SOME ASPECTS OF PERCEPTUAL CODING

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by

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INDEX

	p
PREFACE	i
ABSTRACT	iii
INTRODUCTION: MODELS AND PARADIGMS IN HUMAN INFORMATION PROCESSING	1
(a) Serial vs. Parallel Comparison Processes	20
(b) Some Experimental Paradigms - A Review .	31
1. Memory Search	31
2. Visual Search	39
3. Binary Classification	45
4. The Effects of Sequence	63
Summary and Conclusions	85
CHAPTER 1: CONTEXTUAL EFFECTS IN DISCRETE REACTION-TIME TASKS	92
Experiment 1: "The Effect of Noise on Binary Classification"	93
Experiment 2: "The Contextual Effects of Discrimination Difficulty in a Binary Classification Task"	128
Experiment 3: "Further Effects of Context upon Binary Classification"	154
Summary and Conclusions	168
CHAPTER 2: CONTEXTUAL EFFECTS IN A VISUAL SEARCH TASK	173
Experiment 4: "The Effect of Visual Search on "Same"-"Different" Judgements"	175
Summary and Conclusions	189

CHAPTER 3: CONTEXTUAL EFFECTS IN SERIAL CHOICE REACTION TIME..... 190

Experiment 5: "The Effect of Seriality in a Binary Classification Task" ..... 195?

Experiment 5b: "An Additional Factor in Serial Processing" 226

Experiment 6: "The Effect of Partial Stimulus Identity on Serial Choice Reaction Time ... 236

Experiment 7: "Further Effects of Response Stimulus Interval on Choice RT 245

Summary and Conclusions 259

CHAPTER 4: THE EFFECTS OF SERIALITY UPON HIGHER ORDER DECISIONS 264

Experiment 8: "Superordinate Categorisation in a Serial Task"..... 266

Experiment 9: "Further Effects of Stimulus Class Transitions in Serial Choice RT" 276

Summary and Conclusions 294

CHAPTER 5: FILTERING AND RESPONSE COMPETITION AS RIVAL EXPLANATIONS OF A SEMANTIC STROOP EFFECT FOR TRANSITIONS BETWEEN EVENTS IN A SERIAL CHOICE RT TASK 296

Experiment 10: "The Effects of Semantically Related Irrelevant Information in a Serial Choice RT Task 304

Summary and Conclusions 336

CHAPTER 6: IMPLICATIONS 338

APPENDIX: "AN INVESTIGATION OF RESPONSES TO INCONGRUOUS STIMULI UNDER DIFFERENT TASK CONDITIONS" 370

REFERENCES 380

PREFACE

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ABSTRACT

This thesis reports a series of experiments which study various aspects of perceptual coding processes in normal adults. Perceptual coding involves several distinct stages of analysis, which it is possible to separate by suitable experimental manipulations. The importance of "functional" concepts and models, in discussing the nature and relationship of these stages, is a recurrent theme which is illustrated both by review and by experiment.

The opening section of the Introduction examines the relationship between "recognition" and "discrimination" and distinguishes the concepts of "nominal" and "functional" as they are applied to human information processing. Following a brief discussion of "active" and "passive" theories of recognition, the nature of information theory is elaborated and evaluated with particular emphasis upon recent criticisms and its ability to model discrimination performance. The theory is found to be inadequate in several important respects - in particular in its failure to provide a viable account of the functional basis of information processing.

The following section discusses and criticises the concepts of serial and parallel processing which are frequently employed in modelling the way in which pattern discriminations are executed. Although the terminology is useful in certain instances, it is suggested that it presents difficulties which appear to multiply with the increasing conceptual refinement demanded by new data. Several logical and empirical assumptions upon which the dichotomy rests are made explicit and considered. A number of these are found to be questionable, to an extent which makes it difficult to maintain any heuristic value for the original distinction.

The necessity for functional models is further emphasised in a review of data obtained from some well-tried experimental paradigms. Three are selected on the basis of their relevance to the chapters which follow, as well as for their own intrinsic interest.

Section 1 reviews data from high-speed memory search, and section 2 data from visual scanning tasks. Both collectively, and separately, these reviews stress the multi-factorial determination of reaction times, and

consequently, the need for functional models of sufficient flexibility. Section 3 examines data from current work on binary classification. The existence of separate processes subserving "same" and "different" decisions appears to be indicated. Several versions of two-process models are described and discussed.

In a final review section, data from serial self-paced tasks are considered - since they are the basis for much of the work reported in later chapters. It is clear that sequential RT tasks provide a potentially valuable tool for the separation of the stages of stimulus analysis and response selection, and furthermore, that interesting repetition effects occur. A preliminary analogy between these effects and the "same"-"different" effects of non-sequential tasks is presented.

The principal conclusion of the Introduction is that the multi-factorial determination of RTs makes any simple, non-functional models of human information processing futile, and therefore that these factors must be

exposed and studied, with a view to producing more functionally oriented models in future.

Chapter 1 reports three experiments, all of which were made to study "same"-"different" judgements in a non-sequential context.

Experiment 1: This studied the effect of superimposing visual noise on either, neither or both stimuli in a simple discrimination task. RTs were recorded as a function of the four possible combinations of noise and clear pairs, for each response. The results supported the hypothesis of a stimulus pre-processing device which acts upon noisy stimuli before comparisons are made; however, it was not possible to decide whether the noise was simply attenuated, or whether an entirely new noise-free replica of the stimulus was generated. The existence of an order effect ruled out any explanation based on the equation of noise and reduced stimulus discriminability. The results also illustrated the difference between superimposed and interpolated noise. A significant difference between the pattern of RTs for each response supported a two-process account of binary classification.

Experiment 2 was made to investigate the possibility that RTs to one class of stimuli in a binary task could be affected by the nature of other decisions subjects had to make. It was found that "same" judgements were significantly affected by the difficulty of "different" discriminations in the same list. Furthermore, mixing difficult and easy "different" decisions from separate pools of stimuli exerted a levelling effect on RTs to the same stimuli made independently. These effects necessitate the conclusion that "same" and "different" decisions are not wholly independent of each other, although the existence of two processes was reinforced by the data.

Experiment 3 extended the conclusions of Experiment 2, by making the difficulty of the individual comparisons depend upon the particular pairings of stimuli which were made, rather than upon the particular stimulus population from which easy and difficult pairs were drawn. The fact that "same" RTs in "difficult" lists were significantly slower than RTs to the same "same" stimuli in easier lists, demonstrated a clear effect of stimulus context.

The data from Chapter 1 make clear the dependence of RTs to individual stimuli upon extrinsic, contextual factors. Even in a "discrete" type of task, a single comparison cannot validly be regarded as an event isolated from the others which precede it. The experiments demonstrate that it is possible to use the hypothesis of separate stages in the information processing of a single stimulus event as a methodological assumption obtaining, under specific conditions, variations in RT to stimulus properties while the response set remains constant.

Chapter 2 reports an experiment in which subjects were required to scan through a list of "same" or "different" letter-pairs, for an item of the opposite class to that through which they searched. This task represents a further means of driving an empirical wedge between stimulus analysis and response production processes. Searching through "same" items was found to be consistently slower than searching through "different" items - a reversal of the prediction from simple "same"-"different" studies. The data were explained by supposing that separate tests for sameness and difference were available to the subject and that the test which was

applied was that appropriate to the target rather than that corresponding to the non-target context. In demonstrating the existence of totally separate tests for sameness and difference, this result strongly supports a two-process model of binary decision-making. It also indicates that the functional test applied to a stimulus may not correspond to that expected from its intrinsic identity. In this case search-time was determined by the nature of the target, and not by that of the stimulus present to the subject.

A limitation of the search task is its failure to pick up differences between individual S-R transitions. This is taken up in Chapter 3, which reports a series of short experiments designed to study the behaviour of binary decision making in a serial self-paced task. RTs were analysed both by response, and by the quality of the signal transition of which the present signal was the second member.

Experiment 5 shows certain transitions to be significantly faster than others: repeated signals result in very fast RTs, whether the repeated decision be "same" or "different". This facilitation is not found for non-

repeated signals which involve a similar decision-response sequence. Curiously, repeats are not only very fast compared with other transitions, but they are also extremely accurate. This was found to be so for every serial experiment made - and points to a significant qualitative difference between repeats and non-repeats. The suggestion that this difference corresponds in quality to that between "same" and "different" RTs in a discrete task is not wholly supported by the data; whilst the analogy is viable for repeats, it cannot account for the pattern produced by non-repeated signals. Although less marked, potentiation from "decision" and response repetition was also evident.

The relative difficulty of repeated "different" decisions predicted from Experiment 4 was found, and thus supports the explanation offered there.

A replication of Experiment 5 (Experiment 5b) using a long RSI had the effect of abolishing the identical signal facilitation. This is in line with the prediction from what is known, from tachistoscopic matching tasks, of the decay of visual information with time. It indicates the importance of visual persistence in determining RTs in sequential tasks.

Experiment 6 and Experiment 7 were made to further examine the nature of the facilitation for repeated signals, and in particular to determine what, in functional terms, counts as a repeated stimulus. Transitions between five distinct "different" letter-pair-types were rank-ordered for relative facilitation. Reversals (e.g. "AB"-"BA"), while slower than repeats, were significantly faster than non-repeats, or partial repeats (e.g. "AB"-"AC"); their high accuracy also suggested their functional proximity to repeats. These results add complications to any simple analogy between repetition effects and "same"-"different" effects. It was suggested that the intervention of a mediating response might account for some of the anomalies. Although signal facilitation was found, the results reported in Chapter 3 indicate that potentiation is possible at other levels of analysis.

Experiments 8 and 9 were made to investigate the possibility of transitions between higherorder aspects of the stimulus being facilitative. In Experiment 8 repeating a stimulus class (Number versus Letter), was found to be significantly facilitative. In Experiment 9 class repetition was empirically opposed to repetition of a classification based on a common response. Un-

fortunately the data did not bear out the hypothesis that early in practice "superordinate class" would be the principal determinant of RT, whereas later on in practice response-grouping would be of greater importance. The results provided limited support for the suggestion that the simple stimulus:stimulus-one-back comparison, apparently characteristic of serial tasks, may in certain circumstances be extended to include a memory search through the class of which the previous stimulus was a member. It is clear that, in some sense, different stimuli related arbitrarily to a single response may become "perceptually equivalent", in a way which enables them to be classified without exhaustive analysis. The fact that "rule" or "class" repetition effects exist illustrates the multifactorial determination of RT in serial tasks, and negates models based on simple signal comparisons.

The possibility of facilitation attributable to the repetition of a mediating response raised in Experiments 8 and 9 is taken up in Experiment 10. The experiment was made to examine the influence of seriality upon a complex task in which subjects were required to make individual responses to a series of bi-dimensional items where one source of information was known to interfere with the other. Although subjects responded to only one dimension, it was clear that they were unable to ignore the irrelevant information The pattern of RTs for the various possible

transitions between relevant and irrelevant dimensions, demonstrated that the irrelevant source did not simply promote an all-round increase in RTs, but rather interacted significantly with the transition involved. The results were discussed in terms of a current model of word-recognition, and in the light of the controversy over the nature of selective attention.

In a final chapter, the implications of the experiments reported are discussed in more general terms and related to the theoretical considerations presented in the Introduction.

INTRODUCTION

MODELS AND PARADIGMS IN HUMAN INFORMATION PROCESSING

This chapter contains a discussion of experimental data and models relating to the processes of "recognition" and "discrimination" as they occur in human information processing. It reviews the results obtained from a subset of experimental methodologies by which these processes have been examined and which are of special relevance to the chapters which follow. In particular, the discussion is directed to showing that there is no single appropriate model for either recognition or discrimination, and moreover that it is misleading to expect one; the most suitable model will vary significantly with the experimental paradigm used to test it.

(i) Recognition: Before discussing either "recognition" or "discrimination" by itself, it is important to indicate the complexity of the relationship between them. At one level, namely that of task description, they are entirely distinct: recognition experiments result in recognitions, and discrimination experiments in discriminations. Used descriptively in this way, the terms denote achievements, but say nothing of the underlying functional processes.

What is of interest is whether the processes leading to recognitions are the same or different from those leading to discriminations, and this information cannot be had from the assumption that to each independently specifiable experimental task, there corresponds, 1:1, independent functional processes. The point is this: behavioural recognition - e.g. as studied in "word recognition" or "recognition memory" experiments - refers to a type of end-product; it is not valid to argue from separable end-products to the existence of separable functional processes. For example it is unlikely that the processes of encoding and storage which are necessarily involved in any successful recognition, are markedly different from those involved in making discriminations. Again, since it is a minimum requirement of recognition that stimuli have to be stored and matched, it would seem that some operation of a discriminatory kind is inescapably involved in recognising. Conversely, the recognition of features or feature-complexes may frequently be involved in the making of complex discriminations. An identification which results from a failure to discriminate, amounts to a nominal, but not functional, recognition.

It is necessary to emphasise this distinction between achievements and processes at the outset, since it is the functional nature of information processing which is the subject of this thesis, and not the superficial differences between the several tasks from which data are collected.

Recognition may be characterised by different levels of description: e.g. "a house", "my home", "Mandalay"; but however abstract the response, the minimum requirements for a successful recognition - viz. that a stimulus be both encoded and stored in such a way that it can be matched against the perceptual or mnemonic traces of previous input - remain constant.

Traditionally, two kinds of recognition model have been considered. One, the "active" model, is best exemplified by Stevens (1960) and Neisser (1967). The latter author provides a two-stage model: the first stage acts as a crude filter to exclude stimuli which are not required for further analysis; at this "pre-attentive" level, none but gross feature-bound differences are processed. This analysis is followed by an "attentive" process which operates by synthesising constructs which

are then matched against the input selected by the first stage. This "match"-"mismatch" decision, leading to a motor response, is governed by a decision procedure similar to that of classical signal detection theory (e.g. Swets, 1964; Treisman & Watts, 1966). The second "passive" type of model is exemplified by Morton's scheme for word recognition (Morton, 1964, 1969), which postulates a set of neural units - "logogens" - which correspond 1:1 to the possible outputs of the system. These units are highly specific and exist independently of the ability of the system to generate stimulus replicas. They are scanned, "passively", by the system for identity with the input being matched. Each logogen has its own threshold of firing which is determined by the multiple factors normally affecting stimulus recognition - e.g. recency, frequency, expectancy, transition probability etc.

That these two genres of model are truly distinct has been disputed by Cohen (1971). She argues that, while the superficial aspects of the models differ - e.g. in that their terminologies are quite distinct - there is, in fact, no substantial difference between them. They do not differ in the predictions of

performance they make, and the notion of "generation" which is crucial to the "active" type of model, can be adequately parcelled out in the passive terminology of "pre-priming" of neural units. The first point is valid; the second point is invalid, as recent experiments on the effects of probability on perception have shown. The opposition between the active and passive types of model corresponds closely to the opposition between theories which regard the effects of stimulus probability as being mediated by changes in sensory criteria - akin to the criterion changes of signal detection theory - and those theories which regard the effects of probability as being due to biases within the central processing mechanisms which will produce a response on less evidence for more probable events. This opposition is made empirically explicit by Broadbent & Gregory (1971) who studied the relative effects of opposing bias to whole words against bias to the component elements of words (i.e. letters). Correct perception was found to follow word probability rather than letter probability in each of their experiments, although superimposed upon the main effect was a small effect of letter probability - so to this extent the evidence is ambiguous. However, their main conclusion

rather with determining the bases upon which discriminations can be made. There are two principal bases, and correlatively it has distinct, factors which correspond to two distinct senses of the word "discriminability" for (i) the relative separation, on a sensory scale, of the component features in a pair of stimuli; (ii) the number of features offered differentiate a pair of stimuli. Greater discriminability, and thus better discrimination, results from either greater separateness on a sensory scale or from less redundancy within a stimulus. It must be emphasised that these concepts are quite distinct. This review is concerned with pattern discrimination - not with sensory acuity.

Classically, pattern discrimination problems have been dealt with by information theory. First expounded by Shannon (1948) from an engineering background, it has since been extensively elaborated to provide models of pattern perception (cf. Garner, 1962 for review). There are two principal concepts incorporated in the theory: (a) "Information": the information in an event is defined as the negative logarithm of its probability. Thus, for equiprobable signals, the "stimulus" is explicitly defined in terms ofthe total number of stimuli which

might appear. (b) "Redundancy": redundancy refers primarily to the replication of information in a signal - e.g. known symmetry, or a repeated event. Garner also applies the term to a measure of the "extent to which an actual number of stimuli is smaller than the maximum number which could exist..." - this implicitly allows for redundancy between sequent members of a set of stimuli - e.g. transition probabilities varying between successive letters in a known word.

The results of early work within the framework of the theory has been well and extensively reviewed (Alluisi, 1957; Miller, 1956; Garner, 1962) and it is not proposed to duplicate this. In summary, many experiments on absolute stimulus judgement led to the establishment of a multi-dimensional information processing model, according to which departure from the ideal predictions of information theory increased with the number of dimensions upon which a stimulus varied. Dimensions could not be combined without significant loss of transmitted information, either in toto or in information per dimension. However, for uni-dimensional stimuli the invariable finding was that subjects were able to perform to a correct limit of 7 ± 2 stimulus categories; this figure was relatively independent of the particular dimension used, or of the position in the scale of

stimuli that was tested (Miller, 1956). In addition, Garner (1962) has remarked on the impressive lack of effect of stimulus spacing or discriminability on transmitted information.

Although the earlier work was mainly confined to studying tasks of absolute stimulus judgement which are, in essence, recognition tasks, information theory was easily adaptable to the problem of quantifying discriminability, and thus to the study of pattern discrimination. From the point of view of information theory, discrimination differs from absolute judgement in two quantitative ways: (i) the memory load imposed upon the subject is considerably lessened in a discrimination task; (ii) the information in the response is less in a discrimination task - viz. "same" or "different".

Within this framework, recent efforts have largely been devoted to the study of the scaling and speeded classification of multi-dimensional stimuli. The aim of the scaling experiments has been "to discover the psychological dimensional structure that is pertinent to the stimulus dimensional structure" (Garner, 1970). The main controversy has been whether multi-dimensional stimuli are scaled according to an Euclidean or city-

block metric. In the former scheme, the effect of added dimensions, or changes in value on a given dimension, can be computed in Euclidean fashion, that is geometrically; in the city-block structure, the addition is arithmetic. Hyman & Well (1968) report that for stimuli which are "analysable" - that is, for which the component dimensions are "obvious and compelling" the city-block rule holds, whereas for non-analysable stimulus dimensions (e.g. variation in colour chroma and value within a single patch) the familiar Euclidean hypoteneuse relationship characterised the combination of the dimensional scales.

Garner & Felfoldy (1970) have extended this work to studies of dimensionality effects occurring in speeded-classification tasks, where subjects are required to classify stimuli by one of the two or more dimensions represented in a set of stimuli, and the speed of classification is measured. The interesting questions which have been asked, have been concerned with the effects of variation on one dimension upon classification of the other. On the basis of their experiments, Garner & Felfoldy (1970) distinguish between two kinds of dimensional combination, which differ importantly in their behavioural properties ; for <u>separable</u> dimensions,

correlation of the values on each dimension produces no facilitation in classification time compared with the time taken to sort each dimension singly, and, conversely, there is no interference in sorting time from having both values on one dimension occurring with either value on the other dimension (orthogonality). For integral dimensions, however, the reverse is true: correlation produced significant facilitation, and orthogonality significant interference in sorting time. Felfoldy & Garner (1971) have shown that this result is not an artefact of subjects noticing or not noticing the correlations. Garner & Morton (1969), discussing possible models of perceptual independence, point out that limitations of states of the organism and limitations of processes exert different effects upon the processing of redundant dimensions: "perceptual independence is an asset in information processing tasks only when performance is state limited, in which case separable redundant dimensions will aid performance. On the other hand, if performance is process-limited, then integrality rather than independence of dimensions is needed in order to improve performance by the addition of redundancy" (Garner, 1970, p. 356). These results bear directly upon the discussion of stimulus discrimination, in that different types of discriminability

variations give rise to process limitation from those that generate state limitation. In a major theoretical contribution, Garner (1970) makes it clear that he regards the concept of "stimulus" as task-dependant and therefore that "all stimuli are not equivalent and all information cannot be processed in the same way ... for too long we have considered that a stimulus is a stimulus is a stimulus, whose only function is to elicit behaviour" (ibid. p. 357). The chapters which follow will emphasise this point.

If Garner has weakened from the information-theoretical concept of a stimulus, current in the early 1960s, as a simple numerical relation of one event to the total possible population of events, then the heuristic value of the theory as a potential basis for the quantification of information disappears. Additionally, there are excellent independent grounds for criticism:

1. Corcoran (1971) presents a convincing indictment based both on empirical and conceptual considerations: his empirical argument rests upon the contention that

the bedrock assumption of information theory - viz. that each stimulus is independent of every other stimulus - is invalid. Contrast and adaptation, retroactive interference, and memory decay are all possible links between successive stimuli in a random sequence. An experiment by Holland & Lockhead (1968) makes the point clearly: they found, in a loudness judgement experiment, that (a) the response assigned to stimulus N indicated that N tended to be perceived more like N-1 than in fact it was (i.e. assimilation), and (b) that the response N tended to contrast with N-2 and to be affected in a similar way by stimuli presented as far as eight places back (i.e. contrast). Furthermore, the range of stimuli, and the particular values chosen on one dimension, may exert an influence on performance with other stimuli, or with different values on another dimension. It is certainly clear that in card sorting tasks of the kind used in speeded classification stimuli are not immune from mutual influence. These points will be taken up in a different situation in Chapter 1 - where other kinds of stimulus dependence will be examined in a task for which stimuli are supposedly independent.

Corcoran's conceptual argument concerns the alleged failure of Garner to differentiate between two possible definitions of "redundancy": (i) Garner applies redundancy only to sets of stimuli - being defined as "the difference between the information presented if we do not select from the total population and that presented when we do." This is a measure of quantity. (ii) Garner does not allow for redundancy within a single stimulus pattern - e.g. symmetry. It is reasonable to distinguish between the amount of redundancy and the form which it takes. Corcoran's contention is that it is possible to restrict the form of the redundancy while keeping the amount of redundancy constant. He presents evidence to suggest that Garner is in a minority in his view of what counts as a change in form and what counts as a change in amount of redundancy. Much of the differences between Garner and other authors turns on what it is proper to regard as the parent population of stimuli of a given subset, since it is by relation to this parentage that the information is computed.

2. It is doubtful whether information theory can adequately account for subjects' performance with stimuli containing two or more dimensions. Statements of "theory" based on multi-dimensional stimulus scaling experiment, usually in terms of deviations from optimal channel-capacity limitations, tend to be simply re-statements of the facts rather than the explanations they are taken by their authors to be (e.g. Garner, 1962). Posner (1964), however, did provide a useful demonstration of the possibility of using stimulus uncertainty to predict "similarity" judgement behaviour. Using three different statistical rules for the distortion of 12-dot patterns, Posner made his subjects order 12 sets of topological transforms based on a single distortion rule for their degree of similarity to an original pattern. He found that the relation between the actual degree of distortion and the perceived similarity was roughly linear - indicating the relation of uncertainty to perceived distance that would be predicted from information theory. Crossman (1955) attempted to test the information theory of discriminability with a wide variety of stimuli in a single discrimination task, and obtained sufficiently linear functions to lead

him to conclude that the assumptions of the theory were substantially correct. However, if Crossman's logic is sound, a clear correlative prediction would be of a parallel between the functions of stimulus probability and discriminability. Laming (1963), in a paper directed at exposing Crossman's argument, fails to find this parallel.

Although several authors, like Crossman (1955), report results which appear to accord with an information theoretical model of discriminability quantification - e.g. Welford, 1967 - their contributions are not wholly satisfactory: (i) in the first place, the linear function relating discriminability and RT - predicted by information theory, is not by itself conclusive; as McGill (1963) remarks, "a decision mechanism will, in effect, have to wait until a clear difference emerges between the counter associated with the stimulus and the largest count coming from the other alternatives all of which, presumably, are registering noise" (McGill, op. cit. p. 332) - linear functions are therefore hardly surprising. (ii) In the second place, the data produced by information theory protagonists can be equally well accounted for by models based upon considerations of stochastic probability.

Not only are the different versions of such models (Stone, 1960; Audley & Pike, 1965; McGill, op. cit., Nickerson, 1969) currently available entirely compatible with the data, but they have the additional advantage of allowing the flexibility which must be incorporated into any model of an imperfect system. Indeed, Laming (1968) takes information theory to task for requiring perfect observers in order to work satisfactorily.

The principal inadequacy of information theory based models of discrimination lies, not in their failures to account for empirical data, but in the substantial internal difficulties in the theory over questions of "redundancy" and provision of a consistent basis for measuring information a priori. Information models can, moreover, be criticised in more general grounds than this: (a) in general, they fail to provide an adequate model of discrimination; it is apparent from what has been said already that the transition from "discriminability" to "information load" is not a workable one; (b) the theory itself deals primarily with the relationships between stimuli and responses, and makes relatively little contribution to discussions of the nature of

the decision processes intervening between S and R. Signal detection theory, and its derivatives, provide much more viable accounts of discrimination and decision processes, but unfortunately fail to account for the effects of number of alternatives on RT - which information theory does very well. Models which attempt to bridge these mutual failings - e.g. Audley & Pike, 1965; Vickers, 1970 - are as yet relatively unrefined. The idea of a marriage is certainly promising; (c) information theory operates upon the implicit assumption that it is possible to devise a single metric for the measurement of information. It is not at all clear that such an assumption is justified, and, that, even if it were, simple counts of "possible events" would be an integral part of it; (d) the most compelling general criticism of the theory is that it has little or no "functional validity"; that is to say, that the correlations between stimulus events and behavioural characteristics, by themselves, give no guide to the functional properties of stimulus processes, and it is these, ultimately, which are of interest to the psychologist. Information theory is an attempt to correlate

variations in a priori aspects of stimuli with variations in subjects' performance. To this extent, it pays regard only to the actual physical stimulus - "nominal stimulus" - and not to those aspects of the stimulus which are functionally important to the organism in making a particular decision. Garner (1970) recognises this deficiency, but goes no way towards supplanting it.

These criticisms do not necessarily mean that information theory models cannot be useful, or that they may not be found to support other models of greater functional relevance. It is simply that informational models provide a "description" of performance at a different "level" from alternative, functional models, and, at the present moment, it is the functional models which are to be preferred.

Both experimental psychology and neurology generate models of a similar kind, although perhaps at different levels of explanation; over the last decade, however, both have been inhibited and hampered by the relative triviality of the type of informational model discussed above. The case has already been argued for preferring models which have at least some "functional validity" -

that is to say, models which go beyond the simple descriptive stage of producing correlations between physical aspects of the stimuli and some selected factors in the responses.

The remainder of this chapter will fall into two parts: (a) the first part will present a selected review of literature relating to the second of the two major questions which form the "discrimination problem" - viz. how are comparisons which lead to stimulus discrimination executed? (b) the second part will consist of a review of a subset of experimental paradigms which have been used to investigate the nature of stimulus processing, and a discussion of both "what" and "how" comparisons are made.

(a) Serial vs. Parallel Comparison Processes: the basic terminology in which the question "how are comparisons executed?" is currently answered derives originally from computer language. Operations can be carried out one at a time (serial mode) or simultaneously (parallel mode). Any attempt to fit data into either of these processing moulds requires that one first knows the units in which the stimuli are being processed. Normally the serial/parallel problem is discussed in terms of distinct stimulus dimensions - e.g. colour

and shape - where it is reasonable on independent grounds to assume that the physical separability of the dimensions reflects a real separation of the functional processes involved. However, it is also quite sensible to ask of a single uni-dimensional stimulus - e.g. a letter - whether its constituent "features" are dealt with serially or in parallel. In either case one has to assume that the unit of processing one selects as the correct one, is not just physically distinct but also has a functional separateness.

Egeth (1966) elaborated the basic terminology into a working vocabulary for classifying multi-dimensional stimulus processes: he distinguishes, for both S and P processes, "exhaustive" from "self-terminating" comparisons; in the latter the scan is terminated whenever sufficient information is available to justify a response; in the former, a complete analysis of the several dimensions is made. In the serial mode, the further distinction is drawn between fixed and random order comparisons. For the former the order in which dimensions are dealt with remains constant, in the latter it varies "randomly". In the parallel mode Egeth further distinguishes between "constant" and

"distributed" decision times - corresponding to either fixed or "statistically distributed" decision times for each dimension or attribute of the stimulus. Given this basic terminology and conceptual structure, it is possible to devise experiments to make tests between the different logical strategies and to fit the data into the serial/parallel framework. However, there are good reasons for regarding this conceptual scheme with some caution:

1. Logical Assumptions: the validity of the serial-parallel dichotomy depends upon several assumptions for which there is slender justification:

(i) The units of processing are either "dimensions" for multi-dimensional stimuli, or "features" or "templates" for single-dimensional stimuli - or often indeed both (cf. Egeth 1966) - and these are assumed to be totally independent of one another. If it was true either (a) that the value of one dimension affected processing time for another dimension, or (b) that certain sequential stages of processing interacted with one another, whether positively or negatively, then this assumption would be ill-founded. The total

processing time for a given pair of stimuli may be affected by the total number of dimensions to be processed, or the time taken to process a single stage may be altered by the presence of other dimensions which require processing.

(ii) It is also assumed that it is possible to specify or delimit "dimensions" in advance, in such a way as to make it feasible to count, a priori, the number of dimensions on which, for example, two stimuli differ. It is certainly true that, in some cases, counts of stimuli themselves do have validity, in the sense that, for instance, choise RT is indubitably linked to the number of possible S-R alternatives. However, where single stimulus comparisons are concerned the question of what constitutes a dimensional unit is rather more demanding. Shape and colour may be separate dimensions, but is Egeth (1966) correct in considering "form" and "tilt" as independently processed dimensions?

(iii) The strict serial-parallel dichotomy overlooks the possibility of "hybrid" strategies operating within a single stimulus comparison. Selfridge (1958) in his model called "Pandemonium" offers such a scheme - in

which several sets of analysers, functioning in parallel, are linked to each other in a hierarchical series. Hawkins (1969) points to the inadequacy of serial or parallel models to explain his own data, and suggests a parallel interactive model, in which the durations of some processes affectthe durations of others. In allowing this possibility the fundamental "independence" assumption (i) is undermined.

(iv) Conventional parallel models demand that all tests begin simultaneously, and serial models that no two tests are in progress at the same time. These two extreme possibilities omit a perfectly plausible third, intermediate, strategy; namely, the sequential onset of component processes, but with the actual time courses of the processes overlapping. An additional difficulty is that, in self-terminating serial models, it is rarely made clear whether the decision process pre-dates the onset of the next analysis, or whether these are carried out in parallel.

(v) In considering whether a given set of data fit a serial or a parallel model, only the information relevant to making a correct response is taken account of. That is to say, it is normally assumed that subjects

are able to completely and efficiently filter out irrelevant information, either from a relevant dimension or from an irrelevant dimension. The "efficiency" of the supposed filtering implies that this operation is carried out without significant loss of time or channel capacity for processing the relevant information. It is unlikely that either of these assumptions is true; Egeth (1966) makes the assumption explicitly for both parallel and serial models ("it is assumed that Ss are un-influenced by the presence of irrelevant information ... are capable of completely filtering it", Egeth, 1966, p. 245), but from his own data is forced to abandon it: "the results of the present experiment make it painfully clear that irrelevant dimensions of variation are not necessarily ignored at the command of the experimenter" (ibid. p.251).

These logical difficulties are matched by equally compelling empirical ones:

(iv) In order to make sense of data in strict serial/parallel terms, it must be assumed that scanning strategies remain constant, at least over a single run of trials. Both Corcoran (1971) and Egeth (1966) refer to strategies as "being adopted" by the subject; in this sense, it is

quite possible that these may change with practice or in the interests of greater efficiency. Such "covert selection procedures" (Cohen, 1971) are not invariable. In a second sense, the strategy used to process given information may be at least partially determined by the nature of the device by which it is processed; neurological constraints may force one strategy rather than another. Here the possibility of variation may not be so great, but may nonetheless occur. It is not made clear whether strategies are voluntary or involuntary; if the former then it should be possible, by instruction, to examine the effect of different strategies. It is, however, unlikely that the decision whether to process the constituent features of a single letter in series or in parallel can ever be a real one for an adult subject, so the latter possibility is not entirely speculative. Furthermore, empirical constraints may force subjects to adopt one strategy in preference to another; Briggs and Blake (1969) and Lindsay & Lindsay (1966) both report data which suggest that under conditions of high memory loading in serial scan tasks, subjects are forced to adopt recheck procedures before committing themselves to a negative response. Clearly such a procedure may obscure the true nature of the

underlying functional processes.

(vii) The decision criterion for same/difference may vary from dimension to dimension, and within a dimension from trial to trial. Furthermore, the addition of dimensions may result in the universal lowering of criteria in order to maintain a constant fast RT - thus comparisons between for example bi- and tri-dimensional stimulus comparisons may be contaminated by error-factors. This variability of criterion is only important if single element processing times, derived from single element comparisons are being used to compute expected processing times for multi-dimensional displays.

(viii) It is not always possible, even given a clear and universally accepted set of data, to determine the model of processing which is appropriate to them. Corcoran (1971) provides an extensive discussion to illustrate theppoint that one cannot validly infer, from a linear function relating the number of inputs or dimensions to be processed and RT, that a serial model is operative, since it is entirely consistent with the data to postulate a parallel process which operates in such a way that the efficiency of the component analysers is inversely

proportional to their number. Conversely, Stone (1970) argues from his search-task data that neither a serial nor a parallel model is appropriate to the results.

(ix) The logical distinctions between different types of serial and parallel process are not always accompanied by clear empirical correlates. For example, the distinction between a serial, fixed-order self-terminating process and a parallel distributed self-terminating process has no clear empirical correlate; both predict that the greater the number of dimensions along which stimuli differ, the faster the average "different" RT; both also predict that the greater the number of dimensions the longer the "same" RT. It is the built-in flexibility in the "distributed" clause of the parallel model which makes clear predictions impossible, and equally, makes the logical distinction less plausible.

(x) The type of stimulus process employed may depend, critically, upon the specific nature of the dimensions involved. Garner & Felfoldy's results (1970) which have already been discussed in the context of information theory, demonstrate beyond doubt that it

possible for complicated interactions between dimensions to occur which may facilitate or impede discrimination between pairs of stimuli. Some combinations of stimulus dimensions lead to "stimulus integrality" and others to "stimulus separability" - each type of combination having specific and highly different behavioural properties. It is therefore meaningless to assess the processes contributing to overall RT by simply counting dimensions, or summing number of differences between two stimuli. It is possible, as Garner & Felfoldy show, for a "stimulus" to possess properties over and above those of its component parts - and therefore any discussion of what constitutes the "stimulus" in a given task which is independent of functional considerations, is futile.

This discussion of the serial-parallel dichotomy has deliberately emphasised the difficulties. Assuming that some form of serial or parallel model must be appropriate for a given task has too frequently led to the testing of "ideal" models which are, in the nature of things, unlikely to be practically correct. The failure of any simple serial or parallel models to describe data derived from a variety of tasks has directly generated greatly more complicated and

sophisticated models, often hybrids between the two extremes, which are highly task specific, and frequently impossible to distinguish empirically, given the natural variance built in to human reaction-times. Lack of consideration of response-selection and the assumption of total independence of constituent processes in RT studies have been persistent weak points contributing heavily to the implausibility of serial-parallel models. It is intuitively reasonable that the sort of operations crystallised in the serial-parallel distinction must be involved in information processing, but model-building within the framework of this terminology will fail (a) until more is known about the units in which information is processed, and (b) until adequate recognition is given to the hitherto implicit assumptions which are necessarily at the back of serial-parallel talk. Unfortunately, as the data available amply indicate, the mode of processing information is highly sensitive to a number of critical factors - and therefore, even if the data in any particular task clearly favour one model, there is no guarantee that another task which is logically equivalent will exhibit the same pattern of RTs and therefore be explicable in terms of the same model.

Thus, at the present moment, the conclusion must be that the distinction between serial and parallel processes is of relatively little value, and will continue to be so until much more is known about the basic units of information processing.

What has been said above in quite abstract criticism, will become clear in the review which follows. The experimental paradigms which form the structure of the review, are a subset of the tasks which have been used to study the nature and methods of perceptual comparisons. They have been selected both for their intrinsic interest and also for their relevance to the chapters which follow.

(b) Some Experimental Paradigms - A Review:

1. Memory Search: some form of search through memory is a logical requirement for recognition; a present stimulus must be compared against already stored traces and checked for similarity. Much of the experimentation in search of a model for memory search has been carried out using a task devised by Sternberg (1966,1967). Subjects are presented with a list of digits in a

tachistoscope, followed at a fixed interval (2 seconds) by a single "probe" stimulus which may or may not have appeared in the original sequence. If the probe was in the original the subject makes one response, if not, then he makes another response. The independent variable is usually the number of items in the original list, the dependent variable being the time taken to search through memory for the probe item, timed from its onset. Sternberg's earlier experiments were made to discover (i) what was compared with what, and (ii) how the comparison was carried out. In a simple task, with one probe item and up to four test items, Sternberg reported that RT increased in a linear fashion with original list ("set") size - averaging at about 40 msecs. per item. The slopes for the functions of both positive (target present) and negative (target absent) RTs were not significantly different after two days of practice, although they had different intercepts on the RT ordinate, indicating that negative RTs were slower than corresponding positive RTs by a constant amount, unrelated to set size. Degrading the single probe item with superimposed visual noise led to a change in the intercept of the linear function on the vertical

axis, but not to a change in slope. Sternberg drew the following conclusions from his data: (a) matching is preceded by a smoothing process, of constant duration - this is clear from the increased intercept for the degraded stimuli, and from the lack of effect which degradation of the probe has upon the slope of the RT/set size function; (b) The smoothed replica is matched with the memory list by a fast serial scan, which is exhaustive. The possibility of a parallel scan is excluded according to Sternberg for the following reasons - (i) a self-terminating parallel scan predicts no relationship between the target set size and RT, (ii) an exhaustive parallel scan would yield a non-linear function relating set size and RT; both these models therefore are incompatible with the data and must be rejected. The linearity of the results points to a serial scan, and the absence of a difference in slope between positive and negative RTs points to its being exhaustive rather than self-terminating. On economic grounds an exhaustive scan seems inappropriate and non-adaptive, but Sternberg defends it against the less adaptive self-terminating

scan arguing that "if the determination of whether or not a match had occurred were a slow operation that could not occur concurrently with scanning, self-termination would entail a long interruption in the scan after each comparison" (1966, p. 653). Briggs and Blaha (1969) have extended Sternberg's data by varying display item load (one or two items) as well as memory load (one two or four items) and their results support a serial exhaustive model of the comparison process. However, they report that the slopes of positive and negative RT functions do in fact diverge as the number of items in the test list is increased; this indicates that, with increased memory load, the mode of processing will vary in a substantial manner.

Sternberg's data and discussion seem to be an exemplary model for assessing the value of the serial-parallel distinction. Recent criticisms of this illustrate the difficulties of model-building of this kind:

(i) Nickerson (1966) reports that there is an inverse relationship between RT and the number of dimensions common to both test list and memorised target list; this suggests that a completely exhaustive search is excluded.

(ii) Sternberg's conclusions are greatly weakened by the finding (Morin, de Rosa & Stulz, 1967) that search times are sensitive to the serial position of the target item in the search list. Clifton & Birenbaum (1970) have shown that list position significantly interacts with the interval between the presentation of the last item in the positive set and the onset of the probe item — serial position effects being most marked at the shortest interval. Further, for 9 out of 12 subjects negative slopes were greater than positive slopes - clearly a S-T search, while in the remaining 3subjects positive and negative slopes were parallel, indicating an exhaustive search. These results are incompatible with a truly exhaustive scan, and therefore with Sternberg's model. The position is complicated, however, by a replication of Sternberg's experiments by Atkinson, Holmgren & Juola (1969) which produced no serial position effect. Results from an unpublished study

by Corcoran, Dennett, Morris & Light (cited Corcoran 1971) which contained an auditory version of Sternberg's paradigm indicate a strong serial position effect for four and six item target lists of digits. Since Sternberg explicitly adduces the lack of serial position effects in his data as a ground for his conclusion of an exhaustive search, unless the later data from Morin & al, and Clifton & Birenbaum are in some way spurious then it is difficult to reconcile these effects with his original model.

(iii) Sternberg rejects the possibility of subjects assigning one or more names to the probe item, whether degraded or not, and then matching this against the names of the items held in store. Cohen (1971), however, points out, with justification, that this strategy is not excluded by Sternberg's data, since a serial name match could be expected to yield a linear function relating RT to the number of items matched. However, the relative rapidity of matching found by Sternberg makes this less plausible than a visual matching strategy of the kind he suggested.

(iv) Theios & Palmagne (1971) find evidence for self-terminating processes "guided by the absolute probability of + and - set items " (Rabbitt, 1971, p. 261). In other words, it appears that, for their data, RTs were found to be independent of target set sizes.

(v) Briggs & Blaha (op.cit) find that the fit of their equations, based upon the assumption of a serial exhaustive search, breaks down when the size of the probe item set is increased to 4 items. They explain the result by a rechecking strategy, which is self-terminating, which subjects adopt when the information load increases beyond a certain point.

(vi) Corcoran (1971) presents an extended argument in defence of the logical indistinguishability of serial and parallel processes (pp. 59-92) with particular reference to Sternberg's work. His main point is that any result which is compatible with a serial process is also compatible with a parallel process based on a "limited power hypothesis". According to this, the relative efficiency of an analyser is inversely proportional to the number of analysers switched in. This is a direct analogue of the channel capacity concept of

classical information theory. It is certainly a feasible alternative to Sternberg's model, although it requires a one:one correspondence between letters and "analysers" for which, as yet, there is no independent evidence.

It is clear from the above (i-vi) that any simple account of memory search processes is unlikely to be, by itself, correct. Performance turns out to be significantly sensitive to several factors - target set size, target frequency, memory load, serial position etc. - and therefore it makes no sense to talk in straightforward serial vs. parallel terms. Increases in the information load may force subjects to change either the unit or the mode of making comparisons in a way which makes it absurd to generalise from one set of data to another. Briggs & Blaha were forced to modify their equations to incorporate results obtained with increased information loads; the difficulty is that there is no evidence to say whether this change represents an underlying alternation of functional processes, or simply a quantitative change induced by increased difficulty. The main point to be derived from these data and discussions is that comparisons, functionally, may be self-terminating or exhaustive, depending chiefly, and perhaps solely, upon the imposed information load, and therefore

any model which does not incorporate this contextual flexibility is bound to be wrong.

2. Visual Search: visual search tasks, in which subjects are required to conduct a systematic scan through a continuously present list of stimuli to locate an item that does or does not contain one or more specified target items, provides a task which is logically equivalent to Sternberg's memory search. Although the display-unit size is greater in the visual search task, both types of experiment require subjects to scan a set of target items stored in memory against a visually present probe list. Again, the dependant variable is search time per item scanned. Neisser (1963) used this technique to investigate search for a variety of targets. He found that the time taken to search for a single target was less than that taken to search for a line containing one of two possible targets. Furthermore, he reports that searching for a presence of a target is uniformly faster than searching for a line which does not contain a given item (searching for absence). Neisser, Novick & Lazar (1963) report that afterheavy practice subjects are able to scan as fast for ten targets as for one, which leads them to conclude that parallel search process is possible.

Superficially, Neisser's conclusion is in direct contrast to that of a serial search propounded by Sternberg using a logically equivalent task. However, there are at least two critical differences between Neisser's and Sternberg's paradigms which make strict comparisons invalid:

(a) The target frequency in Neisser's experiments was very low (2%). Sternberg used frequencies of 25%. Rabbitt (1966a) has shown that the low frequency of target to background items in visual search contributes significantly to the results, and particularly that with high target frequency the range of features analysed increased with the number of items for which subjects searched (Rabbitt, 1967a).

(b) Neisser allowed his subjects to make up to 25% errors, in contrast to Sternberg's limit of 5%. The trade off of increased speed for decreased accuracy has been shown to be an important feature of information processes (Schouten & Bekker, 1967; Rabbitt & Vyas, 1970; Pew, 1969), and it is therefore unrealistic to directly compare results from studies with heavily divergent error rates. As Pew comments: "if one wishes to compare information processing efficiency across a set of tasks,

at the very least it is appropriate to attempt to fix the error rate, or to fix the response time and examine the error rate", op.cit.p.25). A recent experiment by Wattenberger (1969, unpublished) provides an empirical foundation for assessing Neisser, Novick & Lazar's claim that subjects can search for ten targets as rapidly as they can for one: insistence on increased accuracy, and low errors, restored the serial-type function which Sternberg reported for high-speed scans.

