In D. Scarborough & S. Sternberg (Eds.) *An Invitation to Cognitive Science, Volume 4: Methods, Models, and Conceptual Issues.* Cambridge, MA : M.I.T. Press, 1998. Pp. 703-863.

Chapter 14

Discovering Mental Processing Stages: The Method of Additive Factors

Saul Sternberg

Editors' Introduction

One of the first steps in understanding any complex process or system is to determine what its parts (or modules) are. In this chapter Saul Sternberg presents the additive-factor method (AFM), one of the principal methods cognitive scientists have used to discover the modules of a mental process. But what is a module? How do we know when we have divided a process into modules? The AFM is based on the idea that a module should be selectively modifiable: it should be possible to change the behavior of each module of a process without changing any of the other modules.

Another method with similar goals is provided by signal-detection theory (SDT), discussed in chapter 13, where it was important that sensory and decision processes can be selectively modified in detection and recognition tasks. Their selective modifiability tells us that the sensory and decision processes are distinct modules, which permits us to study them separately.

However, the SDT approach described in chapter 13 and the AFM described in this chapter use different kinds of data. Signal-detection theory focuses on *which decisions* people make under conditions where errors are frequent. In contrast, the AFM focuses on *how quickly* people make responses (their reaction times) when errors are rare. Thus, as in chapter 9 on comparing objects, chapter 10 on visual search, and chapter 15 on the use of brain waves to study attention and action, the real-time aspects of a mental process are emphasized rather than the results (the response) it produces. The modules that can be discovered by using the AFM are arranged in sequence, as *processing stages*. Evidence accumulated in recent years supports the idea of such sequentially organized modules or stages, while also indicating that, within a stage, mental operations may occur in parallel.

The AFM is applied to reaction-time data from *factorial experiments*, in which the effects of two or more experimental variables or *factors* are studied. Such experiments, introduced in chapter 12, tell us not only about the effects of each factor, but also about how the effects of different factors combine. Selective modifiability of modules of the mental process under study is demonstrated by finding that the effect of one factor on reaction time stays the same when another factor is varied. If we find that the effect of varying one factor is unchanged by varying another, the combined effect of the two factors on reaction time is simply the sum of their separate effects; that is, the effects combine additively, which gives us the name of the method.

Based on his experience explaining the AFM to a twelve-year old friend, Sternberg introduces the method in the context of a mundane analogy—a shopping trip—in section 14.3. If you work through the numerical details of this example, you will be well prepared for the analyses of mental processes that follow.

In the extensive applications section of the chapter (section 14.5), which you are invited to read selectively, Sternberg describes how the AFM has been used to answer a wide range of questions, such as: Why is it often difficult to do two things at once? Which mental processes are impaired when the prefrontal cortex of the brain is damaged? When you lose sleep, does everything you do suffer, or are there some things you are still good at? Does seeing ahead when you search a display improve performance by permitting some operations to occur in parallel?

In the last part of the chapter, Sternberg comments on various aspects of this approach to the analysis of mental processes, including the relation between processing modules and brain structures, and on the relation between the AFM and other ways of discovering mental modules, including the independent-measures method described in chapter 2.

Chapter Contents

No aphorism is more frequently repeated ... than that we must ask Nature ... ideally, one question at a time. The writer is convinced that this view is wholly mistaken. Nature, he suggests, will best respond to a logical and carefully thought out questionnaire; indeed, if we ask her a single question, she will often refuse to answer until some other topic has been discussed.
—R. A. Fisher (1926), in the paper that reported his invention of the factorial experiment.

14.1 Introduction

14.1.1 The Search for Modules

It can be argued that within the sciences the most important approach to the understanding of complex systems or processes, such as mental processes, is to analyze them into parts or *modules*—to decompose them—and then to discover what each part does, which other parts influence it, and what influence it has on other parts. Thus one of the goals of cognitive science in understanding the mental process that underlies the performance of even a simple task, such as naming a digit, is to determine what the parts of that process are.

This chapter introduces and illustrates the most widely used method for discovering the parts of a mental process. The problem is difficult because we usually have to infer the existence and properties of a part from its effects on performance of the task as a whole, rather than directly observing it. Today, in most analyses of real-time mental processes, the mind has to be considered a "black box" we cannot open.[1] The *additive-factor method* (AFM) uses data from reaction-time (*RT*) measurements in special kinds of experiments. They are special in being *factorial* experiments, in which two or more *factors* (that is, experimental variables) are manipulated in such a way that we can determine how their effects combine. The AFM has the virtues of being applicable to a wide variety of problems, as we shall see, and requiring relatively few theoretical assumptions. The method is limited, however, to discovering modules that are arranged in series, as a sequence of *stages*. This is a serious limitation, of course, as there is good reason to believe that some mental operations we would want to describe as separate parts occur in parallel. But evidence from applications of the method includes numerous findings of serial mental operations. The examples discussed in the present chapter range over naming a digit, retrieving information from memory, searching a visual display, switching between tasks, comparing disoriented objects, producing speech, deciding whether a letter string is a word in English, and others.

What sets a part apart? Given a complex process, what property might we reasonably use to divide it into separate parts, or *processing modules*? Discussions of what might distinguish the modules of a complex process (or structure) often invoke properties that either imply or are equivalent to a form of *independence*: distinct parts are *separately modifiable*. The claim that a process can be divided into two modules is supported if each of the putative modules can be modified without the other. Note that independence here does not mean that there is no manipulation that modifies both modules, only that there exists at least one manipulation that modifies one without modifying the other, and vice versa. Thus we define a processing module as an independently modifiable subprocess of a complex process.

Why should we expect complex processes to have parts? Speculative arguments have been advanced for decomposability. For example, in his "principle of modular design," Marr (1976, 485) proposed that

> any large computation should be split up and implemented as a collection of small sub-parts that are as nearly independent of one another as the overall task allows. If a process is not designed in this way, a small change in one place will have consequences in many other places. This means that the process as a whole becomes extremely difficult to debug or to improve, whether by a human de-

signer or in the course of natural evolution, because a small change to improve one part has to be accompanied by many simultaneous compensating changes elsewhere.[2]

Perhaps it is for reasons like the above that psychologists and other scientists tend to devote considerable effort to identifying the separable parts of complex processes. Or perhaps it is because such decomposition facilitates understanding, so it is worth looking for modules even if there is a chance of not finding them.

Comment 1: Parts of an object.[3] To try to determine reasonable criteria for something being a separate part of a process seems arbitrary and difficult, even though such criteria must be implicit in the way we think. Perhaps we can be guided in thinking about the modules of a process by thinking about what we mean when we talk about the parts of a physical object. What properties distinguish things that we would call separate parts of a machine? Here are some possible criteria:

Locality: The most obvious characteristic seems to be that (in most cases) different parts occupy nonoverlapping regions of space.
Detachability: A part can be detached from the rest relatively easily; the subparts of a part are more coherent than is one part with another.
Replaceability: A part can be replaced by a new part (usually in the same class, somehow defined) without causing large changes in the rest of the object.
Independence (separate modifiability): One part can be modified (e.g., wear out) without altering the other parts.
Functional distinctness: Different parts do different things, have different purposes.

