is possible to predict quite successfully (or sometimes even completely successfully) which stimulus will occur next, if a mental count is kept of the number of stimuli of each type which have already occurred. If such a strategy were used, it would violate the assumptions on which the fast-guess models are based, because it would permit the subject to reliably exceed the expected number of correct fast guesses. In terms of probability theory, the crux of the matter is that the fast-guess models assume a process of sampling with replacement from a hypothetical population of stimulus alternatives. If frequency constraints rather than probabilistic ones are used in the actual selection of stimuli, this is equivalent to sampling without replacement from a strictly limited pool of stimuli.

7.2.4 Subjects

Four subjects were employed in this experiment. All were students from Durham University. Three were male (aged between 20 and 21) and one (EA) was female (aged 27).

7.2.5 Payoff And Feedback

The payoff technique described in Chapter 6 was implemented, with the parameter values for R and M (the expected payoff at optimum performance) set at 1.15 and 540 respectively. Also, a ceiling of 750 points was imposed on the block payoff in order to avoid any undue expense caused by exceptional subject performance, or misestimation of the parameters on which the payoff was based. The subject was required to work at a specified level of Q (the optimum proportion of SCRs) for each of the main sessions. Five levels were used and the order of administration was 0.1, 0.7, 0.3, 0.9, 0.5.

The value for q(a) for each block was estimated very simply from the error total for the block. The value assigned to q(s) for each block was arrived at by comparing the overall mean RT for the block with estimated values for 's' and 'g' derived from the four training sessions. These estimates were made separately for the two different types of block and were recomputed for each subject at the beginning of each training session according to that subject's performance in the previous training session. The starting values chosen for the estimation process were 280 ms for 's' and 200 ms for 'g' for both types of block. Once the main sessions were under way, the estimates for 's' and 'g' were not revised further. Penalty points were deducted from the payoff total for each anticipation or very fast response. The criterion time for very fast responses was varied between subjects as was the penalty for making anticipations and fast responses. For subject EA, the criterion time was 20 ms and the penalty 20 points. For subjects JP, BG and IC, the criterion times were 40, 60 and 80 ms and the penalties were 40, 60 and 80 points, respectively. The purpose in varying these parameters was to try to get some idea of how important it was with this type of payoff system to put some type of constraint on the speed of very short FGs.

Further information on performance was provided by the red and amber lights on the same box. If, at the end of a block, q(s) was found to have exceeded Q by more than 0.1, then the amber light was illuminated, indicating to the subject that he would have done better to go faster. Similarly, if q(a) was found to have fallen short of Q by more than 0.1, then the red light was displayed, telling the subject to make fewer errors in the next block.

7.2.6 Procedure

Each subject was put through a training procedure which took four full sessions. At the first session, a set of verbatim instructions (see Appendix 1) was read to the subject by the experimenter, who then asked if any clarification was required. If so, supplementary explanations were given, ad lib. Also, the basic concept of differential weightings for both speed and accuracy was explained in some detail. When the subject had no further questions, the experimenter worked through four blocks of trials acting as subject, with the real subject watching. If the subject had any further questions, these were answered. The subject was then asked to work through a few blocks with the experimenter sitting close by. Any inappropriate behaviour by the subject was corrected by the experimenter who answered any final questions put by the subject. The remainder of the first training session was spent with the subject working on his own, although the experimenter was monitoring his performance from another room. The whole of the first training session was run with Q (the optimum proportion of SCRs) set at 0.5. The next two training sessions were intended to give the subject some experience of working at other parts of the S-A continuum. They used Q values of 0.1 and 0.9 respectively. The final training session was run with Q set at 0.5. For each of the main (and training) sessions, the subject was informed before the start of each session what the speed and accuracy emphases would be. As a further guide, he was also told the expected optimum number of errors per block.

7.2.7 Data Processing

The data were transferred to an IBM 370 for analysis, using the GFGM model described in Chapter 5. Because it was quite clear from an inspection of the graphs of 'u' and 'v' versus 'x' (see Equations 5.12 and 5.13) that the error variances involved were not drawn from the same population, separate regression procedures were employed. This analysis was followed by two-way analyses of variance performed on each of the three parameters, as described in Section 4.2.3 of Chapter 5.

7.3 RESULTS

The parameter estimates and their standard errors are displayed in Table 7.1. Unfortunately, data for two entire sessions for two subjects were lost due to a breakdown of the paper tape punch attached to the IBM 1130. This meant that the fast-guess analysis for subjects BG and IC had to be performed on the results of four sessions only, rather than five. For this reason, the number of points on which each regression analysis was based was only 48 for each of these subjects.

Subject	Parameter	Fixed FP		Irregular FP		t	df
		Mean	se	Mean	se		
EA	s	250	4.36	263	3.08	7.157***	214
	g	145	3.75	191	5.22		
	a	0.865	0.023	0.965	0.033		
JP	s	224	1.72	237	1.59	6.520***	214
	g	159	4.84	202	4.48		
	a	0.942	0.021	1.012	0.047		
BG	s	267	2.63	278	2.96	2.237*	190
	g	188	7.13	165	7.41		
	a	0.948	0.024	0.975	0.021		
IC	s	240	1.90	254	2.16	3.141**	190
	g	167	6.32	201	8.79		
	a	0.900	0.017	0.992	0.033		

Table 7.1 Parameter estimates and 't' tests for pilot fast-guess experiment.

The analysis of variance summary tables are shown in Appendix 3 (in Table A.3.1). They revealed two important findings. Firstly, SCRs for the constant foreperiod condition were significantly faster than for the condition with irregular foreperiods ($F = 410.684$; $df = 1, 3$; $p < 0.001$). Secondly, the 'a' parameter (the probability of an SCR being correct) was significantly lower in the constant foreperiod condition than with irregular foreperiods ($F = 19.498$; $df = 1, 3$; $p < 0.05$).

The FGs showed a significant interaction with Subjects ($F = 15.094$; $df = 3, 416$; $p < 0.001$). The means in Table 7.1 show clearly that this interaction was due to three subjects having faster FGs with the constant foreperiod than with the irregular ones, whereas the remaining subject (BG) showed the reverse tendency. This idea was supported by the results of separate two-tailed 't' tests carried out on the FG results of each subject. The results of these tests are also shown in Table 7.1.

Finally, it should be stated that when the scatterplots for 'v' versus 'x' (see Equation 5.13) were inspected, they showed no consistent tendency towards non-linearity.

7.4 DISCUSSION

The FG results suggest the expected effect operating for three out of the four subjects, with FGs being substantially faster in the constant foreperiod condition. However, the significant counter-effect obtained by the remaining subject is puzzling and cannot easily be accounted for.

The analysis of variance results for the 's' and 'a' parameters indicate the existence of an effect not envisaged in Section 5 of Chapter 5. The fact that SCRs are both slower <u>and</u> more likely to be correct in blocks with irregular foreperiods than in those with constant foreperiods is strongly suggestive of a tradeoff mechanism of the classical kind and hence of a criterion difference between the two experimental conditions.

The question of <u>why</u> subjects should exhibit greater caution when coping with an irregular foreperiod than when dealing with a constant one, is not easy to answer. The higher levels of expectancy which typically are found with constant foreperiods seem to be involved. However, the mechanism is not obvious. Why should being well-prepared for a stimulus (or response) make a subject more likely to commit an error (fast-guessing apart)? Possibly, another locus of temporal expectancy lies in the criterion settings of the stimulus sampling process, with less accurate criteria being employed when the subject is in a state of high expectancy.

The observation made at the end of the previous sub-section suggests that FG latency was independent of 'q', as required by the GFGM. This, in turn, indicates that the deadline model does not fit the data. The implication of this seems to be that, although some sort of fast-guess model (in the wider sense of the term) seems to be appropriate, the deadline model is not supported by these results.

CHAPTER 8: THE FINAL SERIES OF EXPERIMENTS

8.1 INTRODUCTION

Although the results of the pilot experiment described in the previous chapter were not really as expected, they were thought to be sufficiently interesting to make it worthwhile conducting further experiments in the same area. The five experiments described in this chapter were undertaken in order to determine the loci of various well-known effects in CRT, in terms of the parameters used in fast-guess models. Section 5 of Chapter 5 gives an indication of the ways in which conventional analysis of CRT results might differ from those obtained by the application of fast-guess models.

Thus, when an independent variable (e.g. number of choices) is changed between blocks of trials, the resulting effect could be due to a change in either the SCR latency or the FG latency or some combination of the two. A change in the mean latency could also result from a change in the proportions of SCRs and FGs. In addition, a change in the 'a' parameter might also be observed - indicating a shift in the classical S-A criterion. On the other hand, when an independent variable such as stimulus probability is being investigated in the usual fashion, (i.e. with stimuli of different frequencies occurring in the same block of trials), the relative frequency effect could be due to either an SCR effect or to a fast-guess response bias.

8.2 PLAN OF THE FIVE EXPERIMENTS

8.2.1 Overview

As with the previously described experiments, these five experiments were run on-line using the IBM 1130. All the experiments were controlled by the same computer program, which was specially designed to run a wide range of CRT experiments. The experiments that were actually run only constituted a small subset of those available. Given more time (and money to pay subjects), a much more thorough exploration of CRT research could have been made from the fast-guess viewpoint. The five experiments that were run will be referred to simply by Roman numerals, as Experiments I to V.

The first experiment was designed to estimate the effect of different foreperiod lengths on the three parameters of the GFGM. The second experiment was concerned with the effect of constant versus irregular foreperiods. The third experiment used the RSFGM to analyse the effect of differences in stimulus frequency. The fourth experiment was concerned with sequential effects and also used the RSFGM. These four experiments used two-choice tasks. The fifth experiment looked at the effect of number of choices and used both two- and four-choice tasks. Many aspects of the method were the same, or similar, for all five experiments. These details are given in the following sub-section. The first three experiments were run in two parts. The first part of each experiment used conventional stimuli, while the second part incorporated a proportion of catch trials.

The experiments share most aspects of the Method, the common parts of which are described in the following section. The specific parts of each main experiment then follow, in order. Finally, the catch trial data is dealt with and some additional work on partitioned sequences of trials is reported.

8.2.2 General Method

8.2.2.1 Apparatus -

The apparatus used was similar to that employed in the pilot experiment described in the previous chapter, except that two counter-timers (rather than one) were used, placed one on top of the other.

8.2.2.2 Task -

The manner of use of the stimulus lights was the same as that employed in the previous experiment, except that all four of the stimuli and responses were used for the four-choice condition of Experiment V. As before, light offset was used as the stimulus event, with key-press responses. The event sequence was very similar to that described for the pilot experiment, except when catch trials were presented. On these occasions, the warning light occurred, but its offset was unaccompanied by any stimulus event. As in the pilot experiment, anticipations were recorded on the binary counter. Trials on which anticipations occurred were aborted and presented again.

8.2.2.3 Design -

Trials were arranged in blocks of 72. The first six trials of each block were familiarisation trials - intended to allow the subject some time to adapt to the particular parameters used for that block. After the sixth trial, all the counters were reset to zero and the blue light in the feedback box was illuminated, indicating to the subject that performance on all further trials in that block would count towards the block payoff. All experiments used 18 blocks of trials per session, except the first, which had 12. For a given subject, sessions were generally scheduled at the rate of one per day, on consecutive days, where possible. The first three experiments were split into two parts, with the second (subsidiary) part involving a proportion of catch trials. Each main experiment involved giving five principal sessions per subject, while each subsidiary experiment used three sessions per subject. Each session lasted rather less than an hour and there was a short rest period of about 30 s between blocks of trials. As in the pilot experiment, the inter-trial interval was 500 ms and Experiments III, IV and V used a fixed foreperiod of 500 ms.

8.2.2.4 Subjects -

Three subjects were employed in each experiment, with the exception of Experiment II, which used four subjects. All the subjects were male students from the University of Durham and, with the exception of one, were aged between 19 and 21. The remaining subject (IC) was aged 25 and participated in the first part of Experiment II only.

8.2.2.5 Payoff And Feedback -

The payoff technique was similar to that used in the pilot experiment but used a value of 450 for M. This lower value was chosen because subjects were found to attain the ceiling value of 750 rather too frequently in the pilot experiment. Also, there was a distinct difference in the way that q(s) was estimated. Independent deadlines were set for each of the conditions used and speed points were awarded on a trial by trial basis for each response that beat the appropriate deadline. During each block, one counter-timer displayed a running total of errors made in that block and the other displayed a running total of deadline-beating responses. The latter information was used to estimate q(s) for each block. This procedure was adopted in preference to the one used previously, for the following reason. The payoff function described in Chapter 6 displays an obvious symmetry between q(a) and q(s) and suggests that accuracy and speed should, as far as possible, be treated in an equivalent fashion. Now, the simplest way to do this is to dichotomise speed by using a deadline technique. This had the additional advantage that the speed and error points could be displayed (separately) to the subjects as running totals (as just described) in order to provide <u>within-block</u> feedback. The two counter-timers were used for this purpose. For those sub-experiments which used catch trials, the outcome of these was used (with the ordinary responses) to estimate q(a).