Neisser postulates a two-stage model (described above) to account for these results: the first stage - a "pre-attentive" scan - operates by selecting stimuli for further analysis, on the basis of gross feature differentials - e.g. colour, overall shape etc.. At this level processing is relatively crude, stimuli not being processed to a level at which they can be "identified" or "recognised" but simply selected as "possible" targets. The second level of processing, an "attentive" level, is the level at which items are dealt with to the point at which they can be definitely rejected or accepted as targets. It is presumably at this level that memory search is executed. A corollary of this model is that background items are not processed to the same extent, or in the same way, as target items; Neisser supports this contention with the circumstatial observation that his subjects reported that targets tended to "jump out" from a "blur" of background stimuli. Inother words, non-targets, unlikely to get beyond the first pre-attentive stage of analysis are in some sense"not recognised". There is little concrete

evidence to support this: Neisser himself reports that alternations in the relative visual confusibility of background and target items will significantly affect search times. Also, Gordon (1969) reports that variations in background, as well as target items, are relevant to the speed of search. Kreuger (1970, a,b) used approximations to words as his background context, and found that targets were detected more rapidly in words than in second-order approximations ("pseudo-words"), and, again, faster in second-order approximations to words than in random letter sequences. It is, however, a possible criticism of this result that the subject's familiarisation with the context might lead him to adopt a different strategy and thus increase his search speed. Even if this is so, Kreuger's results seem to contradict Neisser's claims that background stimuli are (a) not recognised, and (b) not processed in the same way as targets. Rabbitt (1967a) examined the effects of changing the background stimulus vocabulary with varying degrees of practice on a given background and target set, and found that alteration of background is more deleterious to search performance late in practice than it is earlier on. This finding supports Rabbitt's view that, with practice, subjects are able to perfect

a "feature list" which enables them to differentiate target and ground. It is also indirect support for Neisser's claim of different target and background processes, but only to the extent that a common response to all background stimuli constitutes a "different process", rather than a different application of the same process that allows subjects to make separate responses to each of several targets. Rabbitt's account, it must be said, relies upon the contentious assumption that with, for example, a 10 letter target set and a 16 letter background set, it is possible to find a set of features which reliably differentiate targets from non-targets.

The distinction between the method of processing targets and non-targets is critical to Neisser's account of visual search, and to this extent stands or falls by the validity of the kind of evidence presented above. However, a series of failures to replicate Neisser's results provides further reason for distrusting his account: Kaplan & Carvellas (1965) and Kaplan, Carvellas & Metlay (1966) produce data which show an increase in search time with the size of the positive "target" set, even with extended practice.*

* Although in slightly different situations, Nickerson & Fehrer (1964) and Chase & Posner (1965) also show an increasing search time with number of targets and number of items scanned.

Furthermore, a recent experiment by Sternberg & Scarborough (1969) in which subjects were paced searching through long sequences which were presented to them in rapid succession shows that any simple parallel model is inadequate. They report that their data are best fitted to a model which assumes that subjects carry out a series of sequential tests upon each item they are shown, but that these sequential tests are not themselves performed in series, but overlap, temporally, with the preceding tests; in other words, a hybrid model, which is neither strictly serial nor strictly parallel.

It is clear from the above discussion that visual search data require a considerably more elaborate model than that offered by Neisser. Neisser's results do not rule out a pre-attentive stage of processing, for which indeed there is some independent evidence (cf. chapter 1), but if such a process exists, it does not appear to have the properties which Neisser is prepared to attribute to it. In modelling search data, account must be taken of factors which are known to significantly influence the results - error-rate, frequency of target and non-target stimuli, and the context in which the targets occur. It would be premature to come to any general

conclusions about the nature of the search process on the evidence available beyond the statement that the processes are much more elaborate and complicated than has hitherto been supposed.

3. Binary Classification: The logically limiting case of Sternberg's multi-target, multi-probe scanning situation is that in which there is one target stimulus and one probe stimulus only; this is basically the paradigm of binary classification experiments. In a typical task, subjects are required to make a single response to signal whether a pair of simultaneously or sequentially presented stimuli are same or different. The response is therefore made on the basis of a relationship between two stimuli, rather than on the basis of the intrinsic or absolute identity of any one stimulus. This therefore makes it possible to study the process of discrimination independently of stimulus identification.

Binary classification experiments are of special importance to the study of discrimination, since they provide information about both the several levels of abstraction at which stimuli can be classified, and also about the units in which information is processed

in making discriminations. They are also important for this thesis, since the experiments contained in Chapter 1 are based entirely on this experimental paradigm. However, since these experiments deal only with the level of physical identity or difference, this review will be a selective cover of pertinent data, and relatively little space will be devoted to reviewing the results of experiments on classification at more abstract levels.

Posner & Mitchell (1967) first attempted to deal with the processes underlying "same"-"different" classification. Their subjects were required to make these responses to stimuli which were either letters or Gibson shapes. These were either physically identical - e.g. AA, mm - or identical in name but physically different - e.g. Aa or Gg - or else members of an identical superordinate category - e.g. Au - both vowels, or Xd - both consonants. The relation of the results Posner & Mitchell, and subsequently Posner & his colleagues, have obtained to name and category comparisons are ably reviewed in several recent publications - Posner (1969); Rabbitt (1971) and by Cohen (1971); this review is principally concerned with the results of classification at the physical identity level.

Posner & Mitchell report that for physical comparisons identity is reliably signalled 60 - 80 msecs. faster than physical difference; this holds for simultaneous and sequential presentation at small inter-stimulus intervals. However, this is not true of other types of stimuli: Bindra, Williams & Wise (1965) report that for pairs of tones, "different" responses are faster than "same" responses. This result is confirmed by Coltheart & Curtoys (1968), although in their experiment the reversal occurred only when "same" trials were more frequent within a stimulus list. Bindra et al.'s result is paradoxical in view of the fact that they used pairs of tones which could differ by as little as 60 cy. per sec. pitch. Under these conditions it would be expected that the low discriminability would elevate the "different" RTs and thus accentuate, not diminish, the difference between "same" and "different" classification times. However, Bindra et al. confound stimulus discriminability with stimulus "codability", and it may well be that the "same" matches are made more difficult for stimuli which are not readily codable. The model they offer is based upon a choice-time model of McGill (1960) and Sekuler (1965), which assumes that subjects accumulate evidence

about the stimuli - or some transformation of it, "in an adder, and matches a running total of this accumulated information against some predetermined criterion value" (p. 1627). They consider it a sufficient explanation of the relative latencies of "same" and "different" judgements to postulate that "the 'same' criterion is more stringent than the 'different' criterion" (p. 1627) - however, this is merely a restatement of the results, and not an explanation of them. The critical question is what is the nature of the information accumulated?

Decision time in binary classification is affected by several factors: the nature of the subject's instructions, the relative frequency of alternative decisions, the difficulty of the task, the form of the required response etc.. All or any of these may contribute to a given experimental result. Four particular areas on which data from "same"-"different" experiments have provided valuable information will be discussed in more detail:

(i) <u>The Units of Information processing</u>: knowledge of the nature of the units of processing is a logical pre-requisite of knowledge of the way in which these units are combined. Neurophysiological data (e.g. Hubel & Weisel, 1965) suggest that simple features - e.g. line slants or stopped edges - are the basic units of figural analysis. However, behavioural data by Eichelmann (1968) suggest that whole letters may serve as integral processing units. His subjects were required to match pairs of stimuli which could be either upper-case letters or two of four line slants. Letters could be matched at least as rapidly as line slants; Eichelmann therefore concluded that a letter cannot be regarded as an integrated bundle of line features for the purposes of matching, but may itself operate as an independent unit. This conclusion is consonant with Posner & Mitchell's finding (1967) that Gibson shapes, though they are unfamiliar stimuli to most subjects, take no longer to match than letters which are highly familiar. Hawkins (1967, 1969) makes the useful point, from experiments on multi-dimensional stimulus matching, that if attributes can in fact be matched in parallel then the rate of matching cannot be used to define a unit, except for an individual task. Letters

may be processed as a unit for single letter matches, but a larger unit may operate where words are involved.

(ii) Instructional Effects: The unit of matching may therefore vary with the type of task studied. Posner and his colleagues report other effects of context on binary classification: the response "different" to physically different stimuli - e.g. "AB" - was about 70-100 msecs. faster in an experiment where subjects matched on the basis of physical identity alone, than when they matched for name identity. Thus the same stimulus-response combination gave different RTs under different instructional sets. However, the reverse effect was not found; name identity did not affect "different" matches under physical identity instructions. Eichelmann (1968) reports that matches of multiple strings of letters are significantly faster when the strings form words than when they aresimply random letters. The effect of "familiarity" is relatively complex: Hochberg (1968) reports no difference in matching time for familiar and unfamiliar strings of letters; this is hardly surprising since he arranged the strings vertically. Egeth & Blecker (1971) make the valuable point that

"same" and "different" RTs are differentially affected by familiarity: "same" processes are slowed, "different" processes remain unaffected. These contextual effects point to the complexity of processes involved in an apparently simple matching task.

(iii) Temporal Characteristics of Matching: there is ample evidence to suggest that physical identity matches become less efficient as the interval between two stimuli to be matched is increased. Posner & Keele (1967) and Posner, Boies , Eichelmann & Taylor (1969) report results of varying inter-stimulus interval which show a broadly similar time course of decay to the loss of visual information from tachistoscopic exposures (Keele & Chase, 1967; Sperling, 1960, 1963). At an ISI of 1500-2000 msecs. physical identity matches are no faster than name identity matches. However, there are good reasons for not equating the visual code studied by the RT method with that studied by tachistoscopic exposure. Effects of stimulus luminance, interpolated noise and exposure duration, which occur in the

latter situation, do not appear to affect "same"-"different" judgement (Posner, 1969). Thus comparison across tasks must be made with some caution.

(iv) Levels of Processing: Stimuli can be matched at three levels of abstraction - physical identity, name identity and super-ordinate category identity. The question is: what are the characteristics of matching at each level and how are the levels related. Since name identity matches are not the concern of this thesis, reference can be made to excellent reviews by Posner (1969), Rabbitt (1971a) and Cohen (1971). Rabbitt provides detailed arguments for the view that physical and name identity matches are functionally distinct, and supplies a useful taxonomy of empirical means by which they may be separated. Posner (1969) also discusses this at length (pp. 84-94).

Superordinate categorisation is not related to nominal categorisation in the same way that nominal categorisation is related to physical identity: stimuli and names are most often related in a 1:1 fashion, whereas a given stimulus may be a member of several distinct categories.

Thus the processes of categorisation may be in part determined by the specific nature of the task. Empirical evidence for this is provided by Posner (1970) who required subjects to classify pairs of stimuli as either "both letters or both digits" on the one hand, or "both vowels or both consonants" on the other hand. His results indicate that the letter-digit classification is able to proceed directly from the visual form, without the intermediate stage of stimulus naming, whereas the vowel-consonant decision cannot be made without subjects first determining the letter names. This contention is based on the finding that acoustic similarity affected the latter but not the former task. Further evidence relating to superordinate classification will be reviewed in Chapter 4.

Models of the Binary Classification Process: accounting for the wealth of data on "same"-"different" judgements presents problems: modelling the differences between category levels is beyond the scope of this review; what is of interest are the differences which exist between "same" and "different" RTs at a single level - namely that of simple physical matching.

That "same" judgements for letters should take less time than "different" judgements is, as Nickerson comments (1968), "counter-intuitive" since "the determination that two visual stimuli are identical seems to require a more detailed comparison than would the determination that they differ...". This disquiet is more concisely put by Bindra et al. (1965) who from their own data which show that "same" matches are slower than "different" matches when the individual stimuli are less discriminable suggest that "same" and "different" decisions do not represent converse and obverse of an unitary judgemental process, that is, they are not alternative decisions made on the basis of a single information process. Posner's account is less enlightening: "the rapid decay function ... and the ability to search the code as a spatial array ... seem to agree with the view that the physical match is based upon the passive trace of the immediate past stimulus"; on the other hand "the lack of effects of luminance, duration and visual noise, the importance of attention and the ability to rehearse and generate the visual code argue that the basis of a physical match is not the trace of the stimulus, but the activation of an abstract code which serves as an internal representation

(abstract idea) of that class of prior visual experience" (Posner, 1969, p. 95). This is hardly conclusive, nor indeed hardly a model.

A view which has been proposed by several authors is that "same" and "different" judgements are subserved by two entirely distinct processes (e.g. Nickerson, 1969; Bamber, 1969; Bindra, Williams & Wise, 1965; Egeth & Blecker, 1971). The stimulus for this view comes from the failure of one-process accounts to explain the relative speed of "same" matches, or the differential effect of stimulus manipulations (e.g. discriminability, familiarity) on "same" and "different" RTs.

The logical precursor of two-process models is a model derived from decision theory; this assumes the existence of an hypothetical decision axis, upon which the "neural datum" - i.e. stimulus differences which will be judged "same" or "different" - appear as two partially overlapping (normal) distributions. Since possible decision outcomes are represented by two distributions, a (flexible) "criterion" is introduced upon which decisions are based. It is further assumed that decision latency is maximal at the criterion point,

since this corresponds to the "point of greatest uncertainty" (Bindra et al., 1965). The specific location of the criterion on the probability-latency function allows one to make predictions about the relative RTs of the two possible decisions. There are difficulties with this model:

(a) There is no means of determining the location of the criterion in advance of obtaining actual RTs - except perhaps by forcing subjects to adopt high or low criteria for one or other decision by manipulating instructions, pay-offs etc. in the traditional decision theory manner; without knowledge, independently, of the criterion point, one can only verify predictions from this model recursively.

(b) An empirical objection to the model lies in the nature of the explanation it offers: Bindra et al. take faster "different" RTs to tones as evidence for a stringent "same" criterion; this manoeuvre, however, only pushes a stage further back the explanation of why "same" criterion for tones should be more stringent than the same criterion for, say, letters, where "same" RTs are faster than "different" RTs. This cannot be a voluntary

matter - it must be a characteristic of the information processing device, since neither Bindra nor Posner's subjects were specifically instructed to maximise speed for one particular decision.

(c) This model is essentially a one-process model; although there are separate information distributions for "same" and "different", the provision of one single criterion common to both decisions makes them functionally dependent. The indications from the empirical data, however, suggest that the two decisions are functionally independent, although under certain circumstances they may interact.

Dissatisfaction with this type of account has led to the introduction of two-process models. The common assumption of these is that there exist two functionally independent information processes, which have separate criteria and accumulate specified information separately. A typical example is that offered by Bindra et al. as an alternative - and better - explanation of their data to the statistical decision model: it is derived from choice models of Stone (1960) and McGill (1963) and Sekuler (1965). In essence "one might conceive of the subject's decision

making mechanism as a statistical computer which receives a stream of information about the stimuli, accumulates this information, or some transformation of it, in an "adder" and matches a running total of this ... against some predetermined criterion value" (Bindra et al., op. cit. p. 1627).

The distinguishing mark of the several varieties of two-process models is the nature of the information accumulated by each process. It is not necessarily implied that because the possible decisions are "same" and "different" that one accumulator is a "same" counter and the other a "different" counter. The models suggested by Bindra et al. and Nickerson (1969) have in common the feature of a single "information accumulator" which collects "difference" information from the stimuli being matched and stores it until a prefixed criterion is reached, when the response "different" is triggered. The second accumulator is a time clock which starts its count at the same time as the "difference" accumulator begins its count; if the "different" criterion has not been reached within a preset time criterion, the decision "same" is made.

This model is not entirely satisfactory, although it provides its proponents with good accounts of their respective data, since it makes one of the two decisions dependent on the failure of the laccumulator subserving the other to reach its criterion - thus one decision is made by default. There are other reasons for looking beyond this model:

(a) It does not allow for experimental manipulations which affect one decision and not the other.

(b) In order to account for the rapidity of "same" decisions for letters, the model would have to be modified so that "same" information was accumulated, the decision "different" being then made by default of the time criterion. Apart from the obscurity of what constitutes "sameness" information, this modification would have the corollary that all "different" decisions would take an equal time; this is known not to be the case - simple manipulations of stimulus discriminability will increase "different" RTs. Sekuler & Abrams (1968), Egeth (1966) and many others have found that "different" RTs are inversely correlated with the degree to which the stimuli differ.

(c) There is no evidence to suggest that either of the decisions is regulated specifically and exclusively by temporal factors.

(d) The model has the unsatisfactory feature of making "same" the logical and functional equivalent of "not-different".

The obvious development of this sequence of models has been made by Bamber (1969) who proposes that "S employs simultaneously, two distinct processes for comparing stimuli. One process would generate the "different" responses; the other process would generate the "same" responses" (Abstract). RTs for correct responses and errors to classifications of successively presented horizontal rows of letters supports a two-process model of the kind Bamber proposes. We examined the functions relating "same" RTs to the number of relevant features, and "different" RTs to the number of features by which each pair of letter strings differed (feature = letter, here), and found that whereas "different" judgements indicated a serial self-terminating process, "same" judgements were too fast to be consistent with such a model. The model Bamber proposes is illustrated

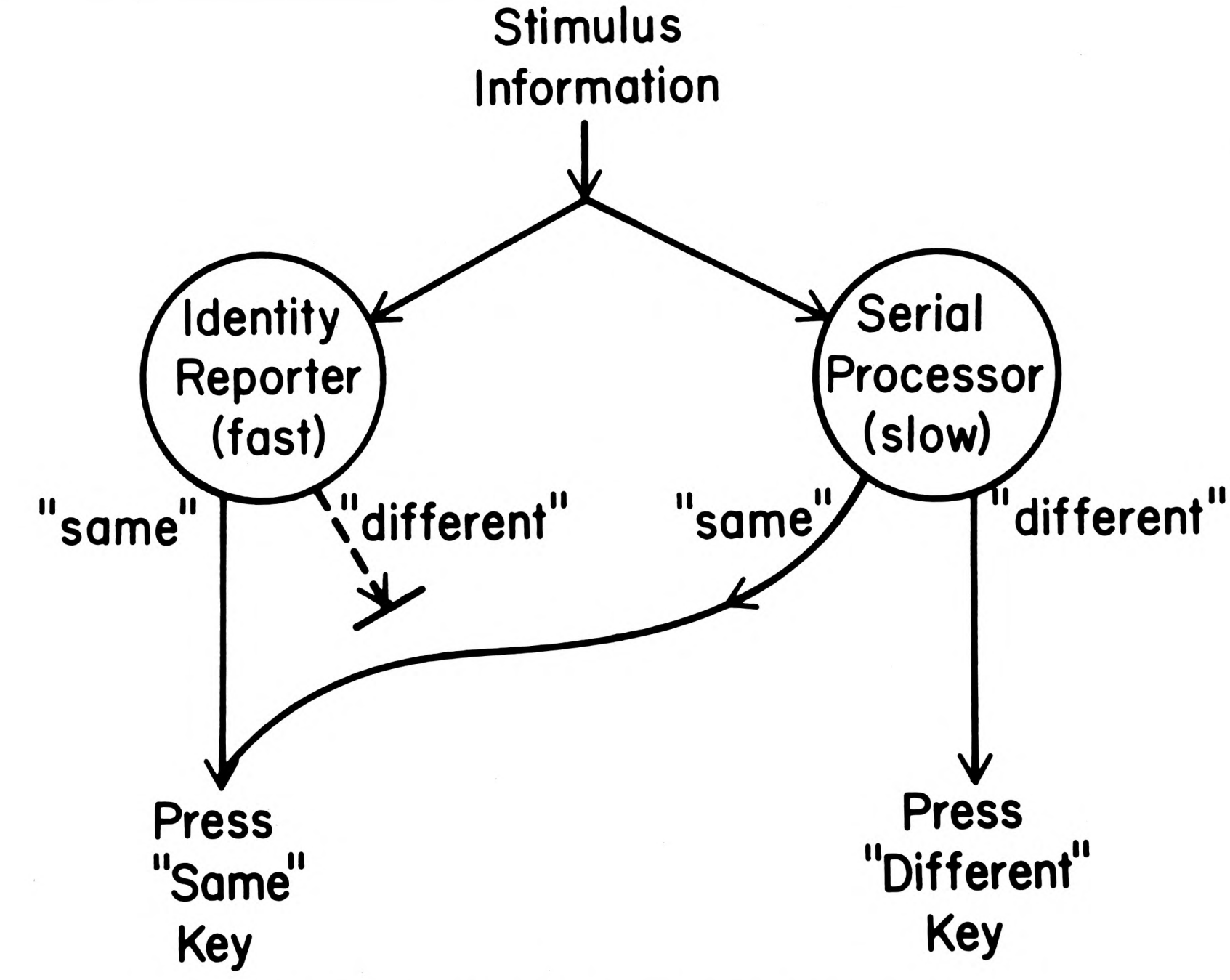


FIGURE 1: 2-Process Model of Binary Classification
(from Bamber, 1969)

in Fig. 1. It consists of two independent analysers - an identity reporter and a serial processor. The former only signals "same"; the latter however can signal either "same" or "different"; the fast reporter is usually, however, the one which initiates the "same" response. Interestingly, Bamber finds that errors of "false same" responses are as fast as correct "same" responses and suggests that these are produced by the fast "identity" reporting device which is temporarily fooled into making a misclassification by the similarity of the letter arrays. This explanation is supported by the negative correlation between false "same" incidence and the number of different letters distinguishing the two arrays being compared. It also suggests an explanation of Posner's finding of RTs intermediate between pure physical identity and nominal identity RTs for stimuli which had the same name and were also visually similar (e.g. Cc, Co).

Another two-stage model is supplied by Tversky (1969); according to this an initial stage - the "compare" stage - compares the stimuli either by a template match or by an exhaustive feature analysis. If this indicates identity, then the "same" response is initiated. If not, the stimulus

comparison is re-checked to locate the difference. This model is excluded by an experiment of Egeth & Blecker (1971) in which subjects matched pairs or trigrams of upper-case letters. An independent variable - stimulus familiarity - was manipulated by either showing the letters orthogonally, or else inverted or rotated. "same" judgements were found to be significantly faster for familiar presentation than for non-familiar presentation. "different" judgements, however, remained unaffected by this manipulation. Tversky's "compare and check" model requires that "different" decision time includes any time taken to make an abortive "same" decision. Therefore, any change which affects "same" should be automatically included in the "different" RT. This was clearly not the case with Egeth's & Blecker's data.

Egeth & Blecker arrive at the necessity for independently operating processes for "same" and "different" judgements, for different reasons from Bamber. Whereas Bamber found "same" RTs to be consistently faster than "different" RTs, Egeth & Blecker find this only for letter pairs, but not for trigrams;

for these "same" RTs were either slower (for unfamiliar stimuli) or equal to (for familiar stimuli) "different" RTs. They conclude that the "sameness detector" is responsive to the "configuration of the presented form", whereas the "difference" detector is "relentlessly featuristic". "It seems that only by being responsive to the properties of the overall configuration can the sameness detector use one of those properties (i.e. familiarity) to guide the feature analysis that is going on at the same time within the complex process of sameness detection" (Egeth & Blecker, 1971, p. 325). So far the substantially parallel accounts of the operation of physical matching given by Egeth & Blecker and by Bamber are the most satisfactory; what is certainly very clear from the data is that some form of two-process theory is demanded.

4. The Effects of Sequence: complications are added to the simple types of model for choice RT reviewed above by the introduction of sequence. It is necessary to consider the effects of sequence for two reasons: (i) stimuli are frequently encountered in sequences, rather than as discrete events; (ii) sequences exhibit properties which are not predictable from knowledge of their

constituent events. There is ample empirical evidence to suggest that the characteristics of a response to a single discrete event may depend, not only upon the intrinsic nature of that event per se, but also upon the temporary state in which the organism finds itself when the event is presented to it; this may reflect not only the individual properties of the information processing system but also the particular sequence of events which have immediately preceded it, which form its "context". This section will examine these relationships.

In recent work on choice RT, a great deal of attention has been devoted to examining the proposition that a subject's response to a present stimulus may be a function not only of the traditional independent variables of reaction-time - e.g. S-R compatibility, S-R ratio, stimulus and response discriminability etc. - but also may reflect the immediate context in which it occurs. The interest in this idea, and consequently in the fine structure of RT sequences, has arisen from the frequent finding that the repetition of a single S-R pairing in a serial RT task leads to a

a significant decrease in RT (Hyman, 1953; Welford, 1959; Bertelson, 1961). These original experiments suggest two distinct categories of investigation concerned with the properties of stimulus-response repetitions:

(a) Experiments dealing with the effects of gross variations in the statistical structure of stimulus-response sequences on overall mean RT, where other variables are held constant (Bertelson, 1961; Kornblum, 1966, 1967, 1968, 1969, a, b). These experiments are of particular relevance to the information theory of CRT (Laming, 1968), since it is a suggested corollary of the findings (Kornblum, op. cit.) that some of the classical RT functions currently attributed to variations in H_s, H_r or H_t might be partially accounted for by correlative variations in the incidence of S-R repetitions. This is disputed by Hyman (e.g. 1969). Falmagne & Theios (1969) present a stochastic model to account for results obtained in a six-stimulus, six-response key pressing experiment by Falmagne (1965), in which RT was found to be an inverse function of the probability of the stimulus (Fig. 2). Essentially, the model assumes

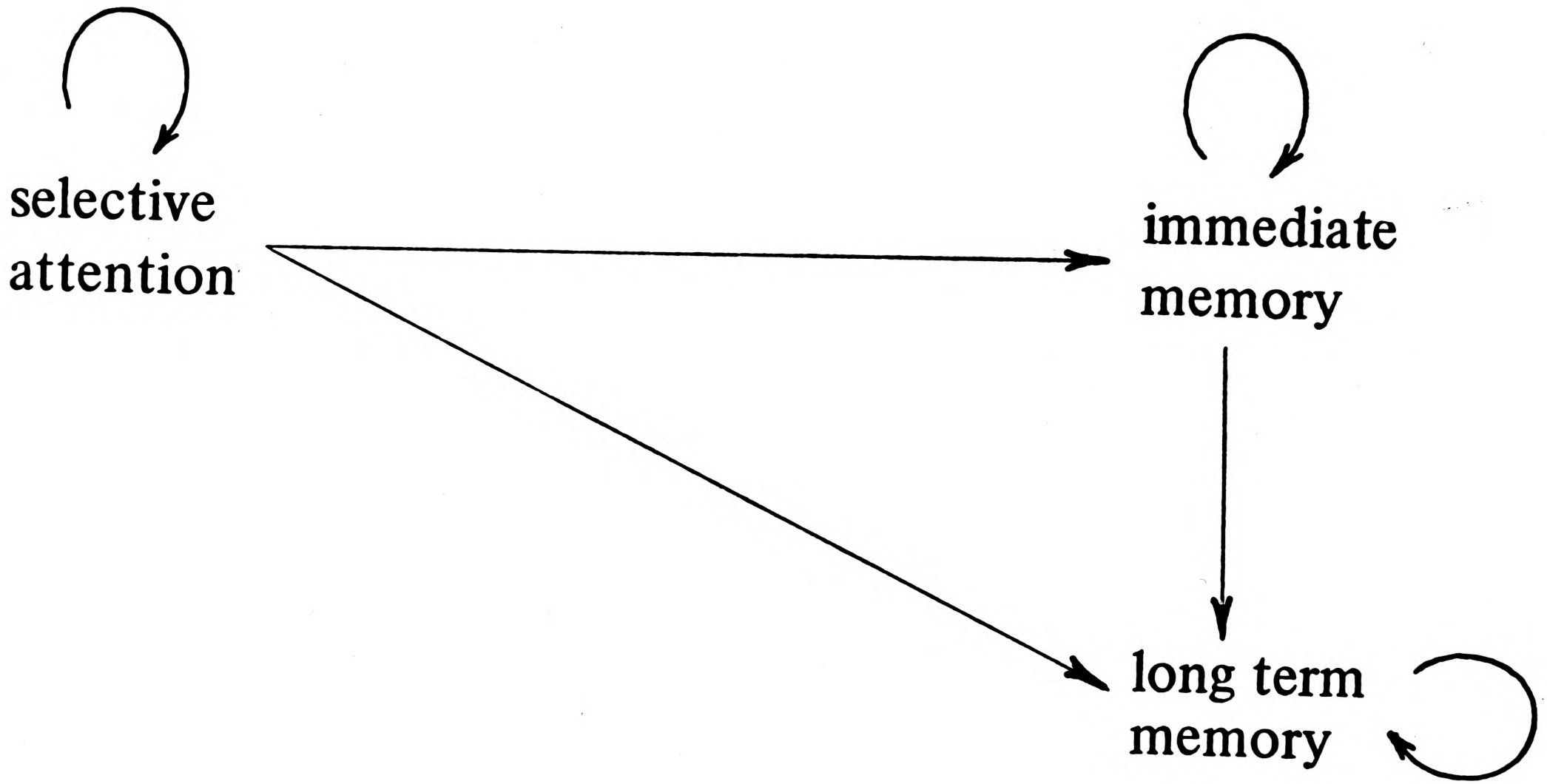


FIGURE 2: Model for Repetition Effects
(from Theios & Falmagne, 1969)

that to each stimulus there corresponds a "template" representation, which can be in one of two states - "selective attention state" or "immediate memory state". The two states are differentiated by the further assumption that on each trial there is exactly one stimulus in the former state, whereas there can be several or none in the latter state. The likelihood of a given stimulus being in the "selective attention state" - and thus enabling a faster RT, since input is first matched against this state before the contents of the immediate memory state are consulted - is related to the probability of that stimulus occurring. The templates of each possible stimulus are assumed to undergo changes of state, governed by probabilistic rules, and this allows for the possibility of sequential effects. Falmagne & Theios do not, however, explain how the model accounts for non-constant probabilities, and hence for second-order sequential dependancies.

(b) The second category of experiment designed to study immediate contextual effects seeks to examine in detail the nature of repetition effects independently of their relative proportions. Balancing within stimulus sequences for frequency of stimulus repetitions and alternations,

variations in RTs to individual transitions are examined as a function of the different aspects of relations between stimuli and responses. It is this class of experiment which is of special interest and which will therefore be reviewed at some length. The basic heuristic of this analysis lies in the assumption that rank-ordering the RTs of various possible equiprobable transitions between successive stimulus events may allow one to say something about the nature of the processes involved. The principal difference, therefore, between experiments of types (a) and (b) lies in the differences between the properties of the stimulus which they respectively reflect. While the former concern statistical properties of lists, and the subjects' ability to take advantage of them, the latter depend wholly upon the specific nature of the repetitions built into the stimulus lists from which they are generated, since single event frequencies are equated in advance. An empirical distinction between (a) and (b) is easily found: Bertelson (1961) points out that the superiority of repetitions in a serial task is apparent in the first block of trials, and therefore cannot be attributed to changes in subjects' overall expectancies resulting from

familiarity with the stimulus sequence. Indeed, expectancies by themselves may result in no new change in RT as long as the situation does not permit the subject to take advantage of them. Even conditions in which the manipulation of sequential dependencies yields equi-information lists, in terms of bits-per-signal, can be shown to result in significantly different mean RTs, and thus to belie the suggestion that these variations may produce changes by acting solely upon subjective expectancies.

It has become very clear that, functionally, repetitions are not equivalent to alternations, even in a simple two-choice task in which their relative incidences are equated across subject groups and fixed at a proportion greatly in excess of chance. The obvious corollary of this observation is that the empirical differences between signal repetitions and signal alternations must be explained in terms of one or more of those aspects of the stimulus-response process which differentiate the two types of transition.

A. The Basic Repetition Effect: As well as the early work already referred to, Bertelson (1963), Rabbitt (1968b), Bertelson & Renkin (1966), Hale (1967) and several other authors have confirmed that in serial tasks, signal repetition (and a fortiori for these particular studies, response repetition) produces faster RTs than signal alternation. The picture is not unclouded, however; Schvaneveldt & Chase (1969) failed to find any repetition effect at inter-stimulus intervals comparable to those used in the studies cited above, with 2 and 4 choice tasks, and, indeed, at 100 msecs. ISI report a slight increase in RT for repetitions over alternations (i.e. a negative recency effect). They also report that repetition of a signal which occurred 2-back in the sequence does produce a significant decrement in RT, in an experiment using 4-choice scrambled numbers-buttons codes, and an ISI of 80 msecs.. It is difficult to find a reason for the striking discrepancy between Schanaveldt & Chase's results and those of other investigators who found large repetition effects under comparable conditions, although it is possible to conceive of the actual moment of change from one stimulus to another, where this involves two identical stimuli, being phenomenally relatively indistinct at an ISI as small as 80 msec.,

and thus delaying the initiation of the response. Both Keele & Chase (1969) and Remington (1969) however support Schansveldt & Chase's results, in that they fail to find one-back repetition effects. Remington in particular is of interest: he varied signal and response biases from one to five back in the sequence, and foudd strong effects of repetition. It is not clear, however, that the functional basis underlying these effects is necessarily identical to that involved in one-back effects, although it is likely that several successive repetitions would tend to induce a powerful expectancy effect, which might diminish with the number of successively identical stimuli.

B. Effects of Signal and Response Transitions: it is not clear from the existence of an effect of stimulus repetition whether the facilitation to repeated signals is a result of repeating the signal, repeating the decision or of repeating the motor response. It is possible to devise suitable situations to examine the relative contributions of these factors to serial effects, and thus to determine the locus of the facilitation within the stimulus-decision-response loop. Bertelson's early

experiments (1961,1963) can be faulted on the ground that, in every case, stimulus repetition and response repetition were confounded. Although this is of no consequence when deciding that, for example, a 1-2//1-2 S//R mapping produces a less marked repetition effect than a 1-2//2-1 mapping, it tells us nothing about the nature of the effect itself. The most efficient means of acquiring this information is from rank-order comparisons of transitions containing different elements of the stimulus-response code in common. Three basic transitions are possible:

(i) Transitions between repeated signals where the response is also repeated.

(ii) Transitions between non-repeated signals requiring a repeated response.

(iii) Transitions between non-repeated signals requiring different responses.

Bertelson (1965) reported that transitions of type (ii) were significantly different from those of type (i) for 2 ex four of his subjects - a finding which led him to the conclusion that the main effect of repetition is linked to repetition of the response rather than of the

signal. Smith (1967), using bi-dimensional stimuli - two numerals and two colours mapped on to two response keys - found equivalent (type ii) transitions to be significantly slower than transitions between different signals requiring different responses (type iii). Smith's conclusion was that the facilitation due to repetition was not to be sought in the peripheral (i.e. motor) system, but rather in the "central" processing mechanisms. However, since Bertelson used an ISI of 50 msecs., and Smith one of 4 seconds, direct comparison between their results is not justified. Further disparity is evident from the fact that Smith's "different" transition category involved repetition of either the numeral or of the colour in which it was printed. This partial stimulus repetition might generate confusion which could lead to response or stimulus competition which might delay responses.

Bertelson's data have been elaborated and expanded by Rabbitt (1968a, 1969); it is clear from his work that transition effects are significantly altered by practice. Rabbitt (1968a) examined the contributions made by stimulus information and response information to repetition effects at two levels of practice and found that, whereas, early

in practice, equivalent (ii) transitions are not significantly different from changed-signal-changed-response (type iii) transitions, after ten runs of practice, there are within 11 to 30 msecs. of identical transition RTs. This directly supports Rabbitt's view that, with practice, subjects are ableto develop a more economical and efficient coding system (cf. Rabbitt, 1967a); it also contradicts Bertelson's findings which attribute the major part of the facilitation of S-R repetitions to repetition of the response.

In addition to these serial studies, several other experiments indicate the possibility of manipulating the stimulus processes independently of the response-producing processes:

1. LaBerge & Tweedy (1964) varied the frequency of occurrence of two stimuli attached to a single response and found that despite constant response frequency, the more frequent stimulus produces a significantly faster RT.

2. Broadbent & Gregory (1964)report that a stimulus with a constant probability of appearance of 75% showed a slower mean RT if it formed part of a four-stimulus alternative set, than if it formed part of a two stimulus alternative set.

3. Lawrence and LaBerge (1956) and Bertelson & Tisseyre (1969) both report strong effects of stimulus repetition, independently of response repetition.

4. It is not possible to adduce the classical effects of set, expectancy and probability bias in this context, since it is arguable whether these operate through the signal analysing or response-producing processes. However, it is unlikely that signal facilitation is not at least a contributory factor to many of these effects. An excellent review is available (Rabbitt, 1971$_a$).

There appears, therefore, to be strong evidence favouring the possibility that stimulus repetition is at least a component factor in the facilitation due to signal-response repetitions. How significant a component is as yet undetermined - but not indeterminate; it is possible to devise experiments which empirically dissociate the contributions of stimulus and response repetition. Rabbitt (1971$_b$, in press) reports two such experiments: in the first, subjects responded to single stimuli on the basis of a comparison between the present signal and its immediate predecessor, the responses being "same" and "different". Only two possible signals were used. In the second experiment groups of 4 signals were used,

signals being treated as "same" or "different" in respect of their belonging to one of two sets of 4 signals each. Signal repetition therefore only involved response repetition where either the two previous signals were physically identical to the present signal (first experiment) or were members of the same arbitrary class (experiment 2).

Under these conditions signal repetition was still significantly facilitative. The rank-order of response-transition classes was found to be different from that predicted by Bertelson's hypothesis of an automatic comparison of each signal in a sequence for identity with the previous one, and by analogy, at variance with the predictions from the existing "same"-"different" literature (Posner, Boies, Eichelmann & Taylor, 1969). This variance was particularly marked for the "different"-"same" response transitions where the stimuli were non-identical; RTs for this class were found to be significantly slower than either "same"-"different" or "different"-"same" transitions involving identical stimuli. The "same"-"different" literature, by contrast, makes the unequivocal prediction that all "same" responses

should be faster than all "different" responses. It is a possible criticism of Rabbitt's experiment that the response "same" was defined as "same class as the previous stimulus"; on this basis, it is not clear that the comparison between his results and the discrete "same"-"different" data is valid.

There have been several suggestions in recent papers that the effects of sequence in serial RT experiments and the discrete "same"-"different" effects - reviewed above - have properties in common. One suggestion (Bertelson, 1963) is that each stimulus in a sequence is classified initially, by direct comparison with the stimulus immediately preceding it in the sequence. Rabbitt (1968b) extended this hypothesis, since equivalent signal transitions were also found to be facilitative after practice, which rules out a simple stimulus-stimulus-one-back comparison by itself, to include a <u>serial</u> comparison of the present stimulus with all stimuli of the class to which the previous stimulus belonged. In recent, unpublished experiments, Rabbitt required his subjects to classify single nouns into one of two semantic categories; RTs to class repetition were significantly faster than RTs to class alternations. More strongly still, it has been

suggested by Entus & Bindra (1970) that not only is the stimulus present to the subject compared with the preceding stimulus, but that there is a significant correlation between the properties of that comparison and the "same"-"different" effects shown in discrete binary classification tasks. Entus & Bindra generate correlation coefficients for pairs of RT tasks - simple RT, choice RT and recognition RT - using both serial and discrete stimulus presentation. Their prediction is confirmed - "same" and "different" decision for the two categories of task do indeed correlate significantly. However, their argument is weak, since from the observation of these correlations they conclude that the serial repetition effects are in fact "same"-"different" effects. Nonetheless, their hypothesis is an interesting one, since it provides a possible link between discrete and serial RT tasks, and the potential basis for a model of sequential effects.

C. Effects of Stimulus-Response Compatibility: it is notoriously difficult to define SR compatibility independently of the particular situation one is discussing. However, for the purpose of this review, it can be thought

of as being greater or less according to whether the mapping of stimuli on to responses involved produces smaller or larger RTs respectively than the simplest, most direct 1:1 mapping.

Bertelson (1963) ran subjects on a 2-stimulus: 2-response lights:keys task, with either direct or reversed mapping, and found that repetitions were fastest on the direct mapping task, although they were most advantageous on the reversed task. Unfortunately the measure used to compute the relative effects of repetitions over other types of transition by Bertelson and subsequently by others - viz. $RT_{alt.} - RT_{rep.}$ - is grossly misleading, for the reason that, since greater incompatibility $\underset{defn.}{=}$ increased RT to individual signals, the relative advantage of repetitions is bound to be artificially elevated, even if the absolute RTs remain unchanged. Bertelson & Joffe (1962) also report RTs to non-repetitions to be disproportionately increased by increasing SR incompatibility, and Bertelson takes this as evidence for the existence of two separate mechanisms, one governing repetitions and the other governing alternations, since

it excludes the possibility of a single mechanism governing both, with facilitation for repetition. Schanaveldt & Chase (op.cit.) report that the magnitude of the RE increased with increasing complexity of the SR code, for two and four-choice tasks. Keele (1969) reports no repetition effect for a compatible task, but a significant RE for an incompatible six-choice task. Curiously, an increase in the RE was found when the intertrial interval was filled by irrelevant interpolated activity.

Explanations of these effects cannot overlook that they may be the result of a simple artefact: it is quite possible that the lack - or weakness - of repetition effects for highly compatible tasks merely reflects the fact that the overall low RTs, which reliably occur with compatible tasks, are masking the small but subtle REs which are nonetheless present. Conversely, the higher repetition effects for incompatible tasks may just reflect the relative difficulty of responding to non-repetitions. What would be of great significance would be the failure to find any repetition effect with a highly incompatible task - an observation which has yet to be made.

D. The Effect of Temporal Factors on Sequential Phenomena: the temporal parameters of stimulus presentation are known to have important effects upon perception and memory (Aaronson, 1967). Rabbitt (1967; 1969), presenting arguments for demanding different functional models for serial self-paced and discrete RT tasks, argues that serial tasks show a contingent variation of RT from preceding signals and responses, which is not shared by discrete tasks. Unfortunately, the distinction between serial and discrete tasks is hard to make. If the absence of contingent variation in RT is to be a criterion of separation, then the distinction becomes task-dependent, since such variations only occur in certain conditions. A further difficulty with a purely behavioural criterion is the finding of REs at stimulus-stimulus intervals as great as 13 secs. (Smith, 1968) and at 4 and 8 secs. (Keele, 1969) - intervals which would normally be unquestionably classified as defining discrete RT paradigms. However, while it may not be possible to drive a hard and fast empirical wedge between serial and discrete situations, there are good reasons for considering the data from experiments at short and long response-stimulus intervals separately: (i) firstly, at very short intervals - up to

about 200 msecs. - there is the possibility of perceptual grouping and refractoriness occurring (Davis,1962); (ii) Secondly, it is known that beyond intervals of 1.5 to 2.0 seconds, iconic persistence of the visual trace (Posner & Keele,1967) is unlikely to occur, thus forcing the subject to employ some other stimulus code if he requires a trace to match against a subsequent stimulus; (iii) At longer response-stimulus intervals, it is more likely that cognitive strategies will play a greater part in determining the way in which subjects deal with successive signals. For these reasons, the results of short and long interval studies will be dealt with separately.

(i) Short Response-Stimulus Intervals: Broadly, the evidence suggests that the magnitude of the repetition effects found are inversely correlated with the length of response-stimulus interval used. Bertelson (1961) finds repetitions to be significantly faster than alternations in a two-choice task in which each type of transition occurred equally often, at an interval of .05 seconds, but not at an RSI of .5 seconds. At the longer RSI repetition RTs actually increased in

absolute value. Bertelson & Renkin (1966) and Hale (1967) also find a decrease in RE with longer RSI, both for two-choice tasks. Schanaveldt & Chase (op. cit.), however, fail to find any RE at an intertrial interval of 1 sec. in a two choice task, and, on the basis of their results, suggest that REs are governed by "sequence memory" - i.e. a verbal trace of the preceding sequence which acts to determine expectancy, and conclude that "these sequential dependencies reveal higher order cognitive processes involved in even the simplest choice RT task" (p.7). Schanaveldt & Chase interestingly fail to find any decrease in RE with increase in intertrial interval; this is in direct contrast to Bertelson, and to Hale. Thus, there is no clear pattern of interaction between repetition effects and short duration response-stimulus intervals.

(ii) Long Response-Stimulus Intervals: while, from the short interval data, we can say (a) that REs genuinely exist, and (b) that they are related in no apparently systematic way to interval between one response and next stimulus, the data from the longer intervals (beyond 1.5-2.0 secs.) present a more complex picture.

Several studies report significant repetition effects in the ITI range beyond 2 secs.. Hyman (1953), for example, using a 10 second RSI, reports an RE for both 4 and 8 choice tasks - although not for a two-choice task, which showed a slight negative recency effect. Smith (1967) finds an effect of stimulus repetition at each of the four intervals he used - 2,4,6 and 10 secs., to bidimensional stimuli mapped arbitrarily on to two response keys. Keele (1969), however, in a six-choice task, finds no RE for ITIs of 2,4 or 8 secs. with compatible SR mapping, although an incompatible mapping did produce a stimulus repetition effect. Williams (1966) also failed to find any effect of stimulus repetition for bimodal stimuli at ITIs of between 12 and 15 seconds; again, alternated signals were significantly faster than repeated signals. Thus it seems to be the case that the presence or absence of a repetition effect at longer intervals, is critically dependent upon the nature of the task involved and the SR relationships concerned.

