

AD-787 880

THE INTERPRETATION OF REACTION TIME  
IN INFORMATION PROCESSING RESEARCH

Robert G. Pachella

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Prepared for:

Air Force Office of Scientific Research

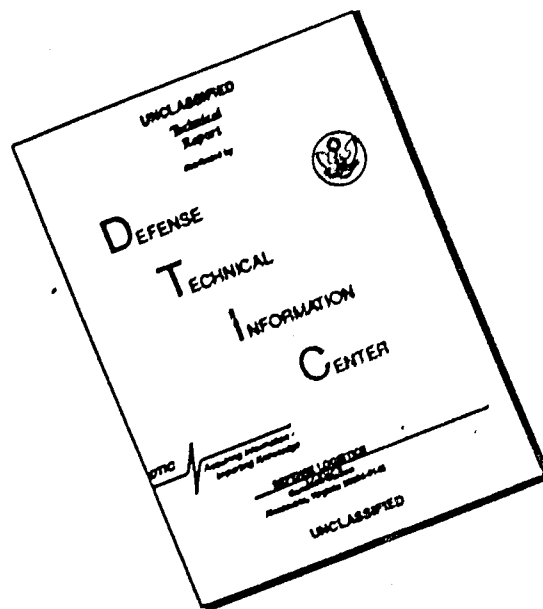
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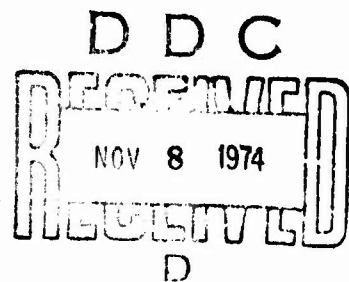
**HUMAN PERFORMANCE CENTER  
DEPARTMENT OF PSYCHOLOGY**

**The University of Michigan, Ann Arbor**

AD 787880

***The Interpretation of Reaction Time  
in Information Processing Research***

**ROBERT G. PACHELLA**



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**Technical Report No. 45**

**November 1973**

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 DOWNS AND WALKER REPORT 100-12 (7b)  
 DATED 10/1/68  
 Technical Information Officer

Unclassified

Security Classification

AD-787880

DOCUMENT CONTROL DATA - R & D

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY (Corporate author)  University of Michigan, Human Performance Center Department of Psychology, Ann Arbor, Michigan	2a. REPORT SECURITY CLASSIFICATION Unclassified  2b. GROUP
---	---

3. REPORT TITLE  
  
THE INTERPRETATION OF REACTION TIME IN INFORMATION PROCESSING RESEARCH

4. DESCRIPTIVE NOTES (Type of report and inclusive dates)  
Scientific Interim

5. AUTHOR(S) (First name, middle initial, last name)  
Robert G. Pachella

6. REPORT DATE November 1973	7a. TOTAL NO. OF PAGES 87	7b. NO. OF REFS 39
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8a. CONTRACT OR GRANT NO F44620-72-C-0019  b. PROJECT NO 1949  c. 61101D  d. 681313	8b. ORIGINATOR'S REPORT NUMBER(S)   8c. OTHER REPORT NO(S) (Any other numbers that may be assigned this report) AFOSR - TR - 74 - 1613
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10. DISTRIBUTION STATEMENT  
  
Approved for public release; distribution unlimited.

11. SUPPLEMENTARY NOTES  TECH, OTHER	12. SPONSORING MILITARY ACTIVITY Air Force Office of Scientific Research 1400 Wilson Boulevard (NL) Arlington, Virginia 22209
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13. ABSTRACT  
  
This chapter discusses the use of reaction time measures in modern experimental psychology. Methodological and theoretical issues are raised concerning the logic of experimentation in which reaction time is the major dependent variable and the limitations of interpretation of reaction time in the presence of variable error rates. The relationship between the speed and the accuracy of performance and theoretical models underlying this relation are also discussed.

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KEY WORDS	LINK A		LINK B		LINK C	
	ROLE	WT	ROLE	WT	ROLE	WT
reaction time Additive Factor Method Subtraction Method Speed-Accuracy Trade-off						

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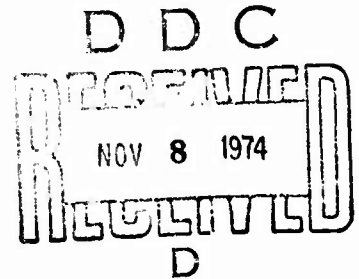
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DEPARTMENT OF PSYCHOLOGY

THE INTERPRETATION OF REACTION TIME IN  
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HUMAN PERFORMANCE CENTER--TECHNICAL REPORT NO. 45  
November, 1973



This research was supported by the Advanced Research Projects Agency, Department of Defense, and monitored by the Air Force Office of Scientific Research, under Contract No. F44620-72-C-0019 with the Human Performance Center, Department of Psychology, University of Michigan.

This paper will appear as a chapter in B. Kantowitz (Ed.), Human Information Processing: Tutorials in Performance and Cognition, New York: Lawrence Erlbaum Associates, 1974.

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The Interpretation of Reaction Time in  
Information Processing Research<sup>1</sup>

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Introduction

Reaction time measures have become increasingly prevalent in human information processing research. This increased use over the past few years has taken two distinct forms. First, a large number of experiments utilizing reaction time as the major dependent variable have appeared in the literature on substantive psychological problems. Included among these problems are such topics as sensory coding and selective attention, the retrieval of information from long and short term memory, psychological refractoriness, parallel and serial information processing, the psychological representation of semantic and logical relations and the selection and execution of responses. Indeed, reaction time has become about as common a dependent variable as there is in human experimental psychology.

The goal of this substantive research has been to understand basic psychological processes. In this endeavor, reaction time measures have sometimes been used as much for convenience as for any particular theoretical purpose. Deriving predictions about the duration of psychological



processes from the theoretical models describing such processes has become a common practice. These derivations seemingly involve only a few straightforward assumptions which link the duration of a process to what it accomplishes. However, as intuitive as these assumptions seem, the implicit acceptance of them on the part of many researchers has limited the potential contribution of their work. Without the explicit statement of how reaction time is related to the ongoing process, it is often difficult to relate the observed variation in the obtained reaction times to the informational transformations and manipulations that are supposed to be going on in the hypothesized processing network. In other words, it is not always obvious how information about process durations is psychologically meaningful. Deese (1969, p. 518), for example, expressing some doubts about contemporary cognitive psychology, has observed that, "We have insisted upon the measurement of behavior to the extent that most of the things we observe in experiments have no relevance for the process of thinking, other than the empty observation that thinking, like most processes, takes a measurable amount of time." Thus, there is a need to examine explicitly the assumptions upon which the use of reaction time measures in substantive research is founded.

A second area of research, also making use of reaction time measures, has concerned itself more directly with the study of reaction time per se. This kind of research has generally been concerned with discovering the conditions and parameters that produce and account for variation in reaction time without regard for specific experimental paradigms and substantive problems. Such factors as the distribution of stimuli (e.g., the

number, frequency and presentation order of specific stimuli), the distribution of responses, the intertrial intervals and the relative emphasis of speed vs. accuracy have all been shown to have systematic effects on reaction time in various different experimental paradigms.

This class of research has been seen by some investigators to be of limited value to the class of research on substantive issues. To some extent work such as this has been considered methodological in nature. It has attempted to discover the conditions and procedures under which reaction time measures can be collected most reliably and interpreted most reasonably. Until recently, parameters which were known to affect reaction time per se were thought to be artifacts to be removed from experiments on problems of substance in which reaction time was simply being used as a dependent variable. Some psychologists have actually found it disquieting that there exist researchers who seem to have actively taken up the study of a dependent variable per se and its associated experimental artifacts. In fact, one psychologist, who was studying problems of human memory, was once asked by a reaction time researcher to describe his own work; he claimed sarcastically that he was engaged in the study of "percent correct." The point, of course, is that reaction time, like percent correct, is simply a measure. It is neither a process nor a mechanism, and thus some people feel it should not be an object of direct attention. Nevertheless, given the prevalence of reaction time measures in contemporary experimental work, it is clear that what is known about reaction time per se should be related to reaction time as it is used as a dependent variable.

The purpose, then, of this chapter will be to examine some reaction time research. It is not intended as a review of literature about particular substantive issues, but rather it is an attempt to extract from that literature the manner in which reaction time measures have been used to draw inferences about psychological processes. Furthermore, an attempt will be made to examine some of the assumptions and principles upon which the interpretation of reaction time measurement is based. It is hoped that the juxtaposition of these sets of issues will lead to the more careful use and interpretation of reaction time measures in the understanding of human information processing.

The use of reaction time as a dependent variable.--To some extent the prevalence of reaction time measures over the past few years can be traced to the concurrent resurgence of interest in cognitive psychology. In contrast to earlier, more behavioristic approaches, modern cognitive psychology can be characterized as the study of events which cannot be directly observed. The events of interest to a cognitive psychologist usually take place when the subject is not engaged in any overt activity. They are events which often do not have any overt behavioral component. Thus, reaction time is often chosen as a dependent variable by default: there simply isn't much else that can be measured.

It is true, of course, that one can make inferences about unobservable mental events by studying subsequent behavior that results from or is dependent upon this cognitive activity. Much cognitive research is done in this manner. However, these indirect procedures are not always useful in situations where a subject is not making a lot of errors or where

experimental conditions have not drastically degraded performance. The only property of mental events that can be studied directly, in the intact organism, while the events are taking place, is their duration.

A second and perhaps more important consideration accounting for the popularity of reaction time measures is the indisputable nature of time as a meaningful and measurable quantity. It is often the case that dependent variables in psychology are only arbitrarily related to the underlying construct for which they are a measure. For example, an investigator interested in the amount of learning that can be produced by some experimental manipulation, might choose the percentage of correct items on a post test as his dependent variable. Percent correct in this case is only a surrogate for the real variable of interest, "amount of learning." Any monotonic transformation of percent correct would likely do as well. By contrast, one would not arbitrarily transform the time scale, because time itself is directly meaningful. The events being studied are considered to be filling real time, and thus real time is the variable of interest.

This point takes on particular significance if the independent variables in an experiment interact in a statistical sense; that is, if the effect of one independent variable is fundamentally changed depending on the level of some other independent variable. For the purposes of constructing theories, multifactor experiments which test for the existence of such interactions have more efficacy than experiments testing the simple effects of individual variables. However, the interpretation of data from multifactor experiments is contingent upon the scalability of the dependent variable. Many statistical interactions can be either

produced or eliminated by a suitable monotonic transformation of the dependent variable. Thus, given the inviolable character of reaction time against such arbitrary rescaling, the interpretation of such interactions or lack thereof, when obtained, is that much more secure.

Reaction time as commonly defined.---What is it that gets measured when the dependent variable in an experiment is reaction time? This question should be kept distinct, for the moment, from the related question of how reaction times are interpreted once they are obtained. The question here concerns simply how reaction times are obtained; what they mean in an operational sense.

Reaction time most typically is defined as the interval between the presentation of a stimulus to a subject and the subject's response. More operationally stated, this interval is usually measured from the onset of the stimulus presentation to the initiation of the subject's response. Certain definitional problems must be solved in order to measure reaction time in this way. To begin with, the constitution of a stimulus presentation and, thus, the definition of its onset is not always psychologically obvious. For example, auditory and visual stimuli have fundamentally different temporal characteristics in the way in which they can be presented; auditory stimuli must necessarily be spread out in time, while visual stimuli can, in principle, be presented instantaneously. Further, occasionally a stimulus presentation will consist of a discrete, temporal sequence of items. In this case reaction time is measured, operationally, from the onset of the last item. In a more functional sense, however,

defining "stimulus presentation" in this manner may be somewhat dubious; especially when such a sequential presentation is compared to one in which all of the items are presented simultaneously.

Similar problems exist at the response end of the reaction time interval. Occasionally, the initiation of a response is also not well defined. Variability in reaction time can be obtained strictly as a function of a response's susceptibility to physical measurement. For example, different initial phonemes in a vocal response will take differential amounts of time to activate a voice key. Furthermore, the amount of time it takes to initiate a response is not always free of the effects of response characteristics, such as response length or complexity, which temporally follow the initiation of the response. Thus, the operational definition of reaction time as the interval between the onset of the stimulus and the initiation of a response is not always a simple matter: it is intimately linked to whatever operational definitions are supplied to the terms "stimulus" and "response." To the extent that these operational procedures become confounded with experimental conditions, the further interpretation of the obtained reaction times is limited.

There is another more subtle procedural question involved in defining the reaction time interval. It is more subtle because it involves the intentions of the experimental subject. Reaction times are generally assumed, on the basis of either explicit or implicit instructions, to be generated under conditions where the subject is trying to minimize his response time. It is generally assumed that the subject in a reaction time experiment waits for the stimulus to be presented, does whatever he

has to do in his usual manner in order to respond correctly, and then responds immediately. In other words, the subject is not rushing his response, nor is he wasting any time: reaction time is taken to be the minimum amount of time needed by the subject in order to produce a correct response.

Since instructions to the subject are not the type of procedure that always produce the desired result, the obtained reaction times sometimes require post hoc editing. These procedures consist of eliminating from the data responses that for one reason or another seem out of the ordinary, and while it is likely that such editing procedures are often carried out for superstitious reasons, they nevertheless tend to have a definitional effect. That is, the data which are most typically reported in experiments utilizing reaction time as the dependent variable are from the responses that seem most likely to represent the minimum intervals needed by subjects to produce correct responses. For example, very fast responses are occasionally eliminated on the grounds that they represent "anticipation" responses that seem to have been initiated without the subject strictly waiting for the stimulus to appear. Sometimes extremely slow responses are eliminated. This can be accomplished by simply removing responses which are longer than some predetermined criterion, or by reporting the median reaction time for a condition. This will tend to eliminate the effect of the generally positively skewed tail of a reaction time distribution.

While the practice of removing responses from data on the basis of their being "too fast" or "too slow" is perhaps arbitrary, a more common

practice, the removal of error responses, has seemed somewhat more justifiable. This is because these trials are well defined and are clearly in violation of the basic definition of reaction time. A trial on which an error is made is apparently the result of activity which is not related to the process under investigation; clearly, if the subject has made an error he has done something out of the ordinary on that trial. Thus, investigators generally examine and report only the reaction times resulting from correct responses.

Whether such editing of experimental data can actually be justified will be the focus of later discussion. Nevertheless, in practice, the reaction times that get reported in experimental literature generally conform to the above requirements, and thus serve as the basis for any further theoretical interpretations that may be called for in an experiment.