The secondary feedback technique that was used in the pilot experiment was also applied, as it seemed to help those subjects whose performance was far from optimal to alter their performance in the appropriate direction.

8.2.2.6 Procedure -

In every experiment, each subject was put through an extensive training procedure. This served two functions. Firstly, it familiarised him with the rather complicated task and ensured that he understood what was required of him. Secondly, the training procedure allowed the experimenter to estimate suitable deadlines for each of the subjects.

At the very first training session, a set of verbatim instructions (see Appendix 1) was read to the subject by the experimenter, who then asked if any clarification was required. If so, supplementary explanations were given, ad lib. The experimenter then worked through three or four blocks, acting as the subject, with the real subject watching. The experimenter then stopped and asked the subject if any further clarification was needed. This was provided if required. The subject was then asked to work through a few blocks with the experimenter sitting close by. Any inappropriate behaviour on the part of the subject was corrected by the experimenter, who then answered any further questions put by the subject. The remainder of the first training session was spent with the subject working on his own, although the experimenter was monitoring his performance from another room. The whole of the first training session was run with Q set at 0.5. The second training session had Q fixed at 0.9 and, if all went according to plan, the third training session used a Q value of 0.1. If the subject did not perform satisfactorily in any of the training sessions, they were repeated. The criterion for satisfactory behaviour was not precisely specified, but one aim was that the subject's performance as regards errors should be somewhere near the optimum for the particular value of Q in operation. Subjects did seem to start out with a pronounced bias towards accuracy, which was quite difficult to break in some cases. These subjects found the training session with Q set at 0.1 to be the most difficult of the three, particularly for long or irregular foreperiods and this condition had to be repeated a number of times for them.

Initially, all deadlines were set at 280 ms but throughout the training process, the deadlines were revised (usually in a downward direction) according to the subject's performance in the previous session. The revisions were made quite simply by observing the difference between q(s) and q(a). If q(s) was found to be less than q(a), this was taken to indicate that the deadline was too high and should be lowered. Separate deadline determination procedures were carried out for the different foreperiod conditions in Experiments I and II and also for the different numbers of alternatives in Experiment V. No further deadline adjustments were made once the training procedure had finished. Each main session used a different value of Q. The values employed were 0.1, 0.7, 0.3, 0.9 and 0.5, in that order. For the subsidiary experiments, the values of 'q' (in order) were 0.9, 0.1 and 0.5.

For those subsidiary experiments involving catch trials, the subjects were introduced to the idea after they had completed the five principal sessions. A further, short set of verbatim instructions (see Appendix 1) was read to each subject, who then performed the three subsidiary sessions without any further training and without any changes in the deadline settings.

The data from each experiment were transferred on magnetic disc to an IBM 370 for various fast-guess analyses to be performed.

Analyses of variance were performed on the results from the fast-guess modeling process and these are all summarised in Appendix 4.

8.3 EXPERIMENT I

8.3.1 Method

Four blocks were run at each of three different fixed foreperiods in each session. The foreperiods used were 500 ms, 1.5 s and 4.5 s. The three types of block were administered in a random order, with the constraint that each type appeared twice and only twice in the first six blocks and likewise in the second six blocks. The GFGM model was used to obtain separate parameter estimates for each of the three conditions. As in the pilot experiment, these were obtained using two regression analyses rather than one because of the lack of homogeneity of error variance. As before, this was followed by two-way analyses of variance.

8.3.2 Results

The parameter estimates and their standard errors are shown in Table 8.1. The analyses of variance did not reveal any significant effects, except that of Subjects on the SCRs. (The summary tables

Subject	Parameter	Short FP		Medium FP		Long FP	
		Mean	se	Mean	se	Mean	se
JP	s	230	7.64	218	5.07	213	3.45
	g	132	10.80	151	12.30	182	10.00
	a	0.895	0.038	0.929	0.043	0.973	0.081
DR	s	279	3.46	282	7.58	269	9.36
	g	171	5.64	167	15.40	168	18.60
	a	0.966	0.021	0.956	0.039	0.939	0.051
JA	s	267	5.07	255	3.29	271	4.18
	g	157	14.30	161	8.31	192	27.10
	a	0.966	0.046	0.975	0.025	0.972	0.039

Table 8.1 Parameter estimates for Experiment I.

are shown in Table A.4.1). However, for two of the subjects (JP and DA) there did appear to be a tendency for the FG latency to increase with foreperiod length. This was checked by carrying out separate one-way analyses of variance on the FG results of each subject. These are also summarised in Table A.4.1, in Appendix 4. Only that for subject JP turned out to be significant.

8.3.3 Discussion

The results obtained were somewhat disappointing and are rather hard to reconcile with those obtained in the pilot experiment, because there was no discernible tendency for the foreperiod effect to be reflected in either the latency or the accuracy of the SCRs. However, the results for the FG latencies for two of the subjects did seem to agree with those obtained in the pilot experiment and also with the unpublished results obtained by White (1976) which strongly suggested that the relationship between RT latency and foreperiod duration was due to differences in FG latency rather than SCR latency.

The problem of poor results could well lie with the experimental design. Because of the fact that three different foreperiod lengths were used and only 12 blocks per session, this meant that only 20 blocks of trials were run at each foreperiod length for each subject. This in turn meant that only 20 points were used in each regression analysis at the parameter estimation stage. This is reflected in the rather high standard errors, particularly for 'g', which indicate rather imprecise estimates.

8.4 EXPERIMENT II

8.4.1 Method

Nine blocks of trials were run with a fixed foreperiod of length 500 ms and nine blocks were run with an irregular foreperiod drawn from a distribution of exactly the same type as that used in the pilot experiment (i.e. a discrete uniform distribution with possible values of 300, 400, 500, 600 and 700 ms). The two types of block were administered in a random order, with the constraint that each type appeared three and only three times in the first six blocks, the second six blocks and so on. The same type of analysis was used as in the previous experiment.

8.4.2 Results

The values of the parameters and their standard errors are displayed in Table 8.2. The ANOVAs (summarised in Table A.4.2) showed three significant effects. Two of these were Subject effects (for 's' and 'a') and were of no interest. The remaining effect was a Subjects x Treatments interaction, present on the SCRs ($F = 3.202$; $df = 3, 352$; $p < 0.05$). However, all subjects showed a tendency for the SCR latency to be greater when the foreperiod was irregular than when it was constant. This observation was tested by carrying out separate two-tailed 't' tests for each subject. As can be seen from the results of these tests (also shown in Table 8.2), two were significant (subjects JD and IC) and two were not. The interaction was due to this effect being larger for subject IC than for the others.

Subject	Parameter	Fixed FP		Irregular FP		t	df
		Mean	se	Mean	se		
MA	s	250	3.06	256	3.24	1.346	88
	g	175	4.17	185	7.30		
	a	0.918	0.020	0.931	0.025	0.406	88
PK	s	275	4.01	280	3.20	0.975	88
	g	168	8.56	171	11.30		
	a	0.950	0.022	0.958	0.017	0.288	88
JD	s	241	1.35	247	1.45	3.029**	88
	g	194	3.15	186	4.27		
	a	0.968	0.019	0.966	0.015	0.083	88
IC	s	277	2.86	297	2.76	5.032***	88
	g	163	13.90	167	18.00		
	a	0.971	0.022	0.989	0.017	0.647	88

Table 8.2 Parameter estimates and 't' tests for Experiment II.

Similarly, three out of the four subjects (i.e. all except subject JD) showed a higher value for 'a' when the foreperiod was irregular. However, in this case, none of the 't' tests achieved significance (see Table 8.2).

8.4.3 Discussion

The results for 'a' and 's' provide some (rather weak) support for the findings of the pilot experiment. However, unlike the pilot experiment, there was no indication of an FG difference between the two conditions. Indeed, for each subject, the results show remarkably close values for the two conditions.

8.5 EXPERIMENT III

8.5.1 Method

All blocks of trials were run with a 2:1 stimulus probability ratio which favoured the left-hand stimulus. Due to an error in procedure, one subject (TJ) participated in <u>six</u> sessions rather than five. (The extra session was run with a 'q' value of 0.7). There seemed nothing to be gained from discarding the extra data, so it was utilised.

The RSFGM model was used to obtain parameter estimates for each subject. As in other experiments that used the GFGM, the lack of homogeneity of error variance dictated that separate regressions were employed. This time, three were used - one for each of the SCR parameters and the third for 'g' and 'a'. The first two of these were weighted regressions, because the same number of left and right responses did not occur in each block.

8.5.2 Results

The parameter estimates and their standard errors are shown in Table 8.3 and the ANOVA summary table appears in Table A.4.3. It did not show high-probability responses to be significantly faster than low-probability ones. However, matched-pair 't' tests, performed separately for each subject, clearly did. (These latter results are reported in Table 8.3).

There was a significant Subjects x Treatments interaction ($F = 39.588$; $df = 2, 285$; $p < 0.001$) but cursory inspection of the means showed it to be merely due to the difference between the two types of SCR being larger for some subjects than for others.

8.5.3 Discussion

Of course, the reason that the F ratio from the analysis of variance did not turn out to be significant (in spite of the fact that all the 't' tests were significant) is due to the mixed effects model employed, which dictated that the main effect concerned be tested against the interaction (with its mere two degrees of freedom) rather than the within-block error term (with its smaller mean square error term and larger number of degrees of freedom).

Subject	Parameter	Mean	se	t	df
TJ	s_1	217	1.88	26.533***	215
	s_2	279	1.37		
	g	137	3.60		
	a	0.983	0.010		
RM	s_1	241	2.61	6.089***	179
	s_2	260	1.71		
	g	133	3.35		
	a	0.945	0.010		
AB	s_1	252	3.72	6.984***	179
	s_2	286	3.14		
	g	140	4.10		
	a	0.948	0.016		

Table 8.3 Parameter estimates and 't' tests for Experiment III.

However, by regarding the results as coming from three, separate single-subject experiments and using 't' tests, it seems reasonable to conclude that manipulating the stimulus probability does have an effect on the SCRs, with high probability responses having faster SCRs than low probability ones. Unfortunately, the RSFGM does not permit separate estimation of the other parameters.

8.6 EXPERIMENT IV

8.6.1 Method

All blocks of trials were run with a sequential bias such that stimulus repetitions were twice as likely as stimulus alternations.

In order to be able to apply a fast-guess analysis, it was necessary to recast the stimulus sequences according to whether each stimulus was a repetition of its predecessor or not. This meant that each response had to be recast too. This resulted in one type of "response" (termed Type I responses) actually consisting of correct responses to stimulus repetitions and incorrect responses to stimulus non-repetitions, whereas the other type of "response" (termed Type II responses) consisted of correct responses to stimulus non-repetitions and incorrect responses to stimulus repetitions.

From this point onwards, the RSFGM model was applied exactly as in Experiment III.

8.6.2 Results

Table 8.4 contains the parameter estimates and their standard errors and the ANOVA summary table appears in Table A.4.4. The ANOVA yielded a significant effect, with Type I SCRs being much faster than Type II SCRs ($F = 106.746$; df = 1, 2; $p < 0.01$). The Subjects x Response Type interaction was also significant ($F = 5.291$; df = 2, 267; $p < 0.01$) but inspection of the means showed this to be due to small differences between subjects in the magnitude of the main effect.

8.6.3 Discussion

At first sight, the method of recasting the stimulus and response sequences that was used prior to applying the RSFGM might seem a little peculiar. However, it seems to be the only way of examining the effect on SCRs of having sequential dependencies in the stimulus sequence. Unfortunately, it was not possible to apply a version of the GFGM and so separate estimates of the other parameters could not be made.

The results clearly show that SCRs have faster Type I responses than Type II responses. Bearing in mind that SCRs have low error rates (estimated at between 2.4% and 4.0% for this experiment), then it seems reasonable to conclude that this large effect (between 80 and 110 ms for the different subjects) is due chiefly to correct responses to stimulus repetitions being faster than correct responses to stimulus non-repetitions.

Subject	Parameter	Mean	se
JD	s_1	254	4.05
	s_2	342	5.69
	g	142	5.06
	a	0.976	0.020
RM	s_1	228	2.60
	s_2	308	3.10
	g	142	4.00
	a	0.957	0.013
RW	s_1	266	5.98
	s_2	376	6.12
	g	113	4.57
	a	0.960	0.015

Table 8.4 Parameter estimates for Experiment IV.

8.7 EXPERIMENT V

8.7.1 Method

Nine blocks of trials were run using a two-choice task and nine using a four-choice task. The two types of block were administered in a random order, with the same constraints used as in Experiment II. The GFGM model was used to obtain separate parameter estimates for each of the two conditions.

8.7.2 Results

Unfortunately, the magnetic disc used to store the data at the end of each session and ultimately transfer it to the IBM 370 became partially overwritten and destroyed some of the data from this experiment before it had been transfered. The result was that nine blocks of trials were lost for each condition for subject MB and four for each condition for subject CD. Nevertheless, the analysis was conducted on the remaining data.