The data available for assessing the effects of increasing or decreasing the RSI at longer intervals is no less inconclusive. Smith (op.cit.) reports a

decrease in the magnitude of the stimulus repetition effect with an increase in RSI from 2 to 10 seconds. Keele (1969) and Schanaveldt & Chase (1969) both report no significant effects of ITI on RE for either compatible or incompatible tasks. Smith attributes the decline in RE at longer intervals to "increased fading (which) necessitates going through the entire search more and more frequently".

The data from both the short and long interval studies is, unfortunately, relatively inconclusive and unsatisfactory. This is largely because the great variety of tasks and variables involved makes any strict comparisons at best tentative. It may be that the differences between seemingly similar situations can arise from differences in subjects matching strategies, but at present there is no direct evidence to suggest that this is ever so.

This extended review of sequential effects has been able to establish little concrete foundation for future discussion. It is clear that sequential effects occur, but their determinants are obscure. This may be partly because they are contributed to by several independent factors, which as yet have not been systematically isolated and studied. Practice, SR compatibility,

response-stimulus interval and task have all been shown to be significant factors in determining sequential performance. This review has been necessary because the experiments reported in Chapters 3, 4 and 5 all employ sequential tasks; the question of models for sequential behaviour will be taken up again in Chapter 3.

SUMMARY AND CONCLUSIONS

In this chapter, data and models relating to the human information processes involved in the recognition and discrimination of stimuli have been selectively reviewed. It has been suggested that any attempt to distinguish two separate processes - one of "recognition" and the other of "discrimination" - is bound to fail, if the distinction is forced on the relatively arbitrary basis of the differences between tasks which happen to bear different arbitrary classifications. These traditional distinctions are both empirically and conceptually invalid, principally because their exponents usually ignore two important facts:

(i) "Recognition" and "Discrimination" are, in psychological literature, ambiguous: they refer both to supposedly distinct hypothetical processes, and also to the results themselves of allegedly distinct operations. They are thus at once, both "achievement" words, and "process" words. (ii) There is no reason why, even if processes and achievements are kept distinct, each distinguishable achievement should require its own functionally separable process. Talk of "recognition processes" and "discrimination processes", as though they were entirely different, has led thought away from the essential point, namely, that what are of heuristic importance are the functional processes underlying performance, and not the dissection of nominal differences between tasks from which the data are collected.

The classical account of discrimination and recognition given by information theory was criticised severely, and found lack on both empirical and conceptual considerations. The principal reasons for distrusting it were that it provided only a limited account of performance and moreover that it lacked functional validity.

Correlations between stimulus variables and response variables are no guide, by themselves, to the underlying processes involved in any particular task, and must be supplemented with some knowledge of the units in which information is processed. That these may vary from task to task, or from stimulus to stimulus cannot be incorporated in a theory which simply equates the number of "stimuli" with the number of "possible events". For these and other reasons, functional models are to be preferred, and must be the goal of worthwhile research.

Two distinct problems were related to the modelling of stimulus discrimination: (i) what are the units of comparison? and (ii) how are these units combined? The terminology usually employed in attempting to answer (ii) - viz. that of serial and parallel processes - was discussed with the aim of evaluating its usefulness as a conceptual tool. It seemed to fall short of what is required on two grounds: (i) not enough is known about the units of processing to make the distinctions between the various available conceptual alternatives wholly workable; (ii) since very simple versions of the dichotomy

are implausible, and the very complicated versions frequently empirically indistinguishable, some intermediate form of processing - e.g. a hybrid process, an interactive process or an hierarchical process - is not necessarily excluded. Until we can say with some certainty what constitutes a stimulus in a given situation, it is meaningless to ask whether stimuli-dimensions-features etc. are processed in series or in parallel. Again, what is critical is the functional and not the nominal nature of the stimulus.

A review of the data and models from a subset of experimental paradigms used to investigate information processing illustrated the flexibility which is required in any adequate models. The data from high-speed memory scanning, visual search and binary classification tasks served to make several points clear:

(i) no single model is appropriate to any of these tasks;

(ii) (i) is true largely because performance in any given task is liable to be determined by several critical factors, which may interact in complex ways, and which at least are rarely independent;

(iii) even under constant conditions of stimulation, performance is frequently sensitive to the context in which a stimulus occurs, as well as to its intrinsic nature. "Context" may take many forms, but it cannot be ignored in accounting for much of the empirical data reviewed.

(iv) Modelling stimulus discrimination in terms of serial or parallel processing is generally a fruitless and indeed futile exercise. This was particularly evident in the binary classification data, where functionally separate processes for "same" and "different" decisions seemed to be demanded.

The final section of the review consisted of a discussion of the data derived from serial choice RT tasks. There are necessary reasons for regarding seriality as an integral part of information processing. Unfortunately, the relatively neat picture provided by the "discrete" RT data is considerably complicated when seriality is added to the task. However, in common with these discrete tasks, the serial data was shown to be sensitive to several factors, and therefore no single model could be extracted to account for all of the variance. A model which postulated an analogy between

the "same"-"different" processes of binary classification and the "repetition" effects of serial tasks seemed to be potentially fruitful, although as it is at present presented it is logically inadequate.

The general aim of this chapter has been to establish two essential propositions:

1. That in discussing any human performance task the stimulus must be specified in functional terms. It is misleading and inadequate to equate the functional stimulus and the nominal stimulus. The critical corollary of this is that it is functional, rather than simply descriptive, models which are to be preferred for describing information processes.

2. That, in any given task, what counts as the "stimulus" depends as much upon the context as upon the individual nature of the material presented. The time taken to make a classification and response cannot be determined from knowledge of the nominal stimulus alone; even knowing the functional stimulus would frequently be insufficient, if considerations of stimulus context were not taken into account.

It is the aim of the chapters which follow to illustrate and emphasise these two conclusions.

Chapter 1 CONTEXTUAL EFFECTS IN DISCRETE RT TASKS

This chapter contains a report of three experiments which were made to demonstrate and study the effects of stimulus discriminability variation upon choice RT in a simple non-sequential binary task. It is hoped to show that RT to a given stimulus may be determined as much by the context in which stimuli occur as by their intrinsic nature, and so correlatively that models which construe each stimulus-response sequence as a discrete "one-off" process whose duration is independent of the context may, in certain cases, be inadequate.

In the present series of experiments, two manipulations are used to vary stimulus discrimination in a binary classification task: (i) the addition of noise, and (ii) variation in the degree of stimulus similarity. It is not intended that comparisons should be made between the results obtained with the two methods, but rather that the more general point, namely, that such systematic variation may produce interesting and significant changes in performance, be convincingly demonstrated. It is further hoped that these changes will provide useful information about the basis of simple binary classification itself.

Experiment 1. THE EFFECT OF NOISE ON BINARY CLASSIFICATION.

There are three principal ways in which stimuli may be degraded or interfered with, by the addition of noise: (i) by interpolating irrelevant stimulus material, which may or may not require a response, but which may compete with the relevant aspects of the stimulus for access to a limited capacity channel; (ii) by masking, in which a fore- or after-coming random stimulus pattern is introduced. Under optimal conditions a signal stimulus may be phenomenally obliterated, otherwise, thresholds for recognition and identification are generally raised; (iii) by superimposition of one pattern upon another, in which both stimulus and "noise" are presented simultaneously. This form of stimulus degradation is to be distinguished from backward and forward masking (type ii) - it has empirical features which clearly define it as a separate phenomenon (Kinsbourne & Warrington, 1962).

The experiments of Posner and his colleagues, which were reviewed in the introduction, have exploited the manipulation of context by the addition of visual noise, in investigation of the properties of "same"-"different"

classification, using noise of types (i) and (ii) above.

(i) Interposing visual material between two successive single stimuli which the subject was required to match (the interpolated material being digits which the subject was instructed to classify in a specified way), abolished the difference between physical and name level matches (Posner et al.,1969). Boies, however (1969), reports that a single auditory digit interpolated in the short (0.5s) delay interval between visual stimuli, failed to alter the pattern of the relationships between physical and nominal matches, although a small effect was recorded on the first day of practice analysed in the direction of a reduction in the difference between physical and name matches (44 msecs. to 20 msecs.).*

(ii) Studies by Posner and his colleagues (cited Posner, 1969) in which the interval between stimuli to be matched was filled with a random black and white noise pattern, suggest that, beyond mediating an all-round increase in RT,

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* An interesting corollary finding of Boies' work is that trace generation appears to be disrupted by presentation of a list of items to be remembered before the presentation of the first stimulus.

such interpolated material had no effect on the relationship between physical and nominal matches. Posner (1969) concludes that "visual noise alone does not affect the visual code" (p.76), although the task of classifying or attending to interpolated visual digits may capture the attention mechanism to a sufficient extent to disrupt the visual encoding of other stimuli, and therefore affect the efficiency of the visual match. (iii) The third method of adding noise has been elegantly exploited by Sternberg, in the experiments reviewed above (p.31 et seq.). His results suggest that degradation of a probe stimulus by the superimposition of noise appears to significantly increase the "smoothing" time required to preprocess a stimulus, but that it has only a slight, transient effect on the slope of the function relating RT to target set size disappearing after the first day of practice. This clearly indicates that this form of noisy interference operates on the early stages of encoding.

Posner did not attempt to evaluate the effects of using the type iii noise, superimposed upon the stimuli which the subject is required to compare. This is a

curious omission, since his account of the "same"-"different" matching process places heavy emphasis on the possibility of "same" matches being carried out at pre-perceptual level, perhaps as a direct comparison between the present stimulus and the trace of that which immediately preceded it, and it is at this level that noise can most plausibly be supposed to be smoothed out. The question therefore arises: how would the "same" comparison be affected if one or other of the stimuli were to be noisy? If the "same" match and the noise "smoothing" process both occur at the pre-perceptual level of processing, then the requirement to smooth might eliminate the relative efficiency of the "same" match. Experiment 1 was therefore made to test this possibility, and the following:

(1) To examine the effect of superimposed visual noise on a two-choice visual comparison, by allowing noise to appear on the first or the second of two successive single stimuli, on neither, or on both. This, essentially, represents a marriage of Sternberg's and Posner's techniques, being a limiting case of the Sternberg paradigm, with a target set size of one, and a single

probe item. In a sense, however, it also goes beyond Sternberg's paradigm, since the effects of noise on the target set were never studied by him.

(ii) To examine the effects of inter-stimulus interval variation (ISI) on same-different matches, a manipulation which Sternberg failed to make for his paradigm. It has been contended that an exhaustive search only occurs at long inter-stimulus intervals (Baddeley & Ecob,1970; Corballis,1967), whereas at short ISIs the comparison process is self-terminating (Sternberg,1967). Posner and his colleagues report consistent effects of ISI upon the superiority of visual "same" matches over name "same" matches - the advantage of visual identity being abolished at ISIs in excess of 1500-2000 msecs.. Three ISIs, covering a wide range, were used in this experiment.

(iii) To determine whether the effects of noise on visual comparisons can be adequately modelled as variation in overall stimulus discriminability. For this reason noise was allowed to occur on a partial or total basis, so that the effects of constant discriminability could be assessed with changes in the position of the noise.

Subjects: 12 subjects, of both sexes, aged 18-25, completed the experiment. They were each run over three hour-long sessions, on consecutive days, and were paid for their services.

Apparatus: All stimuli were viewed in and presented under the control of a Withers 3-field tachistoscope. Viewing was at a distance of 18", and at an average field luminance adjusted for normal acuity. The tachistoscope was connected to two digital timers which were triggered by the tachistoscope timer controlling the onset of the second of the two sequential stimuli. These timers recorded RT to an accuracy of ± 2 msecs.. Also connected to the auxiliary timers were two response keys, mounted on a 6" x 4" x 1" metal box. Each button was connected to one of the two timers, so that depression of one key stopped one timer, and the other key the other timer. This allowed measurement of error-correction responses. The whole apparatus was located in a small room, lit only by low ambient light.

Materials: Stimuli consisted of pairs of upper-case letters presented sequentially to the subject, the first letter appearing on the left and the second on the right of a centrally placed fixation cross. The letters were photographed from original black-on-white stencils, and reproduced on semi-matt paper in the form of a 6" x 4" card. The letters were ½" high, and each subtended an angle of 1° 36' at the fovea. The angular separation between the letters was 2° 24'.

Eight letters were chosen (ADHQLMYZ), and paired into three lists of 104 letter-pairs each. These lists were compiled in such a way that of each of the 104 pairs, half were physically identical and half physically different. The "different" pairs were chosen so as to exclude obvious visual confusions (e.g. HM) and two letter words.

Since it was part of the purpose of the experiment to examine the effect of noise on stimulus matching, either or both of the members of each pair could appear in noise. The noise used was taken from a photographic negative made from a computer-generated 50:50 black:white checkerboard pattern used in studies of visual masking by

Kinsbourne & Warrington (1962). In order to maintain uniformity, especially of the location of the noise in the stimulus field, since it did not necessarily appear with both stimuli of a pair, all 8 letters were photographed in noise - that is to say, with a 1½ cm. square of noise superimposed upon them. Each letter was therefore photographed 4 times; once in clear and once in noise - both on the left and on the right of the fixation cross. Care was taken to ensure that the noise patterning was not identical for any two stimuli - so that identical letters in noise were not in fact strictly identical.

The stimulus lists were arranged so as to contain equal numbers of pairs, having either, neither or both letters in noise: the categories of stimulus were therefore: (i) "Clear"-"Clear" - both letters noise-free. (ii) "Clear"-"Noise" - first letter, on the left of the fixation spot, in clear, the second, on the right, in noise. (iii) "Noise"-"Clear" - the first letter in noise, second in clear, and (iv) "Noise"-"Noise" - both letters in noise. These categories applied to both identical and different letter-pairs, and stimuli were controlled in such a way that the same category did not

occur more than twice in immediate succession. Furthermore, each response category (i.e. "same"-"different") contained equal numbers of each of the four stimulus categories. No response was repeated more than three times in direct succession. The subject was not aware of this restriction, so successful anticipation was unlikely. Thus, for each response-type there were 13 stimuli in each of the 4 stimulus categories (i-iv); this is not as many as would have been ideally desirable, but it was felt to be the maximum number of responses that could comfortably be made in a single hour session, allowing for 20 practice trials and two rest periods.

Design: Three interstimulus intervals were selected - 80 msecs., 600 msecs. and 1500 msecs.. Each session consisted, for each subject, of 104 blocked responses at one of these ISIs. The order in which the ISIs occurred over the three sessions was counterbalanced across subjects, so that two subjects performed in each of the 6 possible orders.

Procedure: Each subject was demonstrated in the working of the apparatus and instructed to maximise speed and minimise errors. He was told of the nature of the

stimuli and given 20 practice trials at the ISI appropriate to the particular session in hand. Correct RTs were fed back to the subject between trials. Errors, which were replaced at the end of the session, were signalled to the subject by a buzzer on the response box. The presentation sequence on each trial was as follows: a verbal "ready ... 1 ... 2 ..." signal, preceding the triggering of the tachistoscope by E; Stimulus 1 appears for 200 msecs.; ISI (being filled by the plain white card containing the central fixation cross); stimulus 2 (simultaneous triggering of the two auxilliary timers); subject's response which stops timers.

The subjects were given a short break between practice and test trials, and as long a pause as time permitted between items 50 and 51 of the latter.

Results:

(i) Errors: unfortunately for analysis purposes, the absolute incidence of errors was sufficiently low to make any statistical comparison meaningless, and therefore none was attempted.

(ii) Correct Responses: cofrect responses were sorted into four categories, corresponding to each of the stimulus categories detailed above, there being one set of categories for each possible response. The mean correct RTs, averaged arithmetically across subjects, are given in Table 1. A four-way AnoVa was carried out on this data (subjects x response x stimulus category x interstimulus interval) - and the results are tabulated in Table 1. Since neither the subject term nor any of its interactions was significant, these are for convenience omitted from the table. These results show:

(a) A significant effect of response("same"-"different") ($p < .01$) and of stimulus category ($CC < NC < NN < CN$), ($p < .01$) although

(b) No overall effect of inter-stimulus interval. There is further

(c) No interaction between interval and stimulus category, but

(d) A highly significant interaction between response and stimulus category ($p < .01$).

Further to this analysis, a Neuman-Keuls analysis was performed upon each set of data, for each response type, in order to determine significant rank-order differences. A summary of these findings is given in Table 3. Several interesting points emerge:

(a) It is very clear that, with the single exception of the 1500 msec. interval, "different" responses - RTs to stimulus pairs of which the second letter is noisy - are significantly longer than the same response to a pair whose second letter is presented in clear; this appears to hold irrespectively of the clarity or noisiness of the first letter. Although the data for the excepted group fail to reach an acceptable level of significance, the trend is strikingly similar - average RTs for CC, NC, CN and NN categories being 454.5, 459.0, 524.6 and 523.3 respectively. This lack of significance might well reflect the effects of temporal uncertainty, which may occur at inter-stimulus intervals of this length, tending to increase both the absolute level of RTs and the overall variance. These data are therefore only marginally exceptional. The remaining results are unequivocal: noise in stimulus 2 significantly retards matching.

RSI	Stimulus Category			
	Clear-Clear	Clear-Noise	Noise-Clear	Noise-Noise
80 msecs.	355.4	509.6	373.0	430.7
600 msecs.	350.7	495.4	364.6	436.2
1500 msecs.	382.8	539.3	382.7	490.0

Mean 'Same' RTs - Experiment 1

RSI	Stimulus Category			
	Clear-Clear	Clear-Noise	Noise-Clear	Noise-Noise
80 msecs.	411.7	497.6	428.5	492.0
600 msecs.	438.8	488.0	440.2	493.4
1500 msecs.	454.5	524.6	459.0	523.3

Mean 'Different' RTs - Experiment 1

Table 1: Mean RTs x Response x Category - Experiment 1.

Source	Sum of Squares	D.F.	Mean Square	F/Ratio
Response (S/D)	148151.40	1	14815.40	25.02**
S-S Interval (RSI)	64976.56	2	32488.28	1.36
Stim. Category (CAT)	616801.50	3	205600.50	176.13**
S/D x RSI	2414.81	2	1207.41	0.83
S/D x CAT	82313.75	3	27437.91	20.02**
RSI x CAT	8011.25	6	1335.21	1.93
S/D x RSI x CAT	5132.69	6	855.45	1.89

Table 2: Analysis of Variance for Experiment 1.

** $p < .01$

(f) With the exception of the 1500 msec. "different" responses, the "clear-noise" and "noise-noise" stimulus categories are always significantly different - at least at the $p \leq .05$ level - from both the "clear-clear" and the "noise-clear" categories. For the "same" responses at both 80 and 600 msecs. ISI, they are also significantly different from each other ($p < .01$); otherwise they are not.

(g) The "clear-clear" and "noise-clear" stimulus categories never differ significantly. The former is always faster (except cf. (h) below) but never significantly so.

(h) The rank-order of categories remains unaffected by either interval or response category - viz. CC NC NN CN. The only marginal exception to this are the "same" responses at 1500 msecs. ISI, where the relative positions of NC and CC are reversed. However, they do not differ significantly.

SAME

	80 msec.			600 msec.			1500msec.		
	N-C	N-N	C-N	N-C	N-N	C-N	N-C	N-N	C-N
C-C	N.S.	0.01	0.01	C-C	N.S.	0.01	0.01	N-C	N.S.	0.05	0.01
N-C		0.05	0.01	N-C		0.01	0.01	C-C		0.05	0.01
N-N			0.01	N-N			0.01	N-N			N-S
C-N				C-N				C-N			

DIFF.

C-C	N.S.	0.01	0.01	C-C	N.S.	0.05	0.05	C-C	N.S.	N.S.	N.S.
N-C		0.01	0.01	N-C		0.05	0.05	N-C		N.S.	N.S.
N-N			N.S.	C-N			N.S.	N-N			N.S.
C-N				N-N				C-N			

Chapter 1, Table 3: Rank-order differences between stimulus categories by Newman-Keuls Analysis - Experiment 1.

In addition to the Neuman-Keuls analysis, T-Tests were performed on the means of "same" and "different" RTs, for each stimulus category, at each ISI, to test for the significant differences between these responses, which might be expected on the basis of Posner's experiments. In every case, this difference appeared ($p < .01$) with, however, one consistent and glaring exception, namely, the "clear"-"noise" category. For this, at no ISI was the difference between "same" and "different" RTs significant. Moreover, whereas for the remaining means "same" had always proved to be faster than "different", the trend was actually reversed for the "clear"-"noise" comparisons, where "same" was always, though non-significantly, slower than "different". This anomaly might account for the interaction between stimulus category and response type mentioned earlier; it is therefore worthwhile trying to specify its nature more precisely. The reversal of the relative efficiencies of "same" and "different" responses could be the result of either an increased "same" RT or of a decreased "different" RT, or of a combination of these. The following

observations seem relevant in the present case:
(1) there is no significant difference at any ISI between the RTs in the "noise"-"noise" and the "clear"-"noise" categories for the response "different"; however (2) for the "same" response, the picture is altered, showing a highly significant difference between "clear"-"noise" and "noise"-"noise" for both the 60 and 600 msecs. ISI conditions. Also, (3) there is no significant difference forthe 1500 msec. condition, although there is a marked trend in the same direction. Thus it appears that the tendency to slower "same" responses for the "clear"-"noise" category is mediated by an increase in the "same" RTs, rather than by a decrease in the "different" RTs.

Discussion:

The main points of issue in these results fall under two distinct headings: (i) the differences between different stimulus categories requiring a common response, and (ii) the differences between similar stimulus categories across responses. Since the results from the 1500 msec. ISI condition differ in several respects from those of the two shorter intervals, which are qualitatively indistinguishable from each other, that condition will be dealt with separately.

(i) Different stimulus categories requiring a common response.

The rank-order of stimulus categories RTs is constant for both responses at both of the shorter ISIs - viz. CC < NC < NN < CN. From this, and the significance tables (Table 3), it is clear that the presence of noise in the second of the two stimuli the subject compares, significantly delays response, whereas its presence on the first stimulus produces no measurable effect. Do these data give any insights into the mechanisms underlying the system's ability to deal with noise?

Noise can be handled in two principal ways: (i) it can be simply regarded as productive of stimulus degradation - the "degraded" stimulus being compared with the other stimulus as if it had been presented in clear. (b) The noise can be "smoothed" prior to comparisons being executed. If this latter process occurs, it is possible to envisage two further distinct alternatives: (b_1) the noise is completely smoothed - e.g. to the extent, if the stimulus is a familiar or readily identifiable one, of the generation of a "clear" replica which can then be matched - before any comparison

is started; (b_{11}) smoothing and comparison process are not sequential, but occur in parallel, perhaps with temporal overlapping and sequential onsets.

Model (a) is clearly not applicable to the present data: if noise is simply modelled as functionally equivalent to stimulus degradation, resulting in reduced discriminability the equally discriminable stimulus pairs should yield equal, or at least non-significantly-different, RTs. The rank-order of stimulus categories, within both responses, is not explicable by reference only to considerations of discrimination difficulty. (i) "Clear-noise" and "noise-clear" categories are equally discriminable, but at both 80 and 600 msecs. they produce highly significantly different RTs for both responses. (ii) "Noise-clear" comparisons are not significantly different from "clear-clear" comparisons, a difference which would be predicted from coordinates of stimulus discriminability. (iii) "Noise-noise" comparisons should be more difficult than "clear-noise" comparisons but they are actually significantly faster for"same" decisions ($p < .01$) and non-significantly different for "different" decisions.

This order-effect must therefore reflect some more complex form of stimulus processing. Were it the case that stimuli were being matched "raw", that is to say, without any preliminary smoothing to reduce or eliminate the effects of noise, then it would be expected that longer ISIs would produce less efficient visual matches (Posner & Keele, 1967); certainly, some difference between 80 and 600 msecs. would be predicted, Added to this consideration it would be reasonable to suppose that the requirement to store a stimulus before matching it would disadvantageously affect noisy stimuli if they were not already smoothed. This logic would lead to the prediction of a difference between "clear-clear" and "noise-clear" stimuli which was not found for either response type. This suggests that an 80 msec. lag may be sufficient for smoothing to occur - a speculation which is supported by Sternberg's own (1966) data - degradation increasing the intercept by between 63.7 and 67.1 msecs. It therefore seems that these variations in RT cannot be adequately or validly modelled as being the results of variations in stimulus discriminability. Some form of "smoothing" operation is indicated. Sternberg's data - reviewed in the

introduction - suggest that smoothing should be modelled as a pre-comparison process which adds a non-interactive constant to RT. The present data produce only limited support for such a model. The marked lack of difference between CN and NN RTs for the "different" responses also is strong evidence against such a model. This argument is consolidated by the significantly faster NN RTs - compared with the CN category - for "same" responses at both the 80 and 600 msec. level. This reverses the prediction from a Sternberg-type smoothing model. Although not significantly different, NN RTs for "different" are faster than CN RTs at every ISI.

If the strict Sternberg-type model, (b_1), is excluded, then if smoothing is to occur it must do so in parallel with the actual stimulus comparison. This need not imply that both operations start and finish simultaneously, but rather that they are not strictly serial; it is most likely that comparison of stimulus 1 with stimulus 2 is initiated before smoothing of either is complete. That smoothing of stimulus 1 occurs is evident from the failure to find a difference in RT between NC and CC categories; if discriminability considerations do not

produce an elevation in RT, then clearly the noise is being processed out before the comparison is made. That smoothing of stimulus 2 occurs is not so obviously evident from the data; the longer RT to CN stimuli compared with CC stimuli does indicate the possibility of S2 smoothing, but the relatively faster NN RTs over the CN RTs appears paradoxical in view of the expectation that stimuli which contained greater amounts of noise should take longer to smooth. At best one would predict no significant difference between CN and NN; one would certainly not predict that the latter would be significantly faster than the former. Although this is paradoxical, it does not logically exclude the operation of a smoothing process on S2, and, on balance, the data support smoothing for both stimuli. However, there is no evidence to suggest that the comparison process is delayed until smoothing is complete.

The existence of a smoothing operation however does not explain why "noise-noise" comparisons should be significantly faster than "clear-noise" comparisons for "same" responses ($p < .01$), and, moreover, why this difference should be abolished for the "different"

responses. An immediate possibility is that the pairs of noisy stimuli were sufficiently alike to make smoothing unnecessary. Although great care was taken to ensure that the noise superimposed on each stimulus letter was such as to make it strictly physically dissimilar from every other letter with which it might be paired, this does not of course exclude the possibility of two noisy letters appearing to be identical, although it is most unlikely that the individual components of noise are processed individually; so even if this extra precaution had been taken, a different result seems remote. A possible additional hypothesis is the "noise-noise" condition is that in spite of the precaution of making noise-fields non-identical for "same" match pairs physical identity matching 8 including noise - is easier than comparisons in the "clear-noise" condition. The disparity between "same" and "different" RT effects fits in well with this supposition.

If the presence of an NN - CN difference is difficult to explain, its abolition for "different" responses is even more puzzling. The lack of any difference between these two stimulus categories constitutes the main

difference between "same" and "different" RT patterns; given the veracity of the hypothesis of some form of pre-processing operation for each stimulus, it is difficult to escape the prediction of identical rank-orders of RTs for both responses. This gross difference therefore suggests that any explanation of "same" and "different" RTs in the same qualitative terms is implausible.

There is a line of speculation from simple binary classification experiments which might prove fruitful in discussing this apparent anomaly: namely, that "same" and "different" responses are subserved by qualitatively different processes with empirically distinguishable operating characteristics. The evidence for this, and a review of current models, has been given in the introduction. There are two versions of the theory which illustrate this line of thought: (i) that "same" decisions may be taken from the results of matches made at a different - more peripheral - level of analysis from "different" decisions; (ii) that the "same" process is, of itself, qualitatively distinct from the "different" process. These versions are not mutually exclusive and indeed may be combined into a single model which postulates

two different comparison processes operating at hierarchically distinct levels of the perceptual process. Corresponding to (i) is Neisser's concept of a "pre-attentive" level of processing, at which gross stimulus characteristics are processed in order to pre-select information which is to be passed on for more detailed analysis. (ii) is supported by binary classification experiments - most recently Egeth & Blecker (1971) - which provide strong empirical grounds for supposing that "same" and "different" decisions are made by distinct processes, which are not qualitatively identical.

To a certain extent, the "level" at which a given comparison is made is irrelevant to determining its nature. In the present case, there is no reason to suppose that it either is possible, or indeed would be possible, for one type of binary comparison to be made at one level, without processing stimuli to a sufficient extent to make a reliable binary judgement. The real and interesting question concerns the possibility of there being distinct processes for each type of decision.

If Bamber (1969) is correct in supposing that "identity" comparisons are initially made by a rapid "identity reporter" which signals identity but not difference, then one could suggest a plausible account of the difference between "same" and "different" patterns: the identity reporting device incorporates a fixed criterion for firing - any comparison not reaching this criterion switching in the relatively slow, feature-operated, "difference" process; stimuli which differ only marginally will be unlikely to be discriminated at the "same" level, and an identity response will be initiated. Stimuli which differ strongly, for example for a noisy and a non-noisy pairing, will always fail to trigger an identity response - even if the actual stimuli are in fact identical - unless of course a smoothing process has already generated a noise-free replica for one or both stimuli. However, where both stimuli are not immediately distinct - e.g. if both appear in similar, though not necessarily strictly identical noise, it might be possible to "fool" the identity reporter into producing an identity response which may, in fact, turn out to be correct. For stimuli which are identical an identity response will be initiated;

for non-identical stimuli, provided the initial comparison fails to reach the identity criterion for the reporting device, stimuli will be passed down for further processing.

This model is far from lacking deficiencies; however it does provide a starting point for further investigation and on a limited scale account for the qualitative differences between the two response category RT patterns.

(ii) Identical stimulus categories requiring different responses: the results of "same"-"different" comparisons within a common stimulus category are clear: "different" responses are always slower than "same" responses with one outstanding exception - the "clear"-"noise" category. For this, "same" and "different" RTs were never significantly different; indeed, "same" responses showed a tendency to be slower than "different" responses. Reasons have been given above for supposing that this curious effect is mediated by an increase in "same" RTs, rather than by a decrease in the "different" RTs. This might be explained, ad hoc, by supposing that the relative speed of "same" matches depended upon the matched stimuli being absolutely, strictly, identical. If smoothing

results in the generation of an upper-case replica of the original letter, which is then matched with the second stimulus, physical identity will only occur in two conditions - CC and NC. Thus, on the CN and NN trials the "same" matches will be slowed. It is tempting to speculate that if, as per Egeth & Blecker (1971), "same" processes are essentially parallel, wholistic comparisons, whereas "different" comparisons are "relentlessly" (= exhaustively?) featuristic, absence of strict physical identity would drive the system to a "different"-type comparison, which would delay the response and therefore increase the RT. This would abolish the significant difference between "same" and "different" decisions in the CN condition, but might also be expected to abolish it for the NN condition, unless, as a further speculation and a possible criticism of the present experiment, the identity of the type of noise employed for the two stimuli on the NN condition was sufficient to allow "same" type comparisons to be made on these occasions, if the noisy stimulus was still available to the subject when stimulus 2 appeared. However if such comparisons are

possible then either the NN condition should be faster than CN and NC conditions, or else earlier statements about constant smoothing times become untenable.

(iii) The effect of interstimulus-interval: part of the purpose of the experiment was to examine the effect of different ISIs upon RT. Posner has shown this to be an important variable in determining the quality of the match which is made (Posner & Keele, 1967), although Sternberg did not include different ISIs in his experimental design. In the present experiment, the results indicate that ISI is not a significant variable. The AnoVa table shows no effect of the ISI factor, and the pattern of results for the two shorter ISIs is identical in every respect of quality. Although Posner & Keele report that the relative superiority of visual matches is abolished in the ISI range 1500-2000 msecs., in a design in which the interval is filled with visual noise, there is no evidence from these present data to suggest a change in the level at which stimuli were being matched at the longer ISI. The results from the 1500 msec. condition can be explained in terms of purely quantitative differences from the 80 and 600 msec. conditions. Both the "same" and "different"

rank-orders for the four stimulus categories are essentially maintained, although no significant differences were recorded for the "different" stimulus categories. The difference between NC and CN "same" categories remained at a high level of confidence ($p. < 01$), and the significant "same"-"different" difference is also maintained for every category with the continued exception of the "clear"-"noise" comparison. The differences in rank-order which do appear, and the failure of the different comparisons to reach significance, are most plausibly explained as the effects of increased temporal uncertainty, which is known to adversely affect RTs - especially their variance - at long intervals, even when the length of the interval is known and constant. There is therefore no reason to suppose that subjects reverted to making "name" matches at this longer interval. It might be possible to disentangle the relative effects of ISI and matching strategy further, if both upper and lower-case letters had been used, but this would have added considerable complication, and was not the primary purpose of the experiment.

Experiment 1 has demonstrated the effect of superimposed visual noise on one or both stimuli of a pair which a subject is required to compare. The results make it clear that the effects of superimposing visual noise are strikingly dissimilar to those obtained with similar noise interpolated during the inter-stimulus interval (Posner & Keele,1967; Posner & al.,1969). It is also apparent that the results of adding visual noise to part or all of a display which requires matching cannot be properly modelled in terms of variations in stimulus discriminability. Directional effects make such an account inadequate. No important effects, beyond an all-round increase in RT, of long interstimulus interval are found, although the results do not preclude the possibility of name matching.

The data are consistent with a system which is capable of rapidly smoothing stimuli, and using a smoothed replica as a match for an after-coming stimulus. However, it is worth emphasising that it appears to be in the "clear-noise" and "noise-noise" conditions only that smoothing time may be added to RT (measured from the onset of S2). The variation in the relative facility of "same" and "different" judgments with stimulus category excluded

Tversky's "compare-and-check" model - since this demands an additivity between the two processes which was not found. The data are entirely consistent with, and indeed in the qualitative differences between "same" and "different" RT patterns actively support, a two-process model of binary classification.

Experiment 1 examined the effects of adding noise to either, neither or both of a pair of stimuli in a binary classification task. The clear order effects which were found implied that the addition of noise could not simply be regarded as functionally equivalent to reducing stimulus discriminability. A smoothing process had to be postulated to explain the data.

This result is capable of being extended in either of two possible ways: (i) to examine the effect of using qualitatively distinct forms of noise, so that two noisy stimuli are visually distinctly different - also to titrate the ISI between 600 and 1500 msecs. (ii) to examine the effects of RT of varying stimulus discriminability by other means than the addition of noise.

A possible source of criticism of experiment 1 lies in the unknown extent to which the presence of more difficult matches in a list might influence relatively easier matches in the same list. For example, RTs for the "clear-clear" matches in experiment 1 may have been artifidally elevated by the matches involving noisy stimuli, thus reducing the differences between these comparisons and the remaining three stimulus categories. Indeed, it may have accounted for the failure of the "clear-clear" and "noise-clear" categories to differ significantly. Since this seemed an interesting speculation, option (ii) above was chosen.

The simple effect of reducing stimulus discriminability is the elevation of RTs; this is a purely <u>local</u> effect being confined to the individual single comparison. A different type of effect is that mentioned above, where the increased difficulty of a particular comparison may be reflected in other comparisons in the list, as well as in that single RT; in other words, a "global" as opposed to a "local" effect. There is good evidence to support the existence of such effects:

(i) Posner & Taylor (1969) report that heavy expectancy of physical identity matches, induced by probability biasses, benefitted physical matches compared with name identity matches at ISIs at which RTs to nominal and physical identity normally approached each other.

(ii) Posner (1970) reports that the time subjects take to make a "different" judgement to physically different stimuli is not wholly independent of the context in which the stimuli occur: specifically, the "different" response takes longer in a context of pure "name" level decisions, than in a context of mixed nominal and physical judgements. Conversely, however, Posner & Mitchell (1967) failed to find any effect on physical identity matches of eliminating the occurrence of name identity matches in a list. This might, however, be explained if the two "modes" of matching operated independently and not necessarily sequentially.

These effects are of interest in their own right, although they originated as by-products of mainstream binary classification work. They show that even in simple "same"-"different" decision tasks, contextual

relativity can be a significant factor in determining RT. However, they are not conclusive, since the changes which they represent always include an alteration in the level at which the subject is required to make the match. What is of particular interest is whether changes in difficulty, at a common level of matching, can produce significant alterations in RT to stimuli which are unaffected by the alteration. Experiments 2 and 3 were made to test for such "global" contextual effects, in a binary classification task.

EXPERIMENT 2. THE CONTEXTUAL EFFECTS OF DISCRIMINATION DIFFICULTY IN A BINARY CLASSIFICATION TASK.

This experiment was made to examine the effects of mixing binary classifications of different levels of difficulty in a single list. Comparisons are made with a control condition in which each of the two levels of difficulty are judged separately.

Posner's report of a global contextual effect in lists which mix physical and nominal identity judgements is unfortunately empirically weak, in that it is possible that the effects are attributable to the operation of

more than one compound strategy. It is therefore interesting to attempt to establish an effect of this kind, which is independent of this criticism; this is the principal aim of experiment 2.

Posner's finding that the same pair of stimuli may take longer to classify "same" or "different" in one context than in another is capable of two interpretations: (i) the increase in RT in the pure name list may reflect an average of pure name identity RTs and pure physical identity RTs. For certain stimuli, it is clearly possible for subjects to make a successful match in the mixed list on either a name match or a physical match basis, whereas, in the pure list, only name matches are possible. If each type of match occurs within a single list the resulting mean RT will fall between the two extreme values. (ii) A second explanation lies in the hypothesis that a subject faced with a set of single binary decisions will be forced to employ a criterion which he is most likely to locate in accordance with a hypothetical pay-off function, but which will be set in such a way as to reflect the difficulty ofthe decisions he is required to make. The higher the

proportion of relatively difficult decisions the greater the upward shift in criterion that will be required to maintain a respectable error rate. The prior explanation (i) is, in a sense, spurious, since it relies upon there being more than one possible basis for matching. Is it possible to demonstrate a criterion-shift effect in a context where there exists only one basis upon which stimuli may be classified?

Several authors have recently canvassed the possibility of qualitatively different mechanisms, with empirically separable properties, being involved in the processing of "same" and "different" decisions, and these have been discussed in detail in the introduction (p.55 et seq). The most common suggestion is that the "same" decision is taken on the result of a "wholistic", Egeth & Blecker, 1971, or "parallel" process, whereas the decision "different" is made on the basis of a serial, and possibly "featuristic", process. In most accounts the two processes have been taken as being functionally independent of each other, although one hypothesis (Nickerson, 1969) does make one decision dependent upon the failure of the analyser accumulating information for the other decision to reach its criterion

in a required temporal interval. If, as seems likely, some form of two-process theory is appropriate for modelling "same"-"different" decision-making, then it might be expected that criterion shifts could be demonstrated which would affect the two processes differentially. It is hoped that manipulation of the context will affect one match (here "different"), and not the other("same"), if the two decisions are indeed independent of each other. The manipulation that was selected was that of differential discriminability, since it is known to produce systematic variations in RT. Posner & Taylor (1969) presented subjects with a three letter array followed by a single probe letter which they were required to match to the middle letter of the array. The "correct same" RTs, to physically identical stimuli, increased when the array was visually similar to the target, compared with either acoustically confusible or neutral arrays. Chase & Posner (1965) found that subjects took longer to scan for a target in a multi-letter array when target and array were visually confusible. This is empirically akin to Neisser's (1964) finding that visual search

times were increased when the target was embedded in a visually similar background context. It is the aim of the present experiment to determine whether, eliminating such gross factors as background similarity, a clean effect of one decision on another can be found which is simply a product of the context in which it occurs.

Subjects: 8 male and 16 female subjects were employed for one session of 45 minutes. All were right-handed, and their ages ranged from 17 to 28.

Apparatus: Pairs of black-on-white upper-case letters were presented simultaneously in a two-field tachistoscope for a duration of 1100 msecs., and subjects required to decide whether the stimuli were "same" or "different", and signal the decision by speaking the appropriate word aloud. The onset of the stimulus coincided with the starting of a Decatron millisecond timer, which was stopped by an impulse from a throat microphone hung round the subject's neck, and which was triggered by throat vibrations from normal speech. This mechanism was adjusted afresh for each subject to a level of sensitivity at which it would respond to a

normally enunciated word, but not to an incidental head or neck movement. It was not always possible to ensure that the apparatus had not fired prematurely, or indeed had not responded to the final, not the initial, syllable uttered. However, where grossly unrealistically long or short RTs were recorded, the stimulus was rerun at the end of the list.

Material: The stimuli consisted of four lists of 24 letter-pairs each, derived from two independent stimulus populations: the visually confusible set (COQG) and the non-confusible set (NRTU). Each letter was $\frac{3}{4}$" high, the angular separation between the letters 1° 18', and the total two-letter display subtended 12°20' at the eye. From each of these sets was constructed one list of 24 letter-pairs - 12 physically identical pairs and 12 physical different pairs. The third and fourth lists consisted of 24 pairs each, derived from both populations of letters, without mixing. That is to say, the "different" pairs were not permitted to contain one component letter from each population.

The mixed lists were randomised for occurrence of letter population, and all lists were randomised for occurrence of response ("same"-"different"), no response occurring more than three times in succession. In addition a practice list was constructed, containing 12 digit pairs in the same stencil-face as the test stimuli. All the stimuli were black, stencilled with a UNO CC6 pen stencil, on to plain white 10" x 8" cards, one character appearing either side of the geometric centre of the card. A small centrally placed fixation cross, also on a white card, occupied the remaining tachistoscope field, and was present before each trial.

Some explanation of the selection of the two letter sets is necessary. Two confusibility matrices were used to provide some metric of relative confusibility: (i) Chase's (unpublished) visual matrix, and (ii) Conrad's (1964) acoustic matrix. The former is derived from a percentage measure of horizontal overlap of a standard typeface of upper-case letters; the latter is the result of an experiment in which listening errors to single letters spoken in noise were recorded. Both are full-alphabet matrices. The average visual confusibility of

the "different" letter pairs used in the confusible list was 874.25, andthat of the non-confusible pairs 103.75.* Both sets of "different" pairs were equated as far as possible, for acoustic confusibility - highly confusible pairs, e.g. "CV" being omitted.

Design: Subjects were run in the following sequence: practice list; Mixed List 1; Pure List A; Pure List B; Mixed List 2. Half the subjects received the confusible list first (i.e. at A), the other half the non-confusible list. The purpose of having two mixed lists was to generate equal numbers of observations in each stimulus category: confusible-same; confusible-different; non-confusible-same, and non-confusible-different. Splitting the mixed lists into 2 x 24 was done simply to avoid lists of unequal length.

Procedure: Subjects were instructed in the working of the apparatus, and the sensitivity of the throat microphone adjusted for each individually. Subjects were told to respond as rapidly as possible, consistent with a low error rate. Errors and stimuli which had produced abnormally

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* These numbers give a rough indication of relative confusibility - they are not intended as implying that a strict quantification of discriminability is possible.

high or low RTs were rerun at the end of each list. A short rest pause followed each list of 64 pairs.

Results: Mean RT for each type of response were computed for each list type (pure confusible, pure non-confusible and mixed). This required that the two mixed lists be collapsed to yield a suitable number of observations in each of the four categories - same/different vs. confusible/non-confusible. Before further analysis all data protocols with standard deviations of excess of 100 msecs. were rejected. This restriction was necessary in order to eliminate several excessively large variances. Before discarding these data a check was made to ensure that the objectionable variances did not derive from any list in particular; it was found that no correlation existed between source and magnitude of the variance, and it was therefore felt justifiable to "smooth" the data in this way. This process preceded any comparisons between stimulus classes. 8 sets of data were rejected on this criterion - leaving 4 male and 12 female protocols. Fortunately, the balance of the design was undisturbed by these exclusions and it was thus felt unnecessary to run replacement subjects.

The data were first subject to a 4-way AnoVa - subjects x context (pure-mixed) x list-type (confusible-non-confusible) x response ("same"-"different"); the table is reproduced below (Table 4). It can be seen that, while context has no effect, subjects, list-type and response are all highly significant ($p < .01$ in each case), and indeed, list-type and response interact significantly ($p < .05$). A significant interaction also appears between context and list-type ($p < .05$).

The data were further analysed in the following way: T-Tests were performed on the column means (i) within a context - i.e. pure and mixed lists, and (ii) between the contexts. These tests (cf. Table 5 for details) reveal that (i) for both pure and mixed lists, considered separately, "same" judgements were always faster than "different" judgements; this is so for both confusible and non-confusible data within the two contexts. Also (ii) considering pure and mixed lists apart, "confusible" list "same" responses were always significantly slower than "non-confusible" list "same" responses ($p < .05$) for pure lists, $p < .02$ for mixed lists. The differences between confusible "different" and non-confusible "different" within each context were not significant.