#### Experimental Logic and Reaction Time Measures

Aside from questions about the conditions under which reaction time measures are obtained, the further interpretation of reaction time is dependent upon the experimental logic involved in specific situations. For example, experimenters often desire to interpret the reaction times obtained in an experiment as "the time to recognize," "the time to deduce," "decision time," or "the time to search memory." In order to allow any of these specific interpretations, an experiment must employ a design which is suitable to the conclusion that the obtained variation in reaction time is related to the variation in the duration of the particular



mental process under study. Since most of these processes are unobserved and mediational, it is almost always necessary to employ the kind of experimental designs classified by Garner, Hake and Eriksen (1954) as converging operations: they must include several conditions, none of which uniquely identify the effect of the process under study, but which taken together define such an effect.

Of course, all of the general principles of experimental design and deductive logic are applicable when reaction time is a dependent variable. However, at present two commonly used types of converging operations have particular significance for information processing research. These are the Subtraction Method and the Additive Factor Method, which will be discussed in some detail below. Each of these methods can be contrasted with a more molar approach to experimentation in which obtained reaction times are of interest essentially without further interpretation. That is, whereas the methods to be described below have as their purpose the subdividing of the time interval between stimulus and response into mediational effects, the molar approach takes the entire interval as its unit of interest. It is, for example, a suitable approach in situations where the immediate function of an experiment is to discover how long it takes to perform some relatively well-defined perceptual-motor task. In such cases converging operations are hardly necessary. Thus, while such an approach is of great value to some areas of research (e.g., applied problems), it is of only marginal significance to information processing research which attempts to investigate the mental processes intervening between a stimulus and a response.

The Subtraction Method.--One of the problems confronting an information processing researcher regarding the interpretation of reaction time measures is the attribution of the effect of some experimental manipulation to a particular mediating process. This is a problem because a reaction time interval always involves other processes besides the one of interest. The Subtraction Method is a set of converging operations that is commonly used, often without being labeled as such, to eliminate this problem. When applicable, it allows for the isolation of a mediating process and the measurement of its duration. The method is applicable when the performance of an experimental task involves the sequential action of a series of discrete mental events. In order to measure the duration of one of these mental events, the reaction time for an experimental task containing the event as a subprocess is compared to that for a comparison task which differs from the experimental task only by the deletion of the process of interest. In other words, the comparison task must involve all of the processes contained in the experimental task except for the process that is to be isolated. The difference in reaction time for these two conditions will then be equal to the duration of the isolated process. Thus, if an experimenter is interested in the effect of an independent variable on the duration of a particular mental process he needs only to compare the difference obtained between an experimental and comparison condition of the type indicated above as a function of the independent variable.

A recent experiment by Eriksen, Pollack and Montague (1970) illustrates the application of this reasoning particularly well. These

investigators were interested in studying the processes involved in the encoding of visually presented words. The experimental task involved having subjects simply name stimulus words which were presented one at a time. Reaction time was measured from the onset of a stimulus word to the initiation of the vocal response naming the word. That is, the first sound emitted in making the response triggered a voice key which terminated the reaction time interval. The independent variable of interest was the length, in syllables, of the stimulus word. The results showed that one-syllable words could be named faster than three-syllable words.

This seemingly straightforward result might have been taken at face value to indicate that it takes more time to encode longer words. The problem with this interpretation is that it equates "reaction time" with "encoding time" and this is not strictly correct. The naming task described above must involve several distinct operations. For one, the stimulus word must be encoded. This is the process in which Eriksen, Pollack and Montague were interested. Additionally, the response, vocalizing the stimulus word's name, must be performed. Variability in reaction time due to the length of the stimulus word on the process of executing the response is irrelevant to the question of the effect of word length on encoding time. Note that it makes no difference that reaction time was measured to the initiation of the response. This is because it may simply take longer to initiate a longer word than a shorter one. In other words, it is possible that it may take no longer to perceptually encode a three-syllable word than a one-syllable word; it may only take longer to initiate a three-syllable response than a one-syllable response.

In order to assess this possibility, Eriksen, Pollack and Montague applied the Subtraction Method. A comparison task was constructed in which the subject was presented with a stimulus word. He did not respond to the stimulus word directly, however; he merely encoded it. Following a short time interval a light was presented to the subject. The subject responded to this stimulus light by emitting the name of the encoded word. Reaction time in this condition was measured from the onset of the stimulus light to the initiation of the vocalized response. Thus, this reaction time interval involved everything the interval in the experimental task involved except for the encoding of the stimulus word. In this comparison condition there was no difference in reaction time for one- vs. three-syllable responses. Therefore, the authors concluded that the entire difference obtained in the experimental condition could be attributed to the perceptual encoding of the stimulus.

Criticism of the Subtraction Method.--The conclusions drawn by Eriksen, Pollack and Montague are dependent upon the applicability of the Subtraction Method. To the extent that general criticisms can be levied against this method their conclusions must suffer. At least two general criticisms have been levied.

First, the Subtraction Method begs one of the most fundamental questions underlying information processing research; namely, the description of the mental events involved in an experimental task. The starting point for the application of the method is a relatively sophisticated one: in order to construct a comparison task, one must already

know the sequence of events which transpire between stimulus and response. Such sophisticated knowledge is rarely available. Rather, it is more often the case that the structure of the mental events is presented with only logical or intuitive (as opposed to empirical) justification. Obviously, the conclusions reached on the basis of the application of the method can then be no stronger than the substantiation of the initial conceptualization of the experimental task. Thus the experiment by Eriksen, Pollack and Montague purports to show the effect of word length on stimulus encoding, but it has presupposed with only the meagerest of justifications the existence of such a process and its independence from response execution. This general class of criticism will be discussed in greater detail below in connection with the Additive Factor Method.

A second general criticism of the Subtraction Method concerns the comparability of the experimental and comparison tasks, or as Sternberg (1969b) has called it, the assumption of pure insertion. This refers to the assumption that it is possible to completely delete (or insert) mental events from an information processing task without changing the nature of the other constituent mental operations. In order for the difference in reaction time between the experimental and comparison task to meaningfully represent the duration of an isolated process, all of the other processes common to both tasks have to be strictly comparable. For example, in order to accept the conclusion of Eriksen, Pollack and Montague, that the effects of word length are localized in the encoding process, it must be assumed that the response execution processes for the two tasks in their experiment are identical. There is nothing in the application

of the method itself, or in the data collected therefrom, that can justify this assumption.

A recent series of experiments by Egeth and his colleagues (Egeth and Blecker, 1971; Egeth, Jonides and Wall, 1972; Egeth, Atkinson, Gilmore and Marcus, 1973; Egeth, Marcus and Bevan, 1972) have demonstrated the importance of this issue. These experiments have utilized subtractive logic for ostensibly methodological purposes. They have employed what are known as "C-reactions" in order to simplify their experimental tasks from the subject's point of view, thereby reducing extraneous variability and thus requiring less data per subject in order to produce stable results.

A C-reaction calls on a subject to respond as quickly as he can when a stimulus from a particular class is presented, and not to respond if any other stimulus is presented. This can be contrasted to the more common practice of having the subject execute a unique, overt response for each potential class of stimuli used in an experiment. For example, in a commonly used experimental task subjects are presented with pairs of letters. They are to judge whether the letters are the same or different. Common practice, utilizing manual responses, would assign "same" responses to the index finger of one hand and "different" responses to the index finger of the other hand. The utilization of a C-reaction makes response type (i.e., same vs. different) a "between subjects" variable. That is, each subject makes only one response. Some subjects respond only on same trials, not responding at all on different trials, and other subjects respond in the opposite manner.

Egeth's argument runs the following way<sup>2</sup>: In the above example, for instance, subjects must decide whether the presented letters are the same or different. It is this decision process that is of interest to most cognitive psychologists. However, in the usual experimental setup, after noting whether the letters are the same or different, the subject must remember which response to make for same stimuli and which response to make for different stimuli. Since this assignment is arbitrary, it must be learned, and this learning is reflected in the amount of practice necessary in order for a subject to produce stable data. Furthermore, variability in the produced reaction times due to deciding what response to make is not relevant to the process of interest--deciding whether the letters are the same or different. On top of all this, subjects undoubtedly make errors in this task, not because they have misclassified the stimulus letters, but because once they have classified the letters they have executed the wrong response. With the C-reaction, once the stimulus pair is classified as the same or different, the subject need only remember whether he is a "same responder" or a "different responder." Certainly this condition also involves memory, but it is much simpler and requires less practice and learning than remembering which overt response goes with each class of stimulus. Thus, by eliminating the response decision process from the experimental task, response decision time has been removed (i.e., subtracted) from the obtained reaction time, thereby reducing subsequent variability.

In order for this argument to be truly persuasive one would need to be assured that this methodological innovation did not fundamentally

modify the processes of interest underlying the experimental task. That is, in order for data collected with C-reactions to be comparable to data collected in the usual way with choice reactions, it must be assumed that the rest of the cognitive processes involved are not affected by the presence or absence of the response decision stage.<sup>3</sup> It is possible, however, to construct an alternative argument that encoding changes as a function of the inclusion of the response decision stage, and in this situation the alternative seems introspectively reasonable. For example, "same responders" (i.e., subjects who respond only on same trials) might be conceived of as subjects who are "looking for" sameness. Their decision processes, as compared to those for subjects who have to respond to both sameness and difference, might be biased in favor of sameness and the duration of the decision process on same trials might be affected by this bias.

Egeth and Blecker (1971) investigated this possibility. In an experiment designed to study the effects of orientation (rightside up vs. upside down) on the type of letter classification described above both C-reaction conditions and choice reaction conditions were utilized. The patterns of results for both sets of conditions were remarkably similar. The only major difference was the fact that the reaction times for the C-reaction conditions were considerably faster than those for the choice reaction conditions, presumably because of the deletion of the response decision stage. Thus, in this situation the logic of the Subtraction Method seemed tenable.

On the other hand, Egeth, Marcus and Bevan (1972) have shown that such good fortune cannot always be expected. In a somewhat different



type of letter classification task they again utilized both C-reactions and choice reactions. This time, however, the two sets of conditions produced remarkably different patterns of results. It appeared that in this situation the deletion of the response decision stage drastically affected the nature of the processing, thus illustrating the lack of comparability that was noted above.

Since, a priori, it is impossible to know when the deletion of a stage will drastically affect processing, this question of comparability can always be put forward in criticism of the Subtraction Method: The method involves the comparison of similar but nevertheless different tasks. It can always be asserted that the information processing for the two tasks, aside from the simple deletion or addition of a particular mental event, is fundamentally different.

Another way of looking at the above criticism is to say that the data collected by using the Subtraction Method generally yield no evidence about the adequacy of the assumptions underlying its use. If one accepts the assumption that the converging tasks are comparable, then one can interpret the data collected. But the data themselves give no indication about the comparability of the converging tasks.

This does not always have to be the case, however. In some situations the data obtained using the Subtraction Method can provide a built-in indication of the reasonableness of the assumption of comparability. The experiments of Egeth discussed above are one such case. In doing his experiments both ways (with C-reactions and choice reactions), he can check the comparability of his tasks. However, it should be noted that the alleged methodological advantages of C-reactions are then lost. Another

situation of this kind has been developed and used extensively by Sternberg in the study of short-term character recognition (Sternberg, 1963a; 1969b). The experimental task utilized in the study of this problem starts with the subject memorizing a short list of characters (usually letters or digits) before each trial. The subject is then presented with a probe character and he is to decide whether or not the probe is contained in the memorized list. Reaction time measured from the onset of the probe character is taken as the dependent variable.

The processing model developed by Sternberg to account for the performance of this task hypothesizes that the internalized or encoded probe character is compared sequentially with each item on the memorized list. Each comparison, then, comprises one mental operation in the processing sequence. Thus, if two conditions which differ from each other in list length by only one item are compared, the difference in their reaction times can be interpreted as the duration of a mental comparison. This is an elementary application of subtractive logic and it is limited, just as the other examples mentioned above were limited, by the assumption of comparability--the assumption that the deletion of one mental comparison leaves the others intact. However, in this case, successive list lengths can be compared. List length 2 can be compared with list length 3; list length 3 can be compared with list length 4; and so on. If strict comparability holds, that is, if the deletion of one mental operation doesn't affect the others, then the successive differences in reaction time should all be identical. The data relating list length to reaction time should be linear. In other words, an obtained linearity of the data can be used to check the applicability of the subtractive logic.

In general, in order to get away from this pervasive criticism of the Subtraction Method (i.e., the comparability of tasks) the critical difference of interest must be embedded in a more complex set of converging operations. In the above instance several conditions were compared, varying the number of identical operations contained from condition to condition. Thus, these repeated applications of the Subtraction Method allow for the detection of any drastic changes in processing as a result of the number of stages--provided, of course, that such drastic changes result in changes in the duration of the affected mental operations. Looking at only one difference in reaction time from one experimental condition and one comparison is tantamount to accepting all of the assumptions underlying the method.

One last point should be mentioned with regard to this criticism about the comparability of tasks in the Subtraction Method. The criticism is not unique to this situation, but rather, it is a criticism that can be applied to the use of converging operations in general. Converging operations, by definition, involve several tasks or conditions that differ from each other in certain systematic ways. They must do this otherwise they wouldn't converge on some theoretical construct. The argument can thus be made that the converging tasks may involve fundamentally different mediational components. In situations of this type, perhaps conservatism is not the best policy, since converging operations of one form or another would seem always to be necessary in the study of mental events. Thus, data collected and interpreted utilizing subtractive logic should not necessarily be dismissed on these grounds alone. Such criticism ought only be paid attention to when it is accompanied by other

persuasive evidence or argumentation indicating that the tasks used to converge are not comparable. In other words, since it is impossible to prove that comparability holds; and since it is, in principle, always possible to demonstrate that it doesn't hold, when in fact it doesn't; an hypothesis garnered via subtractive logic might be maintained, at least in a tentative way until it is disproven. Thus, any particular application of converging operations such as the Subtraction Method, will leave open an obvious direction for further experimentation: The search for data demonstrating that the common components of the converging operations are not comparable.

The Additive Factor Method.--The Additive Factor Method, developed by Sternberg (1969a), has as its principle concern the first of the problems discussed above in conjunction with the limitations on the use of the Subtraction Method. In particular, it concerns procedures for deriving an initial conceptualization of the sequence of cognitive processes that transpire between the presentation of a stimulus and the subject's response to that stimulus. It thus represents a level of inquiry that is somewhat more basic than that of the Subtraction Method, whose starting point is an already well-developed theory about the nature of the processing sequence.