Table 8.5 shows the parameter estimates and their standard errors and the ANOVA summary tables appear in Table A.4.5. Apart from Subject effects, the only significant findings from the ANOVAs were Subjects x Treatments interactions for both the SCRs ($F = 21.046$; $df = 2, 238$; $p < 0.001$) and the FGs ($F = 9.764$; $df = 2, 232$; $p < 0.001$).

Taking a closer look at the data, there was a clear tendency for two-choice SCRs to be faster than four-choice SCRs. This was confirmed by 't' tests, performed separately for each subject, each

Subject	Parameter	Two-choice		Four-choice		t	df
		Mean	se	Mean	se		
DP	s	232	2.78	302	3.24	16.397***	88
	g	253	3.28	280	3.56		
	a	0.937	0.017	0.911	0.034	0.684	88
MB	s	253	3.28	280	3.56	5.578***	70
	g	147	4.40	182	6.52		
	a	0.905	0.019	0.842	0.066	0.917	70
CD	s	251	2.72	312	5.09	10.570***	80
	g	134	4.68	124	5.73		
	a	0.961	0.015	0.946	0.047	0.304	80

Table 8.5 Parameter estimates and 't' tests for Experiment V.

of which turned out to be significant. (The results of these are shown in Table 8.5). The interaction mentioned above was due solely to this effect being larger for some subjects than for others. (It actually ranged from 27 ms to 70 ms).

There were no consistent FG effects. The Subjects x Treatments interaction was found to be due to one subject having a shorter FG under the two-choice condition than with the four-choice task, while another subject showed the opposite effect and the third subject showed no difference at all!

Turning to the 'a' parameter, there was a clear tendency for all subjects to show higher values of 'a' in the two-choice condition than in the four-choice one. Unfortunately, 't' tests (reported in Table 8.5) did not confirm this observation.

8.7.3 Discussion

As regards the probability of an erroneous SCR, it is worth noting that, in spite of the lack of significance of the results, there is a certain uniformity about them. The more likely a subject is to make an erroneous SCR in the two-choice condition, the more likely he is to do so in the four-choice condition. More precisely, the probability of making an incorrect SCR in the four-choice condition is between 1.38 and 1.66 times larger than the corresponding figure for the two-choice condition. Furthermore, the fact that 'a' decreases as the number of choices is increased, whereas 's' increases, means that the effect of number of choices cannot possibly be due to a classical tradeoff. Thus it seems that the traditional finding is again upheld, with information processing really taking longer with four choices than with two.

8.8 SUB-EXPERIMENTS WITH CATCH TRIALS

8.8.1 Introduction

These sub-experiments were undertaken in an attempt to find out more about the nature of SCRs and FGs. For the first two experiments, it was thought that, even though SCRs are not perfectly accurate, subjects would not make such a gross error as a response on a catch trial, unless it were an FG. For this reason, it was envisaged that catch trials that were responded to would constitute a sample of pure FGs, which would have enabled estimates to be made of their variance for each subject and experimental condition. As for Experiment III, it was expected that catch trial responses would only occur on the high-probability response.

8.8.2 Method

For those parts of Experiments I and II that were concerned with catch trials, these occurred randomly with a probability of 0.333, with the stimuli remaining equiprobable. When catch trials were used in Experiment III, they had a probability of occurrence of 0.25 (which was the same as that of the less frequent stimulus). In all other respects, the method was as described for the corresponding main experiments. As regards data analysis, two-way factorial unweighted means ANOVAs were applied to the catch trial data for all three experiments. (Note that, unlike the analysis of the FGM parameters, this model is appropriate for the catch trial data from Experiment III, rather than the split-plot model, because blocks of trials are not being used as replicates and hence Treatments are not nested within blocks).

8.8.3 Results

The mean catch trial RTs for each subject and experimental condition in each of the three sub-experiments are shown with their standard errors in Table 8.6. For the results from Experiments I and II, a mere glance is sufficient to show that the initial assumption of catch trial responses being entirely due to FGs was quite untrue. In fact, the means appear to be much closer to the usual values for SCRs. Also, the catch trial data from Experiment III shows the outcome to be more complicated than was originally hoped. The high-probability catch trial responses were intermediate in latency between FGs and high-probability SCRs, suggesting a mixture of the two types of response.

The ANOVA performed on the results from Experiment II showed no significant results, but that performed on the catch trial data from Experiment I yielded a significant tendency for catch trial latency to increase with foreperiod length ($F = 8.464$; $df = 2, 4$; $p < 0.05$). The Subjects effect was also significant.

The ANOVA from Experiment III also had a significant Subjects effect and, more interestingly, a Subjects x Response Probability interaction ($F = 8.649$; $df = 2, 568$; $p < 0.001$). The table of means clearly shows this to be due to a Response Probability effect

Subject	Foreperiod	Mean	Standard Error	Number
JP	Short	215	4.17	52
	Medium	237	4.98	52
	Long	243	5.49	47
DR	Short	251	11.30	56
	Medium	250	6.36	55
	Long	268	6.65	41
JA	Short	233	9.94	36
	Medium	241	7.71	27
	Long	249	8.04	14

Experiment I

Subject	Foreperiod	Mean	Standard Error	Number
MA	Fixed	226	4.26	52
	Irregular	241	9.81	46
PK	Fixed	241	10.20	67
	Irregular	267	16.30	37
JD	Fixed	262	14.10	14
	Irregular	247	8.86	12

Experiment II

Subject	Probability	Mean	Standard Error	Number	t
TJ	High	184	3.99	119	7.194***
	Low	330	19.90	6	
RM	High	177	5.58	171	2.796**
	Low	210	10.40	14	
AB	High	162	4.44	244	2.986**
	Low	211	15.80	20	

Experiment III

Table 8.6 Parameter estimates for catch trial sub-experiments.

which, although being present for all three subjects, was larger for subject TJ than for the others. This effect failed to be significant in the ANOVA, for reasons which have been discussed previously. However, 't' tests, whose results are displayed in Table 8.6, showed each subject to have a significant Response Probability effect, with catch trial responses on the high-probability response being faster than catch trials on the low-probability response.

Because the catch trial responses appeared to be a mixture of FGs and SCRs, it was decided to attempt to estimate the number of each type for each Subject-Treatment combination for both Experiments I and II and also to estimate the latency of the catch trial SCRs, in order to compare them with the conventional SCRs. This was done quite simply by partitioning the trials into conventional trials and catch trials. The usual fast-guess analysis was applied to the former, with the exception that weighted regressions were used for the parameter estimation, because the number of conventional trials varied from block to block. For each block, an estimate of 'q' was made from the proportion of conventional trials that was correct and the 'a' parameter (estimated for the entire Subject-Condition combination) by applying Equation 5.4. It was then a simple matter to calculate what proportion of the catch trial responses were FGs and what proportion were SCRs. By assuming that the catch trial FGs had the same latency as the FGs from the conventional trials, the catch trial SCR latencies were estimated. They are shown in Table 8.7.

Subject	Foreperiod	SCR	Catch SCR	Number
JP	Short	212	224	43
	Medium	211	244	48
	Long	225	253	44
DR	Short	303	274	46
	Medium	302	285	45
	Long	322	282	35
JA	Short	257	233	36
	Medium	262	242	22
	Long	278	249	14

Experiment I

Subject	Foreperiod	SCR	Catch SCR	Number
MA	Fixed	250	263	18
	Irregular	272	286	14
PK	Fixed	333	293	49
	Irregular	332	290	30
JD	Fixed	242	290	10
	Irregular	247	259	9

Experiment II

Table 8.7 Estimated catch trial SCRs from sub-experiments I and II.

8.8.4 Discussion

The results suggest that the catch trial responses certainly contain incorrect SCRs as well as FGs. This constitutes additional evidence that the value of 'a' is typically less than 1, even for CRT experiments involving easy discriminations, thus contradicting the original assumption that responses to catch trials would constitute a pure sample of FGs. Actually, somewhat similar results were obtained by Cowan and Monroe (1970) but this was not known to the present author at the time this series of experiments was run.

Comparing the conventional SCR estimates with the catch trial SCR estimates reveals an interesting feature. For each subject in both experiments, the catch trial SCRs are either slower for all conditions or faster for all conditions than the conventional SCRs. The two-tailed probability of this occurring by chance is 0.25 for each subject in Experiment I and 0.5 for each subject in Experiment II, giving an overall probability of just less than 0.00~~2~~1.

Of course, this is a post-hoc observation and hence this hypothesis was suggested by the results. Under such circumstances, caution must be exercised in applying such reasoning. The theoretical significance of such a finding is not entirely clear but suggests that subjects may have differed in their strategies for dealing with catch trial SCRs. It is also interesting to speculate whether the finding has any implications for incorrect SCRs in general. Bearing in mind that <u>conventional</u> SCRs tend to be about 95% accurate, is it reasonable to infer that some subjects tend to make faster SCR errors than correct SCR responses and vice versa for other subjects?

8.9 FAST-GUESS ANALYSIS OF POST-ERROR BEHAVIOUR

8.9.1 Introduction

Both Burns (1971) and Laming (1968) observed that RTs following errors tended to be slower than those following correct responses. This was interpreted by Laming (1979) within the framework of the random walk model as being due to error-contingent boundary adjustments in the random walk process.

In Section 4.1 of Chapter 5, the possibility of partitioning blocks of trials was discussed. In particular, the partitioning of trials into those following correct responses versus those following errors was mentioned as being of possible value in helping to elucidate aspects of subject's strategy in dealing with the microstructure of S-A tradeoff.

8.9.2 Method

The data from Experiments III and IV were examined in this way, using the RSFGM on each subset of the data. These experiments only were chosen for further analysis because preliminary investigations suggested that, in the case of the other experiments, the standard errors of the partitioned parameters were too large to allow meaningful comparison. (This was due to the fact that, for Experiments I, II and V, the blocks of trials had first to be split into two or more sets - one for each experimental condition. This was not necessary for Experiments III and IV and hence the parameter estimates were each based on a larger number of blocks of trials).

The parameter estimations were followed by ANOVAs. Strictly speaking, split-plot models should have been used (because the factor of interest was nested within blocks of trials). However, because the within-plot covariance was not known (unlike the case dealt with in Section 4.2.4 in Chapter 5, where it was known to be zero), it was decided not to partition the error term, but to use a two-way factorial design and accept the loss of power involved. For similar reasons, where 't' tests were required, the independent groups method was employed, rather than the matched-pairs technique.

Estimates of 'q' for each condition were also made for each subject in Experiment III, by applying Equation 5.4. This was followed by the usual ANOVA.

8.9.3 <u>Results</u>

The parameter estimates are shown in Table 8.8 and the ANOVA summary tables in Table A.4.7 and A.4.8. Five out of the eight Subjects effects were significant but, of course, were of no interest.

The only SCR effect which even remotely approached significance was for Type I responses in Experiment IV. All the subjects showed a tendency for these to be faster following errors and, for two of the subjects, these tendencies were significant when tested with 't' tests (also shown in Table 8.8). There was also a significant interaction with Subjects for the same effect ($F = 7.517$; $df = 2, 511$; $p < 0.001$) due merely to the main effect being larger for some subjects than for others. Both the low-probability SCRs in

Sub	Par	Post-correct			Post-error			t	df
		Mean	se	n	Mean	se	n		
	s_1	214	1.84	108	223	6.59	94		
TJ	s_2	275	1.17	108	308	6.55	89		
	g	135	3.29	108	149	6.73	94	1.869	200
	a	0.968	0.009	108	1.011	0.029	94	1.416	200
	s_1	241	2.84	90	231	8.47	77		
RM	s_2	261	1.67	90	253	3.89	76		
	g	133	3.16	90	165	8.24	77	3.626***	165
	a	0.950	0.010	90	0.982	0.052	77	0.604	165
	s_1	251	4.68	90	244	5.56	84		
AB	s_2	286	3.67	89	287	7.17	81		
	g	143	4.51	90	160	6.70	84	2.105*	172
	a	0.955	0.020	90	0.963	0.032	84	0.212	172

Experiment III

Sub	Par	Post-correct			Post-error			t	df
		Mean	se	n	Mean	se	n		
	s_1	260	4.95	90	209	9.52	86		
JD	s_2	337	5.89	90	388	10.40	74	4.753***	162
	g	146	5.25	90	152	6.36	86	0.728	174
	a	0.989	0.023	90	1.009	0.040	86	0.433	174
	s_1	228	2.72	90	227	6.70	81		
RM	s_2	305	2.49	90	336	14.00	79	0.138	167
	g	143	3.73	90	153	9.62	81	0.969	169
	a	0.957	0.012	90	0.990	0.044	81	0.724	169
	s_1	268	6.46	90	225	9.37	86		
RW	s_2	379	6.47	90	349	8.08	78	3.778***	166
	g	115	5.27	90	124	4.58	86	1.289	174
	a	0.963	0.017	90	0.977	0.028	86	0.427	174

Experiment IV

Table 8.8 Parameter estimates for post-error partitions in Experiments III and IV.

Experiment III and the Type II responses in Experiment IV also showed significant interactions with Subjects, which were of no interest in the absence of the appropriate main effects.