Source	Sum of Squares	d.f.	Mean Square	F/Ratio
Subjects	275313.70	15	18354.25	44.548 **
Context (Pure/Mixed)	118.19	1	118.95	0.2868
List type (C/NC)	7579.88	1	7579.88	18.397 **
Response S/D)	59125.01	1	59125.01	143.504 **
Subjects x context	7876.43	15	578.94	1.2745
Subjects x list-type	8684.12	15	579.94	1.405
Subjects x response	7223.35	15	481.56	1.189
Context x list-type	2601.01	1	2601.01	6.313 *
Context x response	634.58	1	634.58	1.540
List-type x response	2993.47	1	2993.47	7.266 *
Subjects x context x list-type	5361.86	15	357.46	0.868
Subjects x context x response	5245.05	15	349.67	0.849
Subjectx x list-type x response	1059.49	15	706.33	1.714
Context x list-type x response	13.13	1	13.13	0.032
Subjects x context x list x response	6180.13	15	412.01	
TOTAL	399544.80	127		

** $p < .01$
* $p < .05$

TABLE 4: Analysis of Variance - Experiment 2.

Before discussing these results, the data for a second group of 12 female subjects will be presented. This group was run for two reasons: (a) it was felt that the stimulus duration used for the initial group (1100 msecs.) might have been of sufficient length to have had an adverse effect on both absolute level and also on standard deviation of RTs. There is no objective evidence to support this contention, but as Bergstrom (1907) noted from his subjects "... the duration of the objective stimulus beyond a certain point is felt to be disagreeable" (p. 224). Thus it was hoped that, by using a shorter stimulus duration (500 msecs.) the standard deviations and also absolute levels of RT might be decreased. (ii) The second reason for a replication lay in the low level of practice at which the data for group 1 were obtained. More extended practice might alter the RT pattern. This second group of subjects was therefore run - for two separate sessions of 45 minutes each, on two series of four lists, similar in type and structure to those used before. The second group of lists was not a direct repeat of the first group, but rather a further sequence derived from the same letter populations. Lists were balanced in the same way as before and the experimental procedure identical to that outlined above.

Results - Group 2: Two of the 12 subjects tested failed to reach the criterion of standard deviation less than 100 msecs., and therefore, as before, their data were rejected, leaving 10 subjects with two complete runs for each. A 5-way AnoVa - to include a term for practice (1st or 2nd run) was made - the results are produced in Table 6.

Comparison of this table with that for the previous group shows no substantial points of difference. All the single factors maintain the same levels of significance, and additionally the practice term is significant at the $p < .01$ level. The interaction terms reveal nothing new except that the context - list type interaction here fails to reach significance. T-Tests were performed on the data from each run independently, in the same way as for group 1, with the following results (Table 7):

Run 1: Except for the CS^{P}/ CS^{M} difference ($p < .05$) there were no differences in the between list-type comparisons. In the within list-type comparisons, the only acceptable levels of significance were achieved by the confusible "same":non-confusible "same" term, for the pure lists

	Pure	Mixed
Within Contexts:		
CS/NS	.05	0.02
CD/ND	N.S.	N.S.
Between Contexts:		
CS^P/CS^M	0.05	Pure Slower
CD^P/CD^M	N.S.	Pure Slower
NS^P/NS^M	N.S.	Mixed Slower
ND^P/ND^M	N.S.	Mixed Slower

Table 5: T-Test results for data from Experiment 2.

Source	S.O.S.	D.F.	Mean Square	F/Ratio
A	73831.37	1	73831.37	139.32 **
B	681333.70	9	75703.75	142.85 **
C	35. 16	1	35.16	0.066
D	7439.36	1	7439.36	14 .04 **
E	143341.40	1	143341.40	270.48 **
AB	33210.75	9	3690.08	6.96 **
AC	536.46	1	536.46	1.01
AD	6113.11	1	6113.11	11.54 **
AE	795.94	1	795.94	1.50
BC	12362.34	9	1373.59	2.59
BD	9392.65	9	1043.63	1.96
BE	8942.56	9	993.62	1.87
CD	1076.37	1	1076.37	2.03
CE	0.68	1	0.68	0.0012
DE	2456.81	1	2456.81	4.63
ABC	12433.28	9	1381.48	2.61
ABD	6484.40	9	720.49	1.36
ABE	7258.06	9	806.45	1.52
ACD	345.19	1	345.19	0.65
ACE	0.47	1	0.47	0.0008
ADE	31.76	1	31.76	0.050
BCD	7572.88	9	841.43	1.59
BCE	7233.84	9	803.76	1.52
BDE	6095.98	9	677.33	1.28
CDE	187.07	1	187.07	0.35
ABCD	12142.10	9	1349.12	2.54
ABCE	6020.05		668.89	1.26
ABDE	2962.75	9	329.19	0.62
ACDE	5.24	1	5.24	0.0099
BCDE	1040.43	9	115.60	0.22
ABCDE	4769.50	9	529.94	
TOTAL	1055451.00	159		

** $p < .01$
* $p < .05$

A - Run 1/Run 2.
B - Subjects.
C - Pure-Mixed.
D - Confusible/Non-Confusible.
E - Same-Different.

TABLE 6: 5-way Analysis of Variance, Experiment 2.

	Pure		Mixed
Group 2 Run 1			
Within Contexts			
CS/NS	0.01		0.05
CD/ND	N.S.		N.S.
Between Contexts			
CS^P/CS^M	0.05	Pure	Slower
CD^P/CD^M	N.S.	Pure	Slower
NS^P/NS^M	N.S.	Mixed	Slower
ND^P/ND^M	N.S.	Mixed	Slower
Group 2 Run 2			
Within Contexts			
CS/NS	0.01		0.02
CS/ND	N.S.		N.S.
Between Contexts			
CS^P/CS^M	0.05	Pure	Slower
CD^P/CD^M	N.S.	Pure	Slower
NS^P/NS^M	N.S.	Mixed	Slower
ND^P/ND^M	N.S.	Mixed	Slower

Table 7: T-Test Results for data from Group 2 - Experiment 2.

($p<.01$) and for the mixed lists ($p<.05$), although the "confusible different":"non-confusible different" comparisons did show a non-significant trend in the expected direction for both pure and mixed lists.

Run 2: There were no between-condition significant differences, and the only significant within-lists differences were those relating confusible "same" and non-confusible "same" pure ($p<.01$) and the same term for the mixed list ($p<.02$).

In general, it is fair to summarise the between-context findings by the statement that for "confusible" stimulus pairings pure lists generate slower RTs than mixed lists, whereas for non-confusible pairings the reverse is true - the mixed lists are slower. The within-context findings show that confusible "same" RTs are slower than non-confusible "same" for both lists - there are no differences for either pure or mixed lists for the "different" terms.

Discussion:

There are two distinct questions which this experiment was made to answer: (i) does stimulus context exert any measurable effect on RT? and, (ii) if there is a contextual effect, how does it operate? The first question is largely an empirical one, the second primarily theoretical.

(i) The first question can be applied to the present experiment in two distinct ways: (a) one can ask whether the presence, in an pure list, of confusible "different" items raises RTs for either the "different" category or for the "same" response category in that list, compared with the corresponding RT in the non-confusible list. (b) one can also ask whether a mixture of the two different types of stimuli has any effect on either type of judgement; a stronger form of the hypothesis that there is such an effect takes the form of the suggestion that easily discriminable items in a set reduce RTs for less discriminable items - in other words a levelling process might operate in the mixed lists.

(a) It is quite clear, from the AnoVas, that there is a highly significant effect of the confusible:non-

confusible term throughout ($p < .01$), and further, that this effectis not equally great across list-type: the interaction between confusible-non-confusible and pure : mixed terms is significant (p .05 for both groups 1 and 2). The increase in RT for confusible material is greater in the pure list case; this is apparent from the failure of the confusible : non-confusible comparisons for the mixed lists to achieve an acceptable level of confidence, while, in the pure list comparisons, the confusible "same" : non-confusible "same" term is always significant, for both groups of subjects.

(b) The second aspect of (i) - viz. "is there a pure-mixed effect?" - can be answered by reference to two kinds of observation: (b_1) Comparison between corresponding response categories for each list type under each condition - e.g. confusible "same", pure list, vs. confusible "same", mixed list. This therefore represents a comparison between identical S-R pairs, in different contexts: (b_2) Secondly, it is possible to compare the significance levels achieved by across-category comparisons in the two list-types - e.g. confusible "same":non-confusible "same", pure list versus confusible "same":non-confusible "same", mixed list.

(b_1) The gross mean RTs for the pure and mixed lists clearly show a trend towards <u>decreased</u> confusible and <u>increased</u> non-confusible RTs in the mixed lists. However, while this trend occurs for both groups, and, for group 2, on both runs, none of the differences reaches significance - save that between confusible "same", pure lists and confusible "same", mixed list, for group 1 ($p<.05$). This trend is confirmed by the AnoVas, where the confusible:nom-confusible versus pure:mixed interaction term is significant for both groups of subjects ($p<.05$ in both cases). This seems to indicate a levelling function of mixing the two letter populations, which decreases the confusible list RTs and increases the non-confusible list RTs from their pure list levels. This has the corollary that within list comparisons should achieve less significance than between list comparisons, which, generally, is what was found.

(b_2) The significance levels found for the mixed list comparisons are universally lower than those for the corresponding pure list comparisons. While the confusible "same":non-confusible "same" term is still

significant for group 1 and group 2 (runs 1 and 2) in the mixed list, the levels reached ($p < .02$, $p < .05$ and $p < .02$ respectively) are markedly lower than those for the pure list terms ($p < .05$, $p < .01$ and $p < .01$ respectively). These differences excepted, the only remaining feature of interest in the mixed list is the confusible "different":non-confusible "different" comparison which, while failing to reach significance, indicates that the confusible "different" responses are faster than the non-confusible "different" responses in the same list. Thus, there appears to be a pure-mixed effect in these results - although not an overwhelmingly convincing one.

(ii) It is now possible to attempt an answer to the second question: how do the effects of context operate? In order to evaluate the data, it is necessary to determine whether the contextual manipulations affected both "same" and "different" decisions equally. For the pure lists: while the non-confusible "different" and confusible "different" responses were highly different from the corresponding "same" responses, the differences between confusible "different" and non-confusible "different" decisions were marked by unacceptably low levels

of significance for group 1, and group 2 run 1 ($p < .30$) and by non-significance for group 2 run 2, while the corresponding confusible "same":non-confusible "same" comparisons reached levels of $p < .01$, $p < .01$ and $p < .05$ respectively. For the mixed condition: the confusible "different":non-confusible "different" term failed to reach significance on any comparison, as did the confusible "same":non-confusible "same" terms. Thus the greatest effect of the contextual manipulations appears to be on the "same" response, the confusible RTs being significantly higher than the non-confusible RTs in the pure list, throughout, and, while still (marginally) significant, differ less in the mixed lists. Curiously, the "different" response seems little altered by the context. In view of the considerable differences in RT with changes in stimulus discriminability found in other binary classification experiments (Egeth, 1966; Sekuler & Abrams, 1968) for various classes of stimuli this result is rather surprising. It is open to question whether a high degree of visual overlap is an adequate means of manipulating stimulus discriminability; it can only be said that (a) Chase found the manipulation successful,

and (b) that even without this quasi-quantitative metric, the confusible pairings used were chosen especially for their manifest low discriminability - so the lack of any effect on the "different" RTs to support this manipulation is doubly puzzling. That there is some effect - viz. on the "same" RTs in the pure list - is apparent. It appears that, for these confusible stimuli, the presence of more difficult discriminations than those occurring in the non-confusible list, forces the subject towardsooverall greater caution, and that this disproportionately effects "same" responses. These results require some comment:

1. The fact that judgements of absolute physical identity can be significantly increased by the context in which they occur - that is, in this case, by the presence of difficult "different" discriminations in the same experimental list - is not easy to reconcile with the hypothesis of two functionally independent processes subserving the decisions "same" and "different". If these processes represented independent additive stages, then the expectation would be that the "different" RT would be equally affected by any factor which affected "same" RTs - but this was not found to be the case. Only "same" RTs were affected, and

the traditional difference between "same" and "different" preserved throughout. This result is also at variance with Posner's conception of the identity judgement as the simplest, most primitive form of perceptual operation. It seems that it is capable of highly significant variation - despite constancy of stimuli and responses. Its primitivity is therefore somewhat relative.

2. The experimental set-up allowed subjects the possibility - although this was not explicitly clear to them - of making a superordinate categorisation of stimuli into two classes - corresponding to the two letter sets - confusible and non-confusible. This was so, not by design, but as an artefact of the stimulus selection: all the "confusible" stimuli were pronouncedly curvilinear, whereas the non-confusible stimuli were reliably distinctive in the possession of at least one vertical straight line - a definite, and probably salient, "distinctive feature". This is a possible criticism of the experiment, although it cannot be adduced in explanation of the elevation of confusible "same" RTs, in the pure list.

3. The failure to find an increase in the non-confusible stimulus RTs in the mixed lists might however be mediated by this visual distinctiveness of the two letter sets. For example, if an identical stimulus pair, judged "same", also involved the extraction of information about the overall shape of the stimuli, then the signal indicating angularity, rather than curvilinearity might lead to a change of criterion and thus to a less lengthy comparison process. The present experiment makes it impossible to provide a direct test of this hypothesis.

The results of this experiment indicate two significant and different effects of context on RTs: firstly, although the two letter sets were deliberately not mixed to form new pairs, the effects of using pairs from each set in a common list was found. The confusible list RTs were decreased, and the non-confusible list RTs increased. This "levelling" process suggests that subjects were, in some way, able to alter their decision criteria to coincide with changes in the difficulty of the discriminations they were required to make - this effect may have been partially mediated by the use of

distinctive features which reliably differentiated the easy from the difficult comparisons. The second contextual effect is demonstrated by the finding of a significant difference between the two pure stimulus lists. The curious fact about this difference is that it is largely confined to "same" judgements, the difference between the two "different" RTs failing to reach significance. On previous evidence RTs for "different" judgements should be increased correlatively with decreasing stimulus discriminability; there is however no precedent to support the finding of an increase in "same" RT with a decrease in "different" discriminability. At present, this remains a puzzle - since it is consistent neither with previous data, nor with any two-process model of binary classification - which, on other grounds, appear to be indicated.

EXPERIMENT 3 FURTHER EFFECTS OF CONTEXT UPON BINARY CLASSIFICATION

The results of Experiment 2 demonstrate the existence of contextual effects of a "global" rather than a purely "local" kind. Experiment 3 was made to expand upon these results, and to examine the effects of some additional factors:

(i) Cohen (1968) reported that visual confusibility, by itself, had no effect on the classification time, for either "same" or "different" decisions, of letter trigrams. Acoustic confusibility was equally ineffective; only when discriminability was low on both modalities, did RTs significantly increase. Her results can be criticised on the ground that the stimuli used to obtain them confounded visual and acoustic confusibility, being frequently loaded on both factors - e.g. lower-case "p" and "b". Since experiment 2 demonstrated an effect of visual confusibility, with low acoustic confusibility, it was felt that an extension of the paradigm to include acoustic confusions would be of interest.

(ii) Posner (1969) cites experiments which claim to prove that stimulus duration is not a critical variable in the determination of binary classification RT. Posner & Keele (1967) report that inter-stimulus interval has a highly significant effect on the relative efficiency of visual and nominal matches, implying that longer ISIs act to reduce the superiority of the former over the latter. Since the present experiment seeks to manipulate the efficiency of the visual code in another way, by varying discriminability, it was decided to subdivide each of the principal conditions - visual confusibility and acoustic confusibility - into two subsidiary conditions, in which the relative proportions of processing time available as stimulus duration and inter-stimulus interval were varied. This was effected in such a way that the total time elapsing before the presentation of the second of two stimuli remained constant.

(iii) A direct corollary of the arrangement outlined in (ii) is the sequential presentation of the two stimuli. This contrasts with the simultaneous presentation used in experiment 2. It is likely that staggering the presentation would enhance the effects of confusibility, since, to the

difficulty of direct comparison would be added a decay component : introduced by the requirement to memorise the first stimulus.

(iv) In the previous experiment, it was pointed out that subjects were able to operate successfully by adopting a strategy which enabled them to make a direct classification of stimulus pairs as "easy" or "difficult" comparisons, on the basis of a single distinctive feature. The present study was specifically designed to exclude this option, by making stimulus confusibility dependent upon stimulus pairing, rather than upon parent population.

Subjects: 40 subjects were employed for two separate sessions of approximately 40 minutes each. Their ages ranged from 15-25 and all were right-handed. None had participated in either of the previous experiments.

Apparatus: The stimuli were presented in a withers 3-field tachistoscope, identical to that used in experiment 1. Responses were timed by a single decatron timer linked to the tachistoscope timer that triggered the onset of the second stimulus field. The timer was stopped by the depression of either of two buttons mounted on a horizontal metal box in front of the subject. The accuracy of the

timer was ± 2 msecs.. The display and fixation fields were at equal intensity settings, sufficient for good discriminability, in the dim room light in which the experiment was conducted.

Materials: The stimulus population was composed of upper-case letters, printed in black with a UNO CC10 pen 6 stencil, on plain white 6" x 4" cards. The letters were printed in such a way that their centres would lie ½" either side of the geometrical centre of the card, in a horizontal plane. The first letter of each pair appeared on the left of the central fixation cross, the second on the right.

Two lists of 40 letter pairs each were compiled from each of two separate letter populations. Each list contained 20 "same" - i.e.physically identical - and 20 "different" - i.e. physically dissimilar - pairs, randomly ordered. One letter population - the visually confusible set - consisted of the letters COQGMVAY; the other - the acoustically confusible set - consisted of the letters FSXTPEBMN. The letters in these two sets were never mixed. Each"set" was used to generate two lists of 40 pairs each - a "confusible" list and a "non-confusible" list. For the confusible list, the 20

"different" pairs were chosen for their low within-pair discriminability on the appropriate dimension - i.e. visual or acoustic - and in the non-confusible list they were selected for high discriminability on that same dimension. Thus, in the visual set, CG, OQ, GC, MV, AY etc. would occur in the confusible list but not in the non-confusible list, where other pairs - e.g. MQ, OA etc. would appear.

"Different" pairs on each of the two lists for each stimulus set were scored for both visual and acoustic confusibility, using the two confusibility matrices employed in experiment 2. This was an attempt to equate for confusibility on the non-relevant dimension, between lists generated from the same stimulus population, as well as to measure discriminability differences on the relevant dimension. Unfortunately, it appeared that visually confusible (VC, henceforth) pairs were frequently highly acoustically confusible (AC), so some arbitrary restriction had to be placed on the level of AC permitted in the VC list. Pairs with a rating of more than 40 on Conrad's listening matrix were excluded from either visual list (i.e. either list generated from the VC stimulus set). This left 20 pairs, from a possible maximum of 24 "different" pairs for the VC list. For the AC list the problem of

selection was aggravated by the difficulty of finding AC pairs which were not also VC. This, in fact, made the task of equating between modalities for absolute levels of confusibility impossible so equations were only attempted between lists within each letter population, In other words, it was arranged that confusible and non-confusible lists were equated for confusibility on the non-relevant dimension separately for the two sets. For the VC list, all but 4 pairs were used - these CQ, GQ, MA and AM were excluded by the AC criterion. For the non-confusible visual list, 12 pairs were excluded - those having a mutual visual confusibility of over 130 points on Chase's matrix, and those rating over 40 points on Conrad's matrix - this left, conveniently, 20 "different" pairs.

For the AC list, all 20 possible high-confusible pairs were used. For the acoustic non-confusible list, 20 pairs were selected from the remaining 52 possibilities; these, on average, were as nearly equal to the VC average rating of the confusible list as possible - the argument being that if the lists were equated for confusibility on the irrelevant dimension, it being impossible to find pairs which were highly AC but not highly VC, then, any differences in mean

RT to the two sets should be attributable to the difference on the relevant dimension. The average confusibility ratings of the "different" pairs used in each list, one each dimension, are tabulated below:

	Visually Confusible		Acoustically Confusible	
	C	NC	C	NC
AC Score	14.8	2.0	246.3	9.2
Range	(0-39)	(0-35)	(75-512)	(1-38)
VC Score	562.8	173.0	471.0	466.8
Range	(285-880)	(104-250)	(133-821)	(120-769)

The lists were constructed so that the sequence of responses for the AC and VC lists were identical, as were those for the two non-confusible lists. Each stimulus pair was presented in the following sequence: warning signal ("ready ..1..2" spoken by E) - fixation field - letter 1 - fixation field - letter 2 - fixation field. "stimulus duration" below refers to the duration of letter 1, and "ISI" to the duration of the fixation field between the two stimuli.

The fixation field consisted of a 50:50 black:white computer generated matrix of noise, in a checkerboard pattern, containing a small, red, centrally placed fixation cross. The noise extended to the vertical and horizontal edges of the visible field.

Subjects were randomly assigned to one of four groups, corresponding to the following stimulus-duration: interstimulus interval conditions:

VC Group 1:	ISI 0 msecs.	SD 500 msecs.
VC Group 2:	ISI 250 msecs.	SD 250 msecs.
AC Group 3:	ISI 500 msecs.	SD 500 msecs.
AC Group 4:	ISI 750 msecs.	SD 250 msecs.

The longer overall ISI + SD total for the AC group was selected in the hope that a longer delay before the appearance of the second stimulus would maximise the likelihood of acoustic encoding of the first stimulus, and thus of an acoustic match. Under those conditions the effects of acoustic confusibility would be most likely to appear. There is some evidence to support this expectation: Conrad (1964) found considerable evidence for acoustic errors in short-term memory, and Kinsbourne, in an unpublished study, claims a positive correlation between acoustic errors in short-term memory and delay between presentation and recall. Further, Broadbent and Gregory (1968) show a positive correlation between error-type and delay before recall.

Design: There were ten subjects in each group; half were given the appropriate confusible list first, and half the non-confusible list.

Procedure: Subjects were first instructed in the operation of the apparatus and told of the nature of the task. All subjects responded "same" with the left-hand fore-finger and "different" with the right-hand fore-finger. They were told to respond as rapidly and as accurately as possible. Correct RTs were fed back to the subject after each trial - errors were signalled by a buzzer sounded on the response panel, and the stimulus pair rerun at the end of the list. Each session was preceded by 15 practice trials, with digit pairs, at the appropriate ISI and SD.

Results: Mean RTs and standard deviations for each response for each subject in each condition were calculated. Where the standard deviation of any group of RTs exceeded 100, the entire data of that subject were discarded from further analysis. This left 7 subjects in each group, except group 3 which had 9. The results of each group were then submitted to an AnoVa - 3-way: subjects x list-type x response; the variance tables are presented in Table 8 (i-iv). In addition, T-tests were performed on the RT means within and between lists for each group. These significance levels are shown in Table 9.

Considering the visual-confusibility groups first, the data support the following conclusions: (i) for both group 1 and group 2, list-type and response-type are highly

significant factors ($p < .01$ group 1 and $p < .05$-$p < .01$ group 2); (ii) confusible "same" responses are significantly slower than non-confusible "same" responses ($p < .01$ and $p. < .05$ groups 1 and 2 respectively); (iii) confusible "different" responses are significantly slower than non-confusible "different" responses for group 1 ($p < .05$), but less so for group 2 ($p < .10$); (iv) "same"-"different" differences are preserved for both groups of subjects for both confusible and non-confusible lists ($p < .01$ throughout).

For the acoustic confusibility groups, the picture is obviously different: (i) for neither group 3 nor group 4 is the list-type factor significant, although (ii) response type is significant for both ($p < .01$ and $p < .05$ respectively); (iii) confusible "same" responses are not significantly different from non-confusible "same" responses for group 4 - and, again, do not differ for group 3 where, in fact, confusible "same" RTs are marginally faster than non-confusible "same" RTs; (iv) the confusible "different": non-confusible "different" comparison is significant for neither group; (v) the "same"-"different" difference is maintained at the $p < .01$ level for both list types for group 3, but not for group 4, where it only reaches the unconvincing significance level of $p < .10$.

	Group 1 Source	VISUAL Sum of Squares	D.F.	Mean Squares	F/Ratio		Group 3 Source	ACOUSTIC Sum of Squares	D.F.	Mean Squares	F/Ratio
(i)	Subjects	65509.95	6	10918.32	27.789**	(iii)		24910.88	8	3113.86	-8.37**
	Context (C/NC)	16758.03	1	16758.03	10.93**			1190.25	1	1190.25	0.965NS
	Response (S/D)	25260.03	1	25260.03	93.48**			18632.25	1	18632.25	30.89**
	Subject x Context	9191.19	6	1531.86	3.898NS			9867.03	8	1233.38	3.31NS
	Subject x Response	1621.19	6	270.20	0.687NS			4825.02	9	603.13	1.62NS
	Context x Response	48.89	1	48.89	0.124NS			1308.03	1	1308.03	3.516NS
	Subject x Context x Response	2357.42	6	392.90	-			2976.14	8	372.02	-
	TOTAL	120746.70	27					63709.60	35		

	Group 2 Source						Group 4				
(ii)	Subjects	34773.35	6	5795.56	46.41**			21813.43	6	3635.57	34.120**
	Context (C/NC)	12601.28	1	12601.28	6.91*			585.14	1	585.14	0.73NS
	Response (S/D)	24960.57	1	24960.57	14.08**			7040.57	1	7040.57	7.31*
	Subjects x Context	10939.21	6	1823.20	14.59*			2797.87	6	466.31	4.30*
	Subjects x Response	10639.93	6	1773.32	14.20**			2779.43	6	463.24	-4.35*
	Context x Response	937.31	1	937.31	7.51*			185.14	1	185.14	1.75NS
	Subject x Context x Response	749.28	6	124.88				637.84	6	106.31	
	TOTAL	95600.96	27					56839.42	27		

** $p < .01$
* $p < .05$

TABLE 8: Analyses of Variance for Data from Experiment 3.

Source	G_1	G_2	G_3	G_4
CS/CD	CS(faster) 0.01	CS(faster) 0.01	CS(faster) 0.01	CS(faster) 0.10
CS/NS	CS(slower) 0.01	CS(slower) 0.05	CS(faster) 0.10	N.S.
NS/ND	ND(slower) 0.01	ND(slower) 0.01	ND(slower) 0.01	ND(slower) 0.10
CD/ND	CD(slower) 0.05	CD(slower) 0.10	N.S.	N.S.

TABLE 9: T-Test Levels for Groups in Experiment 3.

Thus, in general, the pattern is the same for groups 1 and 2, and groups 3 and 4 - the second of each pair showing the trends of the first, but at a reduced level of confidence.

Discussion: The results of the AC groups are disappointing: little or no effects of acoustic confusibility appear in the RT pattern, even, surprisingly, at the long ISI used for group 4 (750 msecs.). This confirms Cohen's earlier result, in that, by itself, acoustic similarity was ineffective in producing change in RT. However, it is in apparent contradiction to Conrad's finding of acoustic listening errors in discrimination tasks. The lack of difference between the confusible and non-confusible "different" response RTs is surprising, particularly in view of Posner's (1970) finding that RTs are significantly increased for acoustically similar "different" letter pairs. Unfortunately the letters in question were also visually similar, so it may have been this factor which mediated the increase. Equally, there is no effect of confusibility on the "same" responses - although a small improvement for the "confusible" same RT for group 3 ($p < .10$) appears.

The data from the visual groups reinforce and extend that obtained in the previous experiment: the confusibility manipulation is highly effective. In contrast to Experiment

2, confusible "different" responses are significantly slower than non-confusible "different" responses, and more interestingly, the confusible "same" responses are, again, significantly slower than the non-confusible "same" responses. Thus the "context" of confusibility appears to alterthe time taken to respond to identical stimuli which are common to the two lists of stimuli. This result, obtained in a simultaneous presentation design in experiment 2, is here confirmed in a staggered presentation task. The inference from this finding is clear: that is, whatever model one uses to describe the binary classification data, whether it be a one- or two-process theory, it is possible for factors affecting the latencies of some responses in a series to affect the latencies of others in the same series. This point is made all the more forcibly in this experiment, in view of the fact that the "difficulty" of the "different" comparisons was regulated, not by employing pre-selected but different, stimulus populations, but by altering the pairings of stimuli drawn from a constant, common pool; thus, in this way, the possibility of subjects using overall stimulus characteristics to assess overall difficulty is avoided. In this experiment the increase in the "same" RT in the "confusible" list represents a change in RT to stimuli which remain constant across the two lists for which the comparison is being made.

The temporal manipulations incorporated into the design for the visual groups produced no startling differences. Overall, group 1 RTs tended to be faster than those of group 2, and the significance levels achieved within this group, higher. It therefore seems that the very brief interval (1 msec. approximately) between stimulus 1 and stimulus 2 for the first group did not produce masking or refractory effects which caused RT performance to deteriorate. The fact that experiment 2 showed markedly similar results, qualitatively, reinforces this conclusion, since the simultaneous presentation condition is a limiting case of the sequence used here.

SUMMARY AND CONCLUSIONS

The results of the experiments reported in this chapter demonstrate the effects of two kinds of context upon discrete RTs:

(i) Experiment 1 shows a local contextual effect of superimposing visual noise on one or other, neither or both of a pair of stimuli which have to be judged "same" or "different". The results obtained with this procedure are shown to differ importantly from those obtained with two other methods of manipulating task-difficulty - viz. by introducing a masking pattern, or by interpolating

irrelevant material to which the subject is required to attend, and respond, in the inter-stimulus interval. In particular, an order effect was found which leads to the conclusion that it is inadequate to construe the effects of adding noise in this way as simply a manipulation of discrimination difficulty. The disproportionate difficulty of responding to stimulus pairs in which the first stimulus appears in clear and the second in noise, seems to suggest that the comparison processes for "same" and "different" operate only upon smoothed stimuli. The differences between the four stimulus categories for both "same" and "different" in this experiment are consistent with the existence of such a process. It is impossible to estimate the duration of smoothing from the present design, although the results are entirely consistent with Sternberg's (1967) figure of 60-80 msecs.

Experiment 1 also indicates the likelihood of two distinct processes for "same" and "different" decisions, from qualitative differences between stimulus categories for the two responses. However, the experiment does not provide a direct test of the two-process model. An obvious difficulty for such an account is the general supposition that the "smoothing" process occurs at a pre-perceptual level of processing - which is also the level at which

the "wholistic" type of match thought by some (e.g. Egeth & Blecker, op.cit.) to subserve the "same" response might occur. The logical dependence of the comparison upon the results of the smoothing process does not preclude their occurring in parallel; however, the model could be made more satisfactory if the superiority of "same" decisions was conditional upon both stimuli being in clear, but this, from the present experiment, is manifestly not the case.

(ii) Experiments 2 and 3 demonstrate two more global types of context effect: (a) in experiment 2 the mixed list context showed decreases in "confusible" stimulus RTs and "increases in "non-confusible" stimulus RTs, a levelling process; (b) in experiments 2 and 3 the discrimination difficulty of the "different" stimulus pairs was shown to significantly affect RTs to pairs of identical stimuli; low discriminability raised "same" RTs, high discriminability reduced them, in comparison. This is of interest in that it showsp not only that RTs to identical stimulus comparisons can vary, but also that these RTs may be altered without altering the actual stimuli from which they are generated. These are quite distinct points - the first is significant because it shows that the"same" comparison is not completely functionally independent of other aspects of the stimulus context, nor is it as primitive, basic,

an operation as Posner seems to suggest. The second point is significant because it demonstrates that, given a constant stimulus population it is possible, by selective pairing for the "different" comparisons, to induce changes in RT across the "same"-"different" boundary. In this respect, therefore, "same" and "different" processes, if they be in fact distinct, cannot be wholly independent of each other, or indeed immune from common influence from extraneous factors.

These effects appear to occur only for changes in visual discriminability; they are nowhere evident, even at marginal levels of significance, for material selected for acoustic confusibility. The results of experiment 3 failed to indicate differential effects of trading off stimulus duration for elapsed time between two sequential stimuli in a given pair; the long ISI - especially for group 4 - was intended to force subjects to encode acoustically, when maximum acoustic confusion might be expected to show itself in increased RTs - this did not occur.

This chapter has demonstrated several distinct effects of stimulus manipulations which are nominally changes in stimulus discriminability. Functionally, however, neither at the local nor at the global level, are the effects capable of construction in terms of a single model, nor, more significantly, can they effectively be modelled as the

simple results of systematic changes in stimulus discriminability.

CHAPTER 2 CONTEXTUAL EFFECTS IN A VISUAL SEARCH TASK

In Chapter 1 it was demonstrated that, under certain conditions, RTs to simple stimuli can be significantly altered by suitable manipulations of the overall context in which they occur. Clearly, the particular effects obtained there depended critically upon the nature of the changes made in the "context", but the results were sufficiently significant to show that even in a discrete RT task, single decisions may be strongly influenced by other decisions in the set of which they form a part. The results of chapter 1 also supported the concept of a two-process model of "same"-"different" classification, the suggestion being that separate decision criteria exist for each response.

In this chapter it is proposed to consider the behaviour of "same"-"different" decisions in a serial scanning task. The effects reported so far, have depended upon "local" variations in stimulus discriminability which have aggregately affected RTs to other stimuli. Control of sequence structure in those experiments minimised the likelihood of sequence per se contaminating the results, which appeared, moreover, despite the relatively long inter-decision intervals demanded by the physical difficulty of manual tachistoscopic presentation. Experiment 4 was made to examine the effect

of seriality upon decision-making, and the visual search paradigm was chosen since it allows subjects to make a series of rapid decisions without making an overt response to each. The results of earlier experiments of this genre, by Neisser and others, have been reviewed in the introduction and elsewhere by Rabbitt (1971) and Cohen (1971). Of the data reported so far, none suggest that the requirement for a high-speed scan in conditions where stimulus entropy remains constant produces RT patterns which would not, in principle, be predicted from the results of discrete, single decision tasks, involving the same stimuli. That is to say, sequence, per se, does not appear to be an important variable in determining the type of process employed to make a given decision.*

Experiment 4 collapses the paradigms of Neisser and Posner. That is to say, subjects are required to scan through a vertical list of "same" or "different" letter-

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* This obviously is not true of many:many stimulus response sequences, where systematic sequential effects are known to occur, but is true of tasks in which a single response is made after a sequential scan of the stimulus display. Repetition effects, of the former kind, will be dealt with in the next chapter; this chapter is confined to the effects of sequence on a single decision/response.

pairs to locate a target item. This target is specified only to the extent of its class - i.e. "same" or "different", so the subject is not aware of its actual identity. Thus a rapid sequence of binary decisions is made which terminates on the location of the target. The interesting question is whether or not the sequential context will in any way alter the relative efficiencies of "same" and "different" judgements, and if so, how?

EXPERIMENT 4 THE EFFECT OF VISUAL SEARCH ON "SAME"-"DIFFERENT" JUDGEMENTS

Subjects: 13 subjects, 4 female, 9 male, all between the ages of 18 and 28, were employed for two sessions of approximately 30 minutes each. All were right-handed, and had normal vision.

Apparatus: Stimuli were presented to subjects in a light-tight wooden box, at a distance from the eye of 22½", and viewed through a rubber snorkel-mask, which served to exclude ambient light from the surroundings. Two toggle-switches, one mounted on each side of the wooden box, allowed subjects to initiate a trial themselves, and to terminate the illumination of the stimulus list when they had located the target. The left-hand switch illuminated

the interior of the box, and simultaneously started a Decatron timer, while the right-hand switch both extinguished the light and stopped the timer - this prevented the subject from making an anticipatory response when the target occurred towards the end of the list, although data from Sperling (1963;1967) and others suggest that under conditions where a black-flash terminates an exposure subjects can still read off the target from an iconic trace. A luminous "dot" inside the box located the top of each list, and also served as a fixation spot. Response times were measured to the nearest 100th of a second. The level of illumination inside the box was equivalent to 1.5 log. ft. lamberts. The stimulus lists subtended at the fovea, vertical and horizontal angles of $10^{o}\ 26'$ and $0^{o}\ 38'$ respectively.

Materials: 72 stimulus lists, generated from a computer programme, were mounted separately on sheets of stiff card. Each list consisted of 48 pairs of upper-case letters, arranged vertically, and printed, in heavy black type on non-reflective white paper. 36 lists contained 47 "same" pairs - e.g. AA, BB, FF etc. - and one "different" pair - e.g. XL; the remaining 36 had the opposite arrangement - one "same" pair embedded in 47 "different" pairs.

For each type of list, the incongruous "target" pair occurred three times at each of the following list positions: 6, 11, 16, 21, 26, 31, 36, 41 and 46, so the subject had to search through anything from 5 to 45 items of one type, in order to locate one item of the opposite type.

In constructing the lists, the entire alphabet - with the exception of the letters "I" and "J" - was used, and the following restrictions were met: two letter words were omitted, as were pairs of highly visually confusible letters - e.g. EF, OQ - no letter was repeated in successive pairs; each letter was used with equal objective frequency in the target items on each of the two types of list (target-"same", TS; target-"different", TD); no target was repeated at the same target position; although some "same" targets were perforce repeated, "different" targets were not. The final version of the stimulus lists, incorporating these modifications, was fed into a LINC-8 computer and typed from store, in order to achieve an evenly-leaded type-face. The occurrence of target position, within each list-type, was randomised - each target position occurring 3 times. Thus, of the 36 lists for each type of search, 27 were test lists, the remaining 9 being treated as practice.

Design: Subjects were assigned at random, to run either target "same" or target "different" on the first session. Each session of test searches, was preceded by the 9 practice trials on the relevant condition.

Procedure: Each subject was instructed in the working of the apparatus, and told the nature of the task; it was particularly emphasised that the switch terminating the illumination of the list and stopping the timer, should be depressed immediately he or she thought that the target item had been found. Subjects were told not to track-back through the list in the event of failure to find the target, but to make a steady scan through the list from the fixation spot. As a precaution against false identification, subjects were required to report the name of the target letters to E. Errors, whether of false identification or of total failure to locate the target, were rerun at the end of each session in the order in which they occurred. Subjects were given a short break after the 15th test trial; the entire experiment was run in dim illumination.

Results: Search times for each target position in each condition were computed by averaging the two best response times - out of three - for each list position. Since errors were always rerun, it was inevitably the case that three RTs for each target position were available; this provided an insurance against any particular list being disproportionately difficult.

Error rates varied from 3% to a maximum of 20%, averaging, over subjects, 12.01%. The data of one subject, who made errors in excess of 40%, were excluded from the analysis, leaving 12 subjects in all. It is known that high error-rates tend to be accompanied by abnormal RT patterns, and for this reason the temptation to allow subjects to search as fast as they could without regard to accuracy was stringently avoided (Wattenberger, 1970). The aggregated search times and the differences between "same" and "different" scores at each target list position are set out in Table 10.

A three-way AnoVa (subjects vs. list type vs. target position) was performed on the data. The results are presented in Table 11. Additionally, T-Tests were conducted on the mean TS and TD search scores at each target position.

In every case, searching through "same" was significantly slower than searching through "different" - these significance levels are also given in Table 10.

Several clear and unequivocal trends appear in these results: firstly, although the levels of responding, in absolute magnitude, appear to vary widely between subjects, the pattern within subjects is definite and invariant: that is, search times increase in a broadly uniform way with target item position, although a slowing trend is evident towards the later positions. These results are illustrated graphically in Figure 3. Secondly, it is strikingly evident that, for all the subjects tested, search through "same" background items to find a "different" target is significantly slower than the complementary search through "different" background. Although there are 13 occurrences in all of the opposite result, it will be seen from Table 12 that their incidence is disproportionately confined to the 5 and 10 target positions, where it is reasonable to suppose that extraneous factors, such as the time required to actually initiate a scan, and the visual difficulty in focussing and dealing with the initial items might make the recorded search times somewhat misleading. The remaining six anomalous occurrences are distinguished by the exceptionally small size of the TS minus TD differences (overall

Source	List Position								
	5	10	15	20	25	30	35	40	45
Per "Same"	25.80	40.64	53.40	68.39	88.77	108.77	124.30	143.41	157.24
Per "Different"	22.02	33.81	44.16	57.18	72.92	87.41	99.69	116.02	125.65
Diff. Per S/Per D	-2.78	-6.83	0.24	11.21	15.85	21.36	24.61	27.39	31.59
Significant level of Difference	N.S.	$p<0.05$	$p<0.025$	$p<0.05$	$p<0.05$	$p<0.01$	$p<0.025$	$p<0.05$	$p<0.025$

TABLE 10: Aggregated Search Times (in secs.) - Difference Scores and Significance Levels - Experiment 4.

Source	Sum of Squares	D.F.	Mean Square	F/Ratio	
Subjects	3107812.00	11	282528.30	43.26	**
Context (Per'S'/'D')	14931.04	1	14931.04	2.32	N.S.
List Length	15750240.00	7	2250035.00	173.86	**
Subjects x Context	2255367.00	11	205033.30	37.39	**
Subjects x List Length	996508.00	77	12941.66	1.98	**
Context x List Length	95429.30	7	13632.57	2.05	N.S.
Subjects x Context x List Length	502834.00	77	6530.08	0.05	N.S.
Total	22713130.04	191	118914.85		

** $p < 0.01$

TABLE 11: Analysis of Variance, Experiment 4.

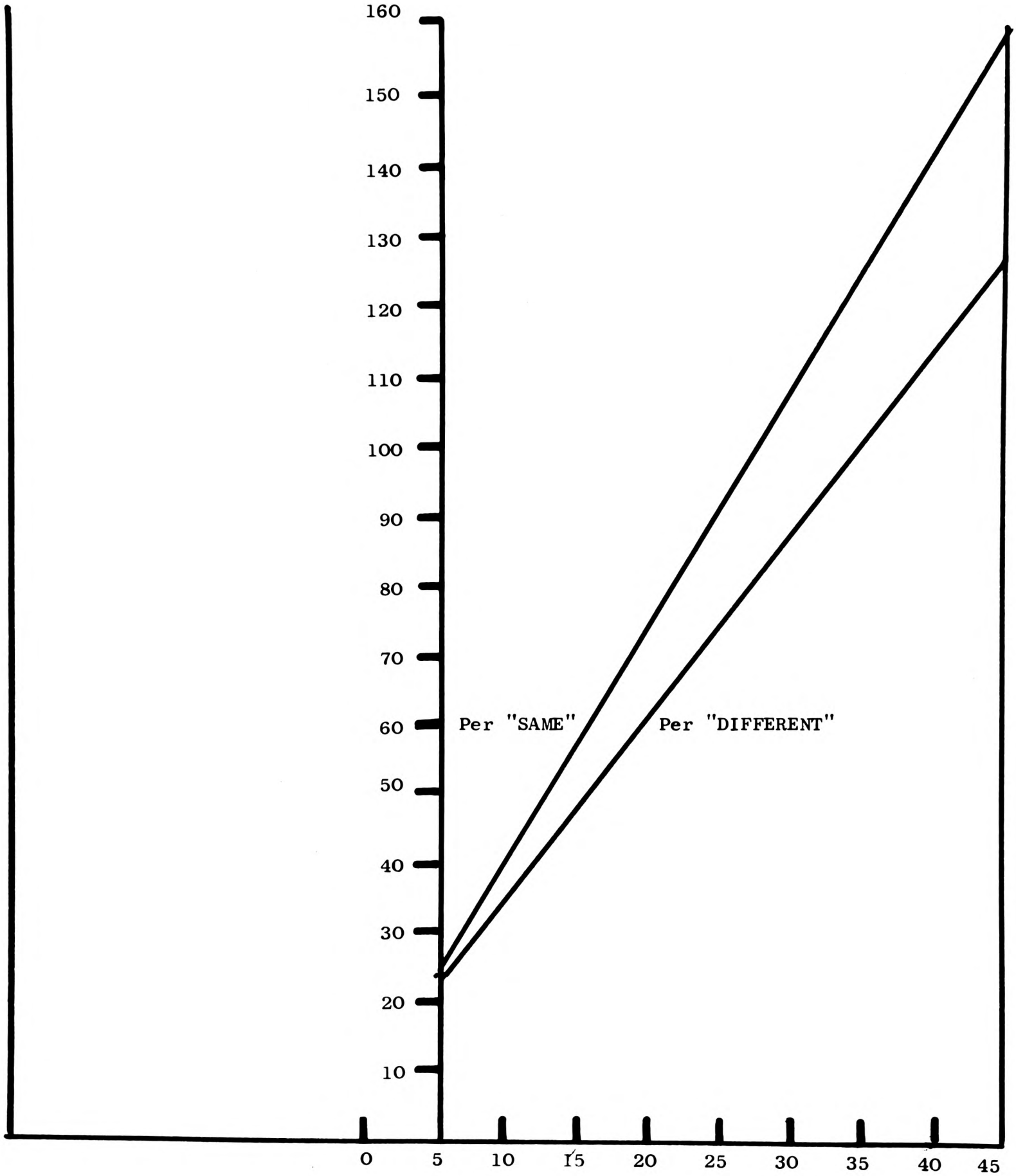


FIGURE 3: Graph of Search Times - Experiment 4.

mean: 0.14 secs.), which are generally considerably less than those for other subjects at the same target position. The overall pattern is clear: while searching through "different" may occasionally take longer than searching through "same", there is a strong systematic trend in the opposite direction, which heavily predominates. A further observation on these results is appropriate: not only do per "same" searches take longer than per "different" searches, but the increase is not absolute but relative. This is reflected in the AnoVa table by the significant interaction between context and list position ($p < .01$). It is therefore probable that the greater difficulty of the former is attributable, not to some constant single factor added to RT, but rather to some process which must be applied to each item that is searched, whether or not it is eventually rejected.