Do any of these properties carry over from the parts of an object to the modules of a process? Because we want to be able to consider parallel as well as serial subprocesses, locality in space does not seem to carry over into locality in time: although the purpose of the AFM is to discover modules that are arranged in series and occupy nonoverlapping epochs, we do not want to be limited to such modules. However, separate modifiability and functional distinctness both seem to be useful defining characteristics of a part of a process. Functional distinctness is a rough concept that is hard to test, while separate modifiability is testable, and it is the latter property that is exploited in the AFM.

14.1.2 The Language of Factorial Experiments

For readers unfamiliar with factorial experiments, it may be helpful to introduce a few of the terms at this point. The *levels* of a factor are the set of its values used in an experiment. For example, the factor "gender" has the levels M (male) and F (female); the factor "size of memory set" might have the levels 1, 2, and 4. In the simplest factorial experiment (the *complete factorial experiment*) with two factors, there is a *condition* for each possible combination of factor levels. For example, consider a pattern-recognition experiment in which one factor is "orientation" of the pattern, with the three levels 0°, 90°, and 180°, and the other factor is its "familiarity," with the two levels L (low), and H (high). The complete factorial experiment would then have the six conditions (0°, L), (0°, H), (90°, L), (90°, H), (180°, L), and (180°, H).

The *effect* of a factor F that has two levels, F_1 and F_2, in a reaction-time experiment is the difference between the $\overline{RT}$s at its two levels: $\overline{RT}(F_2) - \overline{RT}(F_1)$. Thus we would measure the effect of familiarity at orientation 0° by calculating the difference between $\overline{RT}$s in two conditions: $\overline{RT}(0°, L) - \overline{RT}(0°, H)$. Because this is the effect of familiarity at one particular level of orientation, it is called a *simple effect*. The *main effect* is the mean of all the simple effects. It is convenient to number the levels of F—that is, assign values to the *index*, i, of F_i—so that the main effect is positive; we can then refer to the change from F_1 to F_2 as an "increase" in the level of the factor. The concepts of *additivity* and *interaction* to describe how the effects of different factors combine are introduced in section 14.2. And in section 14.3.3 we will see how the idea of an effect, defined here for a factor with two levels, can be generalized to a factor with more levels, such as orientation.

14.1.3 Stages, Selective Modifiability, and Invariant Factor Effects

Processing modules arranged sequentially are called "stages." Suppose a process contains stages **A** and **B**, and we discover factors F and G that demonstrate **A** and **B** are separately modifiable, such that factor F influences the duration of stage **A** (but not **B**), and factor G influences the duration of stage **B** (but not **A**). Now consider the duration of the whole process—the sum of the durations of **A** and **B**. For mental processes we are usually unable to measure the individual durations of **A** and **B** but can determine their sum (by measuring the RT). We shall see that the effect of F on this sum is the same, whatever the level of G (and duration of **B**)—it is *invariant* over the levels of G. Likewise, the effect of G on the sum is invariant over the levels of F. The combination rule for effects of factors that influence stages selectively in this way, and therefore have invariant effects on the sum of their durations is *summation*:

the combined effect is the sum of the individual effects, and the factors are said to be "additive." When, instead of being invariant, the effect of F on the sum is modulated by the level of G, then F and G are said to "interact."

14.1.4 Plan of the Chapter

In section 14.2 I describe two sets of psychological data, one from the apparently simple process used in naming a digit, and the other from three related tasks in which information is retrieved from a short list, seconds after it is memorized. Each data set provides examples of both additive and interacting factors, examples that will be interpreted later in the chapter. These examples should help to clarify the ideas of invariant and modulated factor effects and illustrate how to look at the data from factorial experiments.

In section 14.3 we will consider cases of additive and interacting factors in an everyday process with observable stages—a shopping trip. The goals are to highlight the relations between factor effects, stages, and process durations, and to show how the structure of a process can be revealed by the ways in which factor effects combine, all in a process whose structure is known. (Artificial examples such as this one can help to develop one's intuition.) Also in this section I introduce a notation for process durations and the effects of factors on those durations, a notation that facilitates thinking about invariant and interactive factor effects and will be used later in the chapter. Next, section 14.4 shows how the ideas developed in the shopping-trip example can be applied to learn about the stages of mental processes, stages that are not directly observable. This includes a brief excursion into the history of the idea of mental processing stages and the "subtraction method" that Donders developed around 1868 as a method of measuring stage durations. At this point we will be ready to consider the "additive-factor method" (AFM), which was introduced a century later as an alternative way to decompose mental processes into their constituent stages. By applying the AFM to the digit-naming data introduced in section 14.2.1, I show how it leads to an interpretation of those data that may be important for guiding more detailed theorizing about a well-known but incompletely understood phenomenon in the making of choices.

In section 14.5 I review fourteen additional applications of the method, selected for their diversity. As mentioned in the introduction to that section, these can be read selectively and in most cases are independent. In section 14.6 I stand back and consider the AFM in relation to other methods, try to clarify the kind of reasoning it calls for, consider strengths, limitations, and extensions of the method, and mention some of the issues that would be important in evaluating an existing application or designing a new one.

14.2 Additive and Interacting Factors

The raw material for the additive-factor method is the results of *RT* experiments in which two or more experimental factors have been varied. What is important for our purposes is whether sets of two or more factors have effects on mean *RT* ($\overline{RT}$) that are *additive*, or that *interact*. Because these patterns of factor effects are central to the inferences we shall draw about processing mechanisms, we start by considering some concrete examples from *RT* studies of additive and interacting factors, then go on to consider what such patterns might signify in terms of underlying processes.

14.2.1 Examples from Naming a Digit

Much of our everyday lives consists of responding with appropriate actions to signals we may or may not expect. And in some skilled activities such as driving a car or playing basketball, the speed with which we respond to these signals is important. Psychologists have tried to bring some aspects of these situations into the laboratory, in the form of the choice-reaction experiment. For each member of a set of possible stimuli, a different correct response is defined. The experimenter can manipulate the familiarity of the mapping of stimuli onto responses, for example, and the number of stimulus-response pairs. In one such experiment, the stimuli are digits, and the responses are the names of digits.

The process of naming a digit (or a letter, or a word) is rapid (about 350 ms from display to response for digits), and seems automatic, effortless, and unavailable to consciousness. A mystery that has never been adequately explained is the influence on the *RT* not only of the item presented, but also of the set of items that might have been presented: if the item is drawn from a small set of possibilities, the corresponding response can be produced more rapidly, as discussed in section 14.4.4.2. These properties make the problem of analyzing the naming process into components (without opening the "black box") especially intriguing. In an experiment to look into this (Sternberg 1969a, section 5), I varied the difficulty of the task in three different ways, manipulating the levels of three factors, to determine how their effects would combine in influencing *RT*.

On each trial the subject saw a digit and responded by speaking the name of a digit as fast as possible consistent with high accuracy. In some series of trials the correct response was the name of the displayed digit (if a "3" was displayed, the subject would speak the word "three"). In other series the correct response was the name of the next larger digit (if a "3" was displayed, the subject would speak the word "four"). The factor being varied was the *mapping* of stimuli onto responses, in particular, the familiarity of this mapping. We can call the two levels of mapping familiarity (*MF*) in the experiment "familiar" and "unfamiliar."[4] As shown in figure 14.1A, $\overline{RT}$ increases as the unfamiliarity of the mapping (plotted on the x-axis) is increased.