Like the Subtraction Method, the Additive Factor Method is a set of converging operations. Therefore, the processing schema derived by it can be identified by unique conjunctions of its operational procedures

and assumptions. That is, each process identified by the Additive Factor Method receives its definition from a pattern of data which is the result of a set of operations.

The basic logic underlying this method involves the following conceptions. First, it is assumed that the reaction time interval is filled with a sequence of independent stages or processes. It is the purpose of each of these stages to receive an input from the preceding stage and to perform a particular translation or transformation on it. Its output is then passed along to the next stage or process. Each stage produces a particular transformational effect in the sequence of information processing. That is, while the input to each stage will vary in form from trial to trial, it will nevertheless have had the same transformations applied to it as any other input at that point in the sequence. The output of a stage will likewise have achieved some particular level of information processing. Thus, the purpose of a stage is to produce a constant informational transformation.

The nature of the informational transformation produced by a stage is taken to be independent of the durations of the stages that have preceded it. Furthermore, the output of a stage represents a constant informational translation regardless of its own duration. Thus, the nature of the input and output of each stage is independent of factors which influence its duration. It is the durations of these stages and the factors affecting their durations that are the primary concern of the Additive Factor Method.

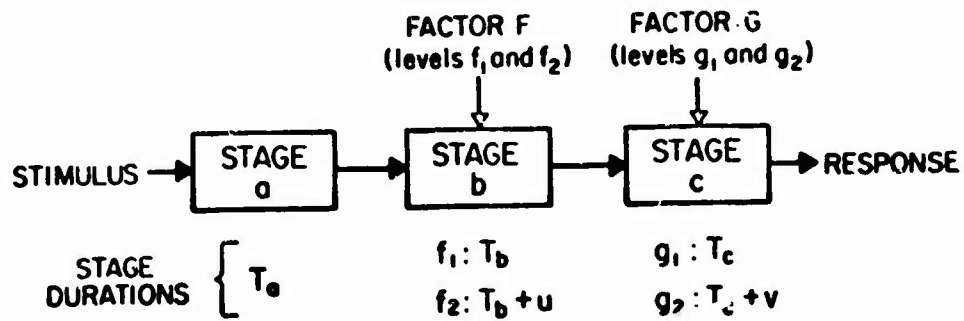
For example, consider an information processing stage which finds the name of a stimulus; that is, a stage which receives an essentially

spatial representation of a stimulus (i.e., a representation with properties isomorphic to the spatial properties of a stimulus) and translates it into an acoustic representation (i.e., a representation with properties isomorphic to the stimulus name). This spatial representation is the result of the preceding stages of processing. It may, of course, vary from trial to trial in its particular form depending on what the actual stimulus is, but it will have been processed on each trial to the same degree (e.g., it will be spatial and it will have similar resolution and detail). Further, the nature of this input is not dependent on how long it took to get through the preceding stages, but only on the fact that it has gotten through them. The input is now processed by the stage in question. The result is an acoustic representation which will go on to the next stage. The nature of this output representation is not dependent upon the duration of this particular translation process, but only on the fact that the process has been completed. Thus, the input and output of a stage are independent of the duration of the stage in question and of those of the preceding stages.

This conception of stage leads to several implications regarding the relationship between the durations of stages and experimental manipulations. First, total reaction time is simply the sum of the stage durations. When an experimental manipulation affects the reaction time for a particular information processing task, it does so by changing the durations of one or more of the constituent stages of processing. Second, if two different experimental manipulations affect two different stages, they will produce independent effects on total reaction time.

the effect of one manipulation will be the same regardless of the level of the other variable. In other words, the effects of the two experimental factors should be additive; they should not interact in a statistical sense. This fact follows quite directly from the relationships described above between the durations of stages and their inputs and outputs. Third, if two experimental factors mutually modify each other's effect, that is, if they interact in a statistical sense, they must affect some stage in common. Figure 1 illustrates the nature of the additivity in a situation where two experimental factors (F and G) affect two different stages of processing (a and b). Figure 2 shows the resultant additivity in graphical form.

In using the Additive Factor Method to interpret reaction time measures the above implications are applied to data from multifactor experiments. That is, in order to apply the method, a well-defined information processing task (where reaction time is being measured as the dependent variable) must be embedded in a multifactor experimental design. The larger the number of experimental factors that are manipulated, the finer will be the analysis of the constituent stages of the experimental task and/or the better will be the definition of each constituent stage. Basically, the data from such an experiment are simply analyzed in order to find pairs of factors which have additive effects on reaction time. Each time such a pair is discovered it is concluded that each factor affects a different stage. Further, from the patterns of interactions it



### TOTAL REACTION TIME

$$RT = T_0 + T_b + T_c + \begin{cases} - & \text{for } f_1, g_1 \\ u & \text{for } f_2, g_1 \\ v & \text{for } f_1, g_2 \\ u+v & \text{for } f_2, g_2 \end{cases}$$

Figure 1. Stage durations and the additive effects of independent factors on reaction time. (After Sternberg, 1971.)



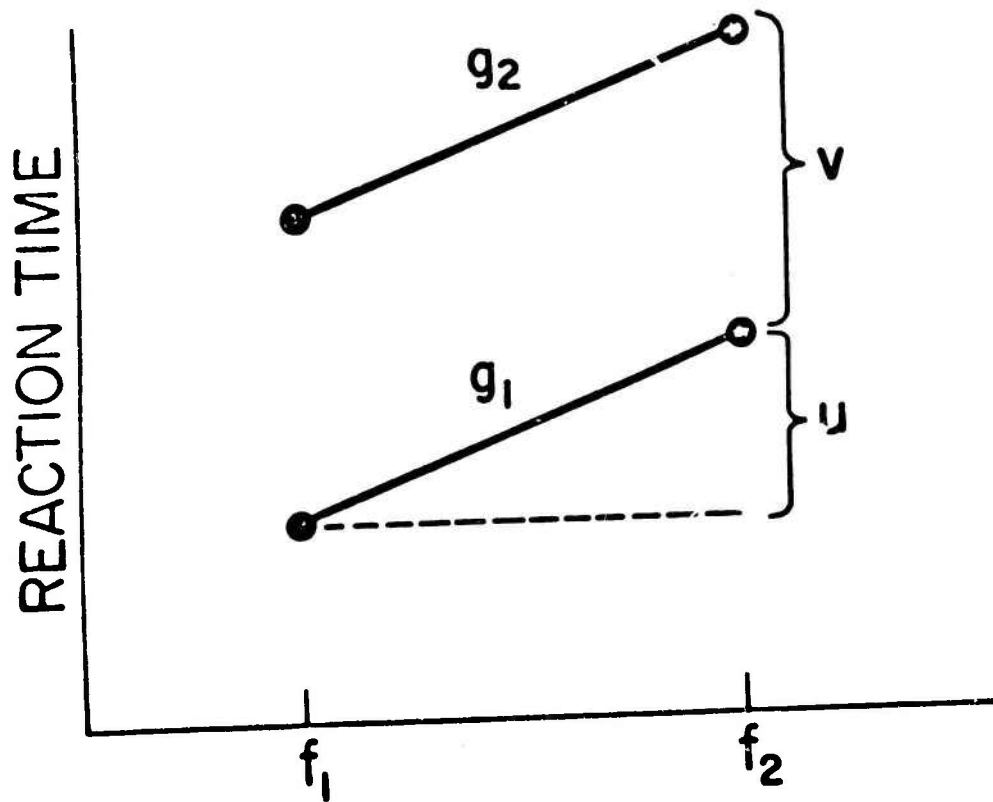


Figure 2. Graphical illustration of the additivity of independent factors.

is possible to give definition to the processing that is accomplished by various stages. Thus, if factors A and B are found to interact, then it can be asserted that there is some stage whose process is such that it can be affected by both Factor A and Factor B. Figure 3 illustrates how the pattern of additivity and interactions among four experimental manipulations (F, G, H, and I) can be used to define a processing schema. The concrete examples which will be given below will also elucidate the methodology.

Prior to looking at these examples, however, three additional methodological generalities should be noted. First, the analysis of an information processing task into component stages by looking at the pattern of additive factors is contingent upon the inherent scalability of reaction time measures that was discussed earlier. Additivity and interaction have meaning only in the context of a dependent variable that is immune to arbitrary monotonic rescaling. In the present context, the obtained reaction time measures are directly of interest because the events that are under consideration (i.e., the stages) are taking place in real time. The additivity is a property of the real time durations of the stages leading to the production of the observed reaction time measures. Consequently, not only are transformations of the time scale inappropriate, but so are measures of reaction time, such as medians, which are themselves in principle not additive.

Second, care must be taken that the manipulation of experimental factors does not redefine the experimental task or its component stages.

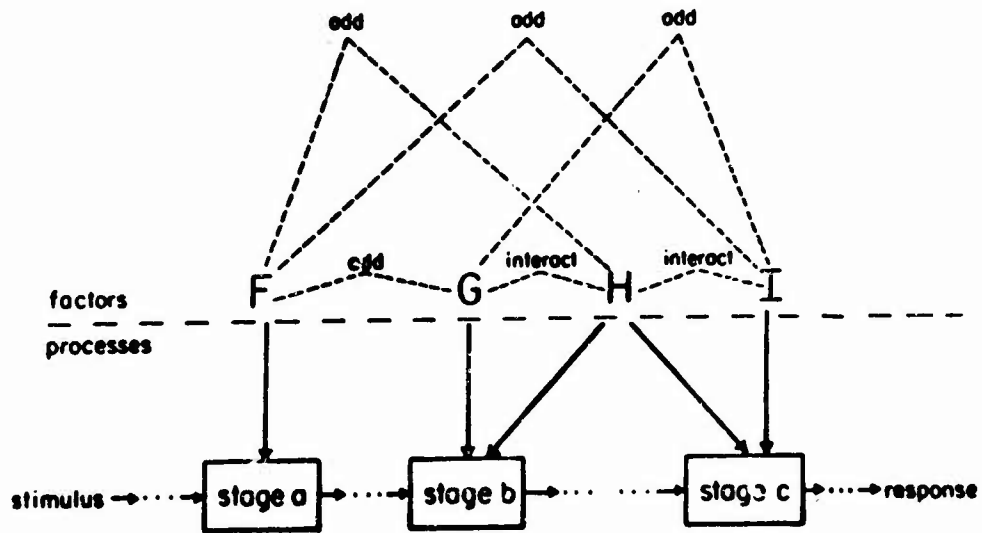


Figure 3. The effect of factors F, G, H and I on stages a, b and c and the pattern of additivity and interaction among the factors (After Sternberg, 1969a.)

The experimental factors should be reasonable within the context of the experimental task. For example, in a certain sense, the construction of a comparison task in the Subtraction Method might be considered as a limiting case of a factor manipulation. Of course, in such a case the limitations of comparability that were discussed earlier would then apply to the present method. More will be said about this below; for the moment it will suffice to note that one of the motivations behind the development of the Additive Factor Method was the notion that changing the levels of a factor is a somewhat weaker manipulation of information processing than the deletion of entire processes.

Finally, the Additive Factor Method represents a clear conjunction of the interest in substantive issues with that of the study of reaction time per se. Clearly, the Additive Factor Method supplies at least one context in which the direct discussion of the effects of experimental manipulations on reaction time are not simply the study of artifacts. The more factors that can be found that systematically affect reaction time in general, the greater the likelihood that the underlying process structure of any information processing task will be elucidated.

An Additive Factor Case Study.--The usefulness of the Additive Factor analysis was originally demonstrated by Sternberg (1969a and b) in connection with the study of short-term character recognition. The typical experimental task used to study this problem requires a subject to make a positive response if the stimulus is one of a previously memorized positive set of stimuli and a negative response otherwise. This simple information processing task was embedded in a series of multifactor experiments

which investigated the effects of the following factors : the quality of the stimulus (either clear or degraded), the size of the previously memorized set (from one to six digits), response type (whether the trial required a positive or a negative response) and relative frequency of a response type (a positive response was required 25, 50, or 75 percent of the time in various conditions). A typical trial from an experimental condition, for example, might have been embedded in a series of trials where the positive response was required 25 percent of the time. The set of digits memorized prior to the trial might have had four members. The stimulus may have been degraded and the correct response might have been negative. Each trial could thus be represented by a configuration of particular factor levels and each possible configuration of factor levels occurred in the series of experiments.

The results of these experiments showed that all four factors had significant effects on reaction time. Furthermore, five of the six possible pairings of the four factors had clearly additive effects on reaction time. That is, their interactions were found to be zero. One of the six two-way interactions, that of stimulus quality and relative frequency of the positive response was not explicitly tested in these experiments. Sternberg, however, presented supplementary considerations which argued for the independence of these two factors (see Sternberg, 1969a, page 295). Thus, Sternberg's tentative description of the information processing involved in this task had four stages, one stage defined by each of the factors tested: an initial stimulus encoding stage in which the visual image of the stimulus is converted into a representation which can be held in short-term memory, a comparison stage in which the stimulus representation is

compared with each memorized positive set stimulus, a response choice stage in which the output of the comparison stage is tested to see whether or not the stimulus matched a positive set item, and a response execution stage in which the motor commands for the appropriate response are carried out.

It should be noted that the Additive Factor Method itself does not supply either the description of each stage, as given above, or the order in which the stages are effective. Those conjectures arise from a consideration of the nature of each factor and a logical argument concerning the dependency of each subsequent transformation on the processes preceding it. For example, it seems reasonable to hypothesize that the stage affected by stimulus quality would be a stimulus encoding stage, and it also seems reasonable to believe that stimulus encoding would precede any of the other stages. Only the study of additional factors and the pattern of interaction obtained with them can supply additional evidence about the adequacy of such a conceptualization.

An example of this type of theoretical substantiation has recently taken place within the problem area of short-term recognition memory. Sternberg (1966) originally hypothesized that the memory comparison stage consists of a serial exhaustive search of the items of the memorized positive set. That is, the comparison stage involves the sequential comparison of the probe with each item from the positive set regardless of whether or not a match is found along the way. The basis for this conjecture involved, first, the linear form of the function relating reaction time to size of the positive set. This linear relation, taken

in conjunction with subtractive logic, implicates the serial nature of the comparisons (see page 20). Second, the additivity of the factors of size of the positive set and response type implicates the exhaustive nature of the search. If the comparison stage were not exhaustive, that is, if the stage terminated itself upon finding a match, then differential effects of size of the positive set would be expected for positive and negative responses. Response type and positive set size would interact. This follows from the fact that in such a self-terminating search all of the items in memory have to be searched on negative trials, whereas on positive trials only about half of the items, on the average, need to be searched in order to find a match. Thus, since set size has the same effect on both positive and negative responses, it was concluded that all of the memorized items are checked on both positive and negative trials.