Of more interest is the fact that, for both experiments, FGs were slower following errors than when following correct responses. Only the FG effect in Experiment IV actually achieved overall significance ($F = 48.077$; $df = 1, 2$; $p < 0.05$) but 't' tests (shown in Table 8.8) suggested that, for two subjects at least, the same effect was present in Experiment III.

It is also worth noting that, for all subjects in both experiments, the value of 'a' is higher following errors than following correct responses. Unfortunately, neither the F ratios nor individual 't' tests showed this result to be significant.

The estimates for 'q' in Experiment III (shown in Table 8.9) revealed a tendency for subjects to be somewhat less likely to make FGs on the trials immediately following errors but the ANOVA did not yield a significant F ratio for this effect. However, the Subjects x Treatments interaction was significant ($F = 4.006$; $df = 2, 537$; $p < 0.05$) and was due to the effect being larger for some subjects than for others. This was checked by conducting 't' tests (also shown in Table 8.9), which gave a significant result for subject AB only.

8.9.4 Discussion

Almost certainly, the fact that Type I responses in Experiment IV were faster following errors than following correct responses was due to the presence of error-correction responses (described in

Subject	Post-correct		Post-error		t	df
	Mean	se	Mean	se		
TJ	0.663	0.0211	0.693	0.0308	0.804	200
RM	0.605	0.0333	0.663	0.0338	1.222	165
AB	0.451	0.0289	0.643	0.0375	4.055***	172

Table 8.9 Post-error estimates for 'q' from Experiment III.

Section 4.1 of Chapter 2) among the correct responses to those stimulus repetitions which followed errors.

Turning to the effects observed on FGs and the 'a' parameter, it seems that both these represent trial-by-trial micro-adjustments made by the subject as a result of his success or failure on the preceding trial. The behaviour of the 'q' parameter in Experiment III is indicative of a similar tendency. This suggests that there are two aspects to the microstructure of S-A tradeoff. One is concerned with the rather coarse strategy of adjusting the relative proportions of SCRs and FGs from trial to trial and the other appears to be the classical type of effect in which the probability of an SCR is adjusted on a similar basis. (However, it must be admitted that the expected covariation with SCR latency did not show up in the data).

Thus it seems that the parameters used in the fast-guess model do not remain constant from trial to trial, but change in a manner which reflect the subject's attempts to maximise his payoff by adjusting his present performance on the basis of the success of his performance on recent trials. It also seems likely that the classical repetition effect may be associated with similar, differential parameter shifts, depending on the identity of the previous response.

This type of finding has devastating implications for the concept of S-A micro-tradeoff, as described in Section 4.2 in Chapter 2. It means that if subjects are apt to change their criterion positions on a trial by trial basis, then the notion of micro-tradeoff is just not applicable and the use of the CAF (described in the same section) is not valid either.

8.10 OVERALL CONCLUSIONS

It seems reasonable to draw the following general conclusions.

1. As far as can be ascertained from the rather poor quality of the results, foreperiod effects seem to have two loci. One of these is FG latency and the other is an S-A tradeoff of the classical type, (i.e. one not involving variation in the proportion of FGs). However, these particular inferences must be regarded with extreme caution because of the inconsistencies in the results obtained.

There was also some indication that subjects found it more difficult to achieve very low values of 'q' when required, particularly with long or irregular foreperiods, suggesting another possible locus for temporal uncertainty effects. Of course, these tendencies did not show up particularly well in the data, because of the type of experimental design used, which actually encouraged subjects to aim for a particular accuracy band at the same time as trying to be as fast as possible. Nevertheless, the tendency did appear to be present.

2. Effects due to the manipulation of stimulus probability and transition probabilities have a component of substantial magnitude located on the SCRs. This means that, although the non-equality of the FG response bias parameters will also contribute to these effects as conventionally calculated (particularly when error rates are high) they cannot be solely due to FG response bias.

3. The effect due to number of choices also appeared to be located on the SCRs and changes in SCR accuracy were also involved. However, unlike the foreperiod effects, the behaviour of the

parameters indicated a genuine increase in information processing difficulty, rather than a criterion shift.

4. The catch trial sub-experiments showed that, under time pressure, subjects can commit the gross error of making SCR responses to null stimuli. The results also indicated that at least some FGs must occur by a mechanism other than that of making detection responses. A distinct possibility is that some sort of time estimation process is involved.

5. The analysis of post-error behaviour indicated that subjects made running adjustments to their S-A criteria within blocks of trials, presumably in order to keep their performance at a near-optimal level as far as payoff was concerned. These adjustments were of two types. Following errors, subjects were less likely to make FGs than when the preceding response was correct and also, SCRs showed a tendency to be more accurate.

CHAPTER 9: CONCLUDING REMARKS

9.1 INTRODUCTION

In this final chapter, an attempt will be made to draw together the strands of reasoning to be found in the other chapters and to suggest an information processing scheme that would account for them. However, the proposals are extremely cautious ones and the result is a loosely structured set of processing stages: i.e. a general specification for a model, rather than a model itself.

9.2 METHODOLOGICAL MATTERS

Certain methodological issues come to mind when discussing experiments of the sort described in the two immediately preceding chapters. First of all, can the use of deadlines in a payoff scheme produce spurious effects, particularly on FGs? On a priori grounds, it does not seem unlikely that the choice of different deadlines for different conditions might cause FGs to be faster for those conditions having shorter deadlines. However, the alternative is to fix the deadlines at the same value for all the conditions, which would make it easier for the subject to achieve a given payoff in some conditions, than others. On balance, the former technique seems to have more in its favour, letting the deadlines be chosen in accordance with each subject's own performance. In the light of the actual results obtained in the various experiments, an assurance can be given that the FG latencies showed no obvious relation to the deadlines chosen, that was apparent from an informal inspection of the data.

However, the FGs themselves seemed to be markedly variable, both within and between subjects and within and between conditions. As a result, the standard errors of this estimated parameter tended to be large and the differences between conditions inconsistent. Possibly, this is due to the fact that FGs are free to vary within quite wide limits - bounded at the lower end by the stimulus occurrence itself (premature responses being disallowed) and at the upper end by the time taken to make an SCR. If the payoff system fails to hold FGs to the upper end of this range, then it is quite possible that the subject will generate them at all parts of the permissible interval.

A related difficulty is that, because the standard error for 'a' is derived from an expression containing 'g' and its standard error, the results for se(a) were somewhat larger (relative to 'a') than was hoped. This in turn led to difficulties in testing the significance of some of the effects observed on 'a'.

Another problem relating to the FGs is the fact that, because they are only correct at chance level, the presence of a large proportion of them in a block of trials tends to produce large variability of the latency for the achieved accuracy level. (This is because the same accuracy level can be attained by different proportions of FGs, with the subject being "luckier" on some blocks than on others). A possible way to alleviate this problem is not to explore the whole of the S-A continuum but to attempt to concentrate the subject's performance at around the 85% accuracy level. Perhaps a good strategy would be to aim for half the errors to be SCRs and the other half to be FGs.

Turning to the actual fast-guess models used, they are necessarily somewhat restrictive in their structure. For example, the fact that correct and incorrect SCRs are constrained to have the same mean latency is not at all desirable. Yellott (1971) puts forward a number of other models which are more flexible than the GFGM and the RSFGM but they cannot actually be applied to experimental data because all their parameters cannot be estimated.

9.3 IMPLICATIONS OF THE EXPERIMENTAL FINDINGS

9.3.1 The Nature And Locus Of Foreperiod Effects

Taken together, the experiments reported earlier on foreperiod effects suggest (albeit somewhat weakly) that manipulations of temporal expectancy achieve their effect via at least two mechanisms. Firstly, it appears that S-A criterion changes of the classical kind occur, so that the latency of SCRs changes inversely with their accuracy. Secondly, it seems that, under certain circumstances, temporal uncertainty can also be reflected in FG latency, although this does not always happen. At present, it is not clear why this inconsistency occurs.

It is interesting to note that the last sub-section of Chapter 2 mentions some studies in which foreperiod variables are manipulated and where error rates differ between the various conditions, in such a manner that they are inversely related to the latencies for these conditions. This is suggestive of the observed effects being due to accuracy criterion differences between the conditions. Of course, another possible locus for the effect (when present in a conventional task) is on 'q' - the probability of a response being an SCR. It is quite plausible that the proportion of FGs declines with increasing time uncertainty. However, there is little point in looking for such an effect in the experiments reported in this thesis because the payoff system is specifically set up to control 'q' and bring the subject's performance close to the optimum value, i.e. Q.

It seems reasonable to conclude tentatively that effects of temporal uncertainty are probably due to criterion shifts of one or two types - one type being that responsible for the conventional S-A tradeoff and the other type being the proportion of FGs generated. There may also be a tendency for FG latency to reflect time uncertainty.

9.3.2 Errors And S-A Tradeoff

The analysis of post-error performance presented in Section 8.9, strongly suggested that subjects achieve S-A tradeoff by the two types of criterion adjustment mentioned at the end of the previous sub-section. This has important implications in the assessment of contending models of stimulus identification. Frequently, reviewers (e.g. Broadbent, 1971) have attempted to choose between different models on the basis of whether they predict that errors are generally faster than correct responses (at least for tasks involving easy discriminations). As a result, the random walk model (which in its simpler forms predicts the same latencies for errors as for correct responses) has come off badly. However, the point about such properties of models is that they depend on criterion settings being the same on errors, as on correct responses, which certainly does not seem to be true. Thus, this type of assessment of models is absolutely pointless.

Another factor is that, even in conventional tasks not dedicated to the investigation of fast-guessing, FGs may be present among the errors, reducing the error latency. In reality, the situation may well be much more complex, with different types of error arising at different stages of processing. Thus, Rabbitt & Vyas (1970) distinguished errors of perceptual discrimination from three types of error in the selection and execution of responses.

In this context, it is worth noting that Falmagne (1972) tried to claim that the fast-guess model could not be true because it did not fit the pattern of error data obtained from his earlier experiment (Falmagne, 1965). However, this claim was based on the assumption that, because the task involved easy discriminations, SCRs must be perfectly accurate. From the experiments presented in Chapters 7 and 8, it can be seen that this assumption is not justified. For the two-choice conditions in the various experiments, 'a' typically took a value between 0.9 and 1.0 but there was a tendency for the value to be somewhat lower for the four-choice condition in Experiment V. Now the error rate in Falmagne's task was marginally less than ten per cent. Bearing in mind that he used a six-choice task which might be expected to give lower values of 'a', it is not implausible that the majority of his errors were in fact incorrect SCRs!

To conclude, it is not claimed that all errors are due to FGs. However, it is argued that FGs should be taken account of, particularly when error rates are high. Finally, as well as being of interest in their own right, FGs are important because of the possible light that they can throw on that other (postulated) class of bypass process: stimulus-controlled response pre-selection.

9.3.3 Other Effects

The other effects studied included stimulus/response probability (Experiment III), stimulus/response repetition probability (Experiment IV) and number of choices (Experiment V). Each of these effects showed an obvious locus on the SCR latencies. In Experiment V, the number of choices seemed also to affect 'a', with incorrect SCRs becoming increasingly likely as the number of choices was increased.

Taking the evidence from these experiments and also from others referred to in earlier chapters, it seems reasonable to draw some general conclusions concerning types of effect. There appear to be three types of effect, although not all effects fit neatly into just one of the categories. Their characteristics are as follows:

1. Type A effects are those which are due solely to one or more aspects of S-A tradeoff, i.e. they are due to various criterion shifts. Temporal uncertainty appears to belong here.

2. Type B effects are those whose principal locus is one or more of the processing stages described in Section 3.3. Effects such as number of choices and their probabilities seem to belong here, and

so does S-R compatibility.

3. Type C effects are those due to selective preparation. The standard repetition effect probably belongs here and so does the prediction effect and all its interactions with other effects. Some effects which are principally of Type B may also be partially located here (e.g. stimulus probability).

Such a classification is an aid to developing a general scheme for an overall model of CRT, which is attempted in the next section.

9.4 AN INTEGRATED SCHEME FOR CHOICE REACTION TIME

9.4.1 Stages Of Processing

In Section 3.2, Sternberg's additive factor method was outlined. This will now be applied to the experimental findings outlined in earlier chapters, using the model shown in Fig. 9.1 as a general framework. The scheme goes beyond a simple stage model in that it includes two bypass mechanisms. It is, however, based on that proposed by Sternberg (1969). The assignment of type B effects to stages may be outlined as follows:

1. S-R compatibility can only reasonably be located at the response selection stage. Any other variable that interacts with it must therefore have a component at this stage.