The second factor varied was the "quality" of the stimulus, *SQ*. The digit was either easy to read—that is, *intact*—or it was *degraded* by a superimposed masking pattern. Examples of intact and degraded digits are shown in figure 14.2. As shown in figure 14.1A, degrading the stimulus increases $\overline{RT}$.

Most important for our purposes, the figure also shows that the *effect* of *SQ*—the change in $\overline{RT}$ as the level of stimulus quality is changed from intact to degraded—is *invariant* over the two levels of the mapping-familiarity factor: for each level of *MF* the effect of *SQ* is close to 42 ms. The *SQ* effect at the two levels of *MF* is represented in the figure by the vertical separations between the data points (the open squares) on the left and on the right. For the filled squares, which have been fitted to the data, the separations are forced to be precisely equal; the lines connecting these points are parallel. To the extent that the filled squares are centered within the open squares, the plot reveals the invariance of the *SQ* effect. The invariance of the effect of one factor across the levels of another is a symmetric relation: not only is the *SQ* effect invariant over the levels of *MF*, but also the effect of *MF*—the 61 ms increase in $\overline{RT}$ as the mapping is changed from high-familiarity to low—is invariant over the levels of *SQ*. Given a data pattern expressed by the parallel lines associated with invariant factor effects (or curves, if factors have multiple levels), the effects are additive; *SQ* and *MF* are "additive factors." I explain this use of "additive" in section 14.3.

The third factor varied in the experiment was the number, *NA*, of different possible digits that might be presented during a series of trials, that is, the number of alternative S-R pairs. In some conditions $NA = 2$, and in others, $NA = 8$. (The values plotted in figure 14.1A were obtained by averaging over the two levels of *NA*; those plotted in figure 14.1B were obtained by averaging over the two levels of *SQ*.) Figure 14.1B shows that increasing *NA* caused an increase in $\overline{RT}$. (This is a case where the set of signals that might occur on a trial, but do not, influence the response to the signal that does occur, a possibility that psychologists became interested in when the theory of information was invented by Shannon (1948). With more uncertainty about what might happen, what *does* happen conveys more "information," and the time it takes a person to make the same

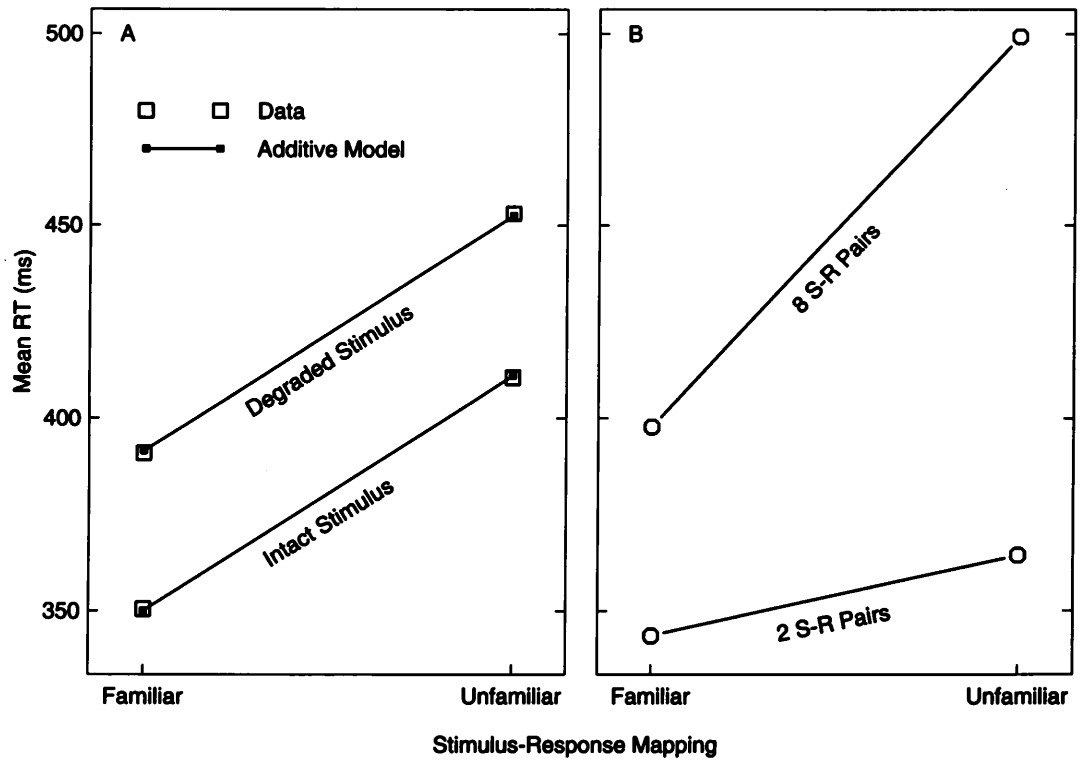


Figure 14.1
Results of experiment with digits as stimuli and names of digits as responses. Panel A shows the effect of mapping familiarity (*MF*) at each of two levels of stimulus quality (*SQ*). An additive model has been fitted to the data, and fits well, indicating that the effect of *MF* is invariant over the levels of *SQ* (and vice versa). Panel B shows the effect of *MF* at each of two levels of number of stimulus-response alternatives (*NA*). Here there is a substantial interaction, with the effect of *MF* strongly modulated by *NA* (and vice versa). Data from table 14.4.

Figure 14.2
Examples of intact (top) and degraded (bottom) digits.

response to the same stimulus increases with the amount of "information" it conveys. Reducing the number of alternatives permits the person to prepare better for the alternatives that remain.) Rather than being additive, factors *MF* and *NA* interact: the effect of each factor depends on the level of the other. Here the 102 ms effect of making the mapping unfamiliar with $NA = 8$ is about five times as great as the 21 ms effect with $NA = 2$.

Why does *MF* modulate the effect of *NA*, but not of *SQ*, and what does this tell us about the underlying process? Section 14.4.4 discusses the implications of these instances of additivity and interaction, and also considers the relation between the factors *NA* and *SQ*, which is hidden by the averaging done to produce figure 14.1. Does *NA* modulate the effect of *SQ*, and if so, what might this mean?

14.2.2 Examples from Searching Memory

For most of us, whether we are dialing a phone number or taking an exam, our greatest concern about our memory is whether it will provide valid information. We may therefore not realize that even under conditions where we are almost always correct, the time taken to retrieve memorized information (for example, the information in a list of items) varies systematically with the kind of information required (item information versus position or context information, for example), the mode of retrieval (recognition versus recall, for example), and the amount of information memorized (such as the length of the list). Figure 14.3 shows results from three kinds of experiments on retrieval of information from a memorized list of digits that reveal such effects on retrieval time (Sternberg 1969b, sections 2, 12, and 14). In all three experiments a trial begins with the subject memorizing a list presented sequentially. The length of this list is plotted on the x-axis.