An extensive analysis of an additional factor, the probability of particular probe stimuli, led Theios, Smith, Haviland, Traupman and Moy (1973) to challenge this position. They found that reaction time in a short-term recognition experiment decreased as a function of stimulus probability, and this fact seemed inconsistent with the hypothesis that the memorized list is searched exhaustively. How could stimuli, because of their particular probability of occurrence, have different reaction times if the probe stimulus had to be compared with all of the stimuli in memory? They concluded that the memory comparison stage must be serial and self-terminating with the order of comparisons determined, at least to some extent, by stimulus probability. Unfortunately, the experimental

design of Theios, et al., did not allow for the clear demonstration of the interaction of stimulus probability and memory set size as the Additive Factor Method would predict if the effect of stimulus probability was localized in the memory comparison stage.

Recently, however, two independent Additive Factor experiments have clarified this issue to some extent. In one, Klatzky and Smith (1972) varied stimulus probability and size of the positive set and found no evidence for an interaction. This would indicate that these two factors have their effects on different stages. In the other experiment, Miller and Pachella (1973) varied stimulus probability and stimulus quality and found that these factors strongly interact. Therefore, it seems reasonable that the stimulus probability effect found by Theios, et al., (1973) is localized, not in the comparison stage, but in the encoding stage. Thus, Sternberg's original conjecture about the nature of the comparison process still seems momentarily plausible. Furthermore, the interaction of stimulus probability and stimulus degradation adds still further definition to the transformation that takes place in stimulus encoding: whatever its nature, it must involve a process such that the effect of stimulus probability can be modified by the clarity of the stimulus.

Criticism of the Additive Factor Method.--The above examples are intended as a demonstration of the usefulness of the Additive Factor Method. It is a useful method because it is unambiguous enough to allow investigators to independently examine findings arising out of its application and either to agree on their interpretation or to express their disagreements in a meaningful empirical discourse. On the other hand,



because of certain limitations underlying the method, it would be more than naive to hold with any tenacity a model having the Additive Factor analysis as its sole justification.

One of these limitations has been alluded to above. It is simply the argument that the manipulation of factor levels may cause a fundamental change in the processing sequence as may happen with the deletion of an entire stage within the Subtraction Method. From a procedural point of view, the difference between the Subtraction Method and the Additive Factor Method can be quite subtle. Thus, "number of overt responses" in a short-term recognition experiment (e.g., one vs. two responses as in Egeth, Marcus and Bevan, 1972) is taken to fundamentally modify the processing sequence, whereas "stimulus quality" (e.g., the presence vs. the absence of a checkerboard noise pattern as in Sternberg, 1967) is taken to simply modify the duration of the encoding stage.

Second, the demonstration of additivity in multifactor experiments often amounts to the acceptance of a null hypothesis concerning the interaction between two variables. Great caution needs to be exercised in such instances. The data needed in order to demonstrate true additivity require a precision that few reaction time experiments obtain. Furthermore, as Sternberg (1969a) has pointed out, experimental artifacts are more likely to obscure true additivity than true interaction. Thus, two factor interactions are best believed when they are obtained in experiments that also contain convincing demonstrations of additivity.

Finally, any particular conception of "information processing stage" and its properties may be more problematic than determinate. On the one

hand, the Additive Factor analysis can be taken as definitional, as it has been in the present paper. The concept of "stage" can be simply defined by the pattern of additivity and interaction that is achieved from sets of multifactor experiments. In such a case a "stage" is operationally defined, albeit from a set of converging operations, and its properties will be those inherent in the assumptions underlying the methodology. This is then a functionally based approach to theorizing. It represents a shorthand for conceptualizing the effects of experimental variables.

On the other hand, there can be independent conceptions of processing stages which have external justification. These may be derivable from other forms of data outside of the context of reaction time and/or information processing based theory (e.g., from psychobiology, psycholinguistics, or psychophysics). In such cases, it becomes an empirical question as to whether or not these "stages" have the properties which satisfy the definition of stage derivable by the Additive Factor Method.<sup>4</sup> In situations where stages have some independent definition, it is perfectly conceivable that two factors might affect a single stage in an additive manner or they might affect different stages and interact. This latter possibility would be the result, for example, when a given factor modified not only the duration of a stage, but also the nature of the output from that stage. In this situation the pattern of data obtained from multifactor experiments would serve not so much for the discovery of the stages themselves, but rather for the determination of their specific properties. Thus, a model of processing in which the stages are defined by an Additive Factor analysis may not be identifiably different from an alternative model based on

a different definition of stage in which the properties of the stages do not lead to the simple patterns described above. They may be just two different conceptualizations of the structure underlying some body of data.

#### Reaction Time and Performance Accuracy

The interpretation of reaction time measures necessarily depends upon the precision of their measurement. With regard to experimental data, this precision is usually discussed in the context of the inter- and intra-subject reliability that is necessary to draw certain statistical conclusions. For example, the last section presented the partitioning of the reaction time interval into stages on the basis of the additivity and interaction among independent variables. The ability to detect such relations depends upon the amount of extraneous variability that is inherent in the obtained reaction time measures. The present section further discusses the precision of reaction time measures. The limitations on interpretability to be presented here, however, concern the variability that can be attributed to a covariate measure, performance accuracy.

The definition of reaction time as an empirical question.--It was stated earlier that reaction time is commonly defined as the minimum amount of time needed for a subject to produce a correct response. For the purposes of theorizing, it is this property that makes reaction time useful. Unfortunately, this definition is not an operational or procedural definition; rather, it simply represents an assumption on the part of the experimenter or theorist. Other than the instructions

given to the subject, there is generally no evidence within a typical reaction time experiment that the times produced by the subject are really the minimum possible while the subject is maintaining accurate performance.

Whether or not subjects actually produce latencies that conform to the above requirements is, in fact, an empirical question. Consider, for example, an information processing experiment in which the subjects are instructed to work at various different rates of speed. That is, in addition to a condition in which the subjects are given the usual instructions to respond as fast as possible without making errors, there are other conditions in which the subjects are induced to work at various speeds which are either faster or slower than this normal reaction time. Thus, on some blocks of trials speed of responding is greatly emphasized; on other blocks speed is only moderately emphasized; and on still other blocks great accuracy and caution are emphasized.

Figure 4 represents the outcome of such an experiment in idealized form. Pew (1969) has called this type of function a speed-accuracy operating characteristic. Such a curve plots the relationship between the average latency of response (e.g., mean reaction time) against the average accuracy of responses (e.g., the percentage of correct responses) for each of the various speed emphasis conditions for a particular experimental task. Virtually all of the speed-accuracy operating characteristics that have been reported in the literature share the basic properties of the hypothetical data of Figure 4. They are monotonically increasing functions

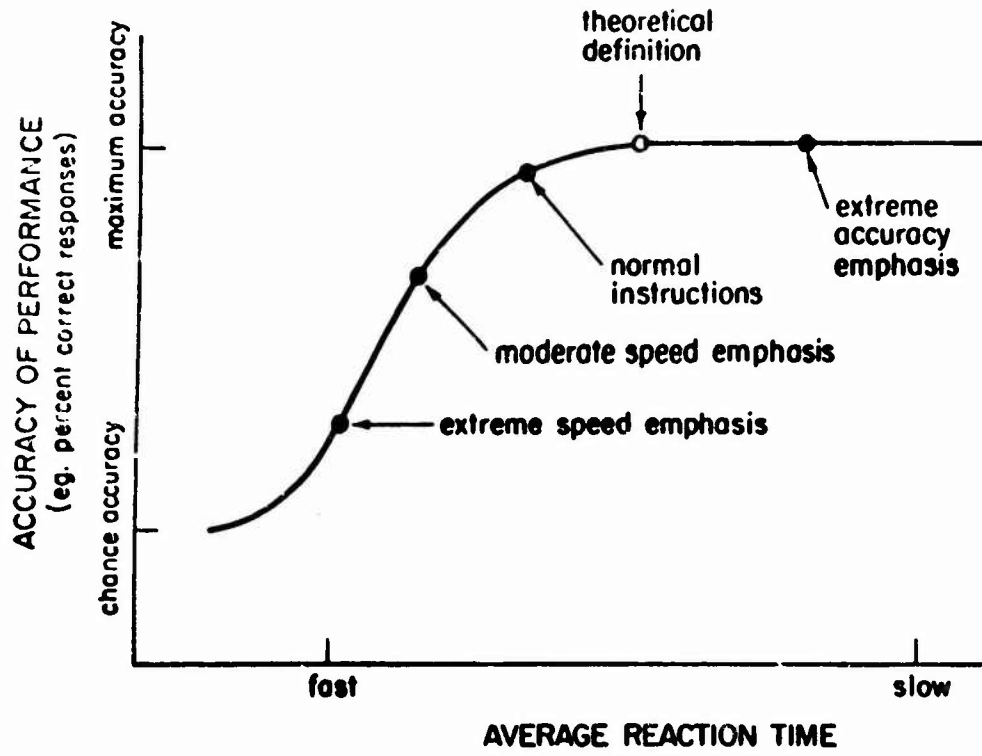


Figure 4. An idealized speed-accuracy operating characteristic.

and, when accuracy is measured in percentage of correct responses, they are typically negatively accelerated.

The open circle on Figure 4 is the point at which subjects are assumed to be operating according to the common definition of reaction time. It represents the fastest reaction time at which maximum accuracy is maintained. It is extremely doubtful that experimental subjects ever adopt this speed-accuracy criterion. Errorless performance is a rare occurrence when the usual reaction time instructions are given to subjects. Even experienced subjects whose data are exemplary in all other respects (e.g., consistent, low variability, etc.) will generally make two to three percent errors in most reaction time tasks, and much higher rates are often reported in published experiments. Thus, experimental subjects almost always operate at a point that is below maximum accuracy.

Several things should be noted about these errors with regard to their limiting effects on the interpretability of reaction time. First, low error rates are generally dismissed by experimenters as inconsequential. They are assumed to be a part of the inherent variability of performance that one can expect of experimental subjects. Such an assumption is undoubtedly wrong and can lead to serious errors of interpretation.

Reaction time measures are usually derived from extremely simple information processing tasks. Subjects would probably never make an error in them if they were not being timed, and if they were not trying to minimize their response time. Consider, for example, the vocal naming task that is often used in information processing research. This task simply requires a subject to say aloud the name of a familiar stimulus, often a

letter or number, when it is presented visually on a screen. In situations where time is not a factor, it is indeed doubtful that a literate adult would ever misname a letter or a number. Note that the occasional, "real-life" instances of mistaking the identity of a letter or number usually occur as a result of not taking enough time in responding. However, the general interpretation of reaction time assumes that subjects are not rushing their response; they are supposed to be taking the amount of time necessary for a successful completion of their task.

Thus, it seems unlikely that even low error rates are a result of inherent subject variability. More likely they result from subjects responding just a little faster than they should because they are trying to minimize their response time. Furthermore, it is not unreasonable that subjects should do this. The problem confronting a subject when faced with the usual reaction time instructions is to find the optimal speed at which to work. If he never made an error, a subject would not know if he could still go a little faster without making errors. Undoubtedly, the subject chooses a speed-accuracy criterion at which he will, in fact, rush his response--at least to the extent that he will make an occasional error, and within a range such that his error rate will seem acceptable to him and/or to the experimenter.

Second, it is not, in itself, a matter of great concern that subjects do not operate at the point assumed by the general definition of reaction time. Of much greater importance is the possibility that differences in speed-accuracy criterion may be correlated with experimental conditions. That is, it is possible that subjects may vary from condition to condition

the extent to which they tend to rush their responses. Thus obtained differences in reaction time may not be due entirely to underlying processing differences. This possibility takes on added significance when the error rates in an experiment are ignored or go unreported, since the differences in reaction time that are obtained, while artifactual in some sense, may be statistically significant.

Third, the limitations on the interpretability of reaction time as a result of variability in error rates is further accentuated by the general form of the speed-accuracy operating characteristic. The practical significance of the negative acceleration of such curves, given error rate as the accuracy measure, lies in the fact that small differences in error rate can lead to large differences in reaction time. This is particularly true for the range of high, overall accuracy (90 to 100 percent) typically found in reaction time experiments. This means, of course, that what may look like relatively meaningless error differences might contaminate reaction time values extensively.

An example of the speed-accuracy problem.--A recent experiment by Theios (1972a) illustrates the problems that can result from the above considerations. Subjects were visually presented with single digits. Their task was to simply name the digits under the constraints usually applicable in reaction time experiments, to be as fast as possible while maintaining maximum accuracy. The particular parameter of interest in the experiment was the effect of stimulus probability. Thus, in various conditions the probability of particular stimuli was varied from .2 to .8.



The results of the experiment are shown in Table I. As a result of the essentially invariant reaction times that were obtained, Theios concluded that stimulus probability had no effect on the information processing involved in this simple naming task.

However, before such a conclusion can be believed several things about the pattern of the error data must be noted. First, across all conditions the subjects averaged about three percent errors. As noted above, this is not unusual for reaction time experiments. But, again, consider the simplicity of this experimental task. These subjects made on the average three percent (and in one condition as much as six percent) errors in a task which simply required them to name a visually presented digit. Outside of the context of a reaction time experiment such an error rate in such a task would be quite unacceptable.

Second, note that the variability of the error rates was quite small. This again is typical. However, the error rates are systematically related to the probability conditions. Thus, the subjects in the .2 probability condition made six percent errors when they averaged 356 msec. per response. The relevant question to ask is: What would their average reaction time be if they were only making one or two percent errors as they did in the .7 and .8 probability conditions? In other words, if subjects had adopted a uniform accuracy criterion for all of the probability conditions, would reaction time have been found to be invariant? From a more practical point of view, the question might be put this way: How much difference in reaction time could possibly result from such small error differences?

Table I  
Mean reaction time and proportion of errors  
as a function of stimulus probability  
(from, Theios, 1972a)

Stimulus probability	.2	.3	.4	.5	.6	.7	.8
Mean naming time (msec.)	356	356	351	357	348	347	346
Mean proportion errors	.06	.04	.03	.02	.02	.01	.02

A second experiment by Theios from the same series of experiments as the one described above will perhaps shed some light on these questions. This experiment contained one condition that was methodologically identical to one of the conditions whose results are presented in Table I. In particular, it duplicated almost exactly the .5 probability condition from the experiment described above. However, in this experiment the subjects performed more accurately than the subjects in the first experiment, making fewer than one percent errors, while their reaction times were considerably slower, by about 100 msec.