2. Number of choices interacts with compatibility and must therefore have a component at the response selection stage. Now, number of choices can be partitioned into number of stimuli and number of responses. These two components themselves interact. It

```
                                    stimulus controlled
                                   response pre-selection
                                   +--------->---------+
                                   |                   |
                                   ^                   v
                                   |                   |
   +----------+    +----------------+    +-----------+    +-----------+
->-| stimulus |->-|    stimulus    |->-| response  |->-| response  |
   | encoding |    | identification |    | selection |    | execution |
   +----------+    +----------------+    +-----------+    +-----------+
              |                                        |
              v                                        ^
              |                                        |
--------------+------------>------------------>--------+
              stimulus independent response pre-selection
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Fig. 9.1 A general scheme for a model of CRT.

is suggested that both have components at the response selection stage. This seems reasonable on a priori grounds, once it it appreciated that increasing either the number of stimuli or the number of responses can increase the difficulty of selecting a response.

3. The foregoing reasoning leaves undecided the issue of whether the number of choices has a component at any other stage of processing. However, Sternberg (1969) found that number of choices interacted with stimulus quality (as well as with S-R compatibility), suggesting that this variable number of choices also affects the stimulus encoding stage. On a priori grounds, it is quite possible that number of stimuli (but not responses) also has an effect at the stimulus identification stage.

4. Although stimulus and response probabilities were classified as type B effects earlier in the chapter, it seems likely that a substantial portion of the stimulus probability effect, at least, is due to stimulus controlled response pre-selection. However, the probability effect cannot be purely of type C, because otherwise the effect would not be observable on incorrect predictions. Both Blackman (1975) and Miller & Pachella (1973) found that stimulus/response probability interacted with stimulus quality (as well as with S-R compatibility), suggesting that this variable also affect the stimulus encoding stage. Once again, it seems reasonable to suppose on a priori grounds that stimulus probability also has an effect at the stimulus identification stage. Because of the interaction with compatibility, a component of the probability effect must lie at the response selection stage, too. The results obtained

by Spector & Lyons (1976) show that stimulus probability interacts with compatibility and it seems reasonable to suppose that response probability does so, as well.

5. As regards sequential effects, there are grounds for distinguishing between manipulated transition probabilities (which should be accounted for in the same way as stimulus/response probabilities) and the general repetition effect which could well be of pure type C, or could possibly be due to some sort of automatic facilitation aftereffect, a possibility mentioned by Vervaeck & Boer (1980).

9.4.2 The First Bypass Mechanism

From Fig. 9.1, it can be seen that there are two bypass mechanisms: stimulus-independent response pre-selection and stimulus-controlled response pre-selection. The former is simply another term for fast-guessing. It will be noticed that, in Fig. 9.1, there are two possible origins for FGs. Thus the broken line represents detection responses, while the bold line represents responses which are entirely stimulus-independent.

Now, the existence of the latter type of FG was demonstrated in the catch trial sub-experiments. Here, it was shown that some of the responses made on catch trials were FGs and it is tempting to embrace the type of deadline model which was described in Section 7.1. Under such a model, the FGs would be produced by a time-estimation process and hence it would be expected that they would reflect time uncertainty in a manner such that their latency would increase with increasing time uncertainty (in order to avoid an increase in the anticipation rate). However, it transpired that the results from the pilot experiment did not support that idea, because the FG latency, 'g', appeared to be invariant across changes in 'q', instead of increasing as 'q' increased, as expected under the deadline model.

Another version of the deadline model was discussed by Ollman (1977). Instead of the race being between a time estimation process and a detection process (which can be termed a detection/deadline model), the race is between a time estimation process and a stimulus identification process (i.e. an identification/deadline model). This means that, on any given trial, the subject sets a deadline and responds by making an FG if the stimulus has not been identified when the deadline expires. Of course, the same criticisms that were made about the detection/deadline model can be levelled against the identification/deadline model.

Also, the identification/deadline model suffers from a serious methodological problem. This becomes apparent when an experiment is carried out which attempts to separate task and strategy effects in order to determine the locus of some experimental manipulation. The difficulty was pointed out by Ollman (1977) and is as follows. The deadline is adjustable, permitting an S-A tradeoff. Such adjustments are purely strategy changes. If a particular experimental manipulation produces an increase in information processing time then, for a given deadline setting, fewer trials will beat the deadline, i.e. there will be fewer SCRs and more FGs. If it is required to keep an approximately constant error rate across changes in experimental conditions, then it is necessary for the subject to make a strategy change, i.e. to adopt a longer deadline. Thus, although the results seem to indicate a straightforward task effect (i.e. an increase in RT latency, with constant error rate), in fact they are partly due to a strategy shift.

Putting the matter in a rather more general way, what is being done is to infer constant strategy from constant accuracy and then to attribute RT differences to task effects. Ollman (1977) points out that the assumption underlying such an inference (when applied to experiments on foreperiod variables) is that variation in foreperiod parameters cannot influence the deadline setting. Now, whether or not this is a reasonable assumption depends critically on the experimental design. If the foreperiod parameters are varied <u>within</u> blocks of trials, then it is obviously true that the deadline setting cannot reflect the corresponding variation in time uncertainty. On the other hand, if foreperiod manipulations are carried out <u>between</u> blocks of trials, then the deadline setting could indeed alter as a result of changes in time uncertainty and thus, in this case, the inference of constant strategy from constant accuracy is not valid for the deadline model.

To summarise, it seems that there are too many problems with deadline models for them to merit further consideration. However, it does seem that FGs really do exist and the problem of accounting for the differential genesis of SCRs and FGs remains. All that can be suggested is a Bernoulli process of parameter 'q', determining whether an SCR or an FG is made on a given trial. Thus the payoff parameters influence the subject's strategy, which in turn determines 'q', the parameter of the Bernoulli process. This will then be the first process which occurs on a trial. If the outcome of the Bernoulli process specifies an FG, then a response is chosen by another Bernoulli process, according to the appropriate bias parameters, as specified by the fast-guess model concerned. This constitutes the first bypass mechanism. On the other hand, if the outcome of the first Bernoulli process specifies an SCR, then the information processing is presumed to proceed by the usual stages (with the possibility of executing a pre-selected response, as described below).

This account leaves unresolved the question of whether detection FGs exist. The results from the catch trial sub-experiments described in Chapter 8 certainly suggested that time estimation FGs were being produced but left unresolved the issue of whether what are essentially SRT responses are ever made in choice situations under time pressure. The way to test this would be to induce subjects to work at a rate which produced 100% FGs on a choice task and then to introduce a small proportion of catch trials in order to see if any responses are withheld. If so, the subject must be making detection FGs on some of the trials. The relative proportions of the two types of FG could then be calculated easily from the proportion of catch trials to which response was made.

9.4.3 The Second Bypass Mechanism

The second bypass mechanism (i.e. that of response pre-selection) is less problematic. Basically, it assumes that subjects can pre-select a response and hold it ready for execution, pending the arrival of the appropriate stimulus. This is rather like loading a computer program into the core of the computer, ready for execution, but not actually running. The advantage gained is that of saving the response selection time in the case where that response does turn out to be the one required. (Presumably, having the wrong response ready for execution adds little or nothing to the RT compared with not having any response ready).

The bulk of the time consumed by response selection is consumed by S-R translation, i.e. applying the appropriate mapping rules to determine the response required, given the identity of the stimulus. The less compatible the S-R mapping, the longer this process takes, because of the greater complexity of the mapping rules involved. This means that the time saved by having the appropriate response pre-selected is greater for tasks of low S-R compatibility. This is exactly what has been found (see Section 3.4 of Chapter 2).

After a potential response has been pre-selected, it is checked against the identified stimulus (when this information becomes available), prior to releasing the response if, and only if, the stimulus was the predicted one. Of course, this checking process involves the application of a rule and takes time to perform. However, the crux of the matter is that because of the simplicity of the check, far less time is required than in the case of the normal S-R translation process.

It is interesting to consider that although the structure of the fast-guess models provides the means for separating effects due to the first bypass mechanism (i.e. FGs) from other effects, no such technique is generally used for removing the effects of response pre-selection. Of course, it is possible to record predictions prior to the arrival of the stimulus and to consider correct and incorrect predictions separately, as was done in various experiments described in the sub-sections of Section 3 in Chapter 2.

9.4.4 Further Comments

It seems that, in the past, theorists have rather ignored the response selection stage and have focused too strongly on stimulus identification. A truly comprehensive model of CRT performance must include both these aspects. According to Teichner & Krebs (1974), a large proportion of the total RT occurs at this stage, which is also where practice effects are located. Recently, however, rather more attention has been paid to the response selection process. Duncan (1977) proposed a process involving a set of transformation rules for deriving responses from stimuli, with RT being a function of the number of different rules in the whole mapping, rather than depending on the difficulty of individual S-R relationships.

It is also worth mentioning that the more stages of processing that a model possesses, the more possibilities there are for different types of error. The existence of various types of error, with different properties, could well allow some error phenomena to be explained by changing proportions of two or more types of error between conditions. An explanation of this sort may well underlie the observation that, in tasks requiring difficult discriminations, errors tend to be slower than correct responses, which is the opposite of that which occurs in tasks using easy discriminations.

The general scheme proposed here is complicated. It makes no attempt to explain all CRT effects in terms of one mechanism at a single stage because this appears to be neither realistic nor possible. It could be criticised on the grounds that it lacks parsimony but, on the other hand, it could be argued that there is no particular reason to suppose that parsimony is an appropriate criterion for judging information processing models. After all, the CNS is itself complicated in both its structure and function.

The scheme makes no definite statements about the nature of the mechanisms operating at the various stages. For example, the stimulus identification stage could be a random walk process, or an accumulator model, or some other type of process altogether. The most appropriate models for the various stages need to be derived by experiment, although further work is probably necessary first to determine the stage location of various effects, bearing in mind that this determination requires removal of bypass effects. Such a complicated scheme might prove awkward from the point of view of parameter estimation but one technique which might be of help is the construction of a computer model of the whole process, which could be used as a test bed for trying out various models for the processes occurring at different stages.