In the item-recognition task the test stimulus is a single item, and the subject's task is to make a "positive" response (by pulling one of two levers) if it is contained in the list and a "negative" response if it is not.

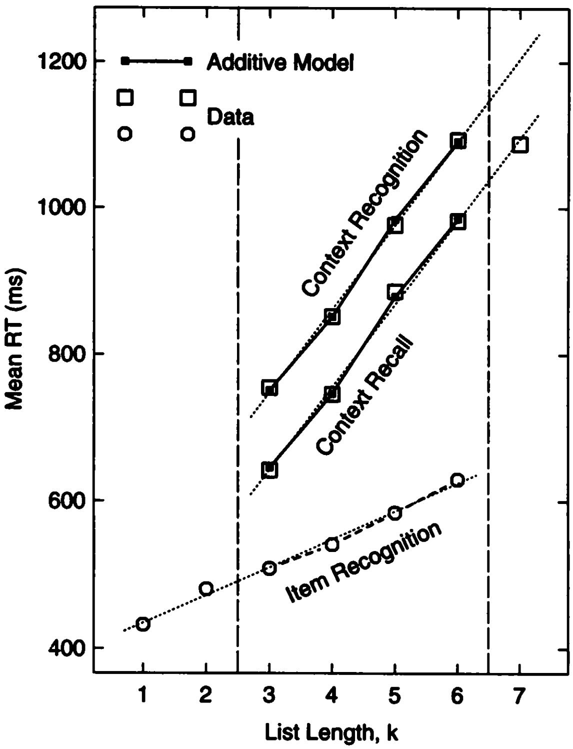


Figure 14.3
Results from three tasks requiring retrieval of information from the memory of a list. The data are represented by open circles and open squares. For the two recognition tasks, means of the *RT*s for positive and negative responses are shown. An additive model (filled squares connected by solid lines) has been fitted to the data from the two context tasks (open squares), and fits well, indicating that the effect of list length is invariant over those two tasks. On the other hand, there is a substantial interaction between list length and the item versus context task difference over the $3 \leq k \leq 6$ range they share. The linear functions fitted to the full range of each set of data (dotted lines) are $\overline{RT} = 397 + 38k$ (item recognition), $\overline{RT} = 305 + 113k$ (context recall), and $\overline{RT} = 407 + 114k$ (context recognition). Data from Sternberg 1969b, experiments 1, 7, and 8.

That is, the subject must recognize whether the test item is one of those in the memorized list. List length ranged from $k = 1$ to $k = 6$.

In the context-recall task the test stimulus is always one of the first $k - 1$ items in the list, and the subject must name the item that follows it in the list. That is, the subject must recall a list item defined by its contextual relation to the test item, and produce its name as a vocal response. In this experiment, list length ranged from $k = 3$ to $k = 7$.

In the context-recognition task the test stimulus is a pair of items displayed simultaneously (rather than a single item) that correspond to a pair of adjacent items somewhere in the list. The subject's task is to make a "positive" response (by pulling one of two levers, as in item recognition) if the left-to-right order of the pair is the same as their time order in the list presentation, and a "negative" response if the order is reversed. That is, the subject must recognize whether the contextual relation between two items is correct, and produce the appropriate manual response. List length in this experiment ranged from $k = 3$ to $k = 6$.

Figure 14.3 shows that in all three tasks retrieval time increases with list length, and, moreover, the increase is almost perfectly linear. The equations of the fitted linear functions (dotted lines) are given in the figure caption. The linearity of the effects of list length is intriguing, and is critical for some aspects of the interpretation of these data (see section 14.5.13, and appendix 2 of chap. 9, this volume). But what is important for the present chapter are the instances of additive and interacting factors that these effects reveal. To consider this, we should restrict our attention to the range of list lengths ($3 \leq s \leq 6$) for which we have data for all three procedures, that is, the data shown between the pair of broken lines in the figure.

Comparison of the two context tasks bears on a classic problem in human memory: the difference between recall (reproducing an item) and recognition (judging whether it was present). If different search processes are used in these tasks, we would expect the sizes of the list-length effects to differ, but they do not. The effect of task (the vertical separation between the top two functions) is essentially invariant—close to 105 ms—across levels of list length. Conversely, the slopes of the fitted linear functions are 113 and 114 ms per digit, essentially identical, which says that the effect of list length is invariant over the recall/recognition task difference. The filled squares connected by solid curves are the best-fitting pair of parallel curves.[5] Thus the deviations between the filled and open squares reflect the discrepancy from perfect invariance of the recognition/recall task effect over list length. A measure of this discrepancy is given by the average distance between each of the eight points and the curves, or the mean absolute deviation. Here it is less than 4 ms, remarkably small relative to the 450 ms range of the eight data points. That is, within the pair of context tasks, list length and retrieval mode (recall versus recognition) are additive factors. In contrast to such invariance of effects, the kind of information sought—item or context—interacts strongly with list length; the effect of list length for item recognition is only one third of what it is for context recognition. (Section 14.5.3 considers the meaning of these instances of additive and interacting factors.)

The orderliness of the data fitted with parallel curves in figures 14.1A and 14.3 is remarkable, and there are many other examples (a few of which are discussed in section 14.5). What might such an additive pattern be telling us about the structure of the process that generates it? And given an interpretation of additivity, what does an interaction—a violation of additivity—mean?

14.3 Effects of Factors on a Shopping Trip: A Process with Observable Stages

14.3.1 Jim and Alice's Story

My goal in this section is to introduce a basis for interpreting additive and interactive patterns of factor effects, like those in section 14.2, by describing some of the properties of processes organized in stages. Normally, we use the effects of factors on *RT* to suggest or test ideas about mental operations that cannot be directly observed. This section presents an analogy—a familiar process in the form of a shopping trip—organized in stages that are observable rather than hypothetical. We consider how the duration of the trip is affected by several variables that influence the stages of which it is composed. Working through the numerical example in this section should sharpen your intuition about ways in which the effects of different factors combine in influencing the duration of a sequence of operations. You will also encounter several principles that relate factor effects to process duration, and a notation by which such effects will be described throughout the chapter.

Jim and Alice, an elderly couple, share an apartment on the eleventh floor of a low-income high-rise building, a bus ride from the supermarket. Once a week, on their drive home from work, Alice drops Jim and their shopping cart at the supermarket. While waiting for Jim to return home from the market by bus, Alice prepares the evening meal. In organizing her cooking, it is helpful to Alice to be able to predict when Jim will arrive.