Discussion: The question raised at the beginning of this chapter was whether or not the introduction of sequence into a series of decision would substantially alter the pattern of RTs which might have been predicted by extrapolation from results of single, discrete RT experiments. In this case, these experiments overwhelmingly indicated faster "same" decisions, than "different", which would lead to the prediction of faster "per same" searches.

List position									
Source	5	10	15	20	25	30	35	40	45
No.of occurrences	6	1	4	0	1	0	1	0	0
Average Per Same-Per Difference in secs.	-0.12	-0.34	-0.12	-	-0.15	-	-0.07	-	-

Table 12: Distribution of Per Different > Per Same Occurrences and Average Difference in Search Times - Experiment 4.

However, since the reverse is clearly the case, the data require some explanation; there appear to be two possible directions in which to look for it - one may search

(i) in the sequence. The obvious element of sequence differentiates this experiment from the discrete studies. The subject is here required to make a series of rapid decisions, only one of which terminates in an overt motor response. Thus, motor responses to each stimulus are eliminated, and a sequential structure is added. It must be noted, however, that the particular type of sequential structure in this search task, represents a heavy bias towards one type of decision, and one cannot necessarily justifiably assume that the decision processes operative on individual stimuli under such conditions are identical to those which operate when either outcome is equally likely. This granted, it must equally be said that there is no a priori reason to suppose that a heavy probability of one decision relative to the other should effect "same" and "different" processes differentially. Moreover, even if probability bias does differentially effect decision time, there is no good reason to expect that it will do so in precisely the way demonstrated by this experiment. It

is further important to note that any possible analogy between the present sequential structure and the relative facilitation found with stimulus-response repetition (v. Introduction) is soundly vitiated by the complete absence of any stimulus repetition in the stimulus sequences used. Great care was taken to ensure that no element of a stimulus pair was repeated, either in the same or in a different position, in the pair immediately following it. The absence of responses to each stimulus pair eliminates possible contamination from motor response repetition (Bertelson, 1961). Thus, there appears to be no reason for attributing the effects to the introduction of sequence per se. One can alternatively seek an explanation

(ii) in the particular class of the item for which the subject is searching and the nature of the decision made. The strict parallel between sequential and discrete paradigms only breaks down, if one assumes, as has hitherto been the case, that the decision processes involved in making the decisions are identical for identical stimuli in the two situations. It is not obvious, for example, that (a) the stimuli "AA" in a sequential search task where the target is e.g. "RL" elicit the mediating response "same", and even if they do, that (b) the process by which the

response is reached is qualitatively similar to that involved in reaching the same decision to the same stimuli in a discrete RT task. Indeed, there is ample evidence to suggest that (a) is false, and, since it is presupposed by (b), to entail the falsity of (b).

Neisser (1964) reports results which show convincingly that the time taken to search for a target line which contains a specified letter is significantly faster than the time taken to search for a line which does not contain that letter. The inference appears to be that in the former case a different type of test is being applied to that in the latter case. Thus, two tests are suggested - one for "presence" and another for "absence" - and they seem to produce radically different search times. An alternative construction of Neisser's results is possible: subjects may be testing for "presence" in both cases, but longer search times will result when the outcome of the test is positive. It is difficult to distinguish these hypotheses empirically. However, Neisser provides a plausible theoretical rationale for the difference he finds: the suggestion is that the "presence" test operates by selecting certain critical features for the identified target set. Should one or more of these appear, a mechanism is triggered which requires the switching-in of a

higher level analyser, which then undertakes a complete analysis of the stimulus. Since this takes time, the need to have to make this extra process on all but one of the stimulus lines in Neisser's experiment will clearly take longer than the case in which the target only occurs once in 50 scans. Indeed, the target frequency is known to be a critical variable in determining search time (Rabbitt, 1966a).

Neisser's model makes the point that the nature of the search required may determine the tests which are carried out. The demands of the situation - e.g. a less discriminable background - may force the number or type of tests which are made to vary. The question is, whether such variations in processing could be acting to produce the effects being discussed. The results of experiments 1-3 have led to the supposition of empirically distinct tests for sameness and difference; if these are assumed to exist independently of each other, the present results may be explained. The subject knows that the target:non-target ratio is very low, and, given that he is set for a rapid search, will most probably

make the test of each letter pair that leads to the

fastest and simplest outcome - which on 47 out of 48 trials was "non-target". The evidence cited from Neisser's experiments suggests that the test which fulfills these requirements is the one which tests for "absence" rather than for "presence" and in this case, since there is independent empirical evidence indicating separate tests for "same" and "different", it seems reasonable to suppose that in searching through "same" items the subject would operate most efficiently by making the repeated test for the target set - i.e. "different", and vice versa for the "per different" searches. In other words, it appears that the assumption of a unitary test, or a simple same-different classification is false for a sequential task of this kind, and that subjects operate by testing for the much less frequent target items rather than by making complete classification of each individual stimulus.

If this is so, then the apparent reversal of the relative efficiency of the two processes becomes explicable, and ceases to be paradoxical. The "per same" search is modelled as a series of"different?" tests, while the reverse is the case for the "per different" searches. Assuming, further, that the classifcal "same"-"different" difference is reflected in the relative times taken to perform the respective tests, then these results follow directly from the hypothesis.

SUMMARY AND CONCLUSIONS

Experiment 4 was made to examine the effects of seriality on binary classification. The results show that searching through "same" items for a single "different" target is significantly more difficult than the reverse task. The most plausible interpretation of the data is that it is possible for subjects rapidly scanning serial lists of stimuli to apply alternative tests effectively, depending upon the type of stimuli through which they are searching. The suggestion that, in this case, tests for sameness are independent of tests for difference, provides support, albeit recursive, for two-process models of "same"-"different" classification. The results also illustrate the point that search is a strongly active, rather than a weakly passive process, and more importantly, underline the main contention of this thesis, namely that RTs can reflect just as much the context in which they are measured as the intrinsic nature of the stimuli to which they are made. To the distinction between the nominal stimulus and the functional stimulus, may now perhaps be added that between the nominal test and the functional test.

CHAPTER 3 CONTEXTUAL EFFECTS IN SERIAL CHOICE RT

The two preceding chapters have reported experiments which demonstrate effects on choice RTs to individual stimuli, and on a serial decision process, of making systematic alterations to the context in which the various stimuli occurred. In Chapter 1, these effects appeared in experimental paradigms which are traditionally referred to as "discrete" - that is to say, each stimulus-response trial is considered as being empirically independent of every other trial. However, it was shown that, even in these cases, one individual event is not necessarily immune from influence from other events with which the subjects has to deal. In Chapter 2, the situation concerned was of a serial decision type, where, while each separate decision did not issue in an overt response, the context involved runs ofa particular single decision, which might have contributed to the overall pattern of performance. This present chapter is concerned with a particular aspect of the results reported in the previous chapter, and with an investigation of one special class of effects which are known to be a distinctive feature of serial choice RT tasks.

It is possible to treat skilled cognitive performance in one of two distinct ways: those who adopt a "molar" approach (Hale, 1969) prefer to regard a skill as a single unitary activity and to study it as such. Those however who adopt a "molecular" approach seek to isolate the component factors involved in a particular task and then to determine the laws which govern their combination. To some extent the differences in these methods reflects differences in ends - applied ends frequently justify less exiguous means; however, since it is concerned with understanding the component processes themselves, this report adopts a firmly molecular approach. Unfortunately, the validity of thechemical analogy is not always upheld: in some instances, the molecule may present properties which occur nowhere in its atomic constituents, and then the types of model which are used to explain one set of results might become inappropriate as an account of the other set. The effects of context, particularly in a serial task (cf. introduction for review) amply illustrate the necessity for a molecular approach to sequential skill. Hitherto, however, the tasks used have not dealt specifically with these kinds of serial effect. Two principal types of contextual effect have been demonstrated:

(i) An Absolute Effect: two mutually visually confusible, or partially noisy, stimuli take longer to classify than two non-confusible - or "clear" stimuli. This effect is a "local" one, since non-confusible pairs in the same stimulus list will be dealt with significantly faster than their confusible counterparts (experiments 2 and 3).

(ii) A Total-Context Effect: constant stimulus-response pairs may have significantly different RTs as a result of other pairs introduced into the stimulus list in which they appear. In experiment 2, for example, "same" response RTs were significantly elevated in a context in which the "different" pairs were more difficult. There are two further classes of context effect which have so far not been discussed:

(iii) A Probabilistic Effect: this effect is divisible into two sub-classes: (a) the effects of absolute probability. Expectancies can be increased for certain items, resulting in a correlative reduction of RT to them, by increasing the absolute probability of their occurrence within a given list (Theios & Palmagne, 1969). This effect is not confined to the frequency of a particular identical stimulus, but can be found in cases where the probability

of a given event - e.g. a particular decision - is altered. It is a possible explanation of some of Neisser's rapid search results, which have resisted later confirmation, that a particular decision, namely the rejection of a line in the display as "non-target", occurred with disproportionate frequency (98%).

(b) The second class of probabilistic effects concerns those properties involved in sequence probabilities. Conditional (Markov) probabilities are known to produce measureable effects on RT, to otherwise independent events (Posner & Chase, 1969).

(iv) A Sequential Effect: in which certain transitions, among otherwise equiprobable events, reliably lead to increased or decreased RTs (Rabbitt, 1968b). This effect is conceptually distinguishable from (iii) by the important requirement that no transition, nor indeed stimulus event, be more or less probable than any other. In practice, this restriction is not always met, but the force of the effect is frequently augmented when it occurs to transitions which are much less probable than others to which it does not occur. In other words, whereas (iii) depends upon relative probability, (iv) does not.

The conceptual distinction between (iii) and (iv) is, however, open to question. Opposing views exist as to whether they are truly separable. Kornblum (1969 a,b) demonstrated that, overall, RT was not the same for equi-information conditions which differed in the relative proportion of repetitions which they contained, and concluded that he had disconfirmed the information hypothesis. If, as Kornblum contends, many of the traditional "effects" of CRT studies, currently attributed to variations in H_s, H_r or H_t, might at least partially be accounted for by correlative variations in the proportions of signal or response repetitions, then effects of type (iii) are logically reducible to those of type (iv). Hyman & Umilta (1969), on the other hand, believe that Kornblum's data reflect not the invalidity of the information hypothesis, however it may be stated, but rather are generated by the sub-optimal conditions of his experiments, which did not allow subjects to achieve "optimal anticipatory processing", and thus to take advantage of the "informational" structure of the stimulus lists. Hyman & Umilta's data, which strictly replicate three of Kornblum's (1968) conditions, accord with the information hypothesis, but are objectionable on the ground that they used a 7.5 sec. response-

stimulus interval throughout. They demonstrate no more than that subjects are, under suitable experimental conditions, able to produce data which the informational approach would predict. Kornblum's strongest claim - that the pattern of RTs in rapid serial RT tasks may often be accounted for by the frequency of event-repetitions - remains intact and is not disproved by Lyman & Umilta's data. A recent experiment (Keele & Chase, unpublished) however serves to establish that Kornblum cannot be wholly correct: variation in conditional probability in a serial task was found to be significant, even at very short RSIs. It is beyond the scope of Kornblum's argument to account for this in terms of transition frequencies.

It is clear, from both the introductory review (p.63 et seq.) and what is said above, that important and significant serial contextual effects occur. This chapter presents a short series of experiments which investigates the effects of seriality on simple binary classification.

EXPERIMENT 5 THE EFFECT OF SERIALITY ON A BINARY CLASSIFICATION TASK

Experiment 5 was made for several distinct reasons: (i) In discussing the results obtained in experiments 1-3, the basic effect, demonstrated by Posner & Mitchell (1967), that "same" responses are invariably faster than "different"

responses, has been taken for granted and not questioned. In experiment 4, however, this result was apparently reversed, in a situation which required subjects to make a serial scan of items of one class to locate a single target of the opposite class. Discussing possible explanations of this result, it was suggested that subjects rejected non-target items by making the test appropriate to the target class for which they were searching, rather than the test appropriate to the non-target set. Therefore, part at least of the relative difficulty of searching through "same" items to locate a "different" item might be that the successive tests for difference which this account supposes are made on each pair of letters, are slower than the corresponding repetition of successive tests for identity which are made in locating a "same" pair, in a context of "different" pairs. A possible test of this relative difficulty is made in experiment 5, by comparing RTs to sequences of "different" transitions, and sequences of "same" transitions, where the stimuli differ in each case.

(ii) The Repetition effects described in the introduction represent a significant and important category of local contextual effect. They provide a striking demonstration of the fact that a single S-R process may be greatly influenced by the nature of the S-R process or processes

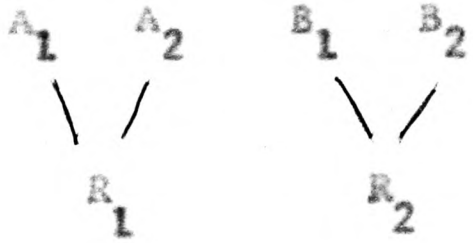
which immediately, temporally, preceded it. Experiments by Bertelson and Rabbitt have made the further point that both stimulus and response repetitions may be contributory factors to the relatively faster RTs to repetitions. In serial self-paced tasks, it is possible, by judicious task selection, to vary stimulus and response repetition independently of each other, so that repetition of a particular stimulus or response does not necessarily entail repetition of the corresponding response or stimulus. Rabbitt (1968b),(1971b), has made such manipulations: his subjects (1971b) compared the presented signal with its immediate predecessor and made the response "same" or "different" on the result of the comparison. In the earlier experiment (1968b), signals were responded to as individuals, being assigned to one of two response classes. The later experiment therefore included the possibility of stimulus repetition without correlative response repetition, a transition which in other experiments was impossible. These possibilities are set out more fully in Table 13.

Unfortunately, Rabbitt's design, in both cases, falls short of conclusiveness: it will be noted from the 1971b experimental structure that all comparisons, from the nature of the stimulus-response code, must necessarily

involve repetition of at least one of the elements of the previous comparison: e.g. the sequence "A-B-A" is equivalent to "AB" followed by "BA". This means that it is not possible to assess the effect of response repetition independently of partial stimulus repetition. Since stimulus facilitation is suspected as the basis - or at least as a heavily weighted factor - of the original repetition effect, repetition of one member of the comparison pair might conceivably facilitate responses when successive responses are identical - i.e. "same"-"same", and "different"-"different" - and hinder responses when they are successively different - i.e. "same"-"different" and "different"-"same". Experiment 5 was therefore designed to examine the effects of repetition of a particular decision independently of the repetition of part or whole of the stimulus upon which it was based.

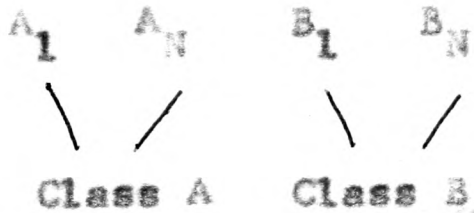
(iii) Much recent discussion has been devoted to forging parallels between the "same"-"different" effect of simple RT classification tasks, and the relative facilitation of repeated signals or responses. The simplest form of the argument (Bertelson,1963) proposes that, for new signals, RTs are determined by the prevailing stimulus-response "compatibility" function, whereas for repeated signals complete recoding is not required, since the system employs a more direct process of analysis involving immediate

Transition	Response Repetition	Response Non-Repetition
Stimulus Repetition	A_1-A_1	Impossible
Stimulus non-Repetition	A_1-A_2	A_1-$B_{\frac{1}{2}}$



RABBITT 1968 b

Transition	Response Repetition	Response Non-Repetition
Stimulus Repetition	A_1-A_1-A	A-B-B
Stimulus non-Repetition	A-B-A	A-A-B



R_1 = Same Class as S_{N-1}

R_2 = Different Class from S_{N-1}

RABBITT 1971 b

TABLE 13 : Signal Transitions in 2-Stimulus Comparisons

repetition of the most recent test successfully carried out on a stimulus which results in reduced RTs to the repeated items. In other words, each new stimulus is first tested for identity with the previous stimulus, a positive result triggering the previous response. In this formulation of the hypothesis, Bertelson does not consider the possibility of a complete analysis of each signal, with the facilitation for repeats being the consequence of increased speed for a repeated procedure - but clearly attributes the facilitation to a different process of stimulus analysis. This account, therefore, makes the repetition effect directly dependent upon a qualitative, rather than upon a quantitative difference in the way a new stimulus in a serial sequence is processed. Furthermore, it locates the facilitation in the stimulus encoding stage of the stimulus-response link, and therefore cannot claim to explain any facilitation which might be attributable to later stages of the processing sequence. Such facilitation exists: Bertelson himself (1961) reports results which lead him to the conclusion that repetition effects are largely response repetition effects, and Rabbitt (1968) reports that at the tenth test run in a sequential task, RTs to "equivalent" transitions - i.e. to signals which are not physically identical, but which, through arbitary class-

assignment, require a repeated response - are within 11 to 30 msecs. of identical signal transitions, whereas, on the second practice run they were not significantly different from new signal transitions - i.e. transitions where both signal and response differed. This result may well be the effect of more efficient perceptual classification, but is certainly not explicable on Bertelson's simple hypothesis of signal: signal-1-back comparison. Rabbitt's (1971_b) results reinforce this contention, since they show response repetition effects to be more marked than signal repetition effects, and further contradict Bertelson's hypothesis in that the rank-order of transition-RTs predicted by his hypothesis differs significantly from that actually obtained in practice. Specifically, Rabbitt found that a stimulus population of 8 neon lights, divided into two response classes, mapped spatially on to two response keys, one for "same class as last signal", the other for "different class from last signal", RTs for "same"-"different" transitions were significantly faster than those for "different"-"same" transitions. Taken as it stands, Bertelson's hypothesis would appear to predict the reverse result, on the principle that "same" judgements are always

faster than "different" judgements (Posner & Mitchell, 1967). However, Bertelson only mentions facilitation for identical signals, and, indeed, to this extent is confirmed by Rabbitt, whose results show "different"-"same" transitions to be faster than "same"-"different" transitions, for transitions which included identical signal repetitions; where signals were not identical, the reverse was the case. Thus, no simple version of the argument for parallelelty between repetition effects in serial tasks and "same"-"different" effects in discrete tasks, will do.

Entus & Bindra (1970) present a radically different version of Bertelson's original hypothesis. It is a marked deficiency of this latter that it only extends prediction, unambiguously, to cases of identical stimulus-response repetition, and moreover only then applied to cases in which stimulus repetition entails repetition of the corresponding response. Entus & Bindra argue from the fact that there can be found weak, but significant, empirical correlations between repetition effects and discrete "same"-"different" effects, in a variety of logically distinct tasks, to the strong hypothesis that the two types of effect are mediated by the same process,

and thence, by the single step of argument, to the specific proposition that the repetition effect is simple a sequential "same"-"different" effect. Entus & Bindra indeed produce the correlations on which they argument is founded, but they then commit the logical error of supposing that these correlations between tasks can be used to demonstrate identity of underlying processes. Their data provide no justification for inferring a causal relationship, and certainly none for inferring identity.

The hypothesis, however, despite Entus & Bindra, is still an interesting and potentially viable one. Experiment 5 attempts to examine it in two different ways: (i) by juxtaposing "same" and "different" judgements made to individual pairs of stimuli, with the transitions of which they form a part. That is to say, will the RT be determined by the stimuli alone, or will it be possible to show significant effects of sequential context? (ii) A special case of (i): if some form of identity matching is used as a preliminary test on each new signal, is it possible to override the traditional "same"-"different" effect by presenting subjects with two identical pairs of letters, in sequence, each requiring the response "different"? This is the critical

test, for if the principal determinant of RT is stimulus identity, then "AB" should not be sensitive to context; if, however, the parallel with "same"-"different" judgements holds, "AB" following "AB" should be significantly faster than "AB" following "YZ".

Subjects: 13 experimentally naive, right-handed subjects were employed for a single half-hour session; their ages ranged between 17 and 25. 8 were male and 5 female - all were paid for their services.

Apparatus and Display: Four stimulus lists were read onto a magnetic tape store, and presented on a slave-oscilloscope screen, under the control of a D.E.C. LINC-8 digital computer. Stimuli appeared, pair-by-pair, in the centre of the small screen, replacing a circular fixation spot which preceded each block of 201 responses. Each pair remained displayed until the subject made his response "same" or "different", by pressing one of two keys beneath his left and right index fingers. Depression of one or other key caused the stimulus to vanish and be replaced at a constant interval of 100 msecs. by the next stimulus pair. Ideally, an R-S interval of shorter duration - in the region of 20-50 msecs. would have been optimal, since it would bring this experiment into line with previous studies on serial performance; unfortunately, however,

pilot studies convincingly showed that intervals below 80 msecs. made the transitions between identical stimulus pairs sufficiently indistinct for them frequently not to be phenomenally apparent to the subjects. This was presumably an idiosyncratic feature of the decay characteristics of the C.R.T. on the oscilloscope. However, the response-stimulus interval of 100 msecs. finally chosen is well within the range where maximum effects were reported by Bertelson & Renkin (1966), so there is no reason to suppose that the results obtained might be abnormal. Responses and RTs to each stimulus pair were recorded by the computer for subsequent print-out and analysis.

Materials: Four lists of 201 upper-case letter pairs were compiled with the following restrictions:

(a) No letter in a given pair to occur either in the preceding or sequent pair - except in the case of a direct repetition.

(b) Excluding the first pair of each block, half the remaining 200 pairs were physically identical, and half physically different.

(c) "Same" and "different" pairs were distributed randomly throughout each block, although neither category was permitted to appear more than three times in direct succession.

(d) For the "different" pairs, all possible pairings of the letters of the alphabet was used, with the exception of the letter 'I' which was omitted, so that no "different" coupling occurred twice, except for deliberate repetitions, in the total set of 804 items. However, where letter pairs were judged to be highly visually confusible - where, particularly, there was likely to be a high degree of horizontal overlap of the dot-matrix which was scanned to provide the oscilloscope display - then, they were excluded. Consideration was also given, in deciding which pairs should be rejected, to the appearance of the letters on the oscilloscope display, which read from a computer type-face, since many letters which were not confusible on straight overlap measures, were highly indistinguishable on the particular type-face used. Two letter words were also excluded.

(e) Transitions: since part of the purpose of the experiment was to examine different stimulus-response transitions, great care was taken to ensure that the various possible transitions occurred with equal objective frequency. Within each sub-block of 40 trials, on each list, the following transitions were counted and matched for objective frequency: "same"-"same"; "same"-"different"; "different"-"same"; "different"-"different". The "same"-

"same" and "different"-"different" transitions were further subdivided into two smaller classes: (i) identical stimulus transitions - e.g. "AA"-"AA", or "GD"-"GD"; (ii) non-identical stimulus transitions: e.g. "AA"-"BB"; or "GD"-"RZ". Each of these subclasses constituted half the trials in each of its parent classes. Thus, each block of 200 transitions contained equal numbers of each principal type of transitions, distributed equally between the two possible response categories (viz. "same" and "different"). Since 25 letters only were being used, the "same" stimulus pairs were made to contain, in total, each letter 4 times. It was not considered necessary to preselect a subset of 25 different pairs.

Method Subjects were instructed in the general nature of the task and responses required of them, and then shown samples of the stimuli on the display screen. "Same" responses were made by the left index finger, and "different" responses by the right index finger; this was fixed for all subjects. In addition to the general instructions, the following points were emphasised: (a) error-correction was fruitless - it would only produce another pair of letters; (b) speed was to be maximised, but not traded off against gross inaccuracy.

Less than 10 errors per 201 responses was suggested as an acceptable norm; (c) runs of 201 responses were to be made without a pause or break.

Design: The four stimulus blocks were run in the same order for each subject. A short rest-pause was given between the first and last two blocks, a longer pause between blocks 2 and 3.

Data-Preprocessing: Subjects overall response-characteristics were found to vary widely. The following criteria were therefore designed to determine whether the data of any subjects should be accepted or rejected for detailed analysis: (i) an average error-rate in excess of 10%; (ii) an overall standard deviation of more than 200 msecs. (ii) might appear to be unduly liberal, but it was found that, on the first block of trials particularly, a more stringent criterion would have excluded more than 75% of the subjects tested. On the 4th block, however, the average standard deviation was nearer the 100 msec. mark, and, if anything, somewhat below it.

Two female subjects were excluded on the first criterion, and two males on the second, leaving a final population of 9 subjects, whose data for the 1st and 4th blocks of trials were printed out and analysed.

Results: The print-outs were analysed by eye. Errors and the three sequent responses, whether correct or incorrect, were omitted from the analysis, since these are known to be atypical (Rabbitt, 1966$_b$; 1967$_b$; 1968$_a$). Also the very first stimulus-pair of each block was discarded - it represents no transition at all. The remaining correct responses were divided into six transition categories: "same"-"same", identical transitions (SSi); "same"-"same" equivalent transitions (e.g. "AA"-"BB") (SSe); "different"-"same" transitions (DS); "same"-"different" transitions (SD); "different"-"different" identical transitions (DDi); "different"-"different" equivalent transitions (e.g. "AB"-"CD"), (DDe). The mean RT for each subject in each of these categories was computed and the mean of these means, over the nine subjects, are presented in Table 14.

Errors: The overall error-rate was constant over the two response blocks examined: 5.34% on block 1 - 5.55% on block 4. It is therefore clear that any differences in average RT between the two blocks does not represent a speed-error trade-off (Schouten & Bekker, 1967; Pew, 1969; Hale, 1969). The distribution of errors between the various transitions is of interest, and is set out in Table 15.

Analyses: Two AnoVas were carried out on the results of each level of practice. The first, computation of the studentized range statistic (S^2), was made to determine whether the means for the six transition categories were significantly different from each other, overall. The second analysis, a Neuman-Keuls test, tested for significant differences between ordered treatment totals.

Both sets of means were found to be significantly different overall, at the $p<0.01$ level. For the first block, the Neuman-Keuls analysis recorded only a single significant difference - that between DDe and SSi ($<.01$). The other means, for this block, failed to show any significant differences. For the fourth block, however, several differences of significance appeared: these are shown in Table 16. Since these will be referred to extensively in the discussion which follows, they will not be further described here.

Discussion: This experiment is different from paradigmatically similar ones in an important respect, namely that subjects were not, as in previous experiments (e.g. Bertelson, 1965; Rabbitt, 1968) required to make some arbitrary classification of single stimuli, but rather, to make a particular response which was contingent upon

Transition-Type	S-S_i	S-S_e	D-S	S-D	D-D_i	D-D_e
Mean RT Block 1	532	608	596	599	584	654
Mean RT Block 4	450	513	546	513	457	576

TABLE 14: Mean of Mean RTs for first and fourth blocks x transition-type, Experiment 5.

Transition-Type	S-S_i	S-S_e	D-S	S-D	D-D_i	D-D_e
Total no. of errors	6	25	23	14	5	22
Mean Error RT	947.2	594.8	565.1	1036.3	773.2	657.4

Error Distribution Block 1 - Experiment 5

Transition-Type	S-S_i	S-S_e	D-S	S-D	D-D_i	D-D_e
Total no. of errors	6	30	28	17	4	15
Mean error RT	620.8	463.0	534.9	489.9	430.0	520.8

Error Distribution Block 4 - Experiment 5

TABLE 15: Error Distributions and Mean Error RTs, Experiment 5.

NOTE: Errors immediately following errors are not included in this analysis, since they were frequently rapid enough to be regarded as error correction responses and not as true errors per se.

the relationship between two simultaneously presented single stimuli. It is therefore unlikely that the overall facilitation found between blocks 1 and 4 might represent an improvement which could be attributed to greater efficiency at any kind of operation which makes for "perceptual equivalence", where this is taken to imply the acquisition of a more economical or more reliable set of discriminating features to separate target from non-target, or to differentiate targets from each other. In this experiment each response-class was open-ended, being defined by a relationship between two stimuli, and not by the intrinsic nature of the stimuli themselves.

The most striking result from these data is the apparent absence of any evidence to suggest facilitation due to response repetition. This is in marked contrast to Bertelson's (1965) results which indicated that for a four-stimulus:two-response task, the difference between equivalent and identical signal transitions was not significant; in other words, that it was the repetition of the response and not of the signal that was facilitative. Admittedly his results are equivocal, since they are true for only two of his four subjects. In the present case, the lack of response-repetition effect is reinforced by several observations:

Transition-Type	S-S_i	D-D_i	S-S_e	S-D	D-S	D-D_e
SiS_i	-	N.S.	x	x	xx	xx
D-D_i		-	x	x	xx	xx
S-S_e			-	N.S.	N.S.	x
S-D				-	N.S.	x
D-S					-	N.S.

xx $p < .01$

x $p < .05$

TABLE 16: Significant differences between signal-transitions for the 4th block of trials.

1. SSe is not significantly different from either DS or SD transitions. This is particularly curious, in view of the fact that taken singly, "same" decisions are known to be reliably faster than "different" decisions (Nickerson,1965; Posner & Mitchell,1967; Posner, Boies, Eichelmann & Taylor,1969). On the hypothesis of facilitation from response repetition, SS transitions of any kind should be significantly faster than SD or DS transitions, for which both stimulus and response alter.

2. DDe is not only significantly slower than SD, it is also the slowest transition of the group. This confirms Rabbitt's previous finding and strengthens the case against significant potentiation from decision-response repetition.

3. SSe transitions are significantly slower than SSi transitions, and DDe transitions significantly slower than DDi transitions. If repeated responses accounted for the major part of the variance, as Bertelson suggests, then disparities of this sort are difficult to explain. It is noteworthy that the present failure to find a significant response-repetition effect also implies the lack of a "decision" repetition effect, although in this particular experimental design the two are confounded.

Conversley, these results demonstrate an equally striking stimulus repetition effect: identical transitions for both "same" and "different" categories are significantly faster than the corresponding equivalent transitions. Despite the fact that a DDi transition involves two"different" responses, which, singly, might be expected to be slower than the two "same" responses involved in either an SSi or an SSe transition, the clear indication is of a stimulus repetition effect which is powerful enough to override, or mask, the differential difficulty of its component decisions. DDi and SSi are non-significantly different from each other,while SSe and SD, DS and DDe, are all uniformly significantly slower than DDi. The fact that stimulus repetition should prove facilitative to such an extent has theoretical implications which are worth exploring:

Posner & Mitchell (1967), discussing their results, which showed that matching stimuli for physical identity is significantly and reliably faster than matches made on any other basis - e.g. name or dass matches - strongly implied that the RT to physical "sameness" is relatively invariant across stimulus and task conditions. Their experiments demonstrated, they claim, that "the comparison

process at the first node remained relatively stable when embedded in more complex second and third node judgements" (op.cit. p.408). Again: "perhaps the most striking feature of the results is that the rate of matching at node 1 does not change with the familiarity of the stimulus pair" (p.407). The main implication of Posner & Mitchell's discussion seems to be that the processes of "comparison and recognition" must be regarded as "elementary units of more complex cognitive processes" - that the recognition of stimulus identity is somehow the simplest kind of perceptual operation. The problem is, therefore, that if this identification process is in fact the simplest and fastest available, at least as far as physical identity is concerned, then there appears to be little room left for facilitation of the kind found to occur between successive identical pairs in the present experiment: "AA" should take no longer to classify whether preceded by "AA" or "BB". The difference in magnitude between the identical and equivalent transitions for both "same" and "different" are so large and convincing that, even allowing for some considerable flexibility in the stability of the "relatively stable" first node comparisons, it is hard to reconcile Posner & Mitchell's dicta with the present evidence.

This conflict, however, does not render the search for an explanation of these results, pointless; it merely sharpens up the distinction between serial and discrete tasks, and emphasises the importance of taking account of contextual factors.

There is an explanation of the significant difference between SSi and SSe: consider a subject responding to a series of stimulus pairs; either he can attend to the classification of each pair as it appears on the screen, independently of the pair which preceded it, in which case no difference would be predicted between SSi and SSe; or, alternatively, he can first compare the present stimulus pair with its immediate predecessor, in the hope of decreasing RT - he has been instructed to maximise speed - by repeating the previous response directly if they turn out to be identical. If both the present stimulus and the one-back stimulus turn out to be identical, then responding should be fast; if, however, as here, 75% of the stimuli differ totally from their immediate predecessor, then the stimulus:stimulus-one-back comparison will yield the result "mismatch". The practical result of this may be the generation of interference between this mediating response and the appropriate response to the current stimulus, analogous to the interference found

between colours and words in the Stroop effect (Stroop, 1935). Were such a strategy adopted, interference would only occur in instances where the mediating response produced by the "X":"X-I" comparison, was a potentially suitable response for the current stimulus. Thus, the SSi - SSe difference could be attributed to a slowing of the SSe responses - since interference would not occur on the SSi transitions. Unfortunately, however, if this strategy occurs at all, its generality is questionable; it would not, for example, predict the relative difficulty of the DDe transitions compared with, for example, the SSe transition. Again, assuming a conflict between mediating responses, DS should be significantly slower than DDe, which it is not. On this hypothesis there would be no hindrance of DDe transitions from a mismatch of X and X-I, since the result of the match would yield the mediating response "different" which, in this particular case, is the correct response for the current stimulus.

A feature of these results which is not straightforwardly explicable is the lack of a significant difference between SD and DS transitions. Analogy with the results of discrete "same"-"different" experiments would predict DS to be faster than SD; the mediating match/mismatch hypothesis however would predict the reverse.

This finding confirms Rabbitt's (1967b) similar result, but goes nowhere towards providing an explanation of it. Rabbitt explains his result by supposing that the comparison process operates by generating the entire last class of which the last stimulus (i.e. X-I) was a member, and then comparing the present stimulus, presumably serially, with it. The difficulty with such an account in the present case, lies in the size of the stimulus classes. The classes of "same" and "different", even for letter populations, are far too large to regard this strategy as an empirically feasible one. The explanation works well for experiments in which each response is subserved by a finite, delimitable class of stimuli, such as Rabbitt used, but otherwise it becomes implausible.

What can be concluded from these results about the nature of the stimulus analysis processes which produced them? Firstly, the evidence is indecisive between two possible explanations of the stimulus effect found. On one hypothesis, the current stimulus can be directly matched against some stored replica of the preceding stimulus; if the result is a mismatch, then the current stimulus has to be dealt with separately, which presumably causes some delay in responding. A match on the other hand would simply trigger a repeated response. An

alternative explanation to this might be that the facilitation for repeated stimuli is the result of repeating a particular comparison between the two stimuli, rather than the repetition of a particular ordered pair of letters. The difference in these two accounts lies in the relative emphasis which they place upon the order in which the members of a stimulus pair appear. In the first case "AB" is regarded as a different stimulus from "BA", whereas, in the second case, they are equivalent, since they involve the same comparison. What is critically important, in understanding repetition effects of this kind, is to determine the precise nature of the functionally important features, in virtue of which a stimulus is counted as a repetition or an alternation. If the critical feature for a repetition to be signalled is the repetition of a comparison per se, then facilitation should be independent of the spatial ordering of the component stimuli. This possibility is hinted at in Rabbitt's results - where significantly faster responding was reported for one-back identity comparisons to singly occurring stimuli if the sequence was of the form "A - B - A", but not of the form "A - B - C". This amounts to facilitation for direct stimulus-pair reversals, since "A - B - A" is equivalent to "AB"-"BA". These transitions

were not included in the present experiment, and it is therefore impossible to draw any definite conclusions. This point will be taken up in more detail later on in this chapter, but it is interesting to note here that there exists the probability of facilitation being the result of repetition of elements of a higher-order than simple physical features.

Bertelson (1963) put forward the "repeat question" hypothesis to explain the repetition effects he found in serial CRT tasks. This said, that, on any given trial, the stimulus present was directly and immediately compared with the stimulus which preceded it. For single stimulus comparisons, this hypothesis predicts no significant difference between SSe and SD transitions, since both matches would yield the mediating response "different". The significantly slower DDe transitions therefore demand a modification of the hypothesis. One possible amendment, which has some prima facie appeal, is to extend the nature of the comparison made from the simple match of "X" and "X-I", to include a serial comparison of X with all the members of the stimulus class of which X-I is a member. Reasons have already been given for rejecting this hypothesis in the present case, although the original form it took

provides a perfectly viable explanation of the strong stimulus effect found for direct repetitions in both the case of SSi and DDi. Its plausibility is also circumstantially reinforced by the error-distributions for both blocks of trials; there it can be seen that errors are disproportionately more frequent to SSe and DS stimulus transitions. If the initial comparison is between X and X-1, then, for both these transitions the result would be the mismatch decision "different". If this inappropriate mediating response prematurely triggered the corresponding motor response, then an error would result, since, in both the SSe and the DS cases, the correct responses is "same", which is not the mediating response. Such an explanation has been rejected above, on the ground that it does not account for the equal difficulty subjects appear to have with the DDe transitions - where both mediating and correct responses are identical. Furthermore, it might also be expected that, in the present context, errors to SD transitions should also be disproportionately frequent, since again, the initial "A":"X-1" comparison would generate a "mismatch" decision. However, since it is unlikely, for several reasons, that one single factor alone is involved in determining RTs in serial tasks, rejection of the Bertelson hypothesis would be premature until more data are available.

An interesting feature of the error distributions in Table 15 is the positive difference between the error RT and the correct RT for the SSi transitions on both blocks. Since, in serial tasks, errors are usually indices of premature responses, and therefore tend to be faster than the corresponding correct RT this is an odd result. Unfortunately, the incidence of errors, overall, is too small to make any statistical comparisons worthwhile, so what is said must therefore be relatively speculative. Rabbitt & Vyas (1970) provide evidence to support the claim that "perceptual" and "motor" errors can be distinguished by their RT relative to the appropriate correct response - motor errors are faster, perceptual errors slower. On this logic, the errors to identical "same" transitions in the present experiment are firmly perceptual. Unless this is a freak result, it is hard to see why the DDi transitions (for block 4) show faster error responses. Nothing more concrete can be said with confidence at the present stage.

In conclusion of this discussion, some reference must be made to the projected purposes of this experiment, outlined on pages 195-204 above:

(i) It was hoped to throw some light on the apparent reversal of the traditional "same"-"different" effect obtained in experiment 4, where a scan through "same" letter-pairs was found to be more difficult than a scan through "different" pairs. In the first place the significant difference ($p < .05$) between SSe and DDe (the latter being slower than the former) excludes any simple account of the relative difficulty of the two kinds of scan. However, this same difference reinforces the explanation ofthe serial scan results given in experiment 4 - namely that the per "same" scan is, functionally, a series of "different?" tests, and the per "different" scan a series of "same?" tests. The SSe - DDe difference confirms their relative difficulty, and, to this extent, strengthens that explanation.

(ii) The experiment was designed to correct a loophole in the design ofprevious serial RT studies, in which response repetition was confounded with total or partial stimulus repetition, and in which decision repetition, by itself, could not occur independently of stimulus or response repetition. In the event, decision repetition was not found to be facilitative, although, in contrast to previous results, heavy stimulus repetition effects were found. There were no effects of response repetition.

(iii) The analogy between discrete "same"-"different" effects and serial "repetition" effects, made principally by Entus & Bindra (1970) was, if anything, supported by these data. The strong facilitation for SSi and DDi transitions was most prominent. However, other aspects of the data were not so encouraging: why, for example, is DDe significantly slower than SD? It is a possible criticism of the experiment as a suitable vehicle for exploring this analogy, that it used stimuli and responses which were potentially productive of interference from mediating responses which would be expected from such a model. It is difficult to see how any "X":"X-I" comparison model might, by itself, supply an adequate account of the processes involved, without an additional assumption to cover the processing of X when the comparison results in a mismatch - this is an inadequacy of the model. There is a logical objection to the model: even if the parallel between discrete and serial tasks holds, it does not provide an explanation of the repetition effect, only an interesting hypothesis about the empirical strategy by which it is brought about.

It was suggested above (iii- p.203-4) that it might be possible by introduction of a serial context to override the traditional difficulty of "different" comparisons. The significantly decreased RT to identical transitions,

even when the stimulus which forms the second member of the dyad itself demands the response "different", again makes the point emphasised in different ways by experiments 1-3, that the RT to a given stimulus is determined as much by the context in which it occurs as by its own intrinsic nature. In the present case, seriality has not only abolished the traditional difference between the "same" and "different" responses, it has, for the comparisons between DDi and SSe/DS actually reversed it.

EXPERIMENT 5b AN ADDITIONAL FACTOR IN SERIAL PROCESSING

Experiment 5 established that, whatever process is responsible for the facilitation obtained with identical stimulus repetitions, it cannot be attributed to the repetition of a particular decision process or to the repetition of a particular motor response, as Bertelson has suggested. The effects found were strongly weighted in favour of stimulus repetition as the predominant facilitating agent. However, while the results can, with confidence, be attributed to stimulus repetition, it is impossible, on the present evidence, to be more specific about whether the nature or location of the effect in the perceptual processing chain. It seems clear, that

more than one process must be involved - that no single factor will account for the variance. Stimulus facilitation by itself will not explain the aberrantly slower DDe response times, in comparison to the SD responses. Some clue to the peculiar nature of these effects can be got by examining the error tables for experiment 5, which reveal that, not only are RTs to repeated stimulus stimulus pairs faster than those to non-repeated pairs requiring the same response, but also that identical stimulus transitions were accompanied by significantly lower error-rates than those of other categories, for each level of practice sampled. This suggests that, whatever process may be critical for producing the classical speed-error trade-off function, the increased speed occasioned by stimulus repetition was not obtained through the same channel, and that, moreover, responses to identical stimulus transitions tended on the whole to be significantly more accurate than those for other transitions where no such repetition occurred. In some way, therefore, repeated stimuli are more easily and accurately processed than non-repeated stimuli. The problem is: in what does this facilitation consist? There are two types of explanation that might be considered:

1. Firstly, it could be said that even for single stimuli, repetition facilitates recognition. This can either be regarded as a perceptual recency effect, or as an hypothesis about the determination of the order in which stimuli are compared (i.e. by "X":"X-1" comparisons): whichever alternative is taken, this hypothesis remains a quantitative one: the process of analysis is speeded by repetition.

2. Alternatively, one can suggest that, once the stimulus has been processed to a sufficient extent for it to be recognised and identified (this is neutral as to whether new and repeated stimuli are dealt with in the same way for this level of processing to be reached), then the system is able to respond to a repeated stimulus more rapidly, by by-passing the usual processes of decision and response selection which normally follow stimulus analysis, and simply repeat the previous response, which, it must be assumed, is still available from short-term memory.

These two hypotheses are clearly distinct: one attributes the effects of repetition to processes which are involved in stimulus processing and identification; the other makes the effects dependent upon processes which follow on from this stage. The former is an hypothesis of quantitative difference, the latter one of qualitative difference.

However, irrespective of which hypothesis one is considering, there is one requirement which is common to both: namely, that the system is able to operate as a "hold-circuit" efficiently enough for the information necessary either to confirm stimulus identity (hypothesis 1), or, given stimulus identity, to select the correct previous response from short-term memory (hypothesis 2), to be retained. In either case, any factor which increases the likelihood of attenuating or destroying the stored information, might also be expected to produce a reduction of the stimulus repetition effect.

A critical factor on the efficiency of recall in short-term memory is the interval elapsing between presentation and recall (cf. Aaronson,1967, for review). In a serial task this corresponds to the response-stimulus-interval,when one is considering the transfer of information from one stimulus-response sequence to the next. Serial data from several sources show that changes in RSI significantly affect RT (Williams,1966; Bertelson,1961; Landauer,1964; Bertelson,& Renkin,1966). Bertelson & Renkin, in particular, present interesting results: they find that the repetition effect for single stimuli at an RSI of 200 msecs. was totally abolished when the interval was increased to 2000 msecs.. The present experiment was therefore made as a straightforward replication of experiment

5, to assess the effects of substantially lengthening the RSI. If, as seems likely, the effects in that experiment are stimulus-bound, then a significant reduction in their magnitude would be expected from an increased delay between one stimulus and the next.

Subjects: 11 subjects, 5 male and 6 female, ages 17-26, were employed for a single session of 30 minutes. All were students, and all right-handed.

Material, Display and Procedure: This experiment was a direct replication of experiment 5, the only alteration being an extension of the RSI from 100 to 3000 msecs.. This meant that stimuli remained on the oscilloscope screen until the subject responded, being then replaced by a blank screen for the three-second interval, at the end of which the next stimulus-pair appeared.

Results: The same criteria for rejecting data protocols was applied as for experiment 5. One male subject was excluded on the error criterion, and one female subject on the standard deviation criterion, leaving a final population of 5 female and 4 male subjects, whose first and fourth blocks were then analysed. The means of means, for each transition category, is set out in Table 19.

As before, two analyses of variance were performed on the results at each level of practice. For the first block of responses the category means were significantly different overall ($p < .01$); they failed to reach significance for the fourth block. Table 18 provides a detailed breakdown of the Neuman-Keuls analysis for block 1.