Theios (1972b) has attributed this performance difference to a difference in the speed-accuracy criterion of the subjects in the two experiments. If this is the case, then it is certainly conceivable that such a speed-accuracy trade-off might produce at least as large a difference in going from the .2 to the .8 probability conditions of the first experiment. That is, in order for the subjects to have reduced their error rate by four percent in the .2 probability condition, they might have had to lengthen their reaction time by as much as 100 msec. Thus, it is doubtful that the data in Table I can be used to substantiate the claim that stimulus probability does not affect naming reaction time. It is alternatively possible that, for some reason, the subjects produced roughly equal reaction times in each of the probability conditions, thereby causing the effect of stimulus probability to appear as error differences.

Some attempted solutions to the problem of variable error rates.--The above example was selected because it illustrates particularly well the potential problems associated with variable error rates in reaction time

research. This ubiquitous problem has received little systematic attention. Nevertheless, when errors occur in reaction time experiments investigators must do something about them. Thus, several procedures have evolved and are in current use even though there exists a paucity of justification for their application.

The first and by far most common procedure for handling errors is to simply ignore them. As noted earlier, the data most often reported in published reaction time experiments are the reaction times from the correct responses only. The data from the trials on which errors are made are simply not included in the analysis of the experimental results. Furthermore, summary statistics about errors (e.g., error rate for each condition) are also sometimes missing. Such a procedure implies obvious assumptions about the relationship between correct and incorrect trials, particularly if such a procedure is believed to "solve" the error problem. In particular, the ignorance of errors assumes that the reaction time for correct responses is not affected by the overall error rate for an experimental condition.

Such an assumption, in most instances, could not be more false. In the type of experiment described earlier, which leads to the kind of speed-accuracy characteristic pictured in Figure 4, reaction time for correct responses follows a speed-accuracy characteristic which is similar to that for total reaction time. That is, when subjects adjust their speed-accuracy criterion from block to block (or condition to condition), average correct reaction time decreases as error rate increases. In fact, one thing which each of the general classes of models to be discussed in

the next section assert is this fact: average correct reaction time is inversely related to error rate. Thus, this procedure does not represent a solution to the error problem. Furthermore, justification for its use in the context of experimental reports is universally absent.

A second procedure, which would be most convenient, would be to make subjects produce the same error rate in all the conditions of an experiment. This, unfortunately, is usually impossible. However, it is often attempted to have the subject produce small differences in error rate by inducing them to be extremely conservative; to have them emphasize accuracy to a great degree. This procedure also has its difficulties. Great caution on the part of the subjects is associated with the flattest portion of the speed-accuracy operating characteristic. Small error differences close to the maximum accuracy level can be associated with large differences in reaction time. In the extreme, if subjects actually produced zero errors in all conditions (as the general reaction time instructions ask of them), the reaction times would be essentially uninterpretable. This is because an infinite number of average reaction times can result in zero errors (i.e., all points to the right of the open circle in Figure 4). Thus, very low error rates, while often the mark of a careful experiment, may also result in artifactual differences in reaction time.

Two statistical approaches to the error problem have occasionally been attempted. The first is the application of the analysis of covariance to experimental data. Such an analysis consists of a standard analysis of variance of adjusted reaction time measures. The adjustment

of the reaction time measures is brought about on the basis of a linear regression of reaction time against errors suitable to the particular experimental design. In other words, the scores that get analyzed are the reaction times predicted by a regression equation for some constant level of error rate (e.g., the mean value).

There are a number of technical statistical considerations that dictate great caution in the application of this covariance technique to the error problem in reaction time studies (see particularly, Evans and Anastasio, 1968). Furthermore, one less subtle difficulty is the assumption on the part of this analysis of a linear relation between accuracy and reaction time measures. As noted above, when the error measure is percentage of error, this relation is not linear, therefore the model underlying this statistical procedure is not suitable. However, it is possible (although not always feasible) to base the predicted reaction time measures on nonlinear functions or to apply a suitable transformation to the experimental data in order to make the relevant speed-error relation linear. The latter possibility is often accomplished by using the logarithm of the error rates, the logarithm of the accuracy odds, i.e.,  $\log \frac{p(\text{correct})}{p(\text{error})}$  or the information transmitted (in bits) by the subject's responses. Each of these transformations has the effect of producing an essentially linear speed-accuracy characteristic. Thus, under certain highly favorable conditions covariance analysis might be an aid in handling the problem of variable error rates.

A second statistical procedure that has occasionally been suggested as a solution to the error problem involves the application of the

multivariate analysis of variance (MANOVA). In short, this analysis treats the reaction time and accuracy measures as a bivariate dependent variable. The variance-covariance matrix for this vector variable can be partitioned into independent component effects in a manner analogous to the partitioning of total variance in the standard analysis of variance. These effects are then tested statistically against the null hypothesis that they simply represent samples from the same bivariate normal population. As illustrated by the Theios experiment discussed in the last section, such a procedure would seem ideal for handling the error problem in reaction time research. However, it should be noted that the constraint of a linear relation between reaction time and accuracy measures is again present. That is, the dependent vector variable is assumed to be sampled from a bivariate normal population which has the linear correlation coefficient,  $\rho$ , as a parameter. Furthermore, in situations where the application of MANOVA techniques seems reasonable the statistical-computer software necessary for complex experimental designs may still need development. Nevertheless, such a technique seems promising, even if it only awakens the realization of the multivariate nature of reaction time-accuracy data.

An experimental approach to the speed-accuracy problem.--A somewhat more drastic approach to the problems cited above is to draw conclusions about human information processing on data other than the usual reaction time measures. As pointed out earlier, reaction time measures, as commonly defined, have much intuitive and theoretical appeal; however, as also noted, it may not be possible to obtain empirical measures which coincide

exactly with these intuitive and theoretical notions. The examples to be presented below utilize as their data base, not ordinary reaction time measures, but rather the speed-accuracy characteristic itself. That is, the conclusions to be drawn are based on the relationship between reaction time and error measures over a wide range of speed-accuracy criteria.

The general experimental paradigm involves inducing subjects to work at various different rates of speed in each of the conditions in an experiment. Functions of the type shown in Figure 4 are then derived for each condition. Conclusions about the relevant information processing are based on the changes from condition to condition of the function that relates reaction time to the accuracy measures.

Inducing subjects to work at different rates of speed has been accomplished in several different ways. Hick (1952) and Howell and Kreidler (1963, 1964) verbally instructed subjects to work at different rates of speed on different blocks of trials or in different conditions. Fitts (1966) and Swensson (1972) utilized explicit payoff matrices providing monetary incentives favoring speed vs. accuracy to produce the same kind of shift in speed-accuracy criterion. Recently, Pachella, Fisher and Karsh (1968) and Yellott (1971) have described a procedure which involves defining for the subject a response time limit and asking that the subject attempt to produce his responses faster than this deadline. Subjects are provided with feedback on each trial as to whether or not they have successfully beaten the deadline and from one block of trials to the next the deadline is manipulated in order to produce shifts in the speed of responding. Each of the above techniques is quite effective in producing



trade-offs that are highly consistent across subjects. The latter deadline technique, however, is most efficient methodologically and is the procedure that is utilized in the examples described below.

At the outset of this presentation, however, one point should be made perfectly clear. While an attempt will be made to show that the data in these examples are consistent with other knowledge obtained using reaction time measures, the techniques illustrated here should not be considered as simply a correction procedure for reaction time experiments. An experiment which derives a speed-accuracy characteristic is potentially a different kind of an experiment than the usual reaction time procedure. Thus, there is no reason to believe that subjects performing under the kind of speed stress present in these experiments are necessarily processing information in the same manner that they might in the corresponding conditions of the more typical reaction time situation. In general, data obtained in this manner must be first evaluated in their own right and then, if consistent with other procedures, generalized accordingly.

Speed-accuracy relations and stimulus discriminability.--Pachella and Fisher (1969) obtained speed-accuracy operating characteristics for a task involving simple absolute judgments of spatial position. On each trial a bar marker assumed one of ten horizontal positions across a uniform visual field. The subject indicated which of the positions he judged the marker to be in by pressing one of ten keys on which his fingers rested. In addition to an unspeeded condition, three deadline conditions were used in the experiment. On some blocks of trials subjects attempted to make each of their judgments in less than 1.0 sec. On other blocks of trials the

deadline was 0.7 sec., and on still other blocks the deadline was 0.9 sec. The subjects were explicitly instructed to try to beat the deadline on each trial and within this constraint to be as accurate as possible. Stimulus discriminability was varied in two different ways: by varying the spacing of the possible stimulus positions (wide vs. narrow spacing) and by varying the contrast of the stimulus against the background (bright vs. dim background).

The speed-accuracy operating characteristics for the three stimulus conditions that were run are presented in Figure 5. Note that the accuracy measure displayed on the ordinate is information transmission which in this situation, where stimulus information is held constant across conditions, becomes a measure of performance accuracy. The results show that varying discriminability by reducing stimulus contrast has a different effect than changing discriminability by increasing stimulus similarity. While both manipulations produce decrements in performance, decreasing similarity changes the rate at which speed is traded for accuracy and reducing stimulus contrast does not.

These data are thus consistent with the stage analysis of choice reaction time tasks arrived at by Sternberg (1969a, Exp. V). This analysis distinguishes between an early stimulus encoding stage which is affected by stimulus degradation (in the present case, stimulus contrast) which adds only a constant to the time needed to attain a given level of accuracy. However, higher level processes which analyze this encoded representation in greater detail are affected by stimulus similarity which thus affects the rate at which performance accuracy is acquired.

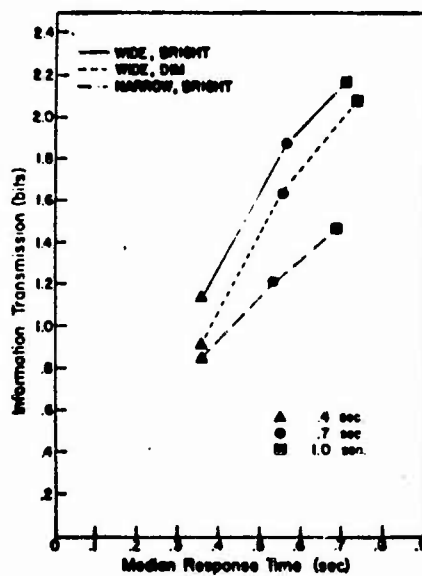


Figure 5. The relationship between information transmission and response time as a function of discriminability and speed constraint conditions. (From Pachella and Fisher, 1969.)

These data are also consistent with the distinction made recently by Garner (1970) between state and process limitations on performance and are similar to data collected by Flowers and Garner (1971) concerning this distinction. State limitations, exemplified by stimulus contrast, affect the opportunities for central information processing but not the nature of the processing itself. Thus, the rate of accuracy acquisition should not be affected by such variables. On the other hand, process limitation concerns the limits on the processing as defined by the experimental task. Thus, stimulus spacing in the present experiment defines the dimension along which the information processing must be carried out, and therefore changes in spacing affect the rate at which accuracy can be obtained.

In any event, the speed-accuracy operating characteristic is diagnostic of differences in stimulus discriminability and permits distinctions between types of discriminability. Further, the data of Figure 5 are not limited in their interpretability the way that regular reaction time measures are.

Speed-accuracy relations and memory scanning.--The short term character recognition paradigm discussed earlier (see pages 19-21 and 26-30) provides a second context in which to illustrate the direct use of speed-accuracy relations.<sup>5</sup> In this experimental task the subject is presented with a short list of letters on each trial. This list is followed by a probe letter and the subject indicates, by pressing buttons, whether or not the probe letter is contained in the list. In the present experiment subjects performed this task under normal reaction time instructions and under instructions to respond faster than response time deadlines. In a typical session the subject started out on a block of trials with normal reaction time instructions. On a second block of the session a relatively lenient response time deadline

was imposed and on each subsequent block the deadline was made more stringent.

Twenty practiced subjects participated in four experimental sessions. Each session contained five blocks of 44 trials (reaction time instructions, plus four deadline conditions). The length of the list presented on each trial was varied from session to session. Thus, within one session a subject saw only one list length. Subjects, days and list lengths were counterbalanced in order to eliminate specific practice effects.

The speed-accuracy operating characteristics obtained for each set size (i.e., list length) are presented in Figure 6. The functions for "yes" and "no" trials (not responses) are plotted separately in each panel. Note that the curves share the general features of the hypothetical curve in Figure 4. Also note that the functions for "yes" and "no" trials for each set size are quite similar--at least for set sizes 2, 4 and 5 (set size 3 seems to be a little noisy). Further, the curves for "yes" and "no" trials tend to separate as a function of set size: For set size 2 the functions are right on top of one another, while for set size 5 they are quite separate. In fact, for the set size 5 condition there is a difference of over 100 msec. between the "yes" and "no" functions for some levels of accuracy.

The import of these features (and others) can be seen somewhat more easily in Figures 7 and 8. Here iso-accuracy contours have been constructed from Figure 6. These functions represent the predicted reaction time (based

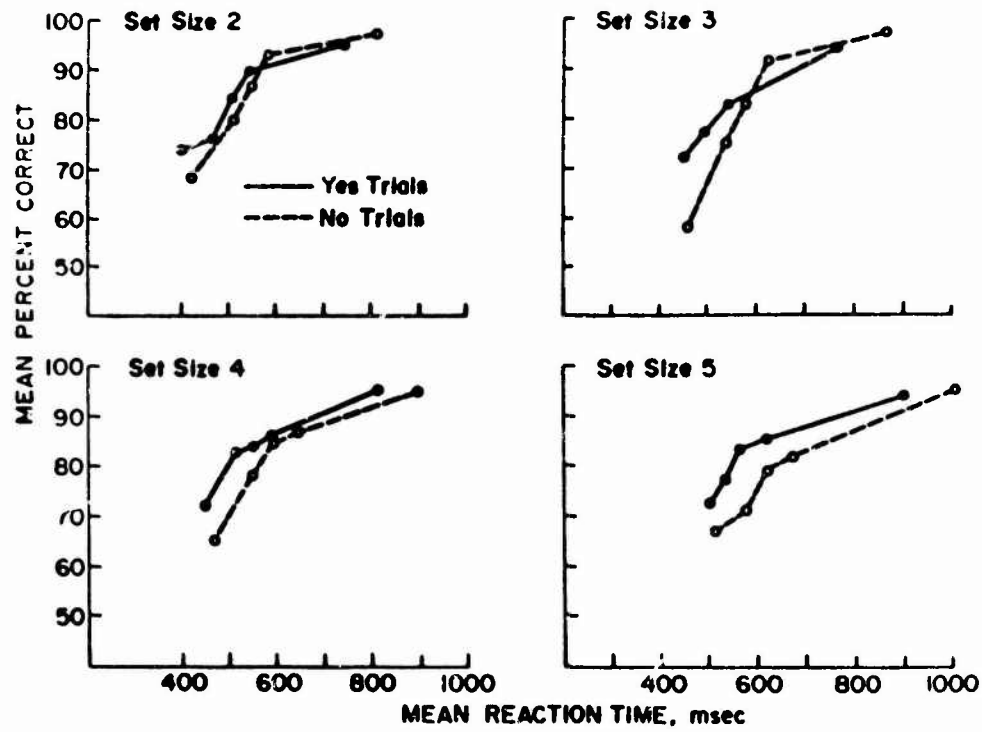


Figure 6. The speed-accuracy operating characteristic as a function of trial type and set size.