14.3.2 The Stages of Jim's Trip and the Factors That Influence Them

The time period from when Alice drops Jim at the market to when he arrives home can be decomposed into four nonoverlapping segments or epochs, in each of which Jim carries out a functionally distinct operation. These operations are *stages* of the shopping trip; one stage begins when

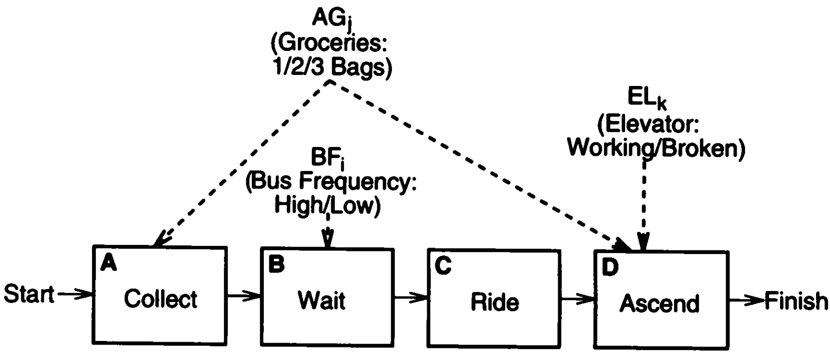


Figure 14.4
A stage model of Jim's shopping trip.

the previous stage has been completed. The stages are shown as a flowchart in figure 14.4, in which the horizontal arrows indicate sequence in time:

A. Collect groceries
B. Wait for bus
C. Ride bus home
D. Ascend to apartment

Because the operations occur in stages, the total duration on the nth trip, T_n, is the sum of the durations of the four operations on that trip:

$$T_n = a_n + b_n + c_n + d_n. \tag{14.1}$$

(I shall use "a_n" to refer to the duration of stage A on the nth trip, and so forth. Without the subscript n, the symbols T, a, b, c, and d will represent mean durations, for which the combination rule is the same as in equation 14.1: $T = a + b + c + d$.) It is worth emphasizing this important principle:

PRINCIPLE 1: ADDITIVE COMBINATION. In their influence on the total duration of a process, stage durations combine additively. (The combination rule is *summation*.)

Alice has not observed the stages, but she has kept a record of how long the shopping trips have taken Jim over the past year, and is now trying to make sense of the data. She has noticed that T varies systematically in response to three ways in which the conditions of the trip vary. That is, there are three *factors* that influence the duration of a shopping trip, also shown in figure 14.4:

BF_i. Frequency of bus service. (If there is a bus strike, there are fewer buses in service, so that the average time that Jim must wait for a bus is longer.) Factor BF_i has two levels, denoted BF_1 (no strike, high frequency, short wait) and BF_2 (strike, low frequency, long wait).

AG_j. The amount (number of bags) of groceries. Factor AG_j has three levels, one, two, or three bags, denoted AG_1, AG_2, and AG_3.

EL_k. Whether the high-rise elevator, which is often on the blink, is working or not. (If not, Jim must climb the ten flights of stairs.) Factor EL_k has two levels, denoted EL_1 (working) and EL_2 (broken).

In figures such as 14.4, factors are connected to the stages they influence by downward-going broken arrows. Let us start our analysis by considering which stages are influenced by which factors. Factor *EL* (elevator working or broken) influences only stage **D**. The extent of this influence increases with another factor, however—the number of bags of groceries (*AG*) that Jim has to carry up the stairs along with the shopping cart, when the elevator is broken. Factor *BF* (bus frequency) influences only the average duration, *b* of stage **B**—how long he must wait for the bus—and has no effect on the other stages. Factor *AG* (number of bags of groceries) influences how long Jim spends in the market (*a*) and also how long it takes him to ascend to his apartment (*d*) on those occasions when the elevator is out of service. But *AG* has no effect on *b* or *c*.

Comment 2: Variability of stage durations. Stage durations are likely to vary from trip to trip, even when the levels of *BF*, *AG*, and *EL* stay the same. In some cases the variation is best thought of as resulting from fluctuation in the levels of particular factors that are not being considered. (For example, one such factor that influences the time to collect the groceries is the number of supermarket cashiers out sick.) In other cases (such as waiting time for the bus), when the number of potential influences is very large, the variation is often treated as inherent randomness. Study of the variability itself and how it depends on the levels of factors can be very fruitful, but will not be discussed in the present chapter. And for purposes of the shopping-trip example I shall assume that the "data" are means of a sufficiently large number of observations so that the variability of these observations can be ignored, and the data can be treated as deterministic.

The information in figure 14.4 can also be expressed symbolically, by elaborating equation 14.1. We shall use *i*, *j*, and *k* to index the levels of *BF*, *AG*, and *EL*, respectively, where values of the indices are 1, 2, ... In general, the duration of stage **A**, for example, can be written a_{ijk}, to indicate the potential relevance of each factor. However, if we know that a factor has no influence on a_{ijk} we replace that factor's index by zero:

$$T_{ijk} = a_{0j0} + b_{i00} + c_{000} + d_{0jk}. \tag{14.2}$$

14.3.3 Details of the Effects of Factors on Stages of the Trip

Now we consider more precisely how the factors influence stage durations. Stage **C** is the simplest: the average duration (c) of the bus ride is 14 min, for all combinations of the levels of the three factors Alice is considering. That is, c is influenced by neither BF, AG, nor EL.

The average waiting time (b) for the bus is 4 min with no strike, and 10 min with a strike. Just as we can consider an effect on $\overline{RT}$, we can define the effect of a factor on the duration of a stage; this is especially simple when the stage is influenced by only one factor, and when that factor has only two levels, as in the case of factor BF and stage **B**. The effect of factor BF on the duration of stage **B** is the difference between the values of b at levels $BF_i = BF_2$ and $BF_i = BF_1$, or $b_{200} - b_{100} = 10$ min $- 4$ min $=$ 6 min. Because an effect is a difference, and because we will soon be taking differences of differences, it is convenient to define a difference "operator" D_i, where the subscript i indicates that the difference is taken over the levels indexed by i, that is, the levels of factor BF. Thus the effect of BF on the duration of stage **B**, just calculated, is $D_i(b_{i00}) = b_{200} - b_{100}$, and its effect on the total duration T is $D_i(T_{ijk}) = T_{2jk} - T_{1jk}$.[6] Because of the additive combination rule (equation 14.2) for stage durations, the effect of BF on T is given by

$$D_i(T_{ijk}) = D_i(a_{0j0}) + D_i(b_{i00}) + D_i(c_{000}) + D_i(d_{0jk}). \tag{14.3}$$

That is, the effect of a factor on T is the sum of its effects on the stages. An important property of D_i is that when it is applied to a quantity that is invariant over changes in index i, the result is zero. For example, $D_i(d_{0jk})$ is the difference between d_{0jk} evaluated at $i = 2$ and at $i = 1$. But the zero in d_{0jk} means that these two values are equal: $D_i(d_{0jk}) = d_{0jk} - d_{0jk} = 0$. Returning to equation 14.3, note that **B** is the only stage influenced by BF. The first, third, and fourth terms on the right are therefore zero; $D_i(T_{ijk})$ reduces to $D_i(b_{i00}) = b_{200} - b_{100}$. The relation between the effects of a factor on the durations of one or more stages and its effect on the total duration T reflects a property of a process arranged in stages that is important enough to embody in

PRINCIPLE 2: FULL EXPRESSION. If the duration of a stage increases by Δt units, and all other stages are unchanged (the increase is *localized*), then the total duration T of the process increases by Δt units.

Other measures of the process may not have the full-expression property. Suppose, for example, that we used a speed measure (number of processes per unit time) rather than a time measure (number of time units per process). A change in the speed of a stage would not be directly expressed by the change it induces in speed of the full process. The additive combination rule (principle 1) is critical; whereas stage durations combine additively, stage speeds do not. More generally, principle 2 applies to individual durations and mean durations, but does not apply to any nonlinear transformation of duration.