Perhaps it should also be mentioned that a stage model is not necessarily the only type of model which could be applied to CRT tasks. For example, McClelland (1979) examines the possibility that the components of an information processing system all operate in an overlapping fashion, rather than a strictly serial one, in a system known as a cascade. However, McClelland's model is extremely complicated and difficult to apply and specifically precludes the bypassing of component processes. For this reason, it was felt to be much more appropriate to adapt the idea of an ordinary stage model, rather than work with anything else.

9.5 THE NATURE OF PREPARATION

This thesis began by referring to two aspects of preparation - temporal and selective. Now, it is clear from what was said earlier in this chapter that the second bypass mechanism is responsible for at least some part of the latter. The subject quite simply exercises selectivity in preparation by pre-selecting the favoured response. The temporal aspects of preparation are more complicated, although it seems safe to assert that they are quite independent of the ~~temporal~~ selective aspects. Some possible loci for temporal expectancy were mentioned in Section 3.1.

Firstly, an S-A tradeoff of the classical kind appeared to be present in at least some of the data from the relevant experiments described in Chapters 7 and 8. However, why subjects should work faster with constant foreperiods than with irregular ones is not immediately clear.

Secondly, the tendency for temporal uncertainty to be reflected in FG latency was also noted. This seems to be due to a time estimation process which, it must be emphasised, has no connection with the deadline model (which has been rejected already). Quite simply, if the first Bernoulli process specifies an FG, a time estimation process is put into operation which results in an FG response being made towards the response selected by the second Bernoulli process (as described in Section 4.2).

Thirdly, a variation in 'q' was mentioned as a possible source of latency differences between foreperiod conditions. The most obvious way in which 'q' could depend on foreperiod type is by the operation of a deadline mechanism - but this has been ruled out. However, the possibility remains that 'q' varies inversely with temporal uncertainty in the same sort of way as the classical tradeoff, mentioned earlier.

Thus it seems that, of the possible loci, that of FG latency is easily explicable as being due to an FG-specific time estimation process. This leaves two remaining loci of more mysterious origin. Both are S-A tradeoffs - one being of the classical type and the other due to FGs. Whether there is a common underlying controlling factor is not clear. At this stage it does not appear to be possible to say anything more about them.

Thus, although the temporal and selective aspects of preparation are controlled by separate mechanisms, they are both concerned with handling speed stress in reaction tasks. Selective preparation saves response selection time on some trials, while two of the mechanisms which seem to be implicated in temporal expectancy involve trading off accuracy for speed. The coarser of these mechanisms involves making FGs on a random basis.

Further research is needed to elucidate the underlying causes of the S-A criterion shifts which appear to underlie the phenomenon of temporal expectancy. It would probably be better to study the two mechanisms separately. The relationship between the classical tradeoff and foreperiod effects might be better studied by tasks involving more difficult discriminations, in order to provide a larger range of variation for SCR accuracy. Also, low fast-guessing rates would be obviously appropriate. For both types of tradeoff, an experimental design is needed which does not attempt to focus the subject's performance on fixed accuracy bands. Accuracy should be free to vary with the experimental condition.

APPENDIX 1: VERBATIM INSTRUCTIONS GIVEN TO SUBJECTS

A.1.1 EXPERIMENT CRT22

1. This is a choice reaction time experiment. It is run on-line from the computer.

2. On each trial, one of these lights [indicate stimulus lights] will go out. Your task is to turn it on again as quickly as possible by pressing the button directly underneath the light concerned [indicate response buttons].

3. It is important that you respond as quickly as you can on each trial because your reaction times are being recorded by the computer.

4. If you make an error by pressing the wrong response button on any trial, press the correct button as soon as you can. Your time to make the correct response will be recorded in this case, as will the fact that you made an error.

5. Now we come to this button [indicate "hold" button]. We call it the "hold" button. It has two functions. One is to standardise the distance that you have to move your finger each time you make a response. The other function is to prevent you from making a response before a stimulus has occurred. If you do this on a particular trial, the trial concerned is cancelled and another is

substituted in its place.

6. When the light incorporated in the "hold" button is illuminated it means that the experiment will not proceed further unless the "hold" button is pressed and held down.

7. The time for which the "hold" button must be depressed before the next stimulus occurs is called the "inter-trial interval" (ITI).

8. Each session consists of 15 blocks of trials and will last rather less than an hour. Each block will have a constant ITI but the value of the ITI will change from one block to the next. Five different ITI values are used in this experiment.

9. When all the lights in the display go out, you have reached the end of a block. The next block will follow after a rest period of approximately 30 seconds.

10. The stimuli are not equiprobable. The light on the left/right [indicate appropriate stimulus light] will go out more frequently than the other one throughout the experiment.

11. Use only the middle or index finger of your right/left [dominant] hand to press the "hold" button and response buttons. If the finger you are using gets tired during the course of the experiment, you may change to the other one but you should only change over fingers <u>between</u> blocks.

A.1.2 EXPERIMENT CRT24

1. This is a choice reaction time experiment. It is run on-line from the computer.

2. On each trial, one of these lights [indicate stimulus lights] will go out. Your task is to turn it on again as quickly as possible by pressing the button directly underneath the light concerned [indicate response buttons].

3. It is important that you respond as quickly as you can on each trial because your reaction times are being recorded by the computer.

4. If you make an error by pressing the wrong response button on any trial, press the correct button as soon as you can. Your time to make the correct response will be recorded in this case, as will the fact that you made an error.

5. Remember that only the end buttons on the bottom row function as response buttons; pressing any of the others will affect nothing.

6. Now we come to this button [indicate "hold" button]. We call it the "hold" button. It has two functions. One is to standardise the distance that you have to move your finger each time you make a response. The other function is to prevent you from making a response before a stimulus has occurred. If you do this on a particular trial, the trial concerned is cancelled and another is substituted in its place.

7. When the light incorporated in the "hold" button is illuminated it means that the experiment will not proceed further unless the "hold" button is pressed and held down.

8. Each session consists of 12 blocks of trials and will last rather less than an hour.

9. A warning light [indicate top light] occurs before the presentation of each stimulus. It stays on for a length of time called the "foreperiod" and goes out when the stimulus occurs.

10. The sequence of events on each trial is as follows:-

(a) The "hold" button is depressed and held down.

(b) After a length of time called the "inter-trial interval", the warning light comes on and remains on for the duration of the foreperiod.

(c) Both the warning light and one of the stimulus lights go out.

(d) The subject responds.

(e) The "hold" button is depressed again, starting the next trial.

11. Both the inter-trial interval and the foreperiod remain constant _within_ each block.

12. When a trial is cancelled, its replacement starts at the beginning of the inter-trial interval when the hold button is depressed.

13. There are three types of block in this experiment and you will get 4 of each in every session. They are as follows:-

(a) Type 1 has a short inter-trial interval and a short foreperiod.

(b) Type 2 has a long inter-trial interval and a short foreperiod.

(c) Type 3 has a short inter-trial interval and a long foreperiod. You will be able to tell which type of block you are on during the first few trials of each block.

14. When all the lights in the display go out, you have reached the end of a block. The next block will follow after a rest period of approximately 30 seconds.

15. The stimuli are not equiprobable. The light on the left/right [indicate appropriate stimulus light] will go out more frequently than the other one throughout the experiment.

16. Use only the middle or index finger of your right/left [dominant] hand to press the "hold" button and response buttons. If the finger you are using gets tired during the course of the experiment, you may change to the other one but you should only change over fingers <u>between</u> blocks.

A.1.3 <u>EXPERIMENT CRT27</u>

1. This is a choice reaction time experiment. It is run on-line from the computer.

2. On each trial, one of these lights [indicate stimulus lights] will go out. Your task is to turn it on again as quickly as possible by pressing the button directly underneath the light concerned [indicate response buttons].

3. It is important that you respond as quickly as you can on each trial because your reaction times are being recorded by the computer.

4. If you make an error by pressing the wrong response button on any trial, press the correct button as soon as you can. Your time to make the correct response will be recorded in this case, as will the fact that you made an error.

5. Now we come to this button [indicate "hold" button]. We call it the "hold" button. It has two functions. One is to standardise the distance that you have to move your finger each time you make a response. The other function is to prevent you from making a response before a stimulus has occurred. If you do this on a particular trial, the trial concerned is cancelled and another is substituted in its place.

6. When the light incorporated in the "hold" button is illuminated it means that the experiment will not proceed further unless the "hold" button is pressed and held down.

7. The time for which the "hold" button must be depressed before the next stimulus occurs is called the "inter-trial interval" (ITI).

8. Each session consists of 16 blocks of trials and will last about an hour. 8 of the blocks will have a constant ITI. 4 of these will have short ITIs and 4 will have long ITIs. The remaining 8 blocks will have irregular ITIs, i.e. 3 different ITIs will be used in a randomised order.

9. When all the lights in the display go out, you have reached the end of a block. The next block will follow after a rest period of approximately 30 seconds.

10. The stimuli are not equiprobable. The light on the left/right [indicate appropriate stimulus light] will go out more frequently than the other one throughout the experiment.

11. Use only the middle or index finger of your right/left [dominant] hand to press the "hold" button and response buttons. If the finger you are using gets tired during the course of the experiment, you may change to the other one but you should only change over fingers <u>between</u> blocks.

A.1.4 <u>PILOT FAST-GUESS EXPERIMENT</u>

1. This is a choice reaction time experiment. It is run on-line from the computer.

2. On each trial, one of these lights [indicate stimulus lights] will go out. Your task is to turn it on again by pressing the button directly underneath the light concerned. [indicate response buttons]

3. If you make an error, do not correct it; just prepare yourself for the next trial.

4. Each trial is preceded by this light [indicate foreperiod light] which will stay on for a length of time called the foreperiod. This light then goes out at the same time as the stimulus light is extinguished.

5. Each session consists of 24 blocks of trials and will last rather less than an hour.

6. There are two different types of block in each session, in a randomised order. One type has a regular foreperiod and the other an irregular one.

7. Points are scored for both speed and accuracy and your score is displayed on this counter [indicate counter] at the end of each block. During the block, the counter records errors.

8. At all times, you should bear in mind that both speed and accuracy are important in determining how many points you score.

9. Each session is arranged so that the payoff is determined by speed and accuracy weighted to different extents. More will be said about this later.