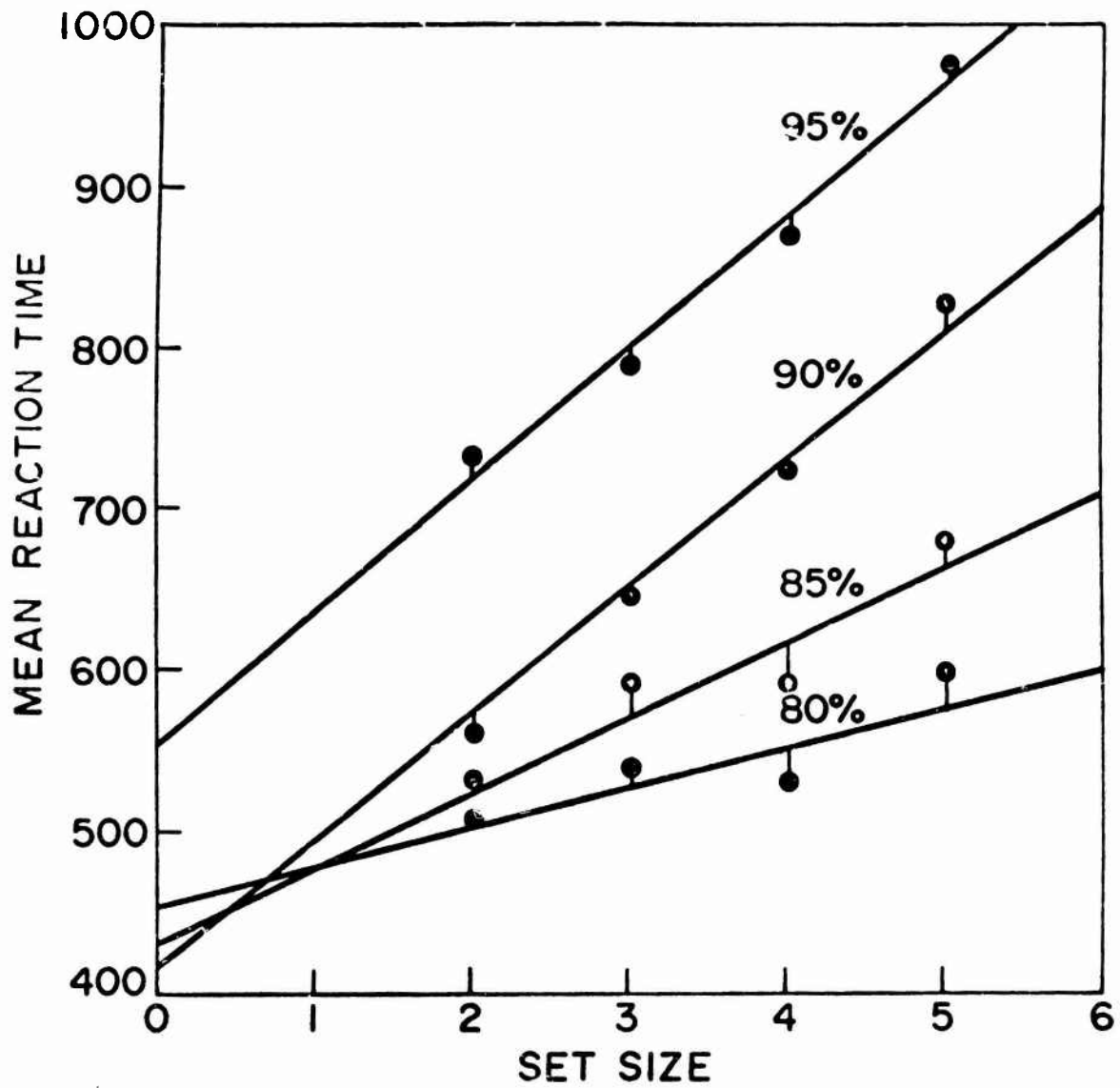


Figure 7. Iso-accuracy contours predicted for reaction time as a function of set size.

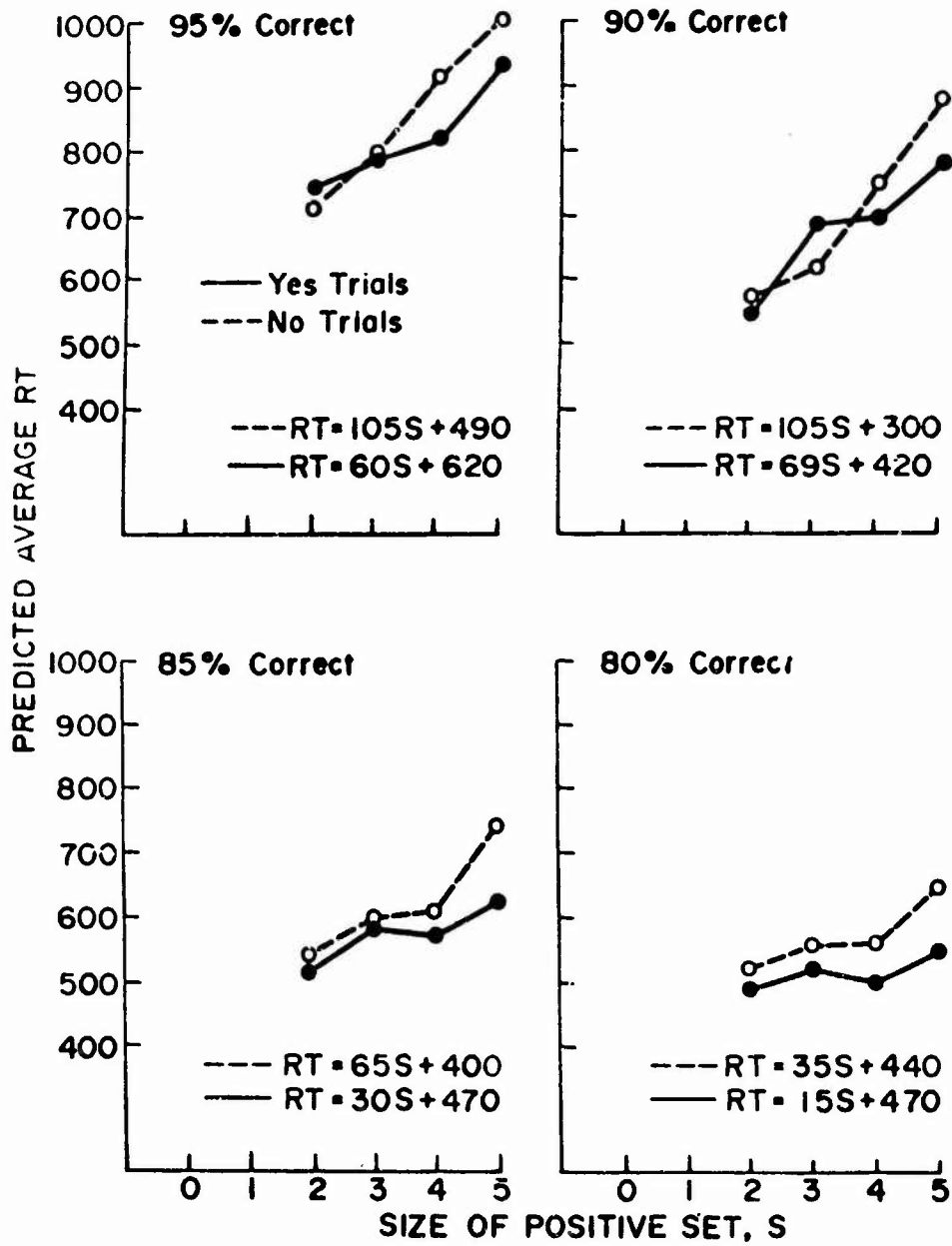


Figure 8. Iso-accuracy contours predicted for reaction time as a function of set size and trial type.



on linear interpolation) for specific accuracy levels as a function of set size.

In Figure 7 four accuracy levels, 95, 90, 85 and 80 percent correct are plotted on the same axes, averaged across trial type. The first thing to note about this figure is that in going from 95% to 90% correct, there is a large effect on the intercept of the function relating reaction time to set size. In other words, the initial effect of stressing speed appears to be located in what was described earlier as the encoding and/or the responding stages of processing. This result agrees well with a previous finding of Swanson and Briggs (1969). They placed subjects under speed stress in a task similar to the present one and, for error rates down to about 10%, found only intercept effects. The present data show, on the other hand, that if subjects are further pressed for speed, no further intercept effects are obtained, but instead the slope of the function decreases. Thus, more extreme speed emphasis seems to affect the memory searching stages of processing.

Figure 8 shows the same four accuracy contours as Figure 7, only here the "yes" and "no" trials have been separated out. In each panel the equation of the best fitting straight line for each function is also presented. First, note that the data are quite noisy when analyzed this finely. Thus, considerable caution should be taken in believing them. Nevertheless, for each accuracy level the slopes of these functions are considerably steeper for the "no" trials than for the "yes" trials. Such a result, if real, would support strongly the possibility of a self-terminating scan of memory. It should be noted, however, that besides being noisy, the present data are also somewhat contradictory. If the data from the normal reaction time

conditions (i.e., the rightmost point for each function in Figure 6) are plotted by themselves, they present evidence which is quite typical of an exhaustive scanning process. That is, these points, as shown in Figure 9, are quite parallel.

One might be tempted to construct a rather elaborate hypothesis for this situation: Under normal reaction time conditions, subjects utilize an exhaustive scan of memory. However, when pressured to emphasize speed they go to a self-terminating procedure. That is, speed stress leads to a basic change in processing strategy.

Before letting this kind of speculation get too far out of hand, though, a less dramatic possibility should be explored. This notion has to do with the general limitations of interpreting reaction time data due to the presence of variable error rates. Note the small differences in accuracy for the various list length conditions of Figure 9. The numbers above each point indicate the appropriate percent correct. These small accuracy differences are slightly correlated with set size (particularly for the "no" trials). Correlations of this type are not only typical, in many memory scanning experiments they are even stronger. By contrast, the data of Figure 8 represent an attempt (albeit somewhat of an approximation) to control for error differences. That is, the functions plotted there are equal-accuracy contours. Thus, the particular slope values that were obtained above, in either case, may be the result of subtle differences in speed-accuracy operating criteria. In other words, reaction time data in general may not be precise enough to make the distinction necessitated by the above hypotheses.

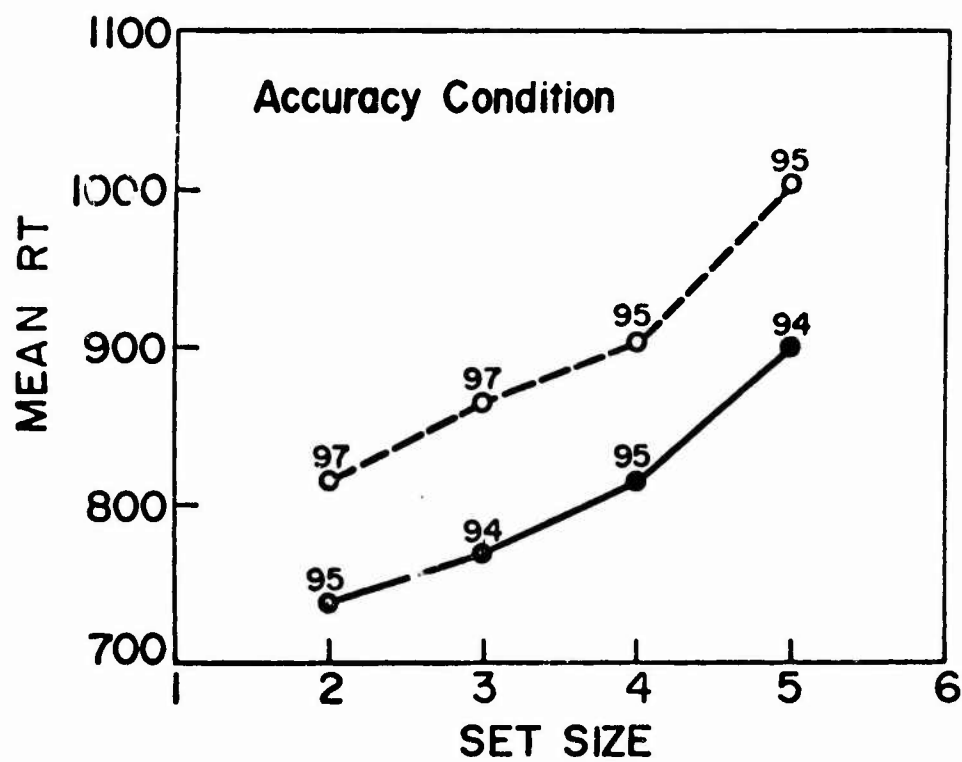


Figure 9. Reaction time as a function of set size and response type for the normal reaction time conditions. The numbers above each point indicate percentage of correct responses.

Figure 10 has been constructed to further illustrate this point. This figure has been constructed in the following way: Reaction times were selected from Figure 6 that would exactly fit the predictions of either an exhaustive or a self-terminating scanning process. The corresponding accuracy levels for each of these reaction times were also noted. These have been indicated above each point in Figure 10. The result shows that either hypothesis can be fit exactly with the data from Figure 6 and a range of only about seven percent errors is necessary to bring this about. The point is to show that either hypothesis might have been supported with data, had this been real data, whose error differences might have been dismissed as insignificant. And while the present data are not really good enough to seriously justify the strong manipulation that went into creating Figure 10, they illustrate the fact that reasonable attention must be paid to even small error rates. Thus, great caution must be exercised in evaluating this kind of data.