One important consequence of principle 2 is that if we know or are willing to assume that a factor (say, *BF*) influences only a particular stage (say, **B**), then we can use the effect of *BF* on *T* as a measure of its effect on *b*. Thus, for example, Alice need not measure the effect of a strike on how long Jim must wait for the bus. Instead, she can infer the effect of *BF* on *b* from the effect of *BF* on *T*, which, without following Jim around, is the only measurement Alice can make. (From Alice's viewpoint, the shopping trip is a "black box": the only temporal information she has is when it begins and ends.)

Collecting the groceries in the market (stage **A**) takes an average of 10 min per bag of groceries. Because *AG* has three levels, we must expand our definition of the effect of a factor.[7] The description of the effect of *AG* on *a* requires two numbers rather than one, the difference between one and two bags, and the difference between one and three bags. The effect of *AG* on *a* can therefore be written as (10, 20) min. Corresponding to this generalization of an effect, we can generalize the difference operator D_j when its subscript takes on more than two values. In this case,

$$D_j(a_{0j0}) = (a_{020} - a_{010}, a_{030} - a_{010}), \tag{14.4}$$

a pair of numbers. More generally, D_i produces a set of numbers, whose size is one less than the number of levels of the factor indexed by *i*.

14.3.4 Trip Duration Data: Additive Factors

Before considering the complexities introduced when the elevator is broken, Alice selected just the data she collected when it was working ($EL = EL_1$), and considered how the effects of *BF* and *AG* combine in affecting the total duration, *T*. When $EL = EL_1$, Jim rides up in the elevator, which takes about one minute regardless of the number of bags in his shopping cart, so that $d = 1$ min. Because *EL* does not vary and *AG* has no effect on stage **D** when $EL = EL_1$, we can simplify the diagram of figure 14.4, to get figure 14.5. The figure shows that factors *BF* and *AG* each satisfy an *assumption of selective influence*: because *BF* influences solely stage **B**, while *AG* influences solely stage **A**, the two factors influence no stage in common. Given a stage model together with selective influence, we shall see that the pattern of effects of the factors on the total duration *T* is especially simple.

Table 14.1 and the points connected by solid lines in figure 14.6 provide Alice's summary of the total durations *T* when the elevator was

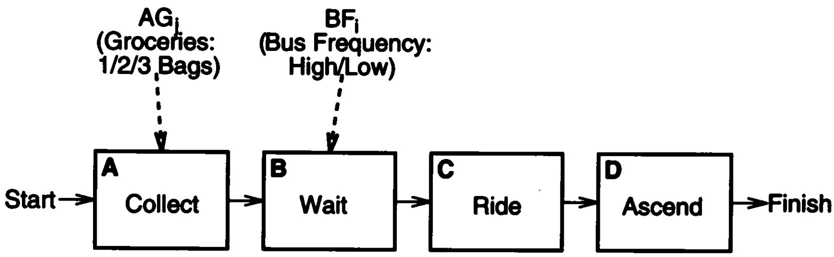


Figure 14.5
Simpler stage model of Jim's shopping trip that applies on days when elevator is working ($EL = EL_1$).

Table 14.1
Mean duration of shopping trip (in min) as a function of factors *BF* and *AG*, with factor effects shown. Factor $EL_k = EL_1$ is fixed at its first level.

Frequency of bus		Groceries (number of bags, AG_j) $AG_1 = 1$ (T_{i11})	$AG_2 = 2$ (T_{i21})	$AG_3 = 3$ (T_{i31})	Mean ($T_{i\cdot 1}$)	Effect of groceries factor
High (no strike, $BF_i = BF_1$)	(T_{1j1})	**29**	**39**	**49**	39	(10, 20)
Low (strike, $BF_i = BF_2$)	(T_{2j1})	**35**	**45**	**55**	45	(10, 20)
Mean	($T_{\cdot j1}$)	32	42	52		
Effect of bus-frequency factor		6	6	6		

working. The table shows the basic data (in boldface) for each of the six conditions, together with the means over the levels of each factor and the effects of each of the two factors. Potentially, each factor has a different effect on *T* for each level of the other factor. For this reason, there are two table entries for the effect of *AG*, and three entries for the effect of *BF*. Note that for the three subscripts, *i*, *j*, and *k* of T_{ijk}, $i = 1, 2$ denotes the row (the level of the *BF* factor), and $j = 1, 2, 3$ denotes the column (the level of the *AG* factor). As for the third subscript, $k = 1$ in every case because we are considering only the days on which the elevator was working ($EL = EL_1$). When a dot replaces a subscript, this signifies that a mean has been taken over the levels of the factor represented by that subscript. For example, $T_{\cdot 31} = 52$ min is the mean of $T_{131} = 49$ min and $T_{231} = 55$ min.

Consider first the effect of *BF* on *T*—the difference between the *T*-values when the bus drivers were on strike versus when they were not. The table shows three measures of this effect. In each case, the *BF* effect on *T* is 6 min; that is, the effect of strike versus no strike is *invariant* over

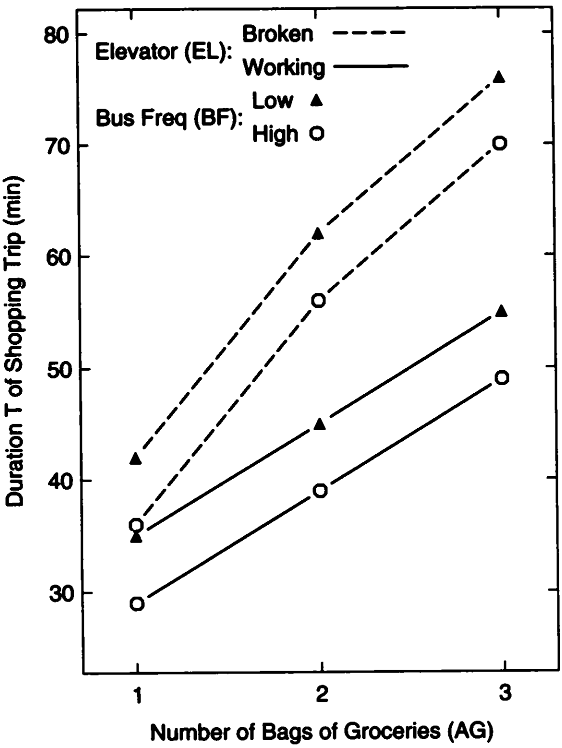


Figure 14.6
Effects on duration of shopping trip of three factors: amount of groceries (*AG*), bus frequency (*BF*), and status of elevator (*EL*).

the levels of factor *AG*. This is also shown in figure 14.6 by the equality of the three distances between each of the circles connected by solid lines and the triangle above it. As mentioned in section 14.2.1, effect invariance is symmetric: if the effect of *BF* is invariant over levels of *AG*, then the effect of *AG* is invariant over levels of *BF*. One way to see this is to note that each of these invariances implies and is implied by the fact that the two bottom curves in figure 14.6 are parallel. The constancy of the vertical separation of parallel curves tells us that the *BF* effect is invariant over levels of *AG*, and the congruence of parallel curves under vertical translation tells us that the *AG* effect is invariant over levels of *BF*. Thus the effect of *AG* on *T* is (10, 20) min, with or without a bus strike (the solid curves in figure 14.6 differ only by vertical translation).