10. The payment received for each session is calculated in pence by dividing the total number of points scored during the session by 240. I shall tell you after each session how much money you earned during that session.

11. If you make a response which is very fast, then a penalty is incurred. Very fast responses are registered on this binary counter [indicate green lights in LH box]. You lose 80 points for every fast response that you make. Very fast responses are those which take less than 80 ms.

12. If you make a response before the stimulus occurs, this is called an anticipation. Trials on which anticipations occur are cancelled and presented again. If you anticipate on a given trial, you lose 80 points.

13. If you score less than 450 points (before deductions for fast responses) on any block, additional information is provided to tell you how you should alter your performance to do better next time [indicate remaining lights in LH box]. A red light means that you made too many errors and an amber light means that you were too slow.

14. When all the lights in the display go out, you have reached the end of a block. The next block will follow after a short pause. This display [indicate starting configuration] indicates that the computer is ready for the next block. You may start by pressing both response buttons together.

15. Use only your two middle or two index fingers to make your response [get S to make choice].

A.1.5 FINAL SERIES OF EXPERIMENTS

A.1.5.1 General Instructions For Main Experiments

1. This is a choice reaction time experiment. It is run on-line from the computer.

2. On each trial, one of these lights [indicate stimulus lights] will go out. Your task is to turn it on again by pressing the button directly underneath the light concerned [indicate response buttons].

3. If you make an error, do not correct it - just prepare yourself for the next trial.

4. Each trial is preceded by this light [indicate foreperiod light] which will stay on for a length of time called the foreperiod. This light then goes out at the same time as the stimulus light is extinguished.

5. See individual expt. details.

6. See individual expt. details.

7. At all times you should try as hard as possible to achieve both speed and accuracy. To encourage you to do this, you are paid according to how well you work. During blocks, this counter [indicate top counter] registers points for speed and this counter [indicate bottom counter] registers points for errors. (The former count in your favour, whereas the latter count against you). At the end of each block, both counters will register your payoff for that block.

8. At all times you should remember that both speed and accuracy are important in determining how many points you score. Each session is arranged so that the payoff is determined by speed and accuracy weighted to different extents. The weightings are arranged on a 5 point scale on the speed-accuracy continuum as follows:

1 speed weighted much more than accuracy

2 speed weighted more than accuracy

3 speed and accuracy weighted equally

4 accuracy weighted more than speed

5 accuracy weighted much more than speed

You will be told at the start of each session which of the 5 conditions you will be working under and the speed and accuracy scores that you should aim for to obtain high payoffs.

9. If you do badly on a given block, additional information is provided to tell you how you should alter your performance to do better next time [indicate red and yellow lights in LH box]. A red light means that you made too many errors and a yellow light means that you were too slow.

10. If you make a response <u>before</u> the stimulus occurs, this is called an anticipation. Trials on which anticipations occur are cancelled and presented again. Anticipations are registered on this binary counter [indicate green lights in LH box]. 50 points are deducted from your block payoff for each anticipation made during

that block.

11. The total payment received for each session is calculated as one tenth of the average block payoff, in pence. I shall tell you after each session how much money you earned during the session.

12. The first few trials in each block are "warm up" trials and do not contribute in any way to your block payoff. A blue light [indicate blue light in LH box] will come on after this warm up period is over and all counters are reset to zero at this point.

13. When all the lights in the stimulus display go out, you have reached the end of a block. The next block will follow after a short pause. This display [indicate starting configuration] indicates that the computer is ready for the next block. You may start by pressing both response buttons together.

14. See individual expt. details.

15. If it is necessary to make a pause in your task for any reason, try to do so between two blocks rather than within one. It _is_ possible to pause within blocks (without incurring any penalty) by holding down a response button after responding in the usual way. However, you should not use this facility unless interrupted.

A.1.5.2 General Instructions For Secondary Catch Trial Experiments

16. Now we come to a slight change in the task. On some trials, the foreperiod light will come on as usual but the stimulus will not occur. These trials are called catch trials. If you make _any_ response at all on a catch trial, it will be regarded as an

error. Also, speed points cannot be won on catch trials.

17. See individual expt. details.

18. There will be only 3 different S-A conditions for this part of the experiment and positions 1, 3 and 5 will be used.

19. In all other respects, the experiment will continue exactly as before.

A.1.5.3 Individual Experimental Details

5. (I) Each session consists of 12 blocks of trials and will last rather less than an hour.

(II - V) Each session consists of 18 blocks of trials and will last rather less than an hour.

6. (I) There are 3 different types of block in each session, each with a different foreperiod length. The blocks are arranged in a random order.

(II) There are 2 different types of block in each session; one type has a regular foreperiod and the other an irregular one. The blocks are arranged in a random order.

(III) Throughout the experiment the stimulus light on the left will be more likely to go out than the other one.

(IV) Throughout the experiment stimulus repetitions will be more likely to occur.

(V) There are 2 different types of block in each session; one type has 2 stimuli and the other has 4. The blocks are arranged in a random order.

14. (I - IV) Use only your two middle fingers to make your responses in blocks where there are only 2 stimuli and your two middle and two index fingers when there are 4 stimuli [demonstrate].

17. (I,II,IV) Catch trials have the same probability of occurrence as individual stimuli.

(III) Catch trials have the same probability of occurrence as the less probable stimulus.

APPENDIX 2: SUPPLEMENTARY TABLES FOR FIRST SERIES OF EXPERIMENTS

Source	SS	df	MS	F
A	121135	3	40378	41.426***
B	871673	4	217918	72.306**
AxB	36166	12	3014	3.092***
S(AxB)	151081	155	975	
C	40818	1	40818	13.028*
AxC	9399	3	3133	24.539***
BxC	1745	4	436	1.899
AxBxC	2758	12	230	1.800
CxS(AxB)	19790	155	128	

First Component Times

Source	SS	df	MS	F
A	69452	3	23151	29.197***
B	66633	4	16658	8.009
AxB	24960	12	2080	2.623**
S(AxB)	122900	155	793	
C	244931	1	244931	40.537**
AxC	18127	3	6042	12.869***
BxC	73938	4	18484	14.215*
AxBxC	15604	12	1300	2.770**
CxS(AxB)	72777	155	470	

Second Component Times

Key - A: Subjects
B: ITI
C: Stimulus Frequency
S: Blocks of Trials

Table A.2.1 Analysis of variance summary tables for each component time in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	63700	4	15925	43.399***
Linear	61439	1	61439	167.434***
Quadratic	118	1	118	0.322
Cubic	2142	1	2142	5.839*
Within ITI	12843	35	367	

Subject PE

Source	SS	df	MS	F
Between ITI	107695	4	26924	47.932***
Linear	88047	1	88047	156.751***
Quadratic	12027	1	12027	21.411***
Cubic	7562	1	7562	13.463***
Within ITI	22468	40	562	

Subject NB

Source	SS	df	MS	F
Between ITI	134773	4	33693	74.617***
Linear	122250	1	122250	270.733***
Quadratic	7576	1	7576	16.777***
Cubic	4680	1	4680	10.364**
Within ITI	18062	40	452	

Subject PKW

Source	SS	df	MS	F
Between ITI	184775	4	46194	47.943***
Linear	162392	1	162392	168.540***
Quadratic	12460	1	12460	12.932***
Cubic	8526	1	8526	8.849**
Within ITI	38541	40	964	

Subject AF

Table A.2.2 Trend test summary table for first component responses to low frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	15207	4	3802	3.146*
Linear	7469	1	7469	6.180*
Quadratic	519	1	519	0.429
Cubic	6408	1	6408	5.302*
Within ITI	42300	35	1209	

Subject PE

Source	SS	df	MS	F
Between ITI	24146	4	6036	6.865***
Linear	9755	1	9755	11.095**
Quadratic	2040	1	2040	2.320
Cubic	9383	1	9383	10.673**
Within ITI	35170	40	879	

Subject NB

Source	SS	df	MS	F
Between ITI	65145	4	16286	17.731***
Linear	54661	1	54661	59.509***
Quadratic	3	1	3	0.003
Cubic	10476	1	10476	11.405**
Within ITI	36741	40	919	

Subject PKW

Source	SS	df	MS	F
Between ITI	68246	4	17061	16.203***
Linear	48534	1	48534	46.092***
Quadratic	6090	1	6090	5.784*
Cubic	11000	1	11000	10.447**
Within ITI	42120	40	1053	

Subject AF

Table A.2.3 Trend test summary table for second component responses to low frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	28951	4	7238	4.061**
Linear	25956	1	25956	14.564***
Quadratic	1125	1	1125	0.631
Cubic	1110	1	1110	0.623
Within ITI	63378	35	1782	

Subject PE

Source	SS	df	MS	F
Between ITI	45477	4	11369	9.651***
Linear	38937	1	38937	33.052***
Quadratic	4183	1	4183	3.551
Cubic	113	1	113	0.096
Within ITI	47123	40	1178	

Subject NB

Source	SS	df	MS	F
Between ITI	22734	4	5684	4.493*
Linear	13493	1	13493	10.668**
Quadratic	7747	1	7747	6.125*
Cubic	1181	1	1181	0.934
Within ITI	50594	40	1265	

Subject PKW

Source	SS	df	MS	F
Between ITI	35032	4	8758	6.856***
Linear	33447	1	33447	26.182***
Quadratic	1201	1	1201	0.940
Cubic	160	1	160	0.125
Within ITI	51099	40	1277	

Subject AF

Table A.2.4 Trend test summary table for total reaction time to low frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	46809	4	11702	28.515***
Linear	41496	1	41496	101.116***
Quadratic	3322	1	3322	8.096**
Cubic	1990	1	1990	4.849*
Within ITI	14363	35	410	

Subject PE

Source	SS	df	MS	F
Between ITI	89015	4	22254	34.575***
Linear	75574	1	75574	117.419***
Quadratic	8817	1	8817	13.698***
Cubic	4424	1	4424	6.874*
Within ITI	25745	40	644	

Subject NB

Source	SS	df	MS	F
Between ITI	142846	4	35711	103.232***
Linear	128218	1	128218	370.643***
Quadratic	7299	1	7299	21.099***
Cubic	7272	1	7272	21.021***
Within ITI	13837	40	346	

Subject PKW

Source	SS	df	MS	F
Between ITI	157428	4	39357	62.965***
Linear	129732	1	129732	207.551***
Quadratic	18896	1	18896	30.230***
Cubic	8585	1	8585	13.734***
Within ITI	25002	40	625	

Subject AF

Table A.2.5 Trend test summary table for first component responses to high frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	1587	4	397	1.247
Linear	1505	1	1505	4.731*
Quadratic	29	1	29	0.091
Cubic	51	1	51	0.161
Within ITI	11135	35	318	

Subject PE

Source	SS	df	MS	F
Between ITI	1195	4	299	1.485
Linear	717	1	717	3.564
Quadratic	235	1	235	1.167
Cubic	224	1	224	1.114
Within ITI	8046	40	201	

Subject NB

Source	SS	df	MS	F
Between ITI	2470	4	618	3.200*
Linear	2382	1	2382	12.343**
Quadratic	60	1	60	0.311
Cubic	2	1	2	0.011
Within ITI	7719	40	193	

Subject PKW

Source	SS	df	MS	F
Between ITI	6703	4	1676	5.389**
Linear	6002	1	6002	19.303***
Quadratic	120	1	120	0.386
Cubic	380	1	380	1.223
Within ITI	12438	40	311	

Subject AF

Table A.2.6 Trend test summary table for second component responses to high frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	64247	4	16062	13.211***
Linear	58861	1	58861	48.413***
Quadratic	2681	1	2681	2.205
Cubic	2703	1	2703	2.223
Within ITI	42554	35	1216	

Subject PE

Source	SS	df	MS	F
Between ITI	105314	4	26328	32.006***
Linear	90440	1	90440	109.944***
Quadratic	12066	1	12066	14.668***
Cubic	2465	1	2465	2.996
Within ITI	32904	40	823	

Subject NB

Source	SS	df	MS	F
Between ITI	178503	4	44626	51.644***
Linear	165551	1	165551	191.588***
Quadratic	5925	1	5925	6.856*
Cubic	7022	1	7022	8.127**
Within ITI	34564	40	864	

Subject PKW

Source	SS	df	MS	F
Between ITI	108155	4	27039	24.381***
Linear	80461	1	80461	72.551***
Quadratic	22373	1	22373	20.174***
Cubic	5321	1	5321	4.798*
Within ITI	44361	40	1109	

Subject AF

Table A.2.7 Trend test summary table for total reaction time to high frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
A	1.338	3	0.446	7.926***
B	7.510	4	1.878	8.273**
AxB	2.730	12	0.227	4.042***
S(AxB)	8.723	155	0.056	

Key - A: Subjects
B: ITI
S: Blocks of Trials

Table A.2.8 Analysis of variance summary table for reciprocal error scores to low frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
Between ITI	3.840	4	0.960	22.102***
Linear	3.294	1	3.294	75.838***
Quadratic	0.371	1	0.371	8.547**
Cubic	0.015	1	0.015	0.348
Within ITI	1.520	35	0.043	

Subject PE

Source	SS	df	MS	F
Between ITI	0.434	4	0.109	1.758
Linear	0.357	1	0.357	5.787*
Quadratic	0.056	1	0.056	0.907
Cubic	0.020	1	0.020	0.327
Within ITI	2.470	40	0.062	

Subject NB

Source	SS	df	MS	F
Between ITI	3.114	4	0.778	11.935***
Linear	2.980	1	2.980	45.694***
Quadratic	0.120	1	0.120	1.834
Cubic	0.007	1	0.007	0.110
Within ITI	2.609	40	0.065	

Subject PKW

Source	SS	df	MS	F
Between ITI	2.691	4	0.673	12.672***
Linear	0.274	1	0.274	5.162*
Quadratic	1.338	1	1.338	25.196***
Cubic	0.920	1	0.920	17.329***
Within ITI	2.124	40	0.053	

Subject AF

Table A.2.9 Trend test summary table for reciprocal error scores to low frequency stimuli in Experiment CRT22.