Errors: Errors, on both blocks, were counted and sorted into the stimulus categories from which they derived. The mean error RTs for each category are shown in Table 19. Errors immediately following errors were not included in this analysis, since they tended to be predominantly very fast-error-correction-type responses which differ both in character and relation to the stimulus from genuine errors.

Discussion:

(a) Practice: over all transition types, there is a reduction of between 17.77 and 67.13 msecs. in the mean of means from block 1 to block 4. Coupled with this difference are small variations in the transition rank-orders, but as the significance tables show, these are not significant. Between the two blocks examined, it is clear that the differences which exist initially between SSi and the other transitions are abolished with practice

Signal-Transition	S-S_i	S-S_e	D-S	S-D	D-D_i	D-D_e
Mean RT. Block 1	550.4	633.4	602.5	611.1	613.8	654.5
Mean RT. Block 4	512.1	566.3	584.2	568.3	567.4	607.1

TABLE 17: Mean RTs for blocks 1 and 4 by signal transition - Experiment 5b.

Signal-Transition	S-S_i	D-S	S-D	D-D_i	S-S_e	D-D_e
S-S_i	-	x	x	x	xx	xx
D-S		-	NS	NS	NS	NS
S-D			-	NS	NS	NS
D-D_i				-	NS	NS
S-S_e					-	NS

xx $p < .01$

x $p < .05$

TABLE 18: Neuman-Keuls analysis ; block 1 - Experiment 5b.

to the extent that, on block 4, only one transition remains significantly different from SSi - namely, DDe ($p < .05$).

(b) Errors: summated over transition category, the error-incidence table clearly shows that transitions between identical stimulus pairs, whether these be DD or SS transitions, have a very low error-rate compared with their non-identical, same-response counterparts. These differences cannot be attributed to statistical differences in the objective incidence of the transitions involved, since these were equal for the 4 categories being discussed. Again, as in experiment 5, there is no tendency for RT to be negatively correlated with the number of errors, as would be expected from speed-error trade-off considerations; this further reinforces the contention that the determinants of stimulus facilitation and speed-error trade-off are not located at the same stages of the coding process. Interestingly, for both SSi and DDi, error RTs are again greater than the corresponding correct RTs, whereas, for all other transitions, the reverse is true. Unfortunately, as before, the relative incidence of errors in the "identical" categories does not allow worthwhile statistical comparisons to be made.

Signal-Transition	$S-S_i$	$S-S_e$	D-S	S-D	$D-D_i$	$D-D_e$
Total no. of errors	1	15	23	20	6	23
Mean Error RT	531.6	550.1	642.5	642.5	730.2	582.0

Error Distribution - Block 1, Experiment 5b

Signal-Transition	$S-S_i$	$S-S_e$	D-S	S-D	$D-D_i$	$D-D_e$
Total no. of errors	2	14	24	5	3	9
Mean Error RT	594.2	471.0	525.0	503.5	601.2	567.8

Error Distribution - Block 4, Experiment 5b

TABLE 19: Error Distribution & Mean Error RTs - Experiment 5b.

NOTE: Errors immediately following errors are not included in this analysis since they were frequently rapid enough to be regarded as error-correction responses and not as true errors per se.

(c) Response-stimulus interval: making a comparison with the results obtained with the shorter RSI, the most striking feature of the present pattern of RTs, is the failure of theDDi transitions to show any facilitation, at either level of practice. Further, in the fourth block of trials, there is no facilitation for either SSi or DDi transitions. In descriptive terms, the effect of increasing the RSI has been to eliminate the repetition effects found in the previous experiment, while maintaining, although not at an acceptable level of significance, the essential features of the rank-orders found at the shorter interval. In this respect, the prediction made in the introduction - i.e. a return to what might be expected from a discrete RT experiment - is confirmed. Indeed, on the basis of what is known of serial performance at long RSIs, (Bertelson, 1961; Bertelson & Renkin,1966; Vale,1967) it would have been surprising had the facilitation not been abolished. However, what is curious is the marked lack of difference between the "same" categories and the "different" categories, which discrete experiments would have led one to expect. While it is possible that these were obscured by artificial elevation in overall RTs produced by the increased temporal uncertainty at the longer interval, this remains puzzling, though not sufficiently convincing to call into question Posner & Mitchell's original results.

EXPERIMENT 6 THE EFFECT OF PARTIAL STIMULUS IDENTITY ON SERIAL CHOICE REACTION TIME

The question was raised earlier that, if as it appears, the repetition effects reported are largely attributable to the immediate repetition of the stimulus item of a stimulus-response pair, then in what particular facet of the stimulus does the effect occur? In other words, what functionally constitutes a repeated stimulus? In particular, does the occurrence of repetition facilitation depend upon there being absolute physical identity between sequent stimuli, or will the repetition of certain critical features be enough to produce decreased RTs to a second stimulus? These questions relate to what has been written above: it is clear from certain accounts of the repetition effect (e.g. Entus & Bindra, op.cit.) that the effects are supposed to be directly analogous, if not identical, to the "same"-"different" effect of simple RT experiments, in the sense that the faster "same" response is tied to physical identity - and in the appropriate cases to physical similarity, and, by extrapolation, to the context in which the stimuli occur. Thus, partial repetition of a stimulus - e.g. by repeating some features and not others - might be expected to lead to an increase, rather than a decrease, in RT, since partial identity would produce an unclear outcome of any "X"-"X-I" comparison.

The problem is complicated by evidence which suggests that under suitable circumstances, stimulus similarity as well as strict physical identity can be facilitative. Sekuler & Abrams (1968) and Posner & Mitchell (1967) both report these effects. In the former case, systematically manipulated dot matrices were compared, in pairs, by subjects, similarity being defined as the relationship holding between two matrices which had at least, but not necessarily at most, one filled square in the same location; in the latter case, although not explicitly defined, "similarity" was taken to mean "having several features in common", and was often associated with differences in size between two stimuli having the same shape and name - e.g. Oo, Cc or Ff. In both cases, similarity was significantly faster than complete physical difference - although slower than strict physical identity. Posner & Mitchell explicitly mention "analogue matches" hinting implicitly at what Egeth & Blecker (1971) were later to conceptualise as "wholistic comparisons". Neither of theseexperiments involved serial stimulus processing, and it would be therefore begging the question of explanation to assume that "similarity" effects reported for discrete presentation tasks would necessarily produce facilitation in serial processing. However, there is some evidence from serial-type tasks to suggest that what counts as

functional identity - in terms of RT measures - is to some extent dependent upon the particular task chosen. Broadbent & Gregory (1962) report that mixed lists of upper and lower case town names took no longer to sort than pure upper case lists, suggesting that for the particular task upper and lower cases were functionally equivalent, although the actual physical stimuli differed markedly.

Experiment 6 was therefore made to examine the effects of physical similarity between sequent stimuli on RTs, and to compare these transitions with the extreme cases of complete identity and complete difference. A pilot study was made, in which upper case letter-pairs were presented sequentially, as in the previous experiment, and transitions were manipulated in such a way that successive pairs requiring the same response could be either identical, similar or dissimilar, according to whether the letters of the first pair were identical, confusible or non-confusible (visually) with the letters in the corresponding positions in the immediately sequent pair. Unfortunately these manipulations failed to show any significant effects, largely, it is suspected, because the typeface used on the LINC-8 computer to code upper case letters made the construction of suitable stimulus

lists impossibly difficult. There was therefore insufficient discriminability difference between the confusible and non-confusible sequences, and no independent check on these relative confusibilities. It was decided to devise an entirely different means of producing stimulus similarity, and this is described below.

In this experiment subjects were required to respond to pairs of upper case letters in the same manner as before. The stimuli were again presented under the control of a LINC-8 computer which recorded RTs and responses. The manipulation which was used to produce stimulus "similarity" operated for the "different" letter-pairs only, and worked as follows: successive "different" pairs were permitted to differ in any one of five ways: (a) both letters identical, in identical positions (e.g. AB/AB); (b) both letters identical, in reversed positions (e.g. AB/BA); (c) one letter identical, in the same position (e.g. AB/AC or AB/CB); (d) one letter identical, in a different position (e.g. AB/CA or AB/BC); (e) both letters different (e.g. AB/CD). In this way, similarity is both graded and strictly controlled; it is also independent of any arbitrary measures of confusibility, which, in any case, did not necessarily correspond to those of the highly-stylised computer letter-code. The similarity used here, therefore, ranges

from complete identity through three intermediate stages, to complete physical difference. Additionally, this serves to provide an empirical separation of the conceptual difference between spatial identity and item identity - "AB"-"BA" contain the same two letter items, but there is no strict identity, since these appear in different spatial positions.

Subjects: 16 student subjects were employed - 9 male and 7 female - for a single session of approximately 30 minutes. Their ages ranged from 15-22; all were right-handed.

Apparatus: as for experiments 5 and 5b.

Materials: six stimulus lists, identical in structure to those used in experiments 5 and 5b, each consisting of 201 upper case letter-pairs, were compiled and read onto magnetic tape storage for presentation by the computer. "Same" and "different" pairs occurred with equal objective frequency, and in a subjectively random order. The only difference from previous lists appeared in the "different" pairs: where a DD transition occurred, it was arranged in such a way that it conformed to one of the five types listed above (a-e). Each type occurred

with equal frequency - that is 10 times in a single experimental list of 201 trials. The same restrictions - i.e. on visual confusibility, two letter words etc. - applied as before. The response-stimulus interval was fixed at 100 msecs.

Method: Since all the subjects were experimentally naive, they were first instructed in the working of the apparatus, and the nature of the task explained to them. The six experimental lists were then run, with a short break between each - six lists were used instead of the four used previously, since, having decided to collapse the last two lists for data analysis, it was felt important to eliminate any gross practice effects which might have contaminated the results.

Data Pre-Processing: Protocols were rejected on violation of either of the following criteria, applied to the last two lists run: (i) error rate in excess of 10%; (ii) standard deviation in excess of 100 msecs.. Together, these criteria excluded one female and three male subjects - leaving a final population of 12 subjects for further analysis. The data of these subjects' last two lists was then printed out, combined, and sorted by eye into transition categories. While it may be noted,

in passing, that the "same"-"same", "same"-"different" and "different"-"same" transitions behaved in exactly the same way as for the previous experiment, these will not be considered further. The discussion will be confined to the behaviour of the five "different"-"different" categories. The means and mean of means for each DD category are presented in Table 20.

Errors: Errors and the three sequent responses were automatically rejected from the analysis. Errors were counted and then classified by division on the basis of the stimulus transition to which they occurred. It is not possible, on the data available, to separate perceptual and motor errors, although a simple attempt was made to exclude aberrant error responses by counting errors only where they were preceded by at least two correct responses. Thus, the second error of an immediately sequent pair would not be included. The error incidence, mean RTs and distribution through the DD categories is also shown in Table 20.

Analysis: A Neuman-Keuls analysis of rank-order was performed on the correct mean RTs - the overall means having been found to differ significantly by the s^2 test

($p < .01$). The results of this analysis are shown in Table 21. Additionally, "T"-tests were carried out on successive pairs in the DD transition rank-order. There were significant differences between AB/AB and AB/BA ($p < .01$), between AB/BA and AB/CD ($p < .01$), and between AB/AB and each of the three slowest transitions (i.e. AB/CA, AB/AC and AB/CD) ($p < .01$).

Discussion: These results require little comment. It is clear that the manipulation of similarity was thoroughly effective, in as much as there appears to be a systematic gradition of RT between the extreme values obtained for complete physical identity and complete physical difference. It is equally evident that the transition between a pair of letters and its direct reversal was significantly facilitative, in the respect of its being significantly faster than the AB/CD condition, although it is slower than the strict identity transition, but still significantly faster than letter-pairs having one letter in common ($p < .01$).

The error-matrix is of interest, in providing at least circumstantial support for the trends apparent in the correct RT data. In this case error RTs for the AB/AB

Signal-Transitions	AB/AB	AB/BA	AB/AC	AB/CA	AB/CD
Mean correct RT	449	482	503	506	519
Total Errors	2	2	6	12	11
Mean Error RT	453	531	488	498	430

TABLE 20: Correct RTs, Error RTs and Error Incidence - Experiment 6.

Signal Transitions	AB/AB	AB/BA	AB/AC	AB/CA	AB/CD
AB/AD	-	*	**	**	**
AB/BA		-	NS	NS	*
AB/AC			-	NS	NS
AB/CA				-	NS

** $p < .01$

* $p < .05$

TABLE 21: Significant differences between Signal Transitions - Experiment 6.

and AB/BA transitions are both marginally slower than their correct counterparts, whereas, for the remaining three transitions, the reverse effect occurs - error RTs are faster than the corresponding correct RTs.

Since the following experiment is an attempt to consolidate and expand upon these results, further comment will be deferred until these data have been presented.

EXPERIMENT 7 FURTHER EFFECTS OF RESPONSE STIMULUS INTERVAL ON CHOICE RT.

The important finding of the previous experiment can be interpreted as demonstrating facilitation for the repetition of a stimulus, as opposed to the repetition of a spatial location. "AB" followed by "BA" does not generate the appearance of physical identity, or even, subjectively, of physical similarity. It seems therefore justified to conclude that some process beyond the level of perceptual (iconic?) comparison must be involved in making the comparisons which produce repetition - facilitation. This is not to say that facilitation is never the result of a straightforward "X":"X-I" comparison, but rather that instances of facilitation can occur which make such an account unlikely as a convincing explanation of the effect.

If the effect of letter-pair reversal is not explicable on the basis of simple physical identity facilitation, then it might be supposed to be resistant to the kinds of experimental manipulation which reduce physical identity facilitation. Experiment 5b demonstrated this reduction for long RSIs; experiment 7 was made to titrate the effects of RSI against stimulus category.

Subjects: 24 student subjects were employed for 2 sessions of approximately 30 minutes each. These sessions as far as possible were held on consecutive days, at the same time of day. All subjects were right-handed and had normal vision.

Apparatus: As for the previous experiment, stimuli were presented on a slave CRT display, controlled by a LINC-8 computer.

Materials: The stimuli consisted of 12 lists of 201 upper case letter-pairs each. The 200 transitions were equally divided between the two response classes "same" and "different". The transitions were controlled as follows:

(i) "Same": identical (AA/AA); equivalent (AA/BB); alternate (CD/AA) - the relative proportions of each being in the ratios 3:3:4.

(ii) "Different": identical (AB/AB); reversed (AB/BA); equivalent (AB/CD); alternate (AA/CD) - relative proportions: 2:2:2:4.

The relative incidence of alternate transitions in each category was larger than would have been ideally desirable, but was necessary to create the impression of subjective randomness, so subjects were unable to make reliable guesses at the next response.

Design: Four response-stimulus intervals were chosen - 80, 200, 600 and 2000 msecs.. These cover a wide range - from the very short (80 msec.) interval at which either post-response refractoriness (Welford, 1967), or refractoriness consequent on S_2 occurring before R_1, is completed (Davis, 1962),* might be expected to occur, to the longer interval (2000 msecs.) which might be correctly described as defining a "discrete" RT paradigm, an interval at which iconic persistence is known to break down in vision (Posner & Keele, 1967).

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* Rabbitt (1969) explored the question of refractoriness in a serial self-paced task with various R-S intervals and found no evidence for a refractory period.

Each subject ran three lists at each of two single RSIs - blocked on each session, thus covering the four intervals in two experimental sessions. The order in which the intervals were encountered was unique for each subject - one subject being assigned to one ordering. There were 24 subjects and 24 possible orders of the conditions.

Procedure: The nature of the task was explained to each subject, and the four lists for the session run with a short break between each. RTs and responses were stored by the computer on magnetic tape for subsequent print-out and analysis. Subjects were generally instructed to respond as rapidly as possible consistent with a reasonably low error rate (an acceptable level of errors of 10% was given to the subject as an upper limit). An addition to the computer facility enabled the experimenter to obtain, at the end of each block of 201 trials, an immediate summary print-out of mean correct RT and gross number of errors. Subjects who were either unduly slow or excessively careless could be informed and suitably admonished. The first of each of the three blocks at each interval were treated as practice, the final two being used for analysis.

Results: A facility enabling the computer to collapse one or more blocks of trials, to sort out the suitably coded responses into transition categories, having first rejected errors and the three responses immediately following them, and finally, to print-out a transition category list containing the number of responses in the category, the mean RTs and logarithmic transforms of the standard deviations, was applied to the stored data. The mean of means for each transition category, at each response-stimulus interval are presented in Table 22. A three-way analysis of variance - subjects x RSI x stimulus transition - was performed on the data and the results appear in Table 23. It will be seen that both RSI and category are highly significant factors. Additionally, a Neuman-Keuls analysis was made to determine significant rank-order differences within each RSI. The resulting significance tables are provided in Tables 24 (a-d). From these, the following factors emerge:

(a) All, save one, of the significant differences obtained at the three shortest RSIs is abolished at the 2000 msec. interval. This confirms the findings of experiment 5b.

(b) The only significant difference at the 2000 msec. interval is that between the DDe and the SSi transitions ($p < .05$).

(c) The DDr (reverse) transitions always show RTs which are intermediate between the DDi and the DDe categories. At the 80 msec. interval, the DDr transitions are significantly slower than the DDi transitions ($p < .01$), and significantly faster than the DDe transitions ($p < .05$). At 200 msecs. RSI, DDr transitions are not significantly different from DDi transitions, although they are in fact slower; however, they remain significantly faster ($p < .01$) than the DDe transitions. At 600 msecs. RSI, DDr transitions are again significantly slower than DDi transitions ($p < .05$), but are non-significantly faster than the DDe transitions.

It is to be noted that these significance levels are products of variance contributed to by the overall pattern of RTs within each category of RSI. More will be said below about category differences based upon the results of "T"-tests which were made to test for differences between single pairs of means. These were performed on the DD transitions only, in pairs, to determine whether the reversed pairing was indeed significantly different from both the identical and equivalent transitions. The results

Source	Signal Transition S-Si	S-Se	D-S	S-D	D-Di	D-Dr	D-De	
Correct Mean RT	453.9	531.0	530.4	527.3	449.5	490.5	534.6	80
Mean Error RT	679.9	490.2	472.6	502.7	471.1	522.9	489.2	msecs
Total Errors	6	47	61	36	2	11	37	RSI
No.of S's contributing errors	3	20	20	19	2	7	17	

Source	Signal Transition S-Si	S-Se	D-S	S-D	D-Di	D-Dr	D-De	
Correct Mean RT	426.3	514.3	509.4	496.7	428.0	458.6	505.8	200
Mean Error RT	467.2	437.6	407.5	446.0	439.1	465.2	454.1	msecs
Total Errors	3	59	92	37	3	12	36	RSI
No.of S's contributing errors	2	21	23	16	3	6	18	

Source	Signal Transition S-Si	S-Se	D-S	S-D	D-Di	D-Dr	D-De	
Correct Mean RT	435.5	513.9	507.4	491.3	437.7	475.0	500.0	600
Mean Error RT	478.6	460.6	427.5	461.0	448.2	511.4	497.8	msecs
Total Errors	9	57	86	40	2	9	32	RSI
No.of S's contributing errors	5	21	21	18	2	9	19	

Source	Signal Transition S-Si	S-Se	D-S	S-D	D-Di	D-Dr	D-De	
Correct Mean RT	509.8	564.3	558.4	551.7	523.1	555.1	576.2	2000
Mean Error RT	627.5	487.9	548.6	490.2	542.0	570.3	521.1	msecs
Total Errors	6	27	59	12	2	10	21	RSI
No.of S's contributing errors	4	18	22	9	1	9	15	

TABLE 22: Correct & Error RT Data - Experiment 7.

i = identical

e = equivalent

Source	Sum of Squares	D.F.	Mean Square	F/Ratio
Subjects	1485281.00	23	64577.43	28.37 **
Response Stimulus Interval	548377.50	3	182792.10	18.59 **
Signal Transition	604905.00	6	100817.50	62.39 **
Subjects x RSI	678201.50	69	9829.01	4.32 NS
Subjects x Sig. Transition	223002.00	138	1615.96	0.71 NS
RSI x Signal Transition	43180.00	18	2398.89	1.05 NS
Subjects x RSI x Sig. Trans.	942247.00	414	2275.96	-
Total	4525194.00	671		

** $p < .01$

TABLE 23: Analysis of Variance, Experiment 7.

Signal Trans.	D-Di	S-Si	D-Dr	S-D	D-S	S-Se	D-De
D-Di	-	N.S.	**	**	**	**	**
S-Si		-	*	**	**	**	**
D-De			-	*	*	*	*
S-D				-	N.S.	N.S.	N.S.
D-S					-	N.S.	N.S.
S-Se						-	N.S.
D-De							-

(a) 80 msecs.

Signal Trans.	S-Si	D-Di	D-De	S-D	D-De	D-S	S-Se
S-Si	-	N.S.	N.S.	**	**	**	**
D-Di		-	N.S.	**	**	**	**
D-De			-	N.S.	*	*	**
S-D				-	*	*	**
D-De					-	N.S.	N.S.
D-S						-	N.S.
S-Se							-

(b) 200 msecs.

Signal Trans.	S-Si	D-Di	D-Dr	S-D	D-De	D-S	S-Se
S-Si	-	N.S.	**	**	**	**	**
D-Di		-	*	**	**	**	**
D-Dr			-	N.S.	N.S.	N.S.	N.S.
S-D				-	N.S.	N.S.	N.S.
D-De					-	N.S.	N.S.
D-S						-	N.S.
S-Se							-

(c) 600 msecs.

Signal Trans.	S-Si	D-Di	S-De	D-De	D-S	S-Se	D-De
S-Si	-	N.S.	N.S.	N.S.	N.S.	N.S.	*
D-Di		-	N.S.	N.S.	N.S.	N.S.	N.S.
S-D			-	N.S.	N.S.	N.S.	N.S.
D-Dr				-	N.S.	N.S.	N.S.
D-S					-	N.S.	N.S.
S-Se						-	N.S.
D-De							-

(d) 2000 msecs.

** $p < .01$
* $p < .05$

TABLE 24 (a-d): Significant differences between signal transitions x RSI, Experiment 7.

of these tests at each RSI are shown in Table 25.

Errors: The errors were analysed by the computer into transition categories. The "transition-category" of an error, here as throughout the previous serial experiments reported, is the same as the transition category that would have obtained had the response been correct. It was impossible to separate out, empirically, perceptual from motor errors. The resulting matrix is therefore an analysis of the stimulus categories into which errors fell, not an analysis of their cause. The means of subjects' mean error RTs, and their proportionate incidence in each category, are tabulated in Table 22, together with a count of the number of subjects contributing to each error total and the total number of errors. The RTs represent untreated means of means.

Discussion: There are two different aspects of these results to be considered:

1. Rank-order changes: firstly, it is of interest to discuss the changes in rank-order of the stimulus transition categories which appear with changes in RSI. The most striking effect, confirming the results of experiment 5b, is the abolition of facilitation attributable to identical stimulus transitions, at the longest

Source / Signal Comparison		$D-D_R$: $D-D_I$	$D-D_E$: $D-D_R$
80 msecs.	t	2.44	2.44
	p <	.01	.05
200msecs.	t	2.59	2.07
	p <	.02	.05
600msecs.	t	2.10	1.50
	p <	.05	0.2
2000msecs.	t	1.60	1.06
	p <	0.2	0.3

TABLE 25: 'T' Tests - significance levels, Experiment 7.

(2000 msec.) interval. Although this is 1000 msecs. less than the longest previously used interval, the result is quite compatible with the known decay in the value of visual information found to affect "same"-"different" judgements at intervals in excess of 1500 msecs. (Posner & Keele, 1967). As before, DS transitions are reliably slower than SD transitions, although both are equivalent in the sense that they both represent signal and response alternation. The directionality of this difference is clearly of some functional significance; possibly, an incongruous mediating response in the former case produces response-competition which would not occur in the latter case, where the mediating response is congruous with the appropriate correct response. Interestingly, the error-matrix procides additional testament to the undue difficulty of these DS transitions - for each RSI the number of errors in this category is substantially greater than the number in the SD category.

The SSe and DDe transitions are also of interest. Excepting the 2000 msec. interval, both transitions are significantly slower than their identical counterparts at each RSI tested ($p < .01$ in all cases). Furthermore, SSe transitions are, at each of the three shorter RSIs,

significantly slower than the DDi transitions. It is plain that identity between successive stimuli functionally overrides any intrinsic difficulty in classification which would otherwise be expected. In no case is there a significant difference between SSe and DDe, which is curious in view of the known superiority of "same" judgements. At the 200 and 600 msec. intervals, SSe transitions are in fact slower than DDe transitions - thus there is not even a tendency in the direction expected from discrete studies.

2. DDr transitions: the second interesting aspect of these data concerns the fate of the DDr transitions. The previous experiment suggested that facilitation was not tied to strict physical identity between successive stimuli but might, to a lesser extent, be found in successive stimuli with either one element in common, or alternatively both elements repeated in reversed spatial order. The reversed pairs appear to produce RTs which invariably lie between the identical and equivalent transition RTs, for each RSI. They approximate neither to identical nor to equivalent categories either in RT or in error incidence; errors to reversed pairs are always greater in number than those to identical pairs (by a factor of 4 to 5 times) and always less in number

than those to equivalent pairs (by a factor of 2 to 3.5 times). The error RT table, however, provides curious evidence to support a claim of greater similarity between DDr and DDi, than between DDr and DDe: Table 24 shows that error RTs for DDi and DDr transitions are uniformly slower than the corresponding correct RTs (a characteristic which has occurred before as a feature of identical signal transitions), whereas, for the DDe transitions, the reverse appears to be the case: correct RTs are always slower than the corresponding error RTs.

The results of the "T"-tests (Table 25) demonstrate the existence of statistical differences between the identical and reversed transitions and between the reversed and equivalent transitions. It was suggested above that support for the existence of higher order processes, beyond the simple physical identity match, as a basis for facilitation, would come from the finding that such facilitation was resistant to the introduction of conditions which destroy the relative availability and advantage of visual information. In the present experiment, the manipulation of RSI constituted the independent variable, and from Table 25 it can be seen that the significant differences between DDr and the two extreme DD transitions

are maintained at a confidence level of $p \leq .05$ or greater for 200 msecs. RSI although the appear to be reduced at the 600 msecs. interval and abolished entirely at the 2000 msec. interval. The Neuman-Keuls analysis (Table 24 a-d) tells much the same story. The conclusion that the DDr facilitation is tied to some storage dependent factor seems inescapable. However, exactly what this factor is, requires further detailed experimentation to determine. What is clear is that the results seem to scotch any model relying on straightforward visual persistence.

SUMMARY AND CONCLUSIONS

The two principal aims of the experiments reported in this chapter have been (i) the exploration of the analogy suggested by Entus & Bindra (1970) between the differences between "same" and "different" responses in discrete RT tasks and the differences between signal repetitions and non-repetitions in serial RT tasks, and (ii) to study the contextual effects of seriality in more detail.

(i) On its simplest interpretation, the analogy between discrete "same"-"different" effects and repetition effects

would lead to the prediction that all transitions between "different" signals would be slower than all transitions between "identical" signals. This was consistently found in every experiment. However, the results were not entirely without complication: it was found that "different"-"same" signal transitions were uniformly slower than "same"-"different" transitions. This is particularly puzzling in view of the fact that the comparison represented by both transitions is the same, and therefore, on X:X-I comparison hypothesis, one would not predict a difference. It was suggested that a conflict in mediating response might account for this anomaly. The strong influence of seriality and context upon signal processing was demonstrated by the finding that identical "different" transitions were extremely fast, and in fact generally faster than "same" responses which came as the second member of a non-identical transition. Thus, the context appears to have convincingly overridden the normal pattern of RTs which might have been expected from consideration of purely "discrete" stimulus comparisons.

The manipulation of response-stimulus interval yielded results which were entirely consistent with what is known of the decay of visual information in

"same"-"different" comparisons (Posner & Keele, 1967). At RSIs in excess of 1500 msecs. the value of "identical" signal transitions was abolished altogether for both "same" and "different" comparisons (experiments 5b and 7). It thus seems that these phenomena are related to the processes involved in the visual storage of information, rather than the effects of response-repetition which had been suggested by Bertelson (1963) to account for this data.

(ii) The transitions between signals studied in these experiments provided some unexpected results: the difference between "different"-"same" and "same"-"different" transitions has already been referred to. The disproportionate difficulty of the "same"-"same" equivalent transitions, also requires mention: this difference - coupled with the difficulty of the "different"-"same" transitions, lead to the supposition of a mediating response which might interfere with the generation of the correct intra-signal response, and thus delay RT in cases where it was inappropriate.

The relative difficulty of "different"-"different" equivalent signal sequences compared with "same"-"same" equivalent sequences supports the explanation of the

search data presented in experiment 4, and explains the apparent reversal of the traditional "same"-"different" effect found there.

Further investigation of signal transitions in experiments 6 and 7 showed that significant facilitation occurred to sequences in which partial signal identity was preserved. Particularly, reversing the letters of a pair between successive pairs was highly facilitative. Experiment 6 also showed a gradation of effect with degree of stimulus identity. It might have been thought that partial replication of a stimulus would be productive of interference rather than facilitation, with a resultant delay in response, but this was clearly not the case. This data points strongly against simple stimulus identity - e.g. via adecaying memory trace - as being the active agent in producing sequential facilitation. It is unlikely, though possible, that subjects learnt to expect reversals in these experiments (despite their very low frequency of occurrence) and therefore were able to prepare for them; but even this would require explanation, since it has not yet been demonstrated that we are capable of generating visual transformations of stimuli - to the extent of producing reversals. This explanation of the superiority of reversed signal transitions over non-reversed transitions seems less plausible in view

of the fact that significant facilitation for reversals was found at and RSI of 80 msecs., an interval at which preparatory generation would be unlikely to be possible.

The experiments reported in Chapter 1 supported the notion of independent processes for "same" and "different" judgements; do the present serial data uphold this on the basis of the suggested analogy? The only pointer in the evidence to a truly qualitative difference between the processes for identical and non-identical transitions comes from the error tables (experiments 5, 5b, 6 and 7). These show that, with fast RTs for identical, or reversed signals, go relatively slow error RTs - that is to say error RTs are, unusually, slower than their corresponding correct RTs. This reinforces the contention that the determinants of stimulus facilitation and speed-error trade-off are not located at the same stages of the coding process, but, as yet, the data available, do not allow a more precise speculation about where they might be located.

CHAPTER 4 THE EFFECTS OF SERIALITY UPON HIGHER ORDER DECISIONS

Serial RT tasks, in which each individual event requires a separate response, demand of the subject a high level of efficiency in processing the information with which he is presented. To a certain extent, it is possible for the system to operate in such a way as to minimise the work-load, by taking advantage of overall probability structure of stimulus sequences, or, as the previous chapters have shown, by making especially rapid responses to certain kinds of signal transitions. However, it is not clear from the results of these experiments whether the substantial advantage gained in RT by directly repeating a stimulus occurs as a matter of cognitive strategy - i.e. a planned test to compare sequent pairs of signals - or whether it is a consequence of a repeated signal somehow "pre-empting" the normal processes of stimulus-response decision-making, and forcing the subject towards a repeated response. The results reported hitherto do not distinguish between these possibilities - although the low frequency of repeats hardly supports a model which incorporates a deliberate "cognitive strategy" to test for repeats; such a test would be both time-consuming, and in view of the high incidence of a negative

result, counter-productive. Further argument against this kind of model comes from Bertelson's (1961) data, which show a significant repetition effect at very early stages of practice.

The results of experiments 6 and 7, which demonstrated that significant facilitation occurs to reversed, or partially repeated stimuli, also makes any simplistic explanation of repetition effect data inadequate. The picture is clearly considerably more complicated than that proposed by Bertelson (1963) or, even in the recent account of Rabbitt (1971b), who suggests that subjects faced with a present serial stimulus, act to classify it, not simply by comparing it with its immediate predecessor but by searching through the last class of which the previous stimulus was a member. This "strategy" has considerable appeal for cases in which stimuli are invariably members of small and finite classes - even if these are relatively arbitrary, e.g. a subset of several neon lights - but is an unlikely alternative for stimuli, which are members of large, even though logically speaking finite classes, such for example as the class of all different upper case letter-pairs. So far, it is these large classes which have been used to investigate

serial effects, and it is therefore of interest to put the "search-through-last-class" hypothesis to a more direct test, by using serial stimuli which fall into small, delimitable, non-arbitrary classes. This is the aim of experiment 8.

EXPERIMENT 8 SUPERORDINATE CATEGORISATION IN A SERIAL TASK

There is already good evidence for supposing that the letter-digit disjunction is an effective basis for classifying information: in short-term memory tasks, grouped presentation of items by class, results in significantly better retention scores than random, or alternated presentation (Broadbent & Gregory, 1964; Sanders & Schroots, 1968), and, further, the classical span of apprehension is significantly increased by class-grouping (Warrington, Kinsbourne & James, 1966). Posner (1970) reports that the behaviour of subjects required to make superordinate categorisation of stimuli, depends upon the level of abstraction from the original stimuli at which they are operating. Subjects were told to classify letter-pairs as either both consonants or both vowels, or mixed pairs as either both digits or both letters. The main finding was that the introduction of acoustic similarity affected the consonant-vowel classification, but not the letter-digit performance. Posner concluded

that for the vowel-consonant classification, subjects "seemed to determine the letter name and then classify the name into its super-ordinate category" (abstract), whereas, for the letter-digit task, they "appeared to classify the visual form directly into one of the two superordinate categories without first obtaining the name" (ibid.). On the basis of this result, Posner suggested that some mechanism exists which enables subjects to test to see whether a signal is a member of a given perceptual class, or not.

Rabbitt (1967a) has shown that subjects can be trained, with practice, to classify individual stimuli as members of one class or another - a class being defined as an arbitrary subset of the total stimulus population - on the basis of the visual pattern of the particular stimulus. This, given the heavily overlearned nature of the letter-digit discrimination, might account for Posner's result.

Further supportive evidence for a functionally distinct letter-digit perceptual facility comes from visual search experiments: Brand (1971) reports that subjects required to scan displays of random consonants for a single target which could be (a) another consonant; (b) a given number;

(c) any number - or v.v. for displays of random digits, scanned faster for a number embedded in a letter display, or a letter embedded in a number display than for a target drawn from the same class as that of the background items. She concludes that "familiar categorisation of responses may be immediate and may provide the basis for the discrimination of relevant from irrelevant items in rapid visual scanning" (Abstract). The conclusion of these data, from several different kinds of experimental task, must be that whether through overlearning, or through some unspecified memorial link, simple binary categorisations, of single stimuli, are facilitated by the grouping of members of a common class. Experiment 8 was made to study the effects of transferring the effects of class membership from the tasks mentioned above, to a serial CRT situation. It enabled the examination of repeating not only a particular individual signal, but also of repeating the class from which it came, without direct stimulus reptation. If the trend suggested by the data summarised above is maintained it should be possible to demonstrate the effects of "class" or "rule" repetition, over and above those of repeating a single stimulus or response.

Subjects: 9 subjects - 5 male and 4 female, between the ages of 15 and 23, were employed singly for two separate, short, sessions of 20 minutes each. All subjects were right-handed and all were students either at the University or at local colleges.

Apparatus: The experiment was run under the control of a LINC-8 computer, the viewing conditions and display being identical to those used in the experiments reported in Chapter 3.

Material: Eight lists of 201 stimuli were compiled from the following population of alphanumeric stimuli: 2, 3, 4, 5, 6, 7, 8; C, D, E, F, G, H, K; B was omitted from the list, since it is known to be visually confusible with 8, and, on the computer type-face available, this low discriminability was accentuated.

The lists were constructed in such a way as to exclude the repetition of a single item on more than two successive trials, and of a given stimulus class (Numeral or Letter), more than four times in succession. Otherwise the occurrences of each stimulus class, each single stimulus, and, since there was one response for each class, each response,

were equated for objective frequency. This gave the subjective appearance of randomness, although the lists were clearly not truly random. Although repeats following repeats (e.g. CC66) were not allowed, direct stimulus repetitions comprised 24 of each setof 200 transitions, add were equally distributed between each stimulus class, and between the members of each class. Alternations between the two classes were also balanced.

Display: Each block of trials was prefaced by a circular fixation spot in the centre of the oscilloscope screen, the subject being given a verbal signal "ready" before this spot disappeared to be immediately replaced by the first stimulus. The response-stimulus interval was constant, fixed at 100 msecs..

Instructions, Design & Responses: Subjects were told of the stimuli he might expect to see, and instructed to make a common response to all members of each class - depression of the left-hand response button with the left index finger for letters, and the right hand response button for all numerals. Each experimental session consisted of four lists of 201 responses, with a short break between each list. The lists on the two sessions were completely different.

Results: Print-outs of the last two blocks of trials on each session were made for each subject - the first two blocks being treated as practice. After elimination of errors and the next three correct responses which followed, transitions between the stimuli were partialled out, by eye, as follows: (a) "letter-letter", identical stimuli; (b) "letter-letter", equivalent stimuli; (c) "number-number", identical stimuli; (d) "number-number", equivalent stimuli; (e) "letter-number"; (f)"number-letter". The mean correct RTs for these six categories were collapsed over two blocks for each subject, for each session, and the means of means for each category, are presented in Table 26. Also included in this table are the error-incidence and mean error RT data for each category.

Analysis: A three-way AnoVa (subjects x run x category) was performed on the data; the results are given in Table 27. This shows a highly significant effect of stimulus category ($p < .01$), but no effect of practice. A Neuman-Keuls rank-order analysis was made separately for each level of practice; these results indicate significant differences between LL (I), NN(I) and both LN-NL at a $p < .05$ level for both runs of practice - however, no other comparisons were significant.

Transition	L-L(I)	N-N(I)	L-L(E)	N-N(E)	L-N	N-L	
Mean Correct RT	418.1	427.1	479.4	465.2	542.2	533.7	
Mean Error RT	-	558.0	458.5	515.0	410.0	416.5	Run 2
Total no. of errors	0	1	17	14	37	35	
Mean Correct RT	411.9	410.1	457.1	460.5	492.6	497.1	
Mean Error RT	446.2	553.5	405.1	458.9	428.6	442.9	Run 4
Total no. of errors	4	2	25	24	22	26	

TABLE 26: Correct and Error RTs & Error Indidence for Experiment 8.

L - letter
N - number
I - identical
E - equivalent

Source	Sum of Squares	D.F.	Mean Squares	F/Ratio
Subjects	226926.40	8	28365.80	25.43 **
Run (2nd & 4th)	27.00	1	27.00	0.0016 NS
Transition	179042.30	5	35808.46	45.90 **
Subject x Run	13312.90	8	1666.41	-1.49 NS
Subject x Transition	31207.87	40	780.20	0.69 NS
Transition x Run	1372.25	5	274.45	0.25 NS
Subject x Run x Transition	44605.50	40	1115.14	-
Total	606494.22	107	-	-

** $p < .01$

TABLE 27: Analysis of Variance for Data from Experiment 8.

Since these show no significant differences between equivalent and identical stimulus transitions, although a clear effect of category is indicated in the AnoVa table, a Wilcoxon's matched-pairs analysis (Siegel, 1956, p. 75) was made to test the significance of the identical-equivalent and the equivalent-alternation boundaries within each level of practice. This analysis shows that for both runs I/E and E/A boundaries are significantly different at the $p<.01$ level.

Discussion: These results are relatively uncomplicated; repetition of a class appears to produce faster RTs than alternation between stimuli from different classes. However, in the design of the experiment, repetition of a class is confounded with repetition of a motor response, and it is therefore impossible to determine how much, if at all, the class-repetition effect is contributed to by the motor repetition which correlated with it. The important question is: "Is there a repetition effect produced by class-repetition, independently of pure stimulus repetition?". These results show some evidence for such an effect: although not as clear-cut as might have ideally been hoped for, the results suggest that equivalent stimulus transitions are less efficiently processed than identical transitions, but considerably

more efficiently handled than alternations. The error data support this conclusion: errors to equivalent transitions have only marginally faster RTs for block 2 (session 1), and either slower ("letter-letter") or equal ("number-number").RTs (session 2) to the corresponding cofrect RTs for that category. This is indeed no more than circumstantial evidence, since the relationship between error RT and transition category, while established in Chapter 3, is itself still in need of explanation. However, the differences in correct RTs between the three principal categories - identical, equivalent, alternation - are strongly in the expected direction, suggesting that, as for results derived from entirely different experimental paradigms, superordinate class can be used to provide a useful cue for rapid stimulus classification. These results also support the view, expressed above, that repetition effects are capable of no simple explanation, since they may depend upon factors far more sophisticated than simple stimulus:stimulu-one-back comparison. However, Rabbitt's "search-through-last-class" hypothesis does receive support from the data, in that the predicted facilitation of equivalent transitions occurs. The confounding of class alternation with motor response alternation makes any more detailed conclusions

from these data unwarranted. However, they provide data for the next experiment, which was made with the purpose of unconfounding these factors, and therefore discussion will be deferred until this has been described.

EXPERIMENT 9 FURTHER EFFECTS OF STIMULUS CLASS TRANSITIONS IN SERIAL CHOICE RT

The term "stimulus class" is ambiguous. The data summarised in the introduction to experiment 8 led to the conclusion that the sets of letters and digits represented classes which are immediately and reliably distinguishable for the purposes of classification and rapid information processing. It is clear that, while there is no "natural" distinction between for example digits and letters, practice and convention have driven an operational wedge between the two sets of stimuli, which has at least some functional significance. In this sense, digits and letters represent different "classes" of stimuli, since they are separable by a simple rule; furthermore, there is empirical evidence, of the kind already discussed, to give force to the view that this distinction has important functional significance. On the other hand, other recent experiments have made it plain that, for the purposes of stimulus classification, items which demand a common

response may exhibit properties which are not shown by items which have no such common bond (e.g. Broadbent & Gregory, 1962). Rabbitt (1967a) provided a convincing demonstration that relatively little practice is necessary to establish an empirically forged link between members of an arbitrary subset of stimuli designated as "targets" in a visual scanning task, even though these items were derived from the same semantic category (letters) as the items comprising the set defined as "non-target". Since it is with "functional equivalence" that experiments on repetition effects are concerned, this distinction - i.e. between <u>classes</u> (e.g. numbers, letters, names of pieces of furniture) and <u>sets</u> (e.g. an arbitrary subset of the alphabet mapped on to a single response key) - is of great interest. Although it is highly possible that sets might show no property which would enable them to be differentiated from the classes, given sufficient practice, the fact that such a difference exists, allows one to discuss data in terms of these two distinct concepts, without begging any questions as to how the differential arose.

The results of experiment 8 would have told a much more elaborate story had it been possible to divorce the effects of class repetition-alternation, from those of response repetition-alternation. Since this is not possible

from those results, a further experiment was made with the necessary refinements to make this empirical separation: four stimuli were arbitrarily assigned to each of two response keys; two of the stimuli on each key were numerals, and two letters. It is therefore possible to examine the relative effects of transitions between classes (i.e. between semantic classes), against those of transitions between sets (i.e. between the members of a particular set of stimuli assigned to a given response). Experiment 8 pointed to the fact that number following number transitions were faster than number following letter transitions, and mutatis mutandis for letters, but it was not possible to decide whether this was because the faster transitions represented a succession of stimuli belonging to a common class, or whether this was an artefact of a design in which classes were mapped onto response-keys in a simple one:one fashion.

It is known (Rabbitt,1967a) that the making of a common response to an arbitrary set of stimuli becomes easier, even if these stimuli belong to the same class as other stimuli requiring a different response. Experiment 9 therefore included provision for fairly extensive practice.

Subjects: 21 subjects were employed for 3 sessions of 30 minutes each. 15 subjects were male, and 6 female. All were right-handed and between the ages of 17 and 29. Most were students, some were laboratory staff.

Apparatus: All stimuli were presented by the on-line LINC-8 computer previously described.

Materials: Eight alphanumeric stimuli (B,C,F,K,2,3,5,6) were used, being assigned to two response classes in the following way: C,K,3,6 - B,F,5,2. Twelve lists of 201 single stimuli were compiled with the following composition and restrictions:

(i) No direct repetition of any signal; (ii) no more than four repetitions of any stimulus class in sequence - i.e. four numbers or four letters were not allowed; (iii) no more than three repetitions of any given response; (iv) each response class, and each stimulus within each response class, was equally represented in frequency, in each list.

Display, Instruction and Design: The display characteristics were identical, in every respect, to those used in the previous serial experiments. There were two principal

factors of interest in this study: firstly, the effects of practice upon transition RTs; secondly, the effects of response-stimulus interval upon repetition effects across categories. Each subject was run over 12 stimulus lists of 201 trials each, and three RSI values (80, 600 and 1500 msecs.) were employed. Subjects were divided into three separate groups, at random, one group being run at one single RSI throughout the experiment. Thus, within subjects comparisons across RSIs were impossible. It was intended to have five subjects in each group, but since the performance of some of the initial group of 15 subjects tested was so poor, either in respect of error-rate or standard deviation - based upon the criteria laid down for previous experiments - six further subjects were randomly assigned to replace those discarded, and run under identical conditions. There was therefore a final group of 15 subjects available for analysis.