Comment 3: Graphical representation of factor effects. Consider a plot that contains just the two bottom curves of figure 14.6. This de-

> scribes a response variable (T) as a function of the levels of two factors (AG and BF). The best way to think of such data is in terms of a three-dimensional figure in which each pair of factor levels is a point in the x-y plane, and the associated T-value is the height of a "response surface" above that plane. That is, the AG level is the x-coordinate, the BF level is the y-coordinate, and the associated T-value is the z-coordinate. The advantage of this kind of plot is that the factors are represented symmetrically. If and only if the factors have additive effects, the response surface is a plane in three-dimensional space. Unfortunately, when such three-dimensional plots are pictured in two dimensions, it is difficult to make quantitative judgments about the factor effects. And furthermore, this approach does not generalize to the analysis of effects of more than two factors. Instead, we represent factor effects asymmetrically, as exemplified by figure 14.6. Here the two bottom curves would be described as "a plot of T as a function of the level of AG, with level of BF as the parameter." The same data can be plotted as a function of the level of BF, with level of AG as the parameter (in which case we would have three parallel lines, each connecting two points). If the curves in one of these plots are parallel, then so are the curves in the other. Note that when factor effects are not invariant, there is a sense in which the graphical patterns may *not* be symmetric. Suppose we have a *crossover* interaction, an extreme form of interaction in which the *direction* of the effect of one factor (say, BF) and not merely its magnitude depends on the level of the other (say, AG): $D_i(\overline{RT}_{i1})$ and $D_i(\overline{RT}_{i2})$ are of opposite sign. When level of AG is the parameter, then the plotted curves will cross. However, when level of BF is the parameter, the curves need not cross; they will do so only if there is a *double crossover.*

The invariance property illustrates a third important principle, which follows from the second:

PRINCIPLE 3: EFFECT INVARIANCE. If a set of factors satisfies the assumption of selective influence such that they influence no stages in common, then the effect of each factor on the total duration, T, is invariant over the levels of the others.

Consider, for example, the effect of BF on T. Whatever the level of AG, the effect of BF on T is the same as its effect on b, by principle 2. Because AG does not influence stage **B**, changing the level of AG cannot change the effect of BF on b. Because the effect of BF on b is thus invariant over levels of AG, this must also be true for the effect of BF on T. Symbolically, for the values in table 14.1, we can write $T_{ij1} = a_{0j0} + b_{i00} + c_{000} +$

d_{0j1}. That the influence of *BF* and *AG* is selective is shown by i and j appearing together in none of the four stage durations. The invariance of each factor's effect on T_{ij1} relative to the level of the other is shown by $D_i(T_{ij1}) = D_i(b_{i00})$ being independent of j, and $D_j(T_{ij1}) = D_j(a_{0j0} + d_{0j1})$ being independent of i. Figure 14.6 also shows that, as it happens, the effect of *AG* on *T* is linear when the elevator is working. This follows from the linearity of its effect on *a*, together with the direct-expression principle and the absence of an effect of *AG* on any other stage.[8]

An equivalent property of the *T*-values in table 14.1 is that factors *BF* and *AG* have *additive effects* on *T*. (To reduce clutter in what follows, the third subscript, which is always 1, will be omitted.) Why "additive"? In general, contributions of two variables i and j to a measure T_{ij} are additive if the measure can be written as a constant plus a sum of functions $r(\ )$ and $s(\ )$ of the two variables:

$$T_{ij} = constant + r(i) + s(j). \tag{14.5}$$

It turns out this is guaranteed to be possible for factors like *BF* and *AG*, where the effect of each factor is invariant over the levels of the other, and conversely.[9] Note first that each of the three differences $\{T_{2j} - T_{1j}\}$ as well as the mean difference $T_{2\cdot} - T_{1\cdot}$ measures the effect of *BF*. If we let the constant in equation 14.5 be T_{11}, we can therefore use $T_{i\cdot} - T_{1}$ as the contribution to *T* due to the level of *BF*; this takes on the value zero when $BF = BF_1$, and the value 6 min when $BF = BF_2$. Similarly, we can use $T_{\cdot j} - T_{\cdot 1}$ as the contribution to *T* due to the level of *AG*; this takes on the values 0, 10, and 20 min, respectively, when $j = 1$, 2, and 3. We can now write

$$T_{ij} = T_{11} + (T_{i\cdot} - T_{1\cdot}) + (T_{\cdot j} - T_{\cdot 1}), \tag{14.6}$$

which has the form of equation 14.5. For an illustration of its validity, let $i = 2$ and $j = 3$ and extract the relevant values from table 14.1: $T_{23} = T_{11} + (T_{2\cdot} - T_{1\cdot}) + (T_{\cdot 3} - T_{\cdot 1})$, or $55 = 29 + (45 - 39) + (52 - 32)$. We can embody this idea in a corollary of principle 3:

PRINCIPLE 4: ADDITIVITY OF EFFECTS. If each of a set of factors has an effect that is invariant over the levels of the other members of the set, then the effects of the factors in the set are additive.

Another corollary of principle 3 is also sufficiently important to be made explicit:

PRINCIPLE 5: IMPLICATION OF INTERACTION. Given a process organized in stages and two factors that interact—that is, each factor modulates the effect of the other—the two factors must influence at least one stage in common.

14.3.5 Trip Duration Data: Interacting Factors

Now let us complicate the situation by considering what happens when the elevator is broken ($EL = EL_2$) and Jim must use the stairs. With the frequent rests he requires, the duration of this process is especially long, and depends, furthermore, on how many bags of groceries he has. Given that $EL = EL_2$, getting the groceries and the shopping cart to the apartment when $AG = AG_1$, AG_2, and AG_3, takes Jim 8, 18, and 22 min, respectively.[10] Thus, with $EL = EL_2$, the effect of AG on d, that is, $D_j(d_{0j2})$, can be described as (10, 14) min. Given a working elevator ($EL = EL_1$), however, the effect of AG on d is $D_j(d_{0j1}) = (0, 0)$. Note that whereas the effect of AG on a is linear, with $D_j(a_{0j0}) = (10, 20)$, because each one-bag increment produces the same 10 min time increment, its effect on d when $EL = EL_2$ is nonlinear (the increase from one to two bags adds more time—10 min versus 4 min—than the increase from 2 to 3 bags). It follows that the effect of AG on the total duration, T, is also nonlinear when $EL = EL_2$ because the effect on T is the sum of the effects on the four stages (equation 14.2), the effects of AG on b and c are nil, and the sum of the effects on a_{0j0} and d_{0jk} is (10, 20) + (10, 14) = (20, 34).

Table 14.2 provides Alice's full summary of her data, including both levels of EL; the data of table 14.1 are included in the upper half. The full data are also included in figure 14.6. The table shows the basic data (in boldface) for each of the twelve combinations of factor levels, along with the effects of each of the three factors.