#### Theoretical Conceptions of Reaction Time and Speed-Accuracy Relations

The last section discussed the difficulty of interpreting reaction time data in the presence of variable error rates. Attempted solutions to this problem depend, either explicitly or implicitly, upon theoretical conceptions of the nature of speed-accuracy relations and these relations, in turn, depend upon conceptions of reaction time in general. The present section will outline a few of these formulations in their simplest form and will emphasize particularly how each conceives of the relationship between speed and accuracy. More complex versions of each of these models are available and the reader is urged to turn to the original sources for detailed analyses. The purpose of the present section is basically pedagogical: to place in some theoretical

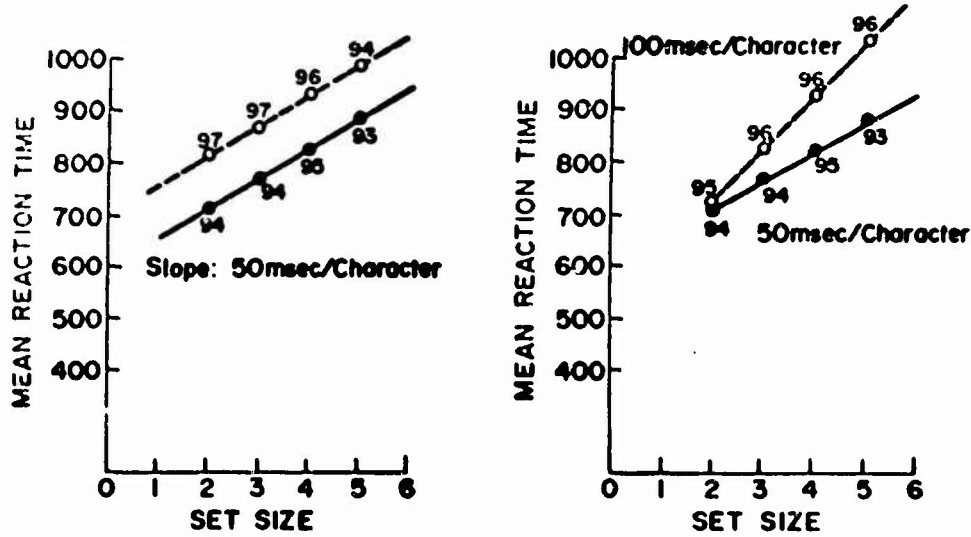


Figure 10. Hypothetical data derived from Figure 6 for two alternative memory scanning hypotheses. The numbers associated with each point indicate the predicted percentage of correct responses.

perspective the notions whose importance were noted in the last section. To be sure, as each model is made more complex it is more capable of accounting for more phenomena, but it is then more difficult to distinguish each from the others. And for the present purposes, it is the distinctions between the models that will be emphasized.

The fast-guess model.--One of the simplest conceptions of speed-accuracy relations in reaction time has been presented by Yellott (1971). This model states that the decrement in accuracy produced by emphasizing speed is due to a failure of stimulus processing on some proportion of the trials. In a series of trials under speed stress there will be two kinds of responses. One class of responses will be the same as those produced under normal reaction time instructions. That is, they will be produced in their usual manner, taking the amount of time necessary for an accurate response. These responses are called stimulus controlled responses. The other class of responses, called fast-guess responses, are initiated without the usual stimulus processing. These responses will be much faster than stimulus controlled responses, but they will have only a chance probability of being correct. The greater the emphasis on speed, the larger will be the percentage of these fast-guess responses. Thus, speed emphasis will increase error rates and decrease average reaction time.

Some of the more subtle implications of this conception become apparent if the basic speed-accuracy relation described earlier is replotted in a somewhat different form. For the moment consider a simple two-choice experiment with various speed emphasis conditions like those which led to the creation of Figure 4. Instead of plotting average total reaction time for each speed-emphasis condition, however, in this case the difference between average correct reaction time and average error reaction time, each weighted by its re-

spective proportion of trials, will be used. Thus, the quantity  $(p_c M_c - p_e M_e)$  will appear on the ordinate where  $p_c$  and  $p_e$  are the probabilities of correct and error responses and  $M_c$  and  $M_e$  are mean correct and mean error reaction times, respectively. Further, instead of simply using percent correct as a measure of accuracy, the difference between percent correct and percent error  $(p_c - p_e)$  will be plotted on the abscissa.

In situations where the model described above is correct, a function such as that shown in Figure 11 will result. The important features of such a graph (and hence, also the reasons for creating it) are as follows: First, a condition consisting of nothing but stimulus controlled responses will yield the highest and rightmost point on the function. In such a condition (for such a simple task) perfect accuracy would obtain and  $(p_c - p_e) = 1.0$ . Furthermore, the value of  $(p_c M_c - p_e M_e)$  will equal the average duration of stimulus controlled responses (shown in Figure 11 as  $\mu_s$ ) since there are neither fast-guess responses nor errors in this condition. Second, a condition consisting of nothing but fast-guess responses will yield the origin of the graph [i.e., the point (0,0)]. In such a case,  $p_c = p_e$  and  $M_c = M_e$ , since all responses are fast-guess responses. Third, Yellott (1971) has derived the fact that the form of the function for the two quantities in question, as shown in Figure 11, is linear. This fact can be seen in an intuitive manner from the realization that the intermediate points on the function result from simple linear mixtures of the conditions at the extremes of the function. Finally, given the ranges of the ordinate (from 0.0 to  $\mu_s$ ) and the abscissa (from 0.0 to 1.0), it can be seen that the slope of the function is equal to  $\mu_s$ , the average duration of stimulus controlled responses. This fact is particularly

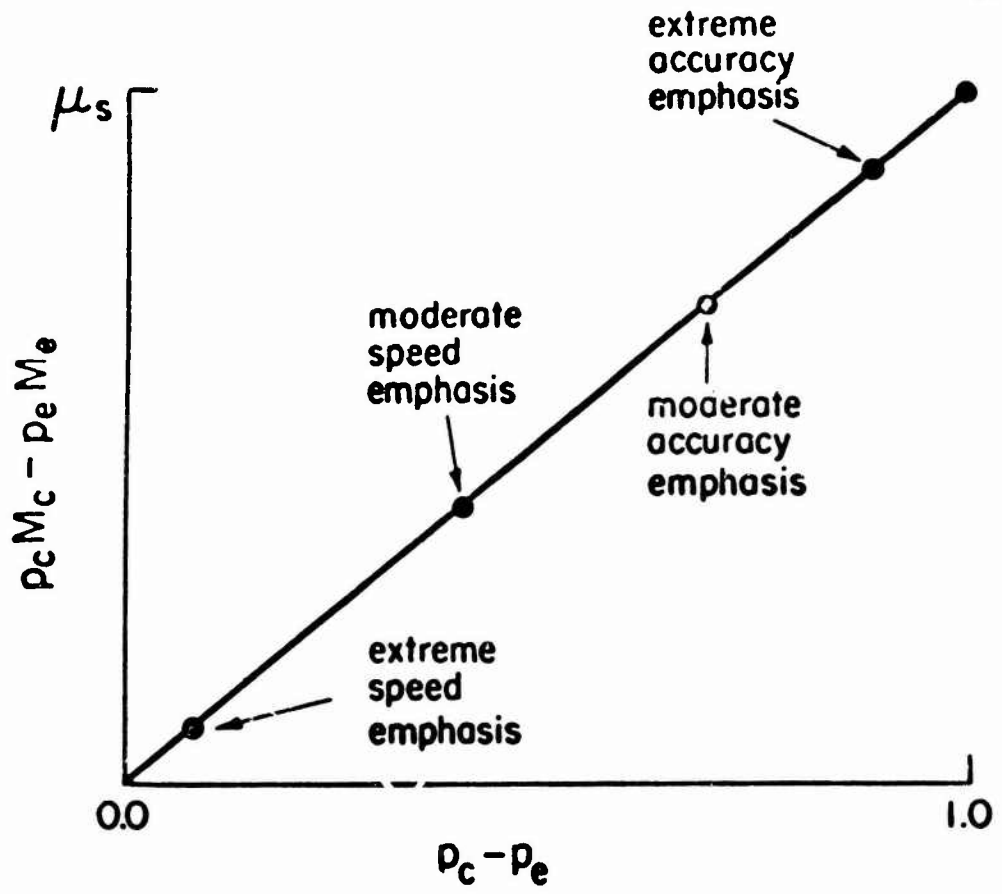


Figure 11. The form of speed-accuracy operating characteristic utilized by the Fast-Guess Model.  $\mu_s$  is the average duration of stimulus controlled responses.



valuable in situations where one wants to estimate the duration of stimulus controlled responses but where perfect accuracy conditions are not available.

On the basis of the presentation made in the last section, the fast-guess model represents a mixed blessing. On the one hand, the basic notion of a speed-accuracy operating characteristic becomes essentially an elaborate artifact if fast-guessing in this simple form were the only mechanism underlying such relations. That is, the mechanism accounting for speed-accuracy trade-offs is not a processing mechanism, but rather it is a gross, relatively peripheral strategy on the part of subjects in order to handle speed stress. Thus, the idea of using speed-accuracy relations directly in the study of information processing as illustrated earlier loses some of its force.

On the other hand, if fast-guessing is the only mechanism underlying the production of errors in reaction time experimentation, then the precise formulation given by Yellott (1971) represents a powerful solution to the problem of variable error rates. The average duration of stimulus controlled responses,  $\mu_s$ , which is easily derivable from experiments in which speed-accuracy criteria are manipulated, is exactly the parameter about which information processing researchers wish to theorize. In other words, the fast-guess model represents a theoretically well-founded correction for variable error rates.

Is fast-guessing enough?--The question must be faced as to whether or not subjects are capable of producing speed-accuracy tradeoffs in some way other than the simple strategy that was outlined above. To begin with, it should be noted that no experimental demonstration can rule out fast-guessing as a potential strategy on the part of subjects. Subjects can

always choose to make fast-guess responses if they want to. Thus, what follows below is simply an attempt to show that subjects are able to produce speed-accuracy trade-offs with some strategy or mechanism other than simple fast-guessing.

This illustration<sup>6</sup> utilizes as its experimental task a simple letter classification of the type which has been studied extensively by Posner. On each trial the subject is presented with a pair of letters. He must decide whether the two letters are both vowels or both consonants, that is, if they belong to the same CLASS, in which case he is to make a same response. If one of the letters is a vowel and the other a consonant, that is, if they don't belong to the same CLASS, he is to make a different response. A same response is to be made, of course, if the two letters differ from each other only in case (e.g., A a), that is, if they have the same NAME, since they will then be both vowels or both consonants. Trials of this type where the letters match in NAME as well as CLASS are typically classified on the order of 70 msec. faster than pairs of letters which match in CLASS only (see Posner and Mitchell, 1967). Likewise, a same response is appropriate if the letters are physically identical (e.g., A A). These PHYSICAL matches are typically 70 msec. faster than NAME matches. The stimulus sequences used in the present experiment had equal proportions of each of these three kinds of same trials, as well as equal proportions of same and different trials. Under the fast-guess strategy described above, errors produced by speed stress should be just as likely for all kinds of same and different trials, since the stimulus is not processed on error trials. In other words, if the decision to produce a fast-guess is made prior to stimulus processing, then the random presentation of trial types should yield a rectangular distribution of errors.

Six subjects were run in the experiment for five sessions, each session consisting of 440 trials. The first two sessions were practice during which subjects simply performed the letter classification task under typical reaction time instructions. The data from the last half of the second session was analyzed and the mean reaction times for the CLASS matches and the NAME matches were computed.

The last three sessions of the experiment were each divided into three blocks. On one of these blocks the subject performed the classification task just as he did during the practice sessions. On each of the other two blocks the subject attempted to produce all of his responses faster than a response time deadline. For one of these blocks the deadline was equal to the mean reaction time of the CLASS matches from the second practice session and for the other block the deadline was equal to the mean reaction time of the NAME matches from the second practice session. Note that these deadlines were individually computed for each subject on the basis of his own data from session two. The order of the blocks was counterbalanced across subjects and sessions. Subjects were given immediate feedback on each trial as to the speed and the accuracy of their responses.

The results are presented in Table II. Across the columns are the various types of trials that could occur in each block. The rows present the average percent error and mean reaction time for each block (accuracy, slow deadline and fast deadline) for the experimental days.

The basic result is quite clear. The error distributions are not rectangular. Speed stress affects CLASS matches more than it affects NAME

Table II

Mean reaction time and error rate for each trial  
type and deadline condition

		PHYSICAL SAME AA	NAME SAME Aa	CLASS SAME AE	DIFFERENT Ab
Accuracy (no deadline)	Percent Error	1.0	2.8	3.9	3.2
	Reaction Time (msec.)	510	577	661	624
Slow Deadline (equal to mean of CLASS matches)	Percent Error	1.8	3.8	16.0	5.8
	Reaction Time	471	531	593	569
Fast Deadline (equal to mean of NAME matches)	Percent Error	2.7	10.5	23.7	11.5
	Reaction Time	463	504	546	533

matches and it affects NAME matches more than it affects PHYSICAL matches. The pattern of results produced here clearly eliminates the simple version of the fast-guess model from being the only source of errors in this situation.

Cumulative speed-accuracy models.--The above experiment would seem to indicate that, at least for this task, subjects accumulate information over time and that they respond when time pressure requires on the basis of whatever information is available at that moment. According to this idea speed stress ought to affect those stimuli first which require the most processing and those stimuli last which require the least processing. Thus, as the present data indicate, CLASS matches, involving a high level of information extraction, should be affected most by speed stress, while PHYSICAL matches should be affected the least.

Numerous cumulative processing models exist as alternatives to simple fast-guessing. A number of these have been summarized by Broadbent (1971) and Audley (1973). Two will be briefly presented here because of the particular way in which they characterize speed-accuracy relations. They will be labeled the Accumulator Model and the Random Walk Model. Again, only very simple versions of these models will be presented.

Both of these models assume that responses are contingent upon central decision processes. These decisions are based on evidence about the stimulus situation that is acquired over time. Both models assume that the accumulated evidence is inherently probabilistic; that it is imperfect or fallible. The models differ from each other in the way in which the evidence is used in making decisions.

The Accumulator Model assumes that evidence regarding each possible (or expected) stimulus alternative is simply aggregated. As evidence pertinent to any particular stimulus is received, it is added in or totaled with all other evidence about that stimulus which has been received up to that moment. A decision to respond is made when the evidence favoring any particular stimulus reaches some critical value. The higher this critical value, the greater is the amount of evidence needed in order to respond. Since it takes time to accumulate evidence, high critical values will lead to long reaction times. However, decisions which result from high critical values will be based on more evidence than those which result from low critical values, thus they will have a higher probability of being correct. Cognitive adjustments to speed stress involve adjusting these critical values to which stimulus evidence must be accumulated in order to respond. Speed stress will lead to low critical values which in turn will lead to fast reaction times and high error rates.