Subjects were instructed in the working of the apparatus which concerned them, and told the stimulus-response assignment, which they were required to repeat several times over to E to ensure reasonable acquaintance; four blocks of 201 responses were then run, with a short break between each block.

Results: Analyses were made of the even-numbered blocks only in each subject's run of 12. This allowed the first and third blocks in each session to be discarded as practice - although the subject was of course not aware of this - which, in the case of the first block at least, it was hoped would eliminate any transient warm-up effects occurring at the start of each session. Thus only blocks 2,4,6,8,10 and 12 were analysed. The computer sorted correct RTs - errors - 3 having been rejected automatically - into six pre-coded transition categories, corresponding to the following stimulus-stimulus events:

1. Letter-Letter, repeated response (e.g.CK,FB or v.v.).
2. Number-Number, repeated response (e.g. 36,52 or v.v.).
3. Letter-Letter, alternated response (e.g. FK,CB etc.).
4. Number-Number, alternated response (e.g. 35,62 etc.).
5. Number-Letter, or Letter-Number,repeated response (e.g. C3,F2.).
6. Number-Letter, or Letter-Number, alt. response (e.g.C5,2K).

The means of RTs in each category were computed and printed-out by the computer, and then collapsed, by hand, within a subject and within a run, into four more comprehensive transition categories - the justification for this being that these latter categories combined represented common event-types, which from the previous experiment did not

differ significantly, and which were logically not distinct. Furthermore, this new categorisation is both a conceptually and empirically more economical description of the data:

1. Class repeated - response repeated (combines 1 and 2 above).
2. Class repeated - response alternated (combines 3 and 4 above).
3. Class alternated - response repeated (as 5 above).
4. Class alternated - response alternated (as 6 above).

This "rationalisation" is seen to be appropriate, and harmless, when it is realised that the collapsed categories combine only like transitions, but over the two classes of stimuli. The means for each category, at each level of practice, in each RSI condition, summed over subjects, are presented in Table 28.

Analysis: a 4-way AnoVa was made - RSI x subjects x practice x transition category, and the results are tabulated in Table 29. It can be seen that each main factor is highly significant ($p<.01$ in every case). The subject term is, however, meaningless, since the results were generated by three entirely distinct subject groups. A check on the validity of the remaining factors was made

80 msecs.

Run / Category	2	4	6	8	10	12
1.	560.00	513.55	494.00	450.65	457.80	485.30
2.	581.35	542.30	516.90	487.70	484.30	470.75
3.	590.40	534.50	495.30	468.10	461.50	443.60
4.	587.80	546.40	518.30	494.40	483.60	469.10
600 msecs.						
1.	574.90	508.40	476.10	444.95	443.60	418.95
2.	570.10	540.90	503.45	486.70	476.10	468.30
3.	572.60	522.50	483.10	468.50	454.60	430.60
4.	577.50	524.60	507.50	484.70	475.20	454.70
1500 msecs.						
1.	635.95	543.60	526.60	494.60	485.50	460.35
2.	663.50	573.50	547.45	541.80	523.50	500.50
3.	678.20	559.40	534.00	503.20	501.10	465.90
4.	671.10	575.00	556.90	545.80	511.80	497.00

TABLE 28: Means of means for each stimulus transition category at each RSI for 6 levels of practice - Experiment 9.

Source	S.O.S.	D.F.	Mean Square	F/Ratio
A	178733.50	2	89366.75	32.59 **
B	280648.80	4	70162.21	25.59 **
C	813737.70	5	162727.50	59.36 **
D	63342.60	3	21114.20	7.70 **
AB	40703.78	8	5087.97	1.85 N.S.
AC	26202.25	10	2620.22	.955
AD	908.33	6	151.38	.066
BC	79892.25	20	3994.61	1.457 N.S.
BD	5157.14	12	429.76	.156
CD	10620.64	15	708.04	.258
ABC	118118.30	40	2977.96	1.096 N.S.
ABD	[illegible]	24	689.39	.251
ACD	4784.42	30	158.28	.057
BCD	11703.65	60	195.06	.071
ABCD	328981.50	120	2741.51	
TOTAL	2046934.00	359		

** $p < .01$

A - Response Stimulus Interval.
B - Subjects.
C - Practice.
D - Signal Transition Category.

TABLE 29: 4-way Analysis of Variance - Experiment 9.

16545.53

by performing separate AnoVas for each RSI. In every case, both practice and transition category were significant at the $p<.01$ level - confirming the original analysis. A further AnoVa was performed on the means given in Table 29 (category x practice x RSI), and the resulting table is reproduced as Table 30. In addition to the $p.<01$ significance level achieved by each single factor, the interactions between RSI and practice, and category and practice, also reach the same confidence level. The contribution of the illegitimate "subjects" term in the original analysis therefore seems to have obscured these effects. A further more detailed analysis will be referred to below.

Discussion: The main question which this experiment was designed to answer is whether the classification of a stimulus is determined to a greater extent by the class from which it comes - e.g. number of letter in the present case - or by the set (frequently arbitrary) of which it is a member, and within which a common response is demanded. Broadbent & Gregory (1962) produce evidence to suggest that stimuli which require a common response are functionally equivalent, in the sense that, whatever their actual differences, they will be treated alike. In their experiment, upper and lower case town names were

Source	S.O.S.	D.F.	Mean Square	F/Ratio
A	35730.65	2	17865.32	536.96 **
B	165167.70	5	33033.54	992.87 **
C	12109.64	3	4036.55	121.32 **
AB	5382.69	10	538.27	16.18 **
AC	159.73	6	26.62	.80
BC	2224.92	15	148.33	4.46 **
ABC	998.12	30	33.27	
TOTAL	221773.50	71		

** $p < .01$

A = Response Stimulus Interval.
B = Practice.
C = Signal Transition Category.

TABLE 30: 3-way Analysis of Variance, Experiment 9.

found to be equivalent for the purposes of sorting by name. Broadbent & Gregory did not, however, control for the proportion of repeated stimuli between the lists which subjects were asked to sort, so subtle differences may have escaped detection in a relatively clumsy type of task. Rabbitt (1967a) also showed the possible effect of treating a "set" of stimuli as equivalent, in a visual search task. His results led him to conclude that the stimulus equivalence between members of a set of stimuli which are grouped under a single response, is to be regarded as representing a more efficient perceptual classification with increased practice, rather than a greater improvement on the motor side of the decision. The common conclusion of both these experiments is that stimuli which, for the purposes of a particular task, are to be classified alike, by a common response, will be more easily and rapidly dealt with than comparable stimuli not requiring an identical response. In Broadbent's task, the stimuli involved were visually dissimilar but had the same name; in Rabbitt's experiment, the target stimuli were simply an arbitrarily chosen sub-class of a given population all of which were members of a common semantic class (letters). In this present experiment, stimuli are indeed arbitrarily combined into response-classes, but these classes are defined by no

common characteristics - their members are neither visually similar, nominally similar, or of a single semantic class. It is therefore possible to use the data as a strong test of the "perceptual equivalence" hypothesis, or of any particular version of it - e.g. Rabbitt's "search-through-last-class" hypothesis.

To determine the nature of the differences between categories at a single level of practice within a single RSI, "T"-tests were made on category pairs. The data from the first and last blocks analysed (i.e. the 2nd and 12th runs) at each RSI were tested for significant differences between stimulus-transition categories, by comparing category means in pairs. Six comparisons were made for each condition - there being six possible pairings of four categories. The results of these comparisons are given in Table 31.

The AnoVa represented by Table 30 indicates clear effects of both transition-type and of practice, and also a strong interaction effect between them ($p \leq .01$). The "T"-test results allow something more specific to be said about the nature of this interaction: for both of the shorter RSIs, the only categories which differ significantly on the first run analysed are categories 1 and 2, and 1 and 4 - i.e. there are differences between

	Comparison	Run 2	Run 12
80 msecs.	1 - 2*	$p < 0.05$	$p < 0.05$
	1 - 3*	NS	NS
	1 - 4*	$p < 0.025$	$p < 0.05$
	2* - 3	NS	$p < 0.05$
	2* - 4	NS	NS
	3 - 4*	NS	$p < 0.05$
600 msecs.	1 - 2*	$p < 0.05$	$p < 0.025$
	1 - 3*	NS	NS
	1 - 4*	$p < 0.05$	$p < 0.05$
	2* - 3	NS	$p < 0.05$
	2* - 4	NS	NS
	3 - 4*	NS	$p < 0.05$
1500 msecs.	1 - 2*	NS	NS
	1 - 3*	NS	NS
	1 - 4*	NS	NS
	2* - 3	NS	NS
	2* - 4	NS	NS
	3 - 4*	NS	NS

TABLE 31: T-Test result for data from Experiment 9.

X indicates the slower member of each pair.

transitions between items of the same semantic category requiring a repeated response, and transitions between items of the same category requiring an alternated response, and also between same-category, same-response transitions and transitions which require both altered category and altered response. In short, the data suggest that the repetition or alternation of the response is a dominant factor early in practice. There is no difference between transitions which require a repeated response, even though they may represent differences in semantic category transition. Later in practice, however, the picture changes sharply for both the 80 msec. and the 600 msec. RSI: there emerges a significant difference between transitions of categories 2 and 3 ($p < .05$), and also between categories 3 and 4 ($p < .05$). In the former case, category 2 is uniformly slower than category 3 - that is to say, response repetition, given semantic category alternation, is slower than response alternation given semantic category repetition. In the latter case, where category 3 is significantly faster than category 4, it is clear that, given equivalent category transitions, response repetition is faster than response alternation.

These results do not present as clear a picture as had been hoped for. Ideally, the early stages of practice might have been expected to show that semantic class was a significant contributor to RT, whereas, later in practice, when the letter-number pairings which constituted the response classes in this task had become more familiar, effects of perceptual equivalence, and thus of response class might be expected to appear. However, the results do present some features of interest:

(i) At all levels of practice, category 2 appears to be disproportionately difficult; that is, a repeated semantic class, coupled with an alternated motor response, is as difficult as alternation of both class and response. This suggests that, even if the conclusions of Posner (1970) and Brand (1971) are acceptable and applicable to the present context, and that the semantic category of a letter or digit is immediately available to the analysers without having prior recourse to the identity of the individual stimulus concerned, this facility is unhelpful, in terms of bringing about a reduction in RT to items which involve a repetition of semantic class especially where the response is alternated. The striking lack of difference between categories 1 and 3, at either level of practice, reinforces this contention. It is a possible

explanation of this that a repeated mediating semantic class response might conflict with the requirement to alternate the motor response. Thus, the expected empirical separation of semantic and response classes is distinctly lacking; there seems to be little or no value to the functional properties of the system of human stimulus classification of repeating items having a common overlearned class membership, over repeating those items which, although of a different semantic class, have the ad hoc property of belonging to the same arbitrary "set", tied to a common motor response. However, it is worth noting that the difference between categories 1 and 2 did reach the $p < .05$ level of significance for the 80 msec. and 600 msec. RSIs at both levels of practice (T-test), which points to a significant contribution from the motor response transition sequence to RT.

The RSI manipulation seemed to have no measurable qualitative effect on the relative difficulty of the different transitions. At 1500 msecs. RSI RTs were uniformly longer, although, as for the shorter RSIs, the 1 and 3, and 2 and 4 category comparisons failed to differ significantly. However, the differences between transitions 1 and 2 and 1 and 4, found at each of the shorter RSIs, were wholly abolished at 1500msecs. RSI. Several subjects

commented spontaneously on the difficulty of the task in the 1500 msec. RSI condition.

(ii) How does the "search-through-the-last-class" hypothesis fare in the light of these data? The evidence will not support an interpretation of class in terms of "semantic class", for the reasons given above, and therefore, the only plausible remaining interpretation, to save the hypothesis, that is consistent with the findings, is that of class in terms of "response-class". The relative ease of transitions between members of the same response class, compared with the difficulty of transitions which cross the classes, gives support to a "perceptual equivalence" hypothesis of the kind suggested by Rabbitt from his serial search data. However, unlike the search data, the present design, forcing one motor response per decision, cannot be used to disentangle the relative contributions of perceptual-class-equivalence and motor response repetition; the uniformly longer RTs for categories 2 and 4 are quite compatible with an explanation of serial effects in terms of motor repetitions and alternations, although the experiments reported in Chapter 3 make any single-factor theory nonsensical.

SUMMARY AND CONCLUSIONS

Experiments 8 and 9 were made to investigate the value of transitions between stimuli which are members of a common class. This adds a further possible basis for facilitation to those discussed and studied in Chapter 3 - namely, facilitation from a mediating response. In experiment 8 mediation based upon membership of a common semantic category was studied and found to be facilitative, although the design was deficient in the respect that equivalent transitions were correlated 1:1 with response alternations. In experiment 9 the effects of semantic class and response class were opposed by combining stimuli from two different semantic categories into a single stimulus set mapped on to a common response. The results showed significant contributions to the overall variance from both stimulus identity and motor response sequence. However, some support for the "perceptual equivalence" hypothesis was forthcoming in the shape of facilitation for transitions between members of the same arbitrary set.

The data from the two experiments reported in this chapter do not therefore unequivocally support the notion of facilitation from functional systems subserving

different semantic categories, which the data from other tasks and other authors might seem to predict (this is particularly striking in the failure of categories 1 and 3 to differ at either level of practice). It may be that a replication of experiment 9 with less visually bound or narrow categories would give a different result; object-naming categories might well demonstrate an effect of searching through the last class, in that, perhaps a bird-name following a bird-name might produce a faster RT than a bird-name following a furniture-name. However, this would not necessarily weaken the present conclusion, since it was part of the purpose of this study to oppose effects of response class and semantic category. Those who have discussed the effects of practice upon categorisation - cf. Rabbitt, 1967a especially - have made their point not of class per se, but of class in relation to a particular individual response, and, to this extent, their functional hypothesis is supported by these data.

CHAPTER 5 FILTERING AND RESPONSE COMPETITION AS RIVAL EXPLANATIONS OF A SEMANTIC STROOP EFFECT FOR TRANSITIONS BETWEEN EVENTS IN A SERIAL CHOICE RT TASK

In modelling the processes involved in complex stimulus discriminations, it is sometimes assumed that subjects are able to effectively filter out unwanted stimulus information, to the point at least that it will not interfere with a successful decision and thus, a fortiori, will not be reflected in the resultant reaction-times. Egeth (1966), in a study of multi-dimensional visual stimulus discrimination, makes this assumption explicitly, but from his own data is forced to abandon it: "it is clear that Ss did not completely ignore the irrelevant dimensions" - (op.cit. p. 250). In this particular instance, the irrelevant information was defined simply as those dimensions of a tridimensional stimulus which were not specified in advance as being critical for the decision the subject had to make.

In other cases however, while by definition "unwanted", the irrelevant information may have semantic associations with the information which is specified by the experimenter as relevant, and therefore be a potential source of active

interference with the production of correct responses. In these cases - e.g. the Stroop effect (Stroop, 1935) and analogues of it (Morton, 1969c) - the irrelevant information while being strictly separable from the relevant aspects of the stimulus - in the sense that it is possible to construct stimuli which contain the relevant information but not the irrelevant - has the additional property of generating responses which are drawn from the same subset as the relevant responses, and this may produce delays in responding, over and above the delays normally expected from the requirement to filter unwanted, but semantically irrelevant, information.

Thus, there are two distinct senses in which information may be said to be irrelevant: (i) by definition of the task, when there is no potentially competitive response from the relevant set; (ii) by consideration of the relationship between various aspects of compound stimuli and the responses required to some but not to others - in this sense "irrelevant" information is also semantically relevant. We have no grounds for supposing that subjects do not respond to the irrelevant information (sense i) in tasks of selective attention, or in tasks of multi-dimensional stimulus discrimination; we only know that these responses, if made, and of whatever nature,

interfere significantly less with correct decisions than irrelevant information drawn from a common semantic category to the relevant information. This is the fundamental observation of the Stroop effect.

It is not clear, however, whether the greater interference from the second kind of irrelevant material merely represents a quantitative change (e.g. a decrease in the rate at which information is being processed) - i.e. an increase in difficulty - or whether some additional factor - e.g. "response competition" - must be introduced to explain the findings. Data available from experiments by Klein (1964) and Morton (1969c) suggest that there exists a gradation of interference between the two stimulus dimensions in Stroop-type tasks, with semantic proximity, which indicates that the differences are explicable in terms of quantitative rather than qualitative factors in the situation.

The previous serial experiments described (Chapters 3 and 4) have demonstrated that correct responses to directly repeated items are significantly faster than responses to either alternated items requiring the same response, or alternated items requiring a different response. This is certainly true for tasks in which

all the items and dimensions are relevant to the response, or at least in which the irrelevant aspects of the stimuli are uniform throughout the experiment. Since it appears that the introduction of unsystematic irrelevant information in discrete tasks involves attentional factors, it becomes of interest to ask what might happen to such information in a serial task. Consider the example of a series of two dimensional stimuli, in which subjects respond to one dimension alone, and are required to ignore the other; there are four possible categories of stimulus transition:

(i) Relevant information repeated - irrelevant information repeated (RsIs).

(ii) Relevant information repeated - irrelevant information altered (RsId).

(iii) Relevant information altered - irrelevant information repeated (RdIs).

(iv) Relevant information altered - irrelevant information altered (RdId).

As long as the subject is required to make a response only to one state of the relevant dimension and consequently this dimension may adopt more than one state, these four categories exhaustively describe the possible transitions. However, with more than one response, these subdivisions become misleading unless they are further fractionated to take account of response transitions in categories (iii)

and (iv). Adding these we get:

(iiia) RdIs - response repeated; (iiib) RdIs - response alternated.

(iva) RdId - response repeated; (ivb) RdId - response alternated.

Categories (i) and (ii) remain unchanged, since it is assumed that repetition of the relevant information always entails repetition of the identical response. Since these categories will be frequently referred to below, they are tabulated completely in Table 32.

Of simple, one-dimensional stimuli occurring in a serial CRT task, the following is normally true: (a) repeated stimuli are responded to significantly faster than non-repeated stimuli (Bertelson,1961,1963 etc.; Rabbitt,1966 etc.; Chapter 3 supra); (b) Response repetition, even given non-repeated stimulus information, is faster than response alternation. However, these statements are only true of situations in which there is a reasonably equal distribution of repetitions and alternations. Heavy bias towards the incidence of alternations can reduce the facilitation of repetitions and vice versa (Kornblum,1969). Taking these into account, it is possible to generate a rank-order prediction of the relative speeds of the four main categories - and the two sub-categories,

Category	Relevant Information	Irrelevant Information	Response
i.	Repeated	Repeated	Repeated
ii.	Repeated	Altered	Repeated
iii_a.	Altered	Repeated	Repeated
iii_b.	Altered	Repeated	Altered
iv_a.	Altered	Altered	Repeated
iv_b.	Altered	Altered	Altered

TABLE 32: Signal - Response Transitions - Experiment 10.

assuming that theirrelevant information is successfully ignored: (1=2) faster than (iiia=iva) faster than (iiib=ivb). This, however, by definition, does not allow for any variation produced by the sequences of irrelevant information involved. If the irrelevant dimension is constantly and completely filtered, then the time taken for this extra operation will appear as an all-round increase in RT for all categories, over a control condition in which no irrelevant information is present; alternatively, in the unlikely event of sufficient channel capacity being available to allow the relevant information and the irrelevant information to be processed in parallel, and no interference occurs, the relevant RTs will remain unaffected by the irrelevant dimension. In practice, however, this is unlikely to occur, unless the irrelevant information is either highly unobtrusive, or else is totally unrelated to the relevant information in the stimuli. Given that an overall increase in RT is the most likely outcome of the introduction of irrelevant stimulus information in to a serial task, it is interesting to ask whether it is possible to predict the relative strengths of the effect upon RTs in cases when it is repeated between two successive stimuli, and cases where it is altered. For example, might one expect to find

significant differences between categories i and ii, or indeed between categories iiia and iiib, or iva and ivb? The fate of the irrelevant information is potentially complex - and, at present, there is no evidence available to suggest in which direction the effects might go. On the one hand, it is possible to envisage the repetition of irrelevant information leading to its more rapid and efficient encoding, and therefore to its more likely interference with the correct response (e.g. by priming a competing response). This logic follows those - e.g. Morton - who believe (a) that the Stroop effect is based upon response competition and (b) that response competition is maximised when the 'wrong' response - that to the irrelevant stimulus aspect - becomes available before the 'right' response - that to the relevant stimulus aspect - or when both responses are simultaneously available and the subject has to select one and ignore the other. On the other hand, one could equally plausibly imagine that the more rapidly it was recognised and identified, the more easy would it be to reject, on the ground that, in a sequential task, much as in a discrete task, one learns to identify a class of stimuli as "irrelevant" irrespective of their particular individual natures (Rabbitt, 1967a, provides an elegant demonstration of such a process).

In this latter case, it would be hard to account for any differences between the various categories partialling out the effects of the irrelevant information transitions. It is also possible to imagine a more complex account, in which the facilitation-interference due to the irrelevant information in a serial stimulus was systematically contingent upon the sequence of relevant information in which it occurred. This possibility is explored further below.

EXPERIMENT 10 THE EFFECTS OF SEMANTICALLY RELATED IRRELEVANT INFORMATION IN A SERIAL CHOICE RT TASK

Experiment 10 was devised to examine the effects on sequences of decisions, of transitions between information on two separate dimensions. In this task the irrelevant dimension was related to the relevant dimension in a specific and exceptional way - viz. they were both members of the same semantic category; some background review of the data relating to such stimuli is therefore demanded.

Morton (1969c) reports several short experiments in which subjects were required to deal, inter alia, with sequences of multiple identical numerals (eg. 555, 22222, 11 etc.) in one of two ways: in one condition, they had to

classify the numbers on the basis oftheir name alone (e.g. 44444 = "four"; 222 = "two"); in the other condition, the classification was based upon the total number of digits presented (e.g. 44444 = "five"; 222 = "three"). In either condition, it is possible to cause interference with the response by making the irrelevant aspect of the stimulus directly conflict with the relevant information; conversely, interference can be substantially reduced by using non-conflicting irrelevant information (e.g. AAAAA, or single digits). Morton's series of experiments provide an illuminating analogue of the well-known Stroop experiments. Since it is numerical material that will be used in this experiment, and since the models which will be discussed are derived from Morton's work, it is his results, rather than the more extensive but less systematic data on the Stroop material, which will be reviewed. Morton reports that:

(a) Both numerosity - i.e. counting - and name-identity tasks produce significant effects upon each other, whether subjects were required either to count the digits or to respond on the basis of the digit names, in a card-sorting task. Numerosity was easier when the items to be counted were letters or abstract figures; conversely, naming the

digits on the cards was significantly interfered with by there being several digits present - in other words "numerosity affected sorting by digit". However, in a tachistoscopic recognition task (cited by Morton, 1969c), Morton found recognition accuracy to increase (by 15% to 69%) when more than one digit was briefly exposed. This result presents no more than a superficial paradox, for there is no a priori reason to suppose that recognition accuracy and sorting responses should behave in a correlated fashion, with respect to potentially conflicting information. The following points should also be noted in discussing the relationship between these recognition and serial studies: (i) subjects were not timed on the recognition task, so RT interference values are not known; (ii) subjects were being presented with redundant information, which, in a tachistoscopic recognition task, is known to produce an increase in recognition accuracy. Morton's use of this control condition is therefore pointless in view of his main argument.

(b) The interference produced between numbers and numerosity information is significantly reduced when the numbers which have to be counted are not taken from the same set as those which are given as counting responses. This effect is also found in the standard Stroop literature

(e.g. Klein,1964).

(c) Regular patterning produced less interference than irregular patterning.

Morton produces a model to account for these results - this is reproduced in its graphical form in Figure 4. It incorporates separate channels for counting and digit-naming, which, in the early stages of processing, operate independently of each other. The outputs of these channels converge upon a system of units called "logogens", which are, by definition, "the place at which all information relevant to a particular response converges, regardless of the source of the information, and from which the response is made available" (1969c,p.338). There is a logogen unit to correspond to each of the possible outputs of the count and digit identity channels, but it is not made clear from Morton's discussion whether or not these are duplicated for each channel, or whether there is simply a single set to serve both channels. From the flow chart, the latter seems the most likely possibility. Morton does not say, however, whether it is semantic or task-defined relevance which regulates the accumulation of information at the logogens; but his logic suggests that there is competition for entry to the

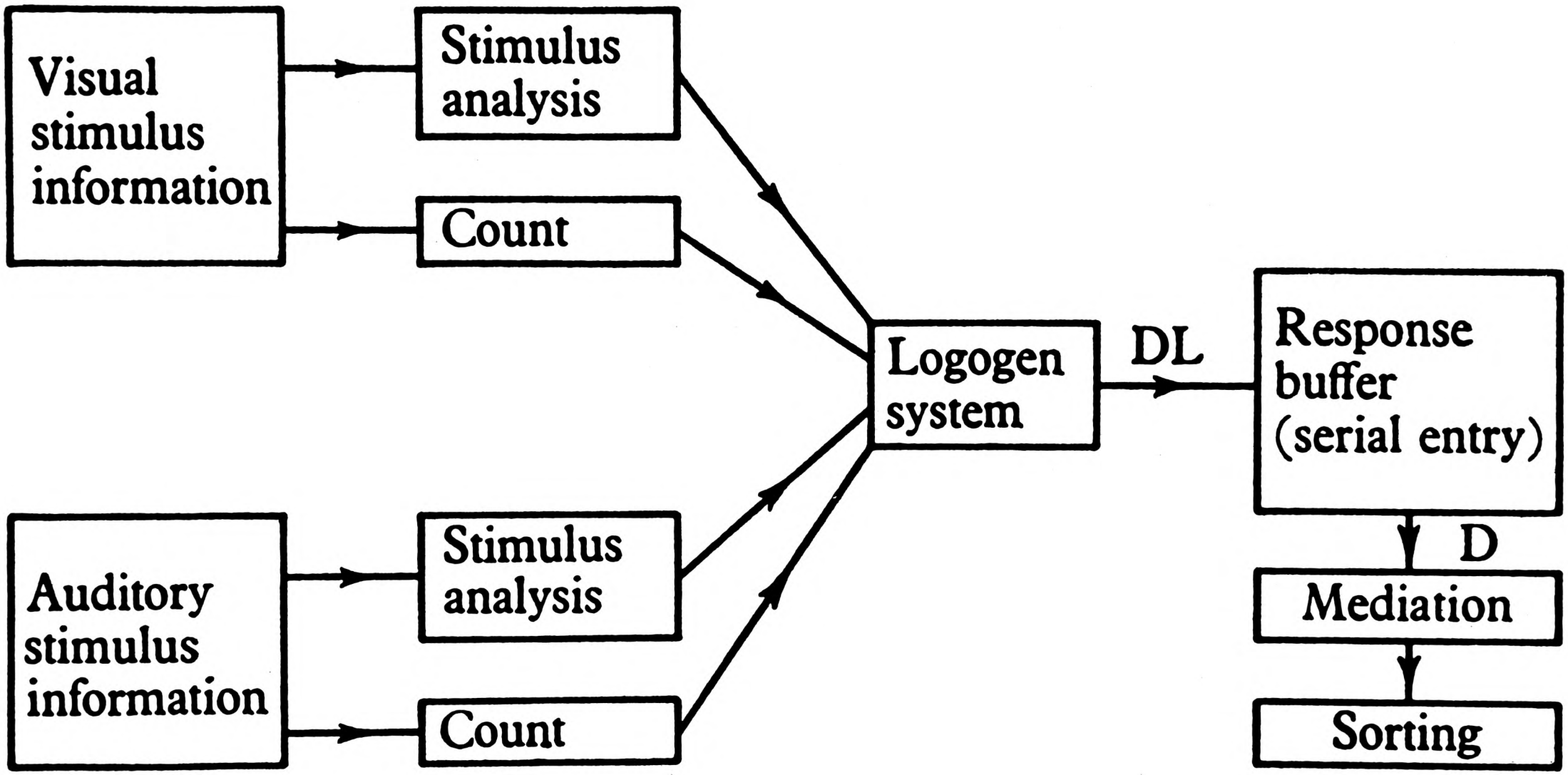


FIGURE 4: Model for Word Recognition
(from Morton, 1969_c)

response buffer system only when logogens of the same subset are receiving input.

It has been suggested, above, that part of the peculiarity of serial CRT effects can be accounted for by supposing that subjects make an automatic match between a stimulus and the one which immediately preceded it in the sequence. Since it is the purpose of the present experiment to examine the effect of seriality on Morton-type Stroop material, it is interesting to discover that several recent experiments have been made to substantiate the claim that the relative difficulty of dealing with the colour of the colour-word, as opposed to producing the word-name, could be either reduced, or in some instances eliminated altogether, if the task was altered so that subjects had to respond to the colour or the word by matching it to a standard, rather than reading the name or naming the colour. Pritchatt (1969) reports just such a result, in an experiment in which subjects matched colours of Stroop colour-words, to colour keys. However, the interference from the word was not completely eliminated, since colour patches were matched faster to the coloured keys than were the colours of the Stroop incongruous colour-words. Unfortunately, Pritchatt's

design included no control for the presence of the word, per se, so it is not possible to determine whether this difference between patches and Stroop material merely reflects the presence of alien material or whether it is a peculiar feature of the particular words employed. Norman (unpublished) incorporated this internal control into a matching task and found that the interference was substantially reduced.* Treisman & Fearnley (1969) used a card-sorting task in which groups of subjects were required to match either across or within the attribute of colour and name. They found that across attributes matching was significantly slower than within attribute matching. Again, the lack of a control makes it impossible to estimate whether or not there was any residual interference from the words per se, in the case where colours are matched to colour. Dorke & Calder (1969) report an experiment in which they find complete elimination of the interference when attributes are matched rather than named. Egeth, Blecker & Kamlet (1969),in an elegant experiment, found that "when Ss had to indicate if the colours of pairs of items from the Stroop test were the same or different, the usual interference effect was completely eliminated. However, when the verbal information consisted

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* These data are presented and discussed in the Appendix.

of SAME and DIFFERENT rather than colour names, interference was re-established" (Abstract). The authors draw two important conclusions from this study: (i) that "the perceptual comparison task does not eliminate interference simply by rendering Ss insensitive to the meanings of words", and (ii) that "the nature of the Ss task is in fact a determinant of the degree to which irrelevant information is interfering".

These two sets of experiments - those of Morton on the one hand, and those of Pritchatt, Treisman & Fearnley on the other hand, provide the background to experiment 10, which was made to study the effect of serial presentation upon performance with two-dimensional information.

Subjects: 18 student subjects took part, 7 male and 11 female. All were right-handed and had normal vision, adjusted, in some cases by correcting lenses normally prescribed for them.

Method: Subjects were presented with sequences of 207 incongruous name-number items on a cathode-ray display, linked to a LINC-8 digital computer, which, as before, both controlled the display and also recorded the responses and RTs to each stimulus. Two conditions were tested, one

in which subjects responded to the number of items presented (NUmerosity), and the other in which they responded to the identity of the digits presented, irrespective of how many identical ones there were on the display (Name Identity). In either case, the subject pressed one of two buttons to signal his response; each button having two number-responses assigned to it. There were four possible numerals - 2,3,4,5 - and four possible conflicting numbers of these which could appear - again 2,3,4,5; there were, therefore, four possible responses in each condition.

Within each main condition (NU and NI), subjects were divided into three groups of four, and randomly assigned to one of the three possible response assignments - 2 and 5 left - 3 and 4 right; 2 and 3 left - 4 and 5 right ; 2 and 4 left - 3 and 5 right. Each subject performed in two short - 20 minute - sessions, each consisting of 4 x 201 responses, with a short break between each block. In one session the condition was NU, in the other NI. Half the subjects did the NU condition first, half the NI condition. Each subject retained his response-assignment across the two conditions,

so that subject groups do not overlap between response assignments. Unfortunately, this makes it impossible to discriminate variance between the response assignments which is due to subjects from that which is due to the response assignments; however, this is of no importance since it is the between-categories variance within a given condition which is of principal interest.

Material: Four stimulus lists, consisting of 201 separate stimulus items each were constructed for each of the two main conditions. Each stimulus item contained from two to five identical numerals, these being chosen from the set 2,3,4,5. The sequences were constructed to conform to the following restrictions:

(a) The maximum number of sequent stimulus items repeating either the constituent numeral or the number of different numerals was three - e.g. the sequence 222-22-22222-2222 was disallowed, as was 22-33-44-55. Both these restrictions applied to each of the conditions.

(b) The number of items in which numeral and number were co-incident was kept to a minimum. These items were not eliminated altogether, but their proportionate incidence was very low (5 per 201 approximately).

(c) Sequences of responses were constructed so as to contain no more than three repeats. These "smoothed" lists were read onto magnetic tape for storage and presentation by the computer.

Display: The stimuli appeared, as for previous experiments, using this display, in the centre of a small oscilloscope screen, each sequence of 201 being prefaced by a small fixation spot. The response-stimulus interval was fixed and constant at 100 msecs..

Procedure: Each subject was told of the nature of the experiment and of the stimuli he might expect to see; he was also instructed in the use of the response buttons. He was then given his response assignment and asked to repeat it several times to E before the start of the first sequence to ensure that he knew it reasonably thoroughly. The sequences were then run, four to a session. The responses and RTs to each stimulus were stored by the computer for subsequent print-out and analysis.

Results: Unfortunately, the data of six of the 18 subjects had to be discarded on account either of excessive errors (over 10%) or for consistently excessively scattered RT distributions - the criteria for this was an arbitrary one, the cut-off point being fixed, as previously, at

100 msec.. The rejected subjects were replaced by others and the condition rerun. The replacements required were not disproportionately contributed to by any of the three response assignments, and therefore can be assumed to be a factor of subjects rather than conditions.

The final two blocks of stimuli for each condition were printed out, collapsed and analysed by eye into the 4 principal and two sub-categories listed in the introduction. Errors and the three sequent responses were, as usual, excluded from the analysis. The means of the four subjects' RTs in each category, by response assignment, are given in Table 33 and the mean error RTs and relative incidence in Table 34.

A three-way AnoVa - conditions x subjects x stimulus-transition-category - was performed on the data, and the results are given in Table 35. It is clear that both main factors are significant, although the interaction between them is not.

In addition, two separate AnoVas, one for each experimental condition, were performed to determine whether or not there was any significant effects of response

Response Assignment	Transition Category 1 (R_SI_S)	RR	2 (R_SI_D)	RR	3a (R_DI_S)	RR	3b (R_DI_S)	RA	4a (R_DI_D)	RR	4b (R_DI_D)	RA
1	413.269		444.279		465.879		546.037		478.437		535.497	
2	511.013		528.345		565.876		594.525		579.574		589.538	NU
3	430.132		456.876		490.291		547.575		510.129		532.463	
MEAN RT	451.471		476.500		507.349		562.712		522.713		552.499	

Response Assignment	Transition Category 1 (R_SI_S)	RR	2 (R_SI_D)	RR	3a (R_DI_S)	RR	3b (R_DI_S)	RA	4a (R_DI_D)	RR	4b (R_DI_D)	RA
1	407.397		435.658		477.452		531.385		490.965		527.125	
2	421.340		441.539		476.077		512.116		505.262		503.835	NI
3	400.431		419.592		461.469		511.044		460.526		495.285	
MEAN RT	409.723		428.929		471.666		518.182		485.584		508.748	

TABLE 33: Category and Overall Mean RTs in Msecs.. in Experiment 10.

Transitions	i	ii	iii_a	iii_b	iv_a	iv_b
No. of errors	4	12	42	87	64	65
Mean Error RTs (msecs.)	479.0	397.5	427.3	456.6	442.1	479.4

Error data - NAME IDENTITY

Transitions	i	ii	iii_a	iii_b	iv_a	iv_b
No. of errors	3	14	54	78	70	69
Mean Error RTs (msecs.)	542.7	408.3	477.4	501.4	493.7	500.6

Error data - NUMEROSITY

TABLE 34: Error incidence & mean error RT - Experiment 10.

Source	Sum of Squares	D.F.	Mean Squares	F/Ratio
Condition (NU vs. NI)	65877.78	1	65877.78	13.58 **
Subjects	213667.80	11	19424.35	-
Transition Category	220088.50	5	44017.70	66.06 **
Condition x subjects	53344.75	11	4849.81	-
Condition x Transition	2815.62	5	56.31	0.19 NS
Subjects x Transition	36649.25	55	666.35	-
Condition x Subjects x Transition	16633.00	55	302.42	-
Total	606545.70	143	-	

** $p < .01$

TABLE 35: Analysis of Variance, Experiment 10.

assignment on RT - particularly in view of the fact that on one assignment (2 & 4 - 3 & 5) subjects could have become more efficient by responding on the basis of the superordinate categorisation of "odd-right; even-left". However, these analyses revealed no significant differences between the response assignments, and it was therefore felt to be justified to treat the different response assignments as equivalent for the purposes of the Neuman-Keuls analyses that were also made. These analyses were performed separately for each main condition (NU and NI) on the mean of subjects' means over all 6 categories - thus, 6 cells in all, to be tested for significant rank-order differences. The results are presented in Table 36 (a & b). T-tests were also performed on the subject means to determine significant differences between identical transition categories across the two different conditions and to test for significant differences between categories which were non-significantly different in the rank order analyses.

These data are relatively complex, and it is therefore worth noting some of the salient features which appear from them:

Category	1	2	3a	4a	4b	3b
1	-	N.S.	**	**	**	**
2		-	*	*	**	**
3a			-	N.S.	N.S.	N.S.
4a				-	N.S.	N.S.
4b					-	N.S.
3b						-

a: NUMEROSITY

Category	1	2	3a	4a	4b	3b
1	-	N.S.	**	**	**	**
2		-	*	**	**	**
3a			-	N.S.	N.S.	*
4a				-	N.S.	N.S.
4b					-	N.S.
3b						-

b: NAME IDENTITY

TABLE 36: Neuman-Keuls Analyses for 2 Experimental Conditions - Experiment 10.

** $p < .01$
* $p < .05$

1. For all categories of transition, NU RTs are significantly slower than NI RTs ($p<.01$ in each case). A simple subtraction of the gross totals for corresponding categories in each condition yields the following absolute differences (NU - NI): i. + 501, ii. + 530, iiia. + 546, iiib. + 515, iva. + 488, ivb. + 524. Thus the greater difficulty of numerosity appears to hold for all categories of transition, and does not seem to covary with their intrinsic difficulty.

2. Repetition of the relevant information is highly facilitative: R_sI_s is the fastest transition for both NU and NI conditions. However,

3. This facilitation is markedly reduced for both conditions if the irrelevant information is not also repeated: R_sI_d is significantly slower for both NU and NI ($p<.025$ and $<.05$ respectively). Conversely,

4. If both relevant information and response are altered, then repetition of the irrelevant information is inhibiting i.e. negatively facilitative. This is shown in the small, but non-significant, negative difference between transition categories iii_b and iv_b - 104 msecs. for NU and -113 msecs. for NI. However, where the response, but not the relevant information is repeated, repetition of the irrelevant

dimension is facilitative (iii_a is just significantly faster than iv_a by T-Test; p.05 for both conditions).

5. The rank-order of categories is the same for both main conditions - viz. i - ii - iii_a - iv_a - iv_b - iii_b - this confirms the order predicted in the introduction.

6. Repetition of a response always produces a faster RT than response alternation: iii_a is faster than iii_b, and iv_a faster than iv_b ($p < .025$ in each case by T-Test). This is so for both conditions, despite the fact that each pair represent equivalent stimulus transitions.

7. A strong effect of strict stimulus identity between successive stimuli is evident. R_sI_c is uniformly faster than R_sI_d. The data do not permit the relative contributions of stimulus change per se, and the presence of irrelevant information to this difference to be assessed, but it is reasonable to assume that since irrelevant information is present in both the transitions of category i and category ii, the difference between them reflects in some measure the change from one stimulus to another which is visually dissimilar.

8. The differences between iiia and iiib, and those between iva and ivb are substantial and consistent for both conditions, and must therefore be taken as a measure of the time required to programme a new response.

Discussion: The present experimental paradigm represents a collapse of two quite distinct effects: (i) the effects of seriality, which have been reviewed in the introduction and studied in Chapters 3 and 4. Its principal characteristics are by now sufficiently familiar to make further exposition unnecessary. (ii) The Stroop effect: each individual item in each list used in this experiment contains both a name-identity and a numerosity value, which are known to interact. This experiment, then, makes a single paradigm from two different sources. It is true that under normal conditions of administration, the Stroop effect, or Morton's analogue of it, is a serial affair, in that it always involves either serial naming or else serial card-sorting. The seriality in these cases, however, is only employed as a convenient means of collecting several responses which are assumed to be totally independent of each other. These results question the veracity of this assumption, but the point of raising it here is to emphasise that the "seriality" per se, of card-sorting experiments in general, and Stroop experiments in particular, is not taken as contributing significantly to those results. They are taken as being generated exclusively from the individual stimuli

and not from the way in which they happen to be presented. The advantage of the present method of presenting the stimuli is that the use of a system of continuous recording for each event makes it possible to partial out transitions effects - a facility which was not made use of by Morton.

1. Serial effects: A question, to which the data of this experiment provide a tentative answer is that of the fate of information from an additional irrelevant dimension in a serial task. Discrete RT experiments - e.g. Corcoran,1967; Egeth, 1967 - suggest that it is difficult, if not impossible, for subjects to completely filter irrelevant information when making direct stimulus comparisons. Since there is no control for establishing a baseline for responding to name or number alone, there is no means of telling whether or not the transition category 1 RTs - where both stimuli are identical on both name and number dimensions - are contributed to by the very presence of the irrelevant dimension. Some clue may be had from the fact that there is a significant difference between the means for NI and NU for this transition ($p < .01$, "T"-test), although it is true that for each category, NU RTs are uniformly slower than NI RTs.

Identical stimulus transitions having relatively rapid RTs cannot therefore be produced by the simple strategy of making a repeated response on recognising that the present stimulus is in fact identical to its predecessor. A complication is introduced into this argument by the fact that not only were the irrelevant aspects ofthe stimulus of the same semantic class as that of the relevant dimension, but also by the fact that the responses appropriate to the irrelevant information are also appropriate to the relevant information. Where this is not so - cf. Morton, 1969c - the absolute magnitude of

	NU	NI	
Relevant information:			
iiia - i.	***789 (65.75)	***744(62.0)	Response
iva - ii.	* 554 (46.1)	***638(53.1)	Repeated
Irrelevant information:			
iia - i.	** 299 (25.0)	* 270(22.5)	Response
iva - iiia.	* 166 (13.9)	* 164(13.7)	Repeated
ivb - iiib.	NS-104 (-8.7)	NS-113(-9.4)	Resp.Non-Repeated

*** $p<.01$

** $p<.025$

* $p<.05$

TABLE 37

This table represents aggregate differences, and these must be divided by the number of subjects (12) to give the actual mean differences (in msecs.) to mean RTs. It can be seen that there is a considerable measure of variability in these differences which suggests that the effects of the irrelevant information are not simply additive, but rather depend upon the transitions involved

in the particular subtraction made. However, the effects of altering the value of the relevant dimensions are predictably greater than those of altering the value ofthe irrelevant dimension. The single exception (viz. the negative difference between categories ivb and iiib) will be discussed below.

2. The Stroop Effect: however, the special and peculiar relationship betweenirrelevant and relevant information makes the situation more complex. If, as the results of Chapter 3 suggest, each new stimulus is compared automatically with that which immediately preceded it, then, as Treisman & Fearnley, Pritchatt and Norman, have found, the interference from the irrelevant channel should be either substantially reduced, or alternatively eliminated altogether. The pattern of RTs makes it clear that on the contrary, the irrelevant source is capable of exerting significant differential effects upon RTs, depending critically upon the transition in which it occurs. This suggests that the hypothesis of a simple comparison between N and N-I is less than adequate.

These data are consistent with the following conclusions:

1. The RT increase observed when irrelevant information changes on successive trials is markedly less than that observed when the relevant information changes.

2. There appears to be no significant difference in the effects of repeating - or alternating - the value of either the relevant or the irrelevant dimension across the two main conditions. However, although numerosity responses are uniformly slower than name-identity responses, the effects of repetition or alternation of either information source appear to contribute equally to both. That is to say, the repetition-alternation effects found are internal to each task, and are not qualitatively affected by intrinsic task difficulty.

3. Where irrelevant information is repeated, but both relevant information and response are changed, there appears a small, but distinct, trend in the direction of increased RT, compared with the case where both types of information and the response are altered (ivb - iiib). This is the direct compliment of a result of Rabbitt's (unpublished) in which alternation facilitation was found when the irrelevant dimension changed in value.