Source	SS	df	MS	F
A	276649	4	69162	37.789***
B	175418	2	87709	34.683**
AxB	20231	8	2529	1.382
S(AxB)	126285	69	1830	
C	54136	1	54136	20.443*
AxC	10593	4	2648	22.492***
BxC	573	2	286	4.421
AxBxC	518	8	65	0.550
CxS(AxB)	8124	69	118	

First Component Times

Source	SS	df	MS	F
A	31045	4	7761	13.571***
B	28338	2	14169	10.892*
AxB	10407	8	1301	2.275*
S(AxB)	39461	69	572	
C	211125	1	211125	81.691**
AxC	10338	4	2584	4.372**
BxC	13624	2	6812	6.024
AxBxC	9046	8	1131	1.913
CxS(AxB)	40788	69	591	

Second Component Times

Key - A: Subjects
B: Block Type
C: Stimulus Frequency
S: Blocks of Trials

Table A.2.10 Analysis of variance summary tables for each component time in Experiment CRT24.

Source	SS	df	MS	F
Between Types	28454	2	14227	31.537***
Contrast II,III	12871	1	12871	28.531***
Contrast I,II	2640	1	2640	5.853*
Within Types	6767	15	451	

Subject CJS

Source	SS	df	MS	F
Between Types	12824	2	6412	3.156
Contrast II,III	5167	1	5167	2.543
Contrast I,II	1587	1	1587	0.781
Within Types	30477	15	2032	

Subject PAT

Source	SS	df	MS	F
Between Types	29827	2	14913	7.187**
Contrast II,III	24571	1	24571	11.840**
Contrast I,II	243	1	243	0.117
Within Types	31127	15	2075	

Subject DHR

Source	SS	df	MS	F
Between Types	43585	2	21792	49.814***
Contrast II,III	19040	1	19040	43.523***
Contrast I,II	4447	1	4447	10.165**
Within Types	6562	15	437	

Subject MHE

Source	SS	df	MS	F
Between Types	4417	2	2208	3.620
Contrast II,III	1513	1	1513	2.480
Contrast I,II	741	1	741	1.215
Within Types	5490	9	610	

Subject MJS

Table A.2.11 Contrast summary table for first component responses to low frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	28065	2	14033	13.591***
Contrast II,III	6211	1	6211	6.015*
Contrast I,II	7854	1	7854	7.607*
Within Types	15487	15	1032	

Subject CJS

Source	SS	df	MS	F
Between Types	2565	2	1283	1.775
Contrast II,III	261	1	261	0.362
Contrast I,II	1121	1	1121	1.552
Within Types	10837	15	722	

Subject PAT

Source	SS	df	MS	F
Between Types	25755	2	12877	7.162**
Contrast II,III	21421	1	21421	11.914**
Contrast I,II	261	1	261	0.145
Within Types	26969	15	1798	

Subject DHR

Source	SS	df	MS	F
Between Types	8227	2	4114	5.474*
Contrast II,III	1801	1	1801	2.396
Contrast I,II	2324	1	2324	3.093
Within Types	11272	15	751	

Subject MHE

Source	SS	df	MS	F
Between Types	592	2	296	0.341
Contrast II,III	78	1	78	0.090
Contrast I,II	231	1	231	0.266
Within Types	7808	9	868	

Subject MJS

Table A.2.12 Contrast summary table for second component responses to low frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	1730	2	865	1.019
Contrast II,III	1200	1	1200	1.413
Contrast I,II	1387	1	1387	1.633
Within Types	12735	15	849	

Subject CJS

Source	SS	df	MS	F
Between Types	4670	2	2335	1.884
Contrast II,III	3201	1	3201	2.583
Contrast I,II	24	1	24	0.019
Within Types	18591	15	1239	

Subject PAT

Source	SS	df	MS	F
Between Types	148	2	74	0.111
Contrast II,III	102	1	102	0.153
Contrast I,II	1	1	1	0.001
Within Types	10039	15	669	

Subject DHR

Source	SS	df	MS	F
Between Types	14982	2	7491	11.000**
Contrast II,III	9130	1	9130	13.406**
Contrast I,II	341	1	341	0.501
Within Types	10216	15	681	

Subject MHE

Source	SS	df	MS	F
Between Types	1913	2	957	0.782
Contrast II,III	925	1	925	0.756
Contrast I,II	145	1	145	0.118
Within Types	11004	9	1223	

Subject MJS

Table A.2.13 Contrast summary table for total reaction time to low frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	20680	2	10340	31.066***
Contrast II,III	10208	1	10208	30.670***
Contrast I,II	1452	1	1452	4.362
Within Types	4993	15	333	

Subject CJS

Source	SS	df	MS	F
Between Types	8671	2	4336	5.772*
Contrast II,III	4181	1	4181	5.566*
Contrast I,II	660	1	660	0.879
Within Types	11268	15	751	

Subject PAT

Source	SS	df	MS	F
Between Types	26436	2	13218	6.853**
Contrast II,III	20584	1	20584	10.673**
Contrast I,II	30	1	30	0.016
Within Types	28930	15	1929	

Subject DHR

Source	SS	df	MS	F
Between Types	32203	2	16101	35.179***
Contrast II,III	14911	1	14911	32.578***
Contrast I,II	2791	1	2791	6.097*
Within Types	6865	15	458	

Subject MHE

Source	SS	df	MS	F
Between Types	4741	2	2370	11.098**
Contrast II,III	2381	1	2381	11.146**
Contrast I,II	313	1	313	1.463
Within Types	1922	9	214	

Subject MJS

Table A.2.14 Contrast summary table for first component responses to high frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	985	2	493	3.514
Contrast II,III	65	1	65	0.466
Contrast I,II	494	1	494	3.523
Within Types	2103	15	140	

Subject CJS

Source	SS	df	MS	F
Between Types	164	2	82	1.042
Contrast II,III	80	1	80	1.017
Contrast I,II	12	1	12	0.152
Within Types	1182	15	79	

Subject PAT

Source	SS	df	MS	F
Between Types	122	2	61	0.265
Contrast II,III	80	1	80	0.347
Contrast I,II	1	1	1	0.006
Within Types	3466	15	231	

Subject DHR

Source	SS	df	MS	F
Between Types	60	2	30	0.548
Contrast II,III	1	1	1	0.024
Contrast I,II	37	1	37	0.670
Within Types	823	15	55	

Subject MHE

Source	SS	df	MS	F
Between Types	483	2	241	7.332*
Contrast II,III	242	1	242	7.352*
Contrast I,II	32	1	32	0.972
Within Types	296	9	33	

Subject MJS

Table A.2.15 Contrast summary table for second component responses to high frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	13777	2	6889	9.591**
Contrast II,III	8640	1	8640	12.030**
Contrast I,II	243	1	243	0.338
Within Types	10773	15	718	

Subject CJS

Source	SS	df	MS	F
Between Types	6500	2	3250	4.872
Contrast II,III	3136	1	3136	4.701*
Contrast I,II	494	1	494	0.741
Within Types	10006	15	667	

Subject PAT

Source	SS	df	MS	F
Between Types	23253	2	11627	4.296*
Contrast II,III	18330	1	18330	6.774*
Contrast I,II	48	1	48	0.018
Within Types	40591	15	2706	

Subject DHR

Source	SS	df	MS	F
Between Types	29632	2	14816	37.631***
Contrast II,III	14421	1	14421	36.628***
Contrast I,II	2187	1	2187	5.555*
Within Types	5906	15	394	

Subject MHE

Source	SS	df	MS	F
Between Types	2172	2	1086	4.283*
Contrast II,III	1105	1	1105	4.357
Contrast I,II	136	1	136	0.537
Within Types	2281	9	254	

Subject MJS

Table A.2.16 Contrast summary table for total reaction time to high frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
A	3.044	4	0.761	14.927***
B	3.379	2	1.690	23.151***
AxB	0.585	8	0.073	1.435
S(AxB)	3.518	69	0.051	

Key - A: Subjects
B: Block Type
S: Blocks of Trials

Table A.2.17 Analysis of variance summary table for reciprocal error scores to low frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
Between Types	3.803	2	1.902	22.241***
Contrast II,III	1.059	1	1.059	12.383***
Contrast I,II	0.847	1	0.847	9.901**
Within Types	6.926	81	0.086	

Table A.2.18 Contrast summary table (all subjects combined) for reciprocal error scores to low frequency stimuli in Experiment CRT24.

Source	SS	df	MS	F
A	276728	3	92243	51.469***
B	453464	1	453464	674.797***
AxB	2016	3	672	0.375
C	470998	1	470998	30.307*
AxC	46623	3	15541	8.672***
BxC	70952	1	70952	5.627
AxBxC	37825	3	12608	7.035***
S(AxBxC)	150544	84	1792	
D	37159	1	37159	15.647*
AxD	7124	3	2375	12.947***
BxD	554	1	554	2.427
AxBxD	684	3	228	1.244
CxD	1213	1	1213	3.462
AxCxD	1052	3	351	1.911
BxCxD	709	1	709	9.000
AxBxCxD	236	3	79	0.430
DxS(AxBxC)	15408	84	183	

Key – A: Subjects
B: ITI Type
C: ITI Length
D: Stimulus Frequency
S: Blocks of Trials

Table A.2.19 Analysis of variance summary tables for first component times in Experiment CRT27.

Source	SS	df	MS	F
A	168249	3	56083	42.344***
B	15692	1	15692	12.223*
AxB	3852	3	1284	0.969
C	21722	1	21722	12.618*
AxC	5164	3	1721	1.300
BxC	24814	1	24814	15.163*
AxBxC	4909	3	1636	1.236
S(AxBxC)	111255	84	1324	
D	101124	1	101124	3.805
AxD	79726	3	26575	66.658***
BxD	11362	1	11362	45.650**
AxBxD	747	3	249	0.624
CxD	13820	1	13820	170.846***
AxCxD	243	3	81	0.203
BxCxD	7908	1	7908	20.500*
AxBxCxD	1157	3	386	0.968
DxS(AxBxC)	33489	84	399	

Key - A: Subjects
B: ITI Type
C: ITI Length
D: Stimulus Frequency
S: Blocks of Trials

Table A.2.20 Analysis of variance summary tables for second component times in Experiment CRT27.

Source	SS	df	MS	F
A	1.651	3	0.550	10.510***
B	5.921	1	5.921	38.954**
AxB	0.456	3	0.152	2.902*
C	1.147	1	1.147	10.427*
AxC	0.329	3	0.110	2.096
BxC	0.687	1	0.687	6.245
AxBxC	0.331	3	0.110	2.108
S(AxBxC)	4.400	84	0.052	

Key - A: Subjects
B: ITI Type
C: ITI Length
S: Blocks of Trials

Table A.2.21 Analysis of variance summary table for reciprocal error scores to low frequency stimuli in Experiment CRT27.

APPENDIX 3: SUPPLEMENTARY TABLES FOR PILOT EXPERIMENT

Source	SS	df	MS	F
Subject(A)	98900	3	32967	81.643***
FP Type(B)	17340	1	17340	410.684***
AxB	127	3	42	0.105
Blocks/(AxB)	171208	424	404	

ANOVA for 's' parameter

Source	SS	df	MS	F
Subject(A)	14885	3	4962	2.726*
FP Type(B)	65401	1	65401	2.381
AxB	82405	3	27468	15.094***
Blocks/(AxB)	757056	416	1820	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.21997	3	0.07333	1.570
FP Type(B)	0.54624	1	0.54624	19.498*
AxB	0.08407	3	0.02802	0.600
Blocks/(AxB)	19.42537	416	0.04670	

ANOVA for 'a' parameter

Table A.3.1 ANOVA summary tables for pilot fast-guess experiment.

APPENDIX 4: SUPPLEMENTARY TABLES FOR EXPERIMENTS I-V

Source	SS	df	MS	F
Subject(A)	105231	2	52616	81.179***
FP Type(B)	2164	2	1082	0.785
AxB	5516	4	1379	2.127
Blocks/(AxB)	110833	171	648	

ANOVA for 's' parameter

Source	SS	df	MS	F
Subject(A)	7858	2	3929	0.987
FP Type(B)	23336	2	11668	3.116
AxB	14980	4	3745	0.941
Blocks/(AxB)	645024	162	3982	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.04276	2	0.02138	0.572
FP Type(B)	0.01037	2	0.00519	0.373
AxB	0.05562	4	0.01391	0.372
Blocks/(AxB)	6.05200	162	0.03736	

ANOVA for 'a' parameter

Table A.4.1a ANOVA summary tables for Experiment I.

Source	SS	df	MS	F
Trial Type(A)	22932	2	11466	5.194**
Blocks/A	119209	54	2208	

Subject JP

Source	SS	df	MS	F
Trial Type(A)	156	2	78	0.021
Blocks/A	199237	54	3690	

Subject DR

Source	SS	df	MS	F
Trial Type(A)	13212	2	6606	1.092
Blocks/A	326578	54	6048	

Subject JA

Table A.4.1b One-way ANOVA summary tables for parameter 'q' in Experiment I.

Source	SS	df	MS	F
Subject(A)	110222	3	36741	101.362***
FP Type(B)	7701	1	7701	6.635
AxB	3482	3	1161	3.202*
Blocks/(AxB)	127589	352	362	

ANOVA for 's' parameter

Source	SS	df	MS	F
Subject(A)	33017	3	11006	2.506
FP Type(B)	446	1	446	0.360
AxB	3713	3	1238	0.282
Blocks/(AxB)	1510648	344	4391	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.14896	3	0.04965	2.926*
FP Type(B)	0.00753	1	0.00753	4.687
AxB	0.00482	3	0.00161	0.095
Blocks/(AxB)	5.83729	344	0.01697	

ANOVA for 'a' parameter

Table A.4.2 ANOVA summary tables for Experiment II.

Source	SS	df	MS	F
Subject(A)	50156	2	25078	43.743***
Blocks(S)/A	163393	285	573	
Resp Type(B)	210044	1	210044	9.255
AxB	45392	2	22696	39.588***
BxS/A	163393	285	573	

Table A.4.3 ANOVA summary table for Experiment III.

Source	SS	df	MS	F
Subject(A)	254280	2	127140	61.947***
Blocks(S)/A	547988	267	2052	
Resp Type(B)	1159260	1	1159260	106.746**
AxB	21720	2	10860	5.291**
BxS/A	547988	267	2052	

Table A.4.4 ANOVA summary table for Experiment IV.

Source	SS	df	MS	F
Subject(A)	11709	2	5854	11.880***
N Choices(B)	167791	1	167791	16.179
AxB	20742	2	10371	21.046
Blocks/(AxB)	117280	238	493	

ANOVA for 's' parameter

Source	SS	df	MS	F
Subject(A)	102400	2	51200	45.543***
N Choices(B)	4096	1	4096	0.373
AxB	21953	2	10977	9.764***
Blocks/(AxB)	260819	232	1124	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.25742	2	0.12871	2.525
N Choices(B)	0.07088	1	0.07088	5.702
AxB	0.02486	2	0.01243	0.244
Blocks/(AxB)	11.82677	232	0.05098	

ANOVA for 'a' parameter

Table A.4.5 ANOVA summary tables for Experiment V.

Source	SS	df	MS	F
Subject(A)	32666	2	16333	6.582**
FP Type(B)	21785	2	10893	8.464*
AxB	5148	4	1287	0.519
Blocks/(AxB)	920623	371	2841	

Catch trials from Experiment I

Source	SS	df	MS	F
Subject(A)	14498	2	7249	1.486
FP Type(B)	2844	1	2844	0.500
AxB	11368	2	5684	1.165
Blocks/(AxB)	1082709	222	4877	

Catch trials from Experiment II

Source	SS	df	MS	F
Subject(A)	118148	2	59074	13.958***
FP Type(B)	169636	1	169636	4.634
AxB	73207	2	36604	8.649***
Blocks/(AxB)	2403881	568	4232	

Catch trials from Experiment III

Table A.4.6 ANOVA summary tables for catch trials from Experiments I-III.

Source	SS	df	MS	F
Subject(A)	75513	2	37756	16.051***
Trial Type(B)	944	1	944	0.204
AxB	9236	2	4618	1.963
Blocks/(AxB)	1249094	531	2352	

ANOVA for 's_1' parameter

Source	SS	df	MS	F
Subject(A)	120650	2	60325	36.236***
Trial Type(B)	9777	1	9777	0.485
AxB	40293	2	20147	12.102***
Blocks/(AxB)	867360	521	1665	

ANOVA for 's_2' parameter

Source	SS	df	MS	F
Subject(A)	8489	2	4245	1.603
Trial Type(B)	57892	1	57892	14.226
AxB	8139	2	4069	1.537
Blocks/(AxB)	1390271	525	2648	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.08935	2	0.04468	0.707
Trial Type(B)	0.10048	1	0.10048	7.168
AxB	0.02804	2	0.01402	0.222
Blocks/(AxB)	33.167414	525	0.06318	

ANOVA for 'a' parameter

Table A.4.7 ANOVA summary tables for post-error analysis for Experiment III.

Source	SS	df	MS	F
Subject(A)	31776	2	15888	3.849*
Trial Type(B)	129414	1	129414	4.171
AxB	62061	2	31031	7.517***
Blocks/(AxB)	2109351	511	4128	

ANOVA for 's_1' parameter

Source	SS	df	MS	F
Subject(A)	199177	2	99589	17.791***
Trial Type(B)	36803	1	36803	0.506
AxB	145388	2	72694	12.986***
Blocks/(AxB)	2726152	487	5598	

ANOVA for 's_2' parameter

Source	SS	df	MS	F
Subject(A)	95438	2	47719	16.022***
Trial Type(B)	8858	1	8858	48.077*
AxB	368	2	184	0.062
Blocks/(AxB)	1504103	505	2978	

ANOVA for 'g' parameter

Source	SS	df	MS	F
Subject(A)	0.08523	2	0.04262	0.607
Trial Type(B)	0.06362	1	0.06362	15.870
AxB	0.00802	2	0.00401	0.057
Blocks/(AxB)	35.483378	505	0.07026	

ANOVA for 'a' parameter

Table A.4.8 ANOVA summary tables for post-error analysis for Experiment IV.

Source	SS	df	MS	F
Subject(A)	1.59173	2	0.79586	9.500***
Trial Type(B)	1.16995	1	1.16995	3.486
AxB	0.67128	2	0.33564	4.006*
Blocks/(AxB)	44.98760	537	0.08378	

Table A.4.9 ANOVA summary table for 'q' for Experiment III.

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