In the Random Walk Model evidence about the various stimulus alternatives is also aggregated. However, the decision to respond is based on a relative criterion rather than an absolute one. When the evidence favoring one alternative exceeds the evidence in favor of any of the other alternatives by some critical amount, a response is initiated. The model is called a Random Walk Model because the state of the evidence from moment to moment can be conceived of as a random walk among the various alternatives. Over time the accumulated evidence will tend toward favoring one of the possible alternatives but it will have vacillations as a result of the probabilistic nature of the datum that is acquired at any moment.

Reaction time will be a function of the size of the critical value by which the evidence for a particular alternative must exceed any of the others. If this critical value is high, that is, if the evidence favoring one alternative must exceed that of the other alternatives by a large amount, then reaction times will be long. Again, however, in this situation the decision will have been based on a large amount of aggregated evidence and thus the probability of error will be small. By contrast, small critical values will lead to fast reaction times and high error rates. Cognitive adjustments to speed stress are made by adjusting the size of the critical value. The more speed is emphasized, the lower the critical value.

The important difference between the two models presented here is this: In the Accumulator Model evidence is aggregated separately for each alternative. A single datum favoring one alternative does not affect the state of evidence favoring other alternatives. In the Random Walk Model, a single datum affects the state of evidence for all alternatives, increasing the chances of one response and decreasing the chances for the others.

The following simple analogy derived from Edwards (1965) may serve to make these models more concrete. Imagine two bags full of poker chips. One bag, which will be called the "red" bag, contains 60 percent red poker chips and 40 percent blue poker chips. The other bag, called the "blue" bag, contains 60 percent blue and 40 percent red chips. One of these bags is presented to a subject and he is to determine whether it is the "red" or the "blue" bag. The subject makes this determination by sequentially drawing single poker chips from the bag. In the analogy the bags correspond to stimuli. The sequential sampling corresponds to the process whereby evidence is aggregated. Reaction time is represented by the number of poker chips

drawn before the subject reaches his decision. The mixture of red and blue chips in each bag causes the data on which a decision is based to be inherently probabilistic.

If the subject behaves in a manner analogous to the Accumulator Model he will draw chips until he has some particular number of red or blue chips, in which case he will respond accordingly. For example, he may continue to draw until he has drawn either five red or five blue chips in the sequence, in which case he will respond "red" bag or "blue" bag respectively. If he is pressed for time he may decide to respond after drawing either three red or three blue chips. In this case, however, his chances of being in error are increased. If accuracy is emphasized, the subject may decide to sample until he has drawn a total of ten or twelve chips of one color. Given the composition of the two bags, it is quite improbable (though not impossible) that this decision will be wrong.

A subject behaving in a manner analogous to the Random Walk Model will continue to sample until the number of chips of one color exceeds the number of chips of the other color by some particular amount. The larger the excess needed for a response, the more time it takes to reach a decision, but the higher the probability that the decision will be correct. Thus, the margin of difference between the accumulated totals determines the operating criterion on the speed-accuracy characteristic. Under this strategy, after some particular number of draws, if the same number of red and blue chips have been accumulated, the subject is no closer to a decision than when he started sampling. By contrast, with the Accumulator strategy each datum (or chip) contributes to reaching a decision.



Macro- vs. micro- speed-accuracy relations.--Thus far three very simple models have been presented. While these models may be unrealistic in their simplicity, their variety allows for the consideration of relationships that are useful in understanding both the performance of subjects and the research strategies employed by experimenters in the study of speed-accuracy tradeoffs.

One of these relationships concerns the average speed of responses and the error rate for those responses for various speed emphasis conditions. Up to this point the present paper has dealt exclusively with this relationship, which for the present purposes will be termed the macro-tradeoff. The three models discussed above make similar statements about this relation: Speed emphasis leads to a reduction in average reaction time and an increase in error rate. Furthermore, the experimental work presented earlier has shown that this relationship can be useful in the study of information processing.

A second relationship, which can be distinguished on the basis of these models, also involves a consideration of the speed and the accuracy of responses. In this case, however, the concern is with the speed of error responses relative to the speed of correct responses within a particular speed emphasis condition. This relation, which will be termed the micro-tradeoff, will be shown to be independent of the macro-tradeoff, at least with regard to the present models, since each of these models makes a different prediction about the reaction time for errors relative to the reaction time for correct responses. In fact, every possible ordering of the speed of errors relative to the speed of correct responses will be

represented among these predictions. Thus, the models agree about the general form of the macro-tradeoff but disagree about the form of the micro-tradeoff.<sup>7</sup>

The Fast-Guess Strategy predicts that error responses will, on the average, be faster than correct responses. This follows simply from the fact that errors occur only as a result of fast-guess responses which are assumed to be faster than stimulus controlled responses. Correct responses represent a mixture of the fast-guess responses that happen to be correct by chance, and stimulus controlled responses. This mixture will have a longer average duration than the average for error responses. However, average correct reaction time will, nevertheless, be a function of speed stress since the proportion of correct-by-chance fast-guess responses in the mixture will increase with speed emphasis.

The Random Walk Model predicts that within any speed emphasis condition, errors and correct responses will have the same reaction time. A reconsideration of the poker chip analogy will make this prediction clear. Imagine that after each draw of a poker chip the subject evaluates the aggregated evidence by using Bayes Theorem. That is, after each draw he uses Bayes Theorem to revise the odds in favor of each alternative. Such an odds revision process can be represented as a random walk between the two alternatives as illustrated in Figure 12. When the odds favoring one alternative over the other alternative reach some critical value or boundary a response is initiated. The important thing to note with regard to the micro-tradeoff is that any random walk terminating on one of the boundaries, regardless of

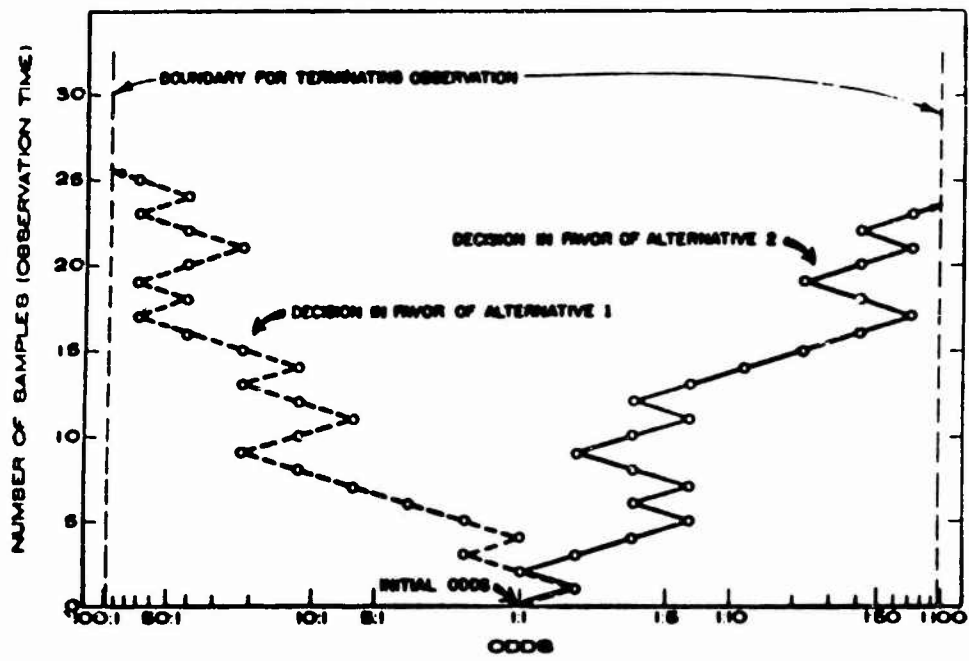


Figure 12. The Bayesian, odd-revision Random Walk Model.

how long it takes to get there, will have the same probability of being correct as any other walk terminating on that boundary. This follows from the basic definition of probability or "odds." Thus, the probability of being correct is independent of the duration of a particular random walk and correct and error responses will have the same average reaction time.<sup>8</sup>

Finally, the Accumulator Model predicts that, within a particular speed emphasis condition, errors will have longer reaction times than correct responses. Again, consider the poker chip analogy. In the Accumulator Model the subject samples poker chips until he has sampled some critical number of red or blue chips; for example, imagine the critical number to be five. A short reaction time will occur when the subject happens to sample only five chips and they all turn out to be either red or blue. Given the composition of the two bags, it would be relatively improbable to sample such a sequence from the wrong bag. By contrast, a sampling sequence consisting of five chips of one color and four chips of the other color will represent the longest possible sequence and it will have a relatively high probability of being in error. Thus, on the average, for a given criterion, errors will be associated with long sequences (by analogy, long reaction times) and correct responses with short sequences.

Quantification of the micro-tradeoff can be carried out in a more continuous manner than the simple relations described above between the means of error and correct responses. In particular, Rabbitt and Vyas (1970) and Lappin and Disch (1972a) have utilized a procedure for acquiring a function relating accuracy to reaction time for the micro-tradeoff in a form similar to that which has been described for the macro-tradeoff (e.g., as in Figure 4). Under this procedure the reaction times for a particular speed emphasis

condition are ranked from fastest to slowest. These reaction times are then grouped within this rank-ordering. Thus, for example, the fastest 100 responses might be grouped together, the second fastest 100 responses together, and so on, down to the slowest 100 responses. For each of these groups the error rate and average reaction time can then be computed and plotted against each other. Each of the above predictions about the micro-tradeoff can be generalized to data of this form: the Fast-Guess Model predicts a monotonically increasing relationship of accuracy as a function of reaction time; the Random Walk Model predicts the relation to be flat; and the Accumulator Model predicts a monotonically decreasing function.

Lappin (Lappin and Disch, 1972a, 1972b; Lappin and Disch, in press) has extensively studied these micro-tradeoff functions and has found them particularly valuable in the investigation of extremely fast processes, such as highly compatible, two-alternative reaction time situations. It has been generally found that such tasks are extremely difficult to study with macro-tradeoffs since subjects seem incapable of cognitively varying speed emphasis for such fast reactions (see, for example, Swenson, 1972). Nevertheless, Lappin has been able to identify a number of experimental variables which seem to systematically affect the micro-tradeoff.

It should be emphasized in this last regard that little is yet known about the relationship between these micro- and macro-tradeoffs. While the simple models presented here make the distinction quite clear, any reasonable complication of these models produces a confounding of the relations. For example, if in the Random Walk and Accumulator Models subjects vary their critical value for responding within a particular speed emphasis condition, the micro-tradeoffs predicted for these models will show a positive correlation.

between accuracy and reaction time. Since it seems reasonable that such variation takes place, the precise untangling of these relations awaits further experimental and theoretical development.

#### Summary

Reaction time measures have been used, and will continue to be used, extensively by researchers interested in substantive issues in experimental psychology. The value of this experimental work is naturally dependent upon the inherent strengths and weaknesses of the experimental logic with which the study of these issues takes place. This logic, in turn, is dependent upon the precision with which reaction time measures themselves can be meaningfully interpreted.

Many of these substantive issues involve questions about processes which are mediational and unobservable. Hence, they require an experimental logic which has come to be known as "converging operations." Conclusions drawn from experimental designs of this type necessitate multifactor or multiple-task comparisons. In such situations it is always possible that the relevant processing on the part of the subject may become task or condition dependent. That is, the subject's processing strategy may change drastically from condition to condition, thus the comparisons needed in order to converge on some theoretical concept may be invalidated. Obviously caution is needed in interpreting any particular set of experiments which involve such a sophisticated mode of investigation. In other words, it may be naive in such situations to allow theories and research strategies to become too dependent upon some limited set of observations. The idea of the single "critical" experiment for deciding between potential hypotheses may not be practicable for this kind of research.

Furthermore, the particular methods of converging upon information processing constructs that have been discussed in the present chapter involve powerful assumptions about the precision of reaction time measures. In particular, they involve the use of interval properties of the reaction time measures that are obtained as data from experiments. The Subtraction Method leads to conclusions based directly upon the magnitude of a difference obtained for pairs of experimental conditions and the Additive Factor Method utilizes differences between differences (i.e., interactions) in order to structure mental events. Given the variable error rates that are obtained in reaction time experiments, and the subtleties of speed-accuracy relations as discussed earlier, it is not clear that such interval properties should be ascribed to reaction time measures as they are obtained from current experiments. Indeed, for the purposes of theorizing, experimentally obtained reaction times may have only ordinal properties and even these can be negated by a large, positively correlated association between reaction time and accuracy.

Of great importance, then, to the development of methodologies involving reaction time measures in the investigation of substantive issues, is the careful study of reaction time as a measure, per se. Much more must become known about the general strategies used by subjects in the generation of reaction times before a high degree of confidence can be had about incorporating these measures in subtle substantive controversies. Until then, great patience and care must be taken in order to limit the possibility of serious error in their interpretation.

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## Footnotes

1. The preparation of this chapter was supported by the Advanced Research Projects Agency, United States Department of Defense and monitored by the United States Air Force Office of Scientific Research, under Contract F44620-72-C-0019 with The University of Michigan's Human Performance Center. The author would like to acknowledge the helpful conversations and correspondence with J. Lappin, R. W. Pew, S. Sternberg, R. G. Swensson, and E. A. C. Thomas.
2. Egeth has made this argument only by way of very informal personal communication with the author. It is reconstructed here for pedagogical purposes and should not be taken as a formal position on Egeth's part.
3. The importance of the comparison of C-reactions and choice reactions is relevant only with regard to the argument constructed earlier about the application of subtractive logic to the methodological advantages of C-reactions. The nature and value of Egeth's research is not directly dependent upon this comparison.
4. It should be noted that the original explication of the Additive Factor Method by Sternberg (1969a) was made in this broader context. The rather narrow description presented in the present chapter is done for tutorial purposes, although it accurately reflects the manner in which the method has been applied by many investigators.
5. This experiment was originally presented at the Forty-fourth Annual Meeting of the Midwestern Psychological Association, May, 1972.

6. This experiment was originally presented at the Thirteenth Annual Meeting of the Psychonomic Society, November, 1972.
7. The terms macro- and micro-tradeoff and this particular manner of discussing this distinction derives largely from many conversations between the present author and E. A. C. Thomas.
8. It should be noted that this Bayesian odds criterion version of the Random Walk Model represents a monotonic transformation of the simple difference criterion version presented earlier. This elegant demonstration of the form of the micro-tradeoff was originally made by Wilfred Kincaid of The University of Michigan's Mathematics Department.

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