The interest in these data lies not in the presence of interference, but in its pattern. It is not necessary that irrelevant information should be interfering, although it is usually the case that information which interferes with the production of a response is irrelevant. On rare occasions, however, one relevant source may interfere with another - for example when visual inputs interfere (binocular rivalry) or when sensory sources interact (masking). The present issue concerns the level at which irrelevant information, which is also semantically relevant, produces delays in RTs. There is a direct conflict between the two kinds of input - what is its nature?

Interference can occur at either of two basic stages of stimulus processing: at the input level, or at the response selection and programming level. If the unwanted information is filtered at the first stage, then it has no opportunity to produce "response competition" which some (e.g. Morton) have suggested as the principal underlying mechanism of the Stroop effect. A previous experiment in this report has shown that facilitation in a serial task can occur as a result of repetition of a decision based upon a common rule - it is therefore likely that this provides a further basis for interference,

in that, as Morton's model for the Stroop effects suggests, mediating responses from different sources may converge upon a common decision point. Given that some form of match occurs between N and N-I, it is clear from the data that while the interference between the channels may be reduced, there is a residual interference which has to be explained. The analysis of variance tableshows the transition category to be a highly dignificant factor ($p<.01$), which must negate any model based upon the assumption that the irrelevant information influences RTs in a purely additive way (Sternberg,1969).

The pattern of facilitation shown by the data cannot adequately be accounted for by the factor of <u>stimulus</u> repetition alone - although it is clear that it contributes to the variance (e.g. ii - i). The difference, for example, between categories iii_a and ii_b, and that between iv_a and iv_b, make such a simplistic hypothesis invalid. Furthermore, these same differences point to a contribution from <u>response</u> repetition which cannot be ignored. Stimulus and response repetition factors account for several of the differences between transitions found. However, there are others which are not so easily explained - chiefly those showing significant differences between categories which involve a change in the irrelevant dimension:

iii_a- iv_a ; iii_b- iv_b. For example, the single difference between categories iii_a and iv_a - viz. the change in the value of irrelevant information - must necessarily account for the significant difference in RTs ($p<.05$; T-test). The fact that such differences appear can be contrasted with the findings, already mentioned, of Treisman & Fearnley (1969), Pritchatt (1969) and Norman (Appendix to this thesis), which show that the requirement to match, rather than to name, the colours of incongruous Stroop items reduces the interference or eliminates it completely. These results are not compatible with the supposition that subjects performing a serial task make a simple "same"-"different" type match between N and N-I. Clearly, if as seems likely from the earlier experiments of this report, such a match does occur, then the assumption that the irrelevant dimension affects all types of transition equally, must, from the present data, be abandoned.

The previous experiments in this report have suggested that facilitation between successive stimuli in serial choice tasks can occur at three separate levels of the stimulus-decision-response link; namely, from signal repetition, from motor response repetition, and (Chapter

4) from repetition of a decision based upon a common rule. Morton's model, however, suggests a fourth, quite distinct possibility: that is, repetition facilitation due to repetition of a mediating response (= insponse). There are two possible interpretations of the model, in this light:

(a) Where the mediating response corresponds to the result of a stimulus to stimulus one-back comparison. In this case the outcome of that match can be either positive ("match") or negative ("mismatch"). If, as Egeth, Blecker & Kamlet (1971) suggest, matches are "wholistic" for "same" judgements, but "featuristic" for "different" judgements, then the identity judgements should not be differentially affected by task. Since it clearly is affected by task - NU category 1 RTs are significantly slower than NI category 1 RTs - it is difficult to avoid the conclusion that some form of separate independent matching process occurs for each dimension of the stimulus. If, therefore, the comparison is a dimension by dimension operation, Morton's model, which incorporporates a channel for symbol-naming and a separate channel for counting, which, in the early stages of processing at least, operate independently

of each other, would predict two mediating responses (either "same" or "different") - one for each channel. Thus there would be four possible outcomes of a two-dimensional match: "same"-"same"; "same"-"different"; "different"-"same" and "different"-"different". If it is these match responses which are considered to be mediating the eventual overt response, then it is possible to account for the relative difficulty of certain transitions when the mediating responses from the two channels conflict. However, there are good reasons for dismissing this version of the model as it stands: (i) there is no evidence for separate mediating responses from the two channels, and, even if there were, it is unlikely that they would consist of anything more than a single "match"-"mismatch" outcome from a parallel comparison on both channels, with the values from the previous stimulus. (ii) Such evidence as there is (cf. Morton,1969c) suggests that the mediating response, as it is described in the logogen model, consists of a response <u>of the same kind</u> as the material which produces it. Thus, the comparison of two numerosity values which were identical would, on this version, produce as a mediating response, not the word "same", but rather the appropriate numerosity value.

For a non-identical transition, the model would predict a mediating response of the category and kind appropriate to the current stimulus. (iii) Morton (1969b) presents evidence from the behaviour of correlated information from two dimensions which strongly supports the view that number and name information from independent sensory analysers converges on a single dimensional point, rather than the outputs of separate dimensional mechanisms being combined.

(b) This suggests a second, more compelling interpretation of Morton's model: if, as the flow-chart Morton gives (Fig. 4) leads one to believe, there is only one set of logogens for number information, from any source, upon which both numerosity and name identity information converges, then repetitions of irrelevant information will disproportionately reinforce the logogen corresponding to the repeated item, and, since the level of information in the logogen is taken, ex hypothesis, as a direct determinant of overt response probability/strength, this repetition will tend to produce a premature and probably erroneous response. Equally, of course, repetitions of relevant information will reinforce the corresponding logogen and the ensuing repetition of the response made to the previous stimulus will be correct and fast. This provides

a clear prediction - namely that the incidence of errors should be increased for transitions which involve repetition of the irrelevant information, but alternation of the relevant information. Inspection of the error incidence table (Table 34) shows this prediction to be strongly corroborated; errors for transitions of category iii_b are far more common than for either category iv_b - where the irrelevant information is alternated, or for category iii_a where the response is in fact also repeated; in this latter case any error due to misperception or miscoding would not show up as an error if it issued in a repeated response, since this, given the transition involved, would, by coincidence, be correct.

The data accord well with the predictions from this second interpretation of the logogen model: (i) where both relevant and irrelevant aspects of the stimulus are repeated, a high degree of facilitation is found - viz. transition i, compared with all other transitions; (ii) where, on the other hand, both aspects of the stimulus are alternated - viz. transitions iv_a and iv_b, maximum interference is found. It is also the case that repetition of the irrelevant information is interfering when the relevant dimension is altered - viz. the disproportionate difficulty of category iii_b.

A subset of the transitions represented by a change on both dimensions, is that when the irrelevant information on trial N-1 becomes the relevant information on trial N. This category of transition provides an interesting and direct test of Morton's model: the prediction is unequivocal - since the appropriate logogen for the relevant dimension on trial N was already primed, by the irrelevant dimension on trial N-1 (although the subject was required to ignore it), the response on trial N will be facilitated, since this same logogen would again be primed - although now by the relevant information. Unfortunately, the very low frequency of such transitions in the present experiment made any analysis unfeasible. Such information would, however, provide a critical test between a model which postulated a single set of logogens, receiving input from both numerosity and name identity channels (as Morton's does), and an alternative model postulating two semi-independent sets of logogens - one for each channel. On the latter hypothesis, both sets of logogens would fire for each bi-dimensional stimulus, the interference being located at the point at which the outputs of the logogens both arrive at the response buffer. A future experiment must decide between these alternatives.

SUMMARY AND CONCLUSIONS

The experiment reported in this chapter investigates the influence of a semantically related irrelevant information source upon a relevant source in a serial choice RT task. Since it is likely that, in serial tasks, an automatic comparison occurs between a stimulus and that one-back in the sequence, and, since independent evidence suggests that for the material of the type used in this experiment, simple matches greatly reduce the level of interference from the irrelevant source, the expectation was that the irrelevant sequence would have relatively little influence on RTs. This was not the case. The sequence of irrelevant information was an important determinant of RT. If, as Morton suggests, unwanted information is not filtered at the input level, then repetition of irrelevant information should prime a competing logogen, which, in its turn, would prime a competing response.

The dats showed that facilitatim occurred from several different sources, but on the whole favoured the hypothesis that the interference from the irrelevant channel occurred at the stage of response-selection-execution, rather than at the stage of stimulus analysis. Two possible versions

of the logogen model were outlined, between which data were indecisive. The data imply that the irrelevant information is active beyond the stage of stimulus encoding, and to this extent support a response competition model of selective attention.

CHAPTER 6 IMPLICATIONS

The experiments reported in this thesis have derived, substantially, from two fundamental and connected premises: (i) that the "identification" of a stimulus depends upon two distinct operations - the analysis of the stimulus, and the programming and selection of a motor response; (ii) that these processes are not only logically distinct, but also capable of empirical separation. The former premise does not exclude the possibility of their being more than two operations involved in stimulus identification, but simply makes the point that there are at least two. In contrast to the relatively uncontroversial nature of (i), premise (ii) has been both strongly stated (Donders, 1868; Sternberg, 1969) and also strongly criticised (Rabbitt, 1971a). Although Donder's original statement of the idea of distinct stages of stimulus analysis, terminating in the execution of a response contains a serious logical fallacy, which has been rightly exposed (Rabbitt, 1971a, p. 259), the basic heuristic is capable of modification in a way which does not undermine the important insight. Sternberg (1969) argues that processing stages are in principle discoverable, and that, even if

against Donder's they are non-additive, it is possible to work out a systematic logic to determine the empirical relations between them. An elegant series of experiments by Briggs & his colleagues (Briggs & Blaha, 1969; Briggs & Swanson, 1969; Briggs & Swanson, 1970) has shown that it is possible to fractionate the parameters of equations describing empirical data, and to meaningfully relate the fractions to various aspects of information processing. This coverage of previous qualifications for premise (ii) is made because the premise is important as a justification of the methodology adopted in the present experiments. Were it the case that the stages themselves interacted significantly, then the methodology would become questionable. However, to the extent that it is supported by the data cited, the second premise allows for the several methods that it has been found useful to employ to achieve a valid empirical divorce between the various stages of the stimulus-analysis - response-selection process. Of these, two have been studied in these experiments:

1. The manipulation of stimulus parameters, under conditions of constant response probability. The argument is that, given constant response conditions, any differences betweendifferent stimulus presentation conditions must be due solely to stimulus variables.

2. The partialling out of different classes of stimulus decision and response transitions in sequential tasks. The fact that S-R events in a self-paced sequence are known not to be independent enables an empirical separation of the stages of processing which is also logically water-tight. Whereas in 1, it is assumed that events in a "discrete" RT task do not directly influence each other - although they may be collectively influenced by changes in some third factor (e.g. absolute probability or subjective expectancy) - in 2 an influence of event N on event N + 1 is expected, and indeed, underlies the logic of the method. By and large, the non-sequential experiments reported dealt with the more widespread "global" influences on performance, whereas the sequential effects studied were concerned with purely "local" effects, confined exclusively to the particular stimuli and responses involved in the various transitions examined.

The experiments reported in Chapter 1 (Experiments 1, 2 and 3) were concerned entirely with the effects of making systematic alterations in stimulus presentation, on RTs in a non-sequential binary decision task. Experiment 1 has strong logical and empirical connections with the task-paradigm used by Posner & his colleagues, and others. Making "same"-"different" judgements to pairs of sequential

single stimuli, where noise could be superimposed on either, neither, or both stimuli, represents a simple extension of the binary classification technique. However, it tested a condition which neither Posner nor subsequent investigators have studied; Posner used (a) masking fields interpolated between stimulus 1 and stimulus 2, and (b) other letters to "confuse" the visual representation of stimulus 2; (c) more recently, Kruger (1970c) has used bracketing lines to partially mask S_2. These manipulations represent the use of indirect noise on the stimulus. In experiment 1, (i) visual noise was directly superimposed on neither, either or both stimuli, and (ii) the form of the noise was distinct from any so far used in studies of binary classification. The results had three principal implications:

1. The existence of some form of stimulus pre-processing facility was strongly supported. It would beg the question to identify this with a "smoothing" operation, e.g. of the kind suggested by Sternberg (1967), where this is taken to mean the attenuation of the noise to allow the signal through. Although the duration of that process, as far as it could be estimated from experiment 1, was well within the limits of Sternberg's results, the data were quite compatible with the alternative hypothesis of some form of generation of a new visual icon

(cf. Posner, Boies, etc., 1969) which is then matched against the other stimulus. It is not very clear what would count as an empirical distinction between these alternatives; but it is clear that experiment 1 did not provide it.

There were, however, good grounds for dismissing a third hypothesis -namely, that some other transformation - e.g. acoustic encoding - is operative. The lack of difference between the "clear-clear" and the "noise-clear" condition for "same" matches makes this an unlikely alternative, in view of the fact that the former match is almost certainly a visual one.

The data from Experiment 1 were consistent with the assumption that the extra time required to process a noisy stimulus, would only increase RTs when the noise occurs on S_2. Noise on S_1 had no significant effect on RTs. Noise on both S_1 and S_2 however, increased RT less than noise on S_2 alone.

The hypothesis of an independent stimulus pre-processing device, was given additional force by the fact that, considering noise simply as a means of decreasing stimulus discriminability the results could not have been predicted. The significant difference between "clear-noise" and

"noise-clear" comparisons showed an order effect which is totally inconsistent with the noise = discriminability hypothesis.

2. The results also made clear the striking differences between the effects of noise superimposed on a stimulus and similar noise interpolated between two stimuli. Posner (1969) reports that, beyond mediating an all-round increase in RTs, interpolated noise had no significant effects on performance. The differences between the four stimulus categories used in this experiment underlined the difference. The result is in line with studies which differentiate forward and backward masking from superimposition masking as distinct qualitative phenomena (Kinsbourne & Warrington, 1962).

3. As well as supporting the dissociation of a preliminary pre-process from the stimulus comparison process, the qualitative differences between "same" and "different" stimulus categories reinforced the idea of a two-process model for binary classification - the genesis of which was reviewed in the introduction (pp. 57 et seq.). The result which was important in this respect was the distinct failure, at any of the three ISIs used, to find a significant difference between "same" RTs and "different"

RTs, for the presentations in which S_1 appeared in clear, and S_2 in noise. If, as Egeth & Blecker (1971) and Bamber (1969) suggest, some form of wholistic - possibly "Gestalt" - comparison is made between S_2 and S_1, then the RTs for "same" in the "clear-noise" condition would be increased. However, the failure to find this effect for the "noise-clear" comparisons weakens this hypothesis.

More convincing support for two-process models of "same"-"different" classification was however forthcoming from experiments 2 and 3. Both these experiments were made to study the effects of mixing easy and difficult discriminations in a single experimental list. In experiment 2, separate stimulus populations were selected to provide the easy and difficult letter-pairs. Two distinct effects were found: (i) RTs to "same" stimuli occurring in the visually confusible - i.e. difficult - list, were significantly longer than "same" RTs to stimuli in the relatively easy list. Curiously, from what is known of the effects of altering stimulus discriminability, "different" RTs were relatively less affected by the manipulation. (ii) Mixing easy and difficult items in the same list exerted a levelling influence - confusible pair RTs being reduced, and

non-confusible pair RTs being increased. (i) appears to be a "local" shift in the criterion for "same" matches; for, if the greater likelihood of a more difficult comparison occurring produced an all-round increase in caution, then the clear expectation would be that "different" RTs would be affected to the same extent as "same" RTs. That this was not the case supports the hypothesis of distinct processes for each decision.

Part, at least, of the results of experiment 2, could be explained by the supposition that subjects made use of distinctive features to differentiate easy from difficult discriminations. While it was not proved that they actually did so, the possibility prompted a replication and extension of the experiment, in which a fixed population of letters was used to compile both the easy and the difficult lists, and, in which both acoustic and visual confusibility were tested - each at two different stimulus-duration/inter-stimulus interval combinations. The use of a key-press response, rather than the voice-key used in Experiment 2, provided an additional modification.

The results of Experiment 3, however, substantially confirmed those of the previous study. Again, "same" RTs for the difficult list were found to be significantly

slower than those for the easier list. The identity of the "same" letter pairs for each list makes this result immune from the criticism that distinctive features might serve to distinguish the two stimulus populations. This can therefore only be an effect of the more or less difficult "different" discriminations. On a simple two-process model, it could be supposed that the difficulty of the "different" discriminations the subject was required to make led to an upward shift in the setting of his criterion for sameness. Even on the hypothesis of an "wholistic" comparison for "same" stimuli, it is reasonable to suppose that the more features "different" letter-pairs have in common, the more stringent the criterion for identity necessary to maintain a respectable error-rate. Since there were no significant differences between the error counts for the two conditions, some form of two-process model is consistent with, and supported by, the results.

Unfortunately, these data did not allow anything very specific to be said about which of the several alternative forms of two-process model was appropriate. What has been shown so far, is that the criterion for sameness and the criterion for difference are capable of independent

variation. The fact that one response - viz. "same" - could be disproportionately affected by manipulating the discriminability of the "different" stimuli, made the data incompatible with the hypothesis that although the criteria are independent, the processes are dependent, or linked. Thus, a corollary of the present findings was that there exist separate processes, as well as separate criteria. This excluded models like Nickerson's (1969) based on the idea of an information accumulator linked to a time-clock. The different betas for the "same" and "different" stages, demand not only that the processes be separate, but also that they be functionally independent.

The experiments of Chapter 1 demonstrated that it is possible to use the hypothesis of separate stages in the information processing of a single stimulus event as a methodological assumption, obtaining, under specific conditions, variations in RT to stimulus properties while the response-set remains constant. While the long inter-trial intervals forced by the practical difficulties of manual tachistoscopic presentation made contamination from sequential effects unlikely, the finding that the relative discriminability of one pair of stimuli can significantly alter RTs to other unrelated pairs, in a

given list, remains a tantalising one, for which at the time of writing there is no precedent in the literature. The finding, in Experiment 2, of a general levelling of RTs in the mixed list, suggested that some kind of "global" criterion shift had taken place. The results of experiment 3 however demonstrated quite another kind of shift, which is highly specific: namely, an upward revision of the "same" criterion resulting from a change in the discriminability of the "different" letter-pairs. A wider implication of Experiments 2 and 3 is that, even in a traditionally "discrete" RT task, individual S-R events may not avoid contamination from outside factors.

Experiment 4 was made as an attempt to examine the effect of converting a Neisser-type search task into a serial "same"-"different" task. It presents a further means of driving an empirical wedge between stimulus analysis and response production processes. Unfortunately the heavy bias towards one particular type of event on any given list makes simple comparisons between discrete and serial situations invalid. The experiment did however make two substantial points:

(i) There is no single test to allow the stimulus analysing mechanisms to answer the question "same or different?". Either one of two apparently separate tests, one for "same", theother for "different", can be applied to the line beingsearched. This conclusion is necessitated by the finding that search times for one type of stimulus list - i.e. where all the items are "different" pairs - is significantly faster than search through lists of all "same" items. The hypothesis of one single test would predict at best no significant differance betweenthe two lists - and at worst, per "same" searches to be faster than per "different" searches - the reverse result to that actually found.

(ii) The data suggested that the test that is applied to each item is not that appropriate to its absolute identity, but rather that appropriate to the class of the target being searched for. In other words, in line with Neisser's own results, the most efficient method of searching is to make the test for the presence of the target - a test for set inclusion.

The demonstration that there are two quite distinct tests - one for identity and another for difference, clearly demand the hypothesis of two correlative independent processes, discussed in the Introduction, and in

relation to the data of Chapter 1. It is evident that the classifications "same" and "different" are not simply the outcomes of the accumulation of information along a single continuum which is terminated when a state set by a single flexible criterion is reached. Were this so, it would be impossible to account for the difficulty subjects experienced in searching through "same" letter-pairs, compared with the ease of the opposite task.

A severe limitation of the serial search task in general, and of Experiment 4 in particular, concerned the failure to pick up data from individual transitions; in some cases, this disadvantage is outweighed by the elimination of variance attributable to motor response transitions, thus allowing any significant effects to be ascribed to factors involved in the processes of stimulus analysis and classification. The particular expense of this advantage, however, is the necessity of a heavy bias towards one type of stimulus item.

Experiment 5 was therefore designed to examine individual transitions between stimuli and responses using "same" and "different" letter-pairs in a sequential, self-paced task. The interest in the experiment lay in three connected areas:

(a) The nature of the sequential effects found.

(b) The extent to which these effects supported a two-process model of binary classification, and

(c) The extent to which they reinforced the analogy between repetition effects and "same"-"different" effects (Entus & Bindra, 1970). These facets of the results will be considered separately:

(a) In a serial RT task there are three main possible sources of facilitation: from the stimulus, from the decision and from the motor response. Stimulus effects were very clearly demonstrated - transitions between identical letter pairs - whether they were "same" or "different" were always significantly faster than transitions between non-identical pairs requiring the same response. This result contrasts strongly with the conclusions of those who claim that the major part of the variance due to S-R transitions can be accounted for by facilitation due to repetition of the motor response. Unfortunately, the design of the experiment did not allow the category of transition in which a repeated signal is accompanied by an alternated response - this would have given another direct test of the relative potentiation of stimulus and response repetition.

Repetition of a specific decision was not obviously facilitative in Experiment 5; moreover, since repeating a decision entailed repeating its correlative response, no uncontaminated assessment of this factor was possible. However, there were differences between "same" and "different" decisions: while the identical and alternated transitions were not significantly different across decisions (i.e. AA-AA was no faster than AB-AB, nor AA-CD faster than CD-AA), the equivalent transitions between "same" pairs were significantly faster than those between "different" pairs. Since both are logically identical in that they involve identical stimulus-decision and response transitions, the difference can only be accounted for by the hypothesis of a difference in the nature of the tests carried out in the two cases. This will be discussed further below.

In contrast to stimulus repetition effects, no response repetition effects were apparent. None of the data suggested that motor response repetition was significantly facilitative. "Different"-"same" transitions were not significantly slower than "same-same" transitions which contained non-repeated stimuli. More compelling

was the finding that "different"-"different" equivalent transitions were significantly slower than "same"-"different" transitions; the trend is not even towards facilitation for a repeated response.

The results of experiment 5 demonstrate unequivocally that stimulus repetition can be highly facilitative, and moreover, that in suitable circumstances can outweigh any effects of response repetition. The position taken by Bertelson (1963), that the "repetition effect" in serial self-paced tasks is principally due to the effects of repeating the motor response, is clearly not generalisable to the present set of data, or indeed, to others such as those by Lawrence & LaBerge (1956), LaBerge & Tweedie (1964), and Theios & Falmagne (1971).

Further, if one wishes to use the serial versus parallel jargon for discussing the data, then the strong point emerges that for serial self-paced tasks, the order of comparisons is not fixed but varies from trial to trial, based mainly upon the nature of the immediately preceding signal.

(b) Two-process models of "same"-"different" judgement gained considerable support from Experiment 5. The very striking difference between "same"-"same" and "different"-"different" equivalent transitions - the latter being significantly slower than the former, made it absurd to suppose that there exists some single process which is responsible for deciding between these alternatives. A further pertinent factor was the qualitative difference between the rank-orders of the transitions for each decision:

Same: Identical * < Equivalent = Alternated.

Different: Identical * < Alternated * < Equivalent.

* = $p < .05$

The characteristics of the identical signal transitions provided further evidence against any single-process hypothesis. These transitions were uniformly accurate and fast - a strongly atypical feature of serial responses. This trait separated them from every other transition in the sequence. Rabbitt & Vyas (1970) present a review of the evidence for speed-error trade off in serial RT tasks, and also a useful taxonomy of possible error responses. While it is impossible to speculate reliably about the aetiology of errors for the identical transitions, since

they were sufficiently infrequent to make statistical comparisons improbable, their constant trade mark - being slower than their correlative correct response - served to differentiate them from errors in the other categories, and indicated that they were errors of perceptual analysis (whether of miscoding or misperception it is impossible to determine) rather than errors of response selection or execution. The invariant combination of speed, on the one hand, and accuracy on the other, which occurred in Experiment 5, and in all the other serial experiments reported in this thesis, in conjunction with transitions involving repeated signals, suggests a qualitative difference in the way in which repeats are handled. If the same process were involved in dealing with repeated signals as with other signal transitions, then there would be no reason for not expecting the traditional speed-error trade-off function to appear, which it did not.

(c) It seemed, from all the available data, that in serial self-paced RT tasks, there is a definite influence of stimulus N on its successor, N - 1. Moreover, it also appeared that this influence was entirely independent of any strategies that subjects might individually have chosen to operate. This is so for two separate reasons:

(i) the trends reported are shown by all subjects alike; (ii) the very low proportion of direct stimulus repeats, relative to equivalent and alternate transitions, in Experiment 5, makes it very unlikely that some form of test for repeats is executed as a matter of premeditated strategy. This effect, which appears to be the result of some involuntary feature of the stimulus analysis behaviour, points to the autonomous nature of the recognition process in serial tasks.

Basically, there are two kinds of hypothesis to account for the facilitation due to identical stimulus transitions:

(a) According to the first hypothesis, the analysis of stimulus N is qualitatively identical to that of N-1, but since it is a direct repetition, it takes less time to execute.

(b) The second hypothesis is that there is some kind of direct comparison of each stimulus with its immediate predecessor; if the result of that comparison is "same", then the normal processes of response selection and programming are by-passed and the response to the immediately preceding stimulus simply repeated.

(a) will not do for several reasons: (i) the abolition of facilitation at longer ISI (e.g. Experiment 5b) suggests that short-term memory is critically involved in determining the degree of facilitation for any particular transition; (ii) the pattern of facilitation for non-identical transitions cannot be explained from this hypothesis; (iii) there is no independent evidence that directly repeating a given process will necessarily accelerate it; (iv) evidence from the present experiments - cited above - points to a clear difference between repeats and non-repeats.

Some form of hypothesis (b) therefore seems demanded. Bertelson's original version of it (1963) simply postulated an automatic N:N-1 comparison; Rabbitt (1971b, in press) has shown why this, as it stands, is inadequate. The present results support that contention. The main objection to Bertelson's version of the hypothesis is that it does not account for the definite and significant facilitation found with practice, to characterise transitions between non-identical stimuli which happen to be mapped onto an identical response. Rabbitt (1968, 1971$_b$, 1971$_b$) extended Bertelson's hypothesis to include a Sternberg-like serial

scan through the members of the entire class of which N-1 was a member. The viability of this hypothesis depends critically upon how one specifies "class"; if, in the present instance, the class of which the letter-pair AB is a member is equivalent to the heavily overlearned class of all upper-case letters, then the hypothesis is viable; if, however, perhaps more plausibly, it is defined as the class of all "different" letter-pairs, then the hypothesis will not do.

Entus & Bindra's hypothesis (1970) would explain the pattern of transition RTs by analogy with the existing literature on "same"-"different" judgements. Unfortunately, evaluation of the idea, for the present results, is complicated by the possibility of mediating responses from the N:N-1 match directly conflicting with the appropriate response from the presented letter pair. Clearly, all the identical signal transitions are consistently significantly faster than every other transition, but this alone is insufficient to prove Entus & Bindra's hypothesis. Although it can deal adequately with the identical transitions, it is the differences between the various transitions for which the N:N-1 comparison yields a "mismatch" decision, which make its

generality questionable. For example, the significant difference between "same"-"different" and "different"-"different" equivalent transitions is not easily accounted for. It is not made clear by Entus & Bindra, whether it is physical or functional identity which determines whether responses are speeded. A possible further test of the model would be a comparison between transitions involving identical, similar and dissimilar stimuli; comparisons between similar stimuli are known to be faster than those between dissimilar stimuli, if they lead to the response "same", but not if they lead to the response "different" (Sekuler & Abrams, 1968).

Experiments 6 and 7 were made in an attempt to delimit, more precisely, the exact basis for stimulus identity which leads to RT facilitation. The abolition of facilitation at 2000 msecs. (Experiment 5b) seemed to indicate that iconic persistence might be a possible factor in the repetition effect. However, the results of Experiments 6 and 7 did not confirm this expectation: transitions between directly reversed letter-pairs were found to be significantly faster than transitions between either pairs with a single letter in common, or pairs which were totally different. Position was found to be

a contributory factor - an identical letter in an identical position was more facilitative than an identical letter in a different position - but the much greater facilitation for reversed pairs in comparison with partially identical pairs argues against strict visual identity as the principal factor. Reversed pairs do not even appear, phenomenally, to be identical physically, and this seems to exclude simple iconic persistence as a major determinant of facilitation. The abolition of facilitation at the longer inter-stimulus intervals tested, does however suggest that some form of short-term memory is a contributory factor, but from the data collected, it is not possible to be more specific. It might be suggested that subjects, as well as testing for strict physical identity, also applied a test for physical reversal; there are good reasons why such a suggestion is probably false: either the test would be a deliberate one, planned in advance, or else it would be an involuntary feature of the stimulus analysing mechanisms. If the former then it is difficult, given the very low incidence of letter-reversals included in the lists, to explain why without exception every subject tested showed facilitation to reversals; if the latter, then one has to have a reason

for supposing that such a test is an integral part of stimulus comparison processes, and so far, there is no data available to show that this is the case. It is an unsatisfactory, although in the present context compelling, explanation of the finding, to attribute it to the repetition of a particular stimulus comparison. The reason that it is unsatisfactory, is that there is no evidence to indicate how such facilitation might be mediated. However, that the effect is genuine, is clear both from the significance levels obtained and, interestingly from the error RT data, which ally reversals with stimulus repeats, rather than with alternations, in that they exhibit the common distinctive features of slower error RTs than their corresponding correct RTs, and of great accuracy.

It is very clear, from the serial experiments discussed so far, that no single factor could be considered responsible for the several differences which exist between the various SR transitions. Given suitable circumstances, repetition of the signal, repetition of a particular decision, and repetition of a specific motor response, may all contribute to the total variance.

This does not mean that the stages in information processing are totally interactive, but rather that reaction times in serial tasks are frequently multifactorially determined.

In Chapter 3, it was suggested that part of the overall variance might be explained by the supposition that a mediating response from a comparison between the present stimulus and the stimulus one-back in the sequence, might directly conflict with the appropriate response to the present signal. Chapter 4 investigated the possibility that repetition of a mediating response might be a potential contributor to serial RTs. Experiment 8 made the simple point that transitions between stimuli belonging to the same semantic class - numerals or letters - were faster than transitions between signals of different classes. This confirms the wealth of data from other sources, reviewed in the Introduction and in Chapter 4 which shows that common class membership is an effective basis for more efficient classification.

Experiment 9 was set up specifically to oppose membership of an overlearned, familar class, and membership of an arbitrary set, defined as its relation to a common response. It was expected that inttially a semantic

class effect would predominate, making transitions between stimuli belonging to the same class more rapidly dealt with than transitions between stimuli from different classes, whether or not these required an alternated or a repeated response. Later in practice, it was predicted that the effects of response-grouping would show themselves, and the semantic class effect would be overridden. Unfortunately, these expectations were not realised. Right from the start of practice the maximum stimulus equivalence effect - which Rabbitt failed to get with arbitrary sets - was found. To a certain extent the data bears out the hypothesis that subjects operating in a serial task of this kind will compare N not only with N-1 but also with all the members of the class of which N-1 is a member - in the sense that transitions between members of a common semantic class were always faster than transitions between members of different semantic classes. However, later in practice the convenience of common class membership was significantly negated by the need to make a motor alternation - this appears to be a feature of many serial tasks - and also of those in which stimuli are given in blocks. The result demonstrates that facilitation due to transitions at one level - e.g. between signals - can be inhibited, or

traded off against, transitions at another level - e.g. the response level - of the stimulus-decision-response link. It further emphasises the futility of seeking a single-factor explanation of repetition effects.

The possibility of facilitation attributable to the repetition of a mediating response raised in Experiments 8 and 9 was taken up in a more complicated situation in Experiment 10. This dealt with the intermediate case between signal and response repetition, in which mediating responses at one level or overt responses at another level are repeated or not repeated on successive trials. The serial method employed where each stimulus event requires a separate motor response, gave the advantage of providing continuous recording to enable the relative effects of stimulus, mediating response and response repetition to be disentangled.

Treisman & Fearnley (1969) and Pritchatt (1969) have both shown that mediating responses play a significant part in the production of interference between relevant and irrelevant dimensions of incongruous stimuli in the Stroop effect. The contribution of the mediating response to the variance appears to exist over and above that attributable to either stimulus or response bias, in that, given constant stimuli and response suitable instructions

lead to the reduction or total abolition of the interference. Both the studies mentioned report that matching within a single dimension leads to reduction or abolition of the greater difficulty of identifying the colours of Stroop items. The intervention of mediating responses if further emphasised by the finding that the interference is not reduced if the subject is instructed to match across dimensions (Treisman & Fearnley, op. cit.).

In Experiment 10 subjects responded to a sequence of analogue Stroop items, on the basis of either the name of the numbers presented, or alternatively, on the basis of the total number of stimuli present. The data confirmed the superiority of a directly repeated item, but offered a more complex picture for transitions which consist of partial repetitions or of no repetition at all. It is clear that facilitation may occur at several levels of the stimulus-response process, and that these can be partialled out by comparing RTs for appropriate transitions. The data did not support the idea that the presence of an irrelevant source of information adds a simple, non-interactive constant to reaction-times. Neither was the expectation that the comparison carried out between stimulus N and stimulus N-1 would result in the relative

abolition of interference from the irrelevant source, confirmed. The contribution of the interfering dimension appeared to vary significantly according to the transition in which it occurred.

This result had implications for theories of selective attention. If an irrelevant source is filtered out at a relatively early stage of stimulus encoding, it is then assumed that it is not available at any subsequent stages. In the present case, the fact that the incongruous elements in the stimulus interfered more with some motor-response transitions than with others, clearly implied that this stimulus information was still available at the stage of response-selection and execution. This is irreconcilable with a filtering theory of selective attention, and strongly supports a response-competition model.

The data from experiment 10 were interpreted in terms of Morton's model for word-recognition. It was found that, while the results did not provide a critical test of the model, they gave it limited support. Two alternative interpretations of the model were discussed, between which the data were indecisive: (i) on one

reading, a single set of logogens receives input from both name and number channels, competition arising between alternative outputs to the limited access response buffer; (ii) on a second interpretation, two semi-independent sets of logogens, one for each signal channel both fire, on the basis of the information they receive, and competition occurs within the buffer storage system. An experiment was suggested to test between these versions of Morton's theory.

General Implications:

The wider implications of the data reported in this thesis can be evaluated by reference to what was said in the Introduction. Data and models reviewed there emphasised the importance of directing attention towards exposing the functional aspects of reaction processes. This is not simply a pragmatic point: the experiments reported here, as well as the wealth of previous studies, amply bear out the contention that not only are models basedon functional considerations of more value than those based for example on derived correlations between a priori aspects of the stimulus and RTs, but also that, as a matter of empirical fact, they are more likely to be correct. Every experiment made in Chapters 1-5 illustrates, in one

way or another, the futility of counting S-R events, or stimulus "dimensions" abstracted from the context in which they occur. This is true in two different senses:

1. It is clear that, in many cases, the nominal stimulus is not equivalent to the functional stimulus: this was shown, for example, in Chapter 2, where the data could be best explained by supposing that each stimulus was tested for membership of the target class, rather than for membership of its own intrinsic class.

2. The RT to any given stimulus is contributed to by several factors, not least contextual ones. The experiments of Chapter 1 demonstrated this point for "discrete" RT tasks, and Chapters 3, 4 and 5 elaborated this theme for serial tasks. The case, for both kinds of task, is obviously complex; yet, complex as it is, it admits of one sufficiently clear distinction:

(i) The "stages" in processing which constitute a single stimulus-response event, are not necessarily immune from mutual influence. Although encoding must necessarily precede response selection, it was a fatal assumption of Donder's technique, that they are ipso facto independent.

(ii) Even if the stages of analysis are non-interactive, the S-R events themselves are .manifestly not. The serial data of Chapter 3 in particular provide a demonstration of the several levels of analysis at which a given stimulus may exert an influence on the RT to that which follows. At present, there is no easy way of quantitatively assessing the relative contributions of the disparate effects which combine to produce the rank-orders of transitions in serial tasks. The limited aim of this research has been to identify some of the factors, and to show that they make a qualitative contribution to the RT.

This does not mean that one is driven to the ultimate pessimism of assuming that we are dealing with a system of perceptual analysis which is totally interactive, and for which, therefore, law-like models from which systematic and reliable predictions can be made are out of the question. The point is, rather, that the component atoms - "stages" - which constitute the S-R molecule, are sufficiently flexible to demand that we construct <u>functional</u> models, since it seems to be the functional nature of the comparisons that are made which is decisive.

Appendix:

An Investigation of Responses to Incongruous Stimuli Under Different Task Conditions

Pritchett (1969), Derks & Calder (1969), Treisman & Fearnley (1969) severally present evidence to suggest that the interference normally experienced to naming the colours of Stroop incongruous colour-words can be at least substantially reduced, and at most, totally abolished by altering the response which the subject is required to make to signal the identity of the colour. It seems that, provided the demand is not for a verbal colour name, then the incongruous word will generate little interference with the response to the colour. This finding is taken as support for the conclusion that however the interference is produced, it cannot be simply a matter of stimulus-competition, or a failure of selective attention at the input level, for were this the correct explanation then the particular nature of the overt response required of the subject should make little or no difference to the RT.

The usual demonstration of reduced interference consists of some form of "matching" task, in which subjects are asked to make matches, rather than emit words to the

successive colours on a Stroop-list. In Pritchatt's situation matches were made by pressing keys labelled with either colours or words; Treisman & Fearnley, however, used a card sorting task, in which subjects had to match either the colour or word of a Stroop card against either a row of coloured X's or a colour-word printed in black, on the same card. Egeth, Blecker & Kamlet (1971) have shed interesting additional light on the process involved in making such matches. They report that "when Ss had to indicate if the colours of pairs of items from the Stroop test were the same or different, the usual interference effect was completely eliminated. However, when the verbal information consisted of SAME and DIFFERENT, rather than colour names, interference was re-established" (Abstract). The Authors draw two conclusions from this study: (i) that "the perceptual comparison task does not eliminate interference simply by rendering Ss insensitive to the meanings of words" and (ii) that "the nature of the S's task is, in fact, a determinant of the degree to which irrelevant information is interfering" (ibid.).

This experiment was made to compare directly, three possible types of response to either colour or verbal information in series of two dimensional stimuli. In

addition to examining the effect of task on response time, the nature of the relationship between the colour and the verbal aspects of the stimulus was also manipulated as an independent variable.

Subjects: 8 subjects, three of whom were male, were employed for three sessions of approximately 25 minutes each. All were right-handed, with normal vision and were students at a college of further education.

Material: 6 white cardboard wall charts, 18" x 30", were used to display 16 lists of 100 stimuli each, arranged in columns of 10. Horizontal spaces of 1" were left between each column, and vertical spaces of ½" between each row. The lists were compiled as follows:

1. 100 colour words, printed in black upper case letraset, from the set BLUE, GREEN, YELLOW and RED. No word was repeated, and each occurred with equal frequency.

2. 100 coloured single upper case letters. The colours were bright and saturated and corresponded to the words used in list 1. The letters were the initial letters of the colours - i.e. B, G, Y and R - occurring always with their appropriate colour (B for example, would never occur except in blue). The order of stimuli was the reverse of that for list 1.

3. As for list 2, except that the letters here never appeared in their proper colour, being always incongruous with the colour in which they were printed. No colour or word was immediately repeated.

4. A set of 100 congruous colour and colour-name combinations.

5. A set of 100 incongruous (Stroop) colour and colour-names.

6. A set of 100 rectangular colour patches, 1¼" x ¼". The spacing was identical for each list; each list had a practice list of 2 columns of 10 items each on the reverse side.

Apparatus: The charts were displayed, individually, on a blackboard easel, lit by two 100 watt angle-poise lamps, in an otherwise dimly lit room. Protruding from the base of each chart was a sheet of card (white) on which were mounted, horizontally, a large (6" x 4") patch of each colour used. These patches matched the colours exactly, and could be removed if they were not required. When displayed, they were angled towards the subject, to make them more prominent. The subject was supplied with two light woodenpointers of equal length. Responses were timed in seconds by a simple laboratory clock.

Design: Each subject was required to perform three basic types of task: reading the names, naming the colours, or matching the colours of the words, letters or patches on each card. One type of task was performed only at each session. Half the subjects did reading followed by colour-naming (CN) followed by colour-matching (CM), in that order, and half in the reverse order. Sessions were well spaced apart in order to avoid deleterious transfer effects, or indeed, equally unwanted facilitation between runs. Within each session the tasks were ordered from easiest to most difficult (on the basis of a small pilot experiment) so that any practice effects would benefit the more difficult tasks and thus make any significant differences between the lists more convincing. This order was : 1. Naming or matching the colour patches, or reading the words printed in black. 2. Reading, naming or matching the congruous colour-letters. 3. The incongruous colour-letters. 4. The congruous colour-words and 5. The incongruous colour-words. Each list, in each condition, was presented four times in all, by repeating the order four times. This, it was hoped, would further act to reduce any differential effects of practice. Short breaks were given between each run, and, in the matching task, were deliberately lengthened, since more physical effort was required.

Instructions: Subjects were told of the nature of the task and were tested for basic colour-discrimination and naming with a selection of colours from the Ishihara test. Each was told that only the names GREEN, BLUE, RED and YELLOW were to be used. Speed was emphasised, but subjects were discouraged from making more than 4% or 5% errors on any list (particularly in view of the fact that individual RTs were not being recorded). The lists were to be read in vertical columns, subjects working from the base of one column to the base of the next, in a turret-pattern. This was essential since subjects in the pilot experiment had reported difficulty in locating the top of the next column from the base of the previous one. The practice lists were given, for each card, only on the first run of each session. Trials were started by a verbal signal from E. which coincided with the illumination of the two lamps and the start of the clock. Subjects were required to use a pointer to indicate which stimulus he was responding to, and in the matching task, another pointer to indicate the response. This was awkward at first, but subjects rapidly accustomed themselves to the routine and subsequently had no difficulty with this procedure. E followed the subjects' progress through each list and in the few cases of excessive errors, administered a suitable admonition to the subject, at the

conclusion of the list.

Results: The best RT of the four taken for each list in each condition was recorded; the means, over subjects, are presented in Table 1. An AnoVa (3-way: subjects x task x material) was performed on the raw data, and the result presented in Table 2. It is clear that, not only are the three main factors highly significant ($p < .01$ in each case), but also the interaction between task and material is significant at the same level.

A cursory look at Table 2 is sufficient to establish the disproportionately greater difficulty of the matching task, for all types of stimulus list. This may be the result of one or both of two factors: greater difficulty of making the appropriate decision, or alternatively, greater motor difficulty inherent in the operation itself. Since theavailable data suggest that matching reduces interference, it is likely that, in the present situation, the increase in RT is heavily contributed to by motor factors. Therefore, to forestall possible criticisms, comparisons will only be made within a single type of task, and no attempt will be made to compare values for corresponding lists on different tasks.

Neuman-Keuls analyses were made on each of the tasks, for rank-order differences: the reading and matching conditions generated no significant differences between the conditions. In the colour-naming task, however, both sets of incongruous colour-word letter combinations generated significant interference with naming the colours ($p < .01$), and in addition were significantly slower than with congruous colour-word letter combinations ($p < .05$).

Discussion: This experiment was made to provide a base-line for the work reported in Chapter 5. The point that it makes is relatively simple, namely, that matching colours of incongruous colour-word pairs eliminates the interference from theword. This interference is shown to be strongly present in the colour-naming condition, both for incongruous words, and also, more surprisingly, for the incongruous letter condition, in which the verbal information consisted of a single letter, which was always the initial letter of a conflicting colour-name. As far as is possible in such an experiment, care was taken to minimise the occurrence of obviously significant stimulus sequences - e.g. those in which the word in one stimulus was the same as the colour-name in

the next. Such sequences are known to have abberrant properties (Dirks + Calder, 1969). The technique of this experiment is relatively crude, but it has served to make its point, namely, that matching does eliminate colour-word interference, and thus provides a supporting reference for the more complex data of Experiment 10.

Material	Task	Word Reading(1)	Colour Naming(2)	Colour Matching(3)
Words in black(1); Naming Patches(2); Matching Patches(3)		37.4	53.0	76.6
Initial letters of colours in their appropriate colour		43.0	69.6	83.8
Initial letters of colours in an incongruous colour		40.5	83.8	85.0
Colour words in their respective colours		42.2	69.0	80.8
Colour words in an incongruous colour		40.6	83.8	82.1

TABLE 1: Mean RTs (msecs.) per 100 stimuli x task x material - Appendix

Source	Sum of Squares	D.F.	Mean Square	F/Ratio
Task	36486.31	2	18243.15	747.42 **
Material	3007.39	4	751.85	30.80 **
Subjects	5740.94	7	820.13	33.60 **
Task x Material	2708.80	8	338.60	13.87 **
Task x Subjects	629.47	14	44.96	1.84 NS
Material x Subjects	510.59	28	18.24	0.75 NS
Task x Material x Subjects	1366.86	56	24.42	-
Total	50450.36	119	-	-

TABLE 2: 3-way Analysis of Variance - Appendix.

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