The data pattern became more complicated when Alice added the second level of EL. Now we see that for AG and EL, the effect of each factor is modulated by the level of the other. Consider first the effect of AG on T. This effect, under the four conditions defined by the combinations of levels of the other factors, is shown by the four functions in figure 14.6. The change from AG_1 to AG_2 increases T by twice as much when the elevator is broken (each broken line rises by $T_{i22} - T_{i12} = 20$ min from $AG_j = 1$ bag to $AG_j = 2$ bags) as when it is working (each solid line rises by $T_{i21} - T_{i11} = 10$ min), and the increment in T due to raising the AG level from AG_2 to AG_3 is similarly greater. The effect of EL on T (distance between corresponding solid and broken lines) is only $T_{i12} - T_{i11} =$ 7 min with one bag, but $T_{i22} - T_{i21} = 17$ min with two, and $T_{i32} - T_{i31} =$ 21 min with three. As we have seen in sections 14.2.1 and 14.2.2, such modulation of the effect of one factor by the level of another is termed an *interaction*: we say that factors AG and EL "interact."

In thinking about how an interaction might be measured, it is useful to keep in mind that a two-factor or two-way interaction is a failure of the invariance of the effect of one factor across the levels of another. Thus it makes sense for the size of the interaction to measure the extent to which

Table 14.2
Mean duration of shopping trip (in min) as a function of three factors, with magnitudes of the factor effects indicated. The symbolic expression for the effect of *AG* (rightmost column) is $(T_{i2k} - T_{i1k}, T_{i3k} - T_{i1k})$.

Status of high-rise elevator	Frequency of bus		Groceries (number of bags, AG_j)			Effect of groceries factor
			$(AG_1 = 1)$	$(AG_2 = 2)$	$(AG_3 = 3)$	
Working $(EL_k = EL_1)$	High (no strike, $BF_i = BF_1$)	(T_{1j1})	**29**	**39**	**49**	(10, 20)
	Low (strike, $BF_i = BF_2$)	(T_{2j1})	**35**	**45**	**55**	(10, 20)
	Mean	$(T_{\cdot j1})$	32	42	52	
	Effect of bus-frequency factor	$(T_{2j1} - T_{1j1})$	6	6	6	
Broken $(EL_k = EL_2)$	High (no strike, $BF_i = BF_1$)	(T_{1j2})	**36**	**56**	**70**	(20, 34)
	Low (strike, $BF_i = BF_2$)	(T_{2j2})	**42**	**62**	**76**	(20, 34)
	Mean	$(T_{\cdot j2})$	39	59	73	
	Effect of bus-frequency factor	$(T_{2j2} - T_{1j2})$	6	6	6	
Effect of elevator factor, given high *BF*		$(T_{1j2} - T_{1j1})$	7	17	21	
Effect of elevator factor, given low *BF*		$(T_{2j2} - T_{2j1})$	7	17	21	

the invariance fails. The difference operator D_i introduced in section 14.3.3 provides a way to do this. Consider factors AG and EL and, for simplicity, suppose initially that AG has only its first two levels, AG_1 and AG_2. We want to measure the extent to which the effect of AG is modulated by the level of EL, rather than being invariant over the levels of EL. We could ask this question separately for each level of BF, but because BF is additive with the other two factors, the answers would be the same. For simplicity, let us consider the data averaged over levels of BF. When $EL = EL_1$, the effect of AG is $D_j(T_{\cdot j1}) = T_{\cdot 21} - T_{\cdot 11} = 42 - 32 = 10$ min. On the other hand, when $EL = EL_2$, the effect of AG is $D_j(T_{\cdot j2}) = T_{\cdot 22} - T_{\cdot 12} = 59 - 39 = 20$ min. The 10 min difference between 10 min and 20 min is usually called an "interaction contrast"; it measures the extent to which invariance fails, and takes on the value zero when the factors are additive. If you think of the difference of 10 min between the two differences as the "effect" of EL on the effect of AG, then it becomes clear that in terms of the difference operator, this measure of interaction is $D_k[D_j(T_{\cdot jk})]$. You should be able to persuade yourself that the difference operator is commutative, so that the "effect" of AG on the effect of EL is equal to the "effect" of EL on the effect of AG. Thus $D_k[D_j(\ )] = D_j[D_k(\ )]$, which we can define to be $D_{jk}(\ )$; this permits us to use $D_{jk}(T_{\cdot jk}) = 10$ min to represent the size of the interaction.

Comment 4: Application of the difference operator. It is helpful to see how the interaction measure $D_{jk}(T_{\cdot jk})$ can be developed using the difference operator in this simple case, still assuming only its first two levels for AG. To reduce clutter, the dot that indicates averaging over the levels of BF_i has been omitted (as if the basic data are the means given by the third and seventh rows of numbers in table 14.2): $D_{jk}(T_{jk}) = D_j[D_k(T_{jk})] = D_j[T_{j2} - T_{j1}] = (T_{22} - T_{21}) - (T_{12} - T_{11})$. How do we deal with the third level of AG? As indicated by equation 14.4, when a factor indexed by i has n levels, $D_i(\ )$ is a set of $n - 1$ values. In the present instance, the effect of AG has two components, $D_j(T_{jk}) = (T_{2k} - T_{1k}, T_{3k} - T_{1k})$, and therefore, $D_{jk}(T_{jk}) = [D_k(T_{2k} - T_{1k}), D_k(T_{3k} - T_{1k})] = [(T_{22} - T_{12}) - (T_{21} - T_{11}), (T_{32} - T_{12}) - (T_{31} - T_{11})] = (10 \text{ min}, 14 \text{ min})$. To demonstrate the commutativity of the difference operator, we can reverse the order of application: $D_{jk}(T_{jk}) = D_j(T_{j2} - T_{j1}) = D_j(T_{j2}) - D_j(T_{j1}) = (T_{22} - T_{12}, T_{32} - T_{12}) - (T_{21} - T_{11}, T_{31} - T_{11}) = [(T_{22} - T_{12}) - (T_{21} - T_{11}), (T_{32} - T_{12}) - (T_{31} - T_{11})]$, as before.

In contrast to the complexity of the $AG \times EL$ interaction, the invariance of the effect of BF we saw in the more limited analysis above carries over to the full data. All six measures of this effect shown in table 14.2 (and the corresponding distances between the circle and triangle in each of the six pairs of points in figure 14.6) are equal. And the converse also extends to the full data: the effect of each of the other factors is invariant over levels of BF. Thus, if $EL = EL_2$, then the effect of AG on T is (20, 34) min, with or without a bus strike (the broken curves in figure 14.6 are parallel). The consequence is that at each level of EL, the effects on T of BF and AG are *additive*; you should be able to convince yourself that a representation like equation 14.5 is possible for the six durations in the lower half of the table, but with the last term taking on the values 0, 20, and 34. Similarly, BF and EL are additive factors. The evidence for this in figure 14.6 is that for each level of AG (value on the x-axis), the separation between the pair of circles (effect of E with no bus strike, i.e., $BF = BF_1$) is the same as the separation between the pair of triangles (effect of E with $BF = BF_2$).

To show the additivity of factors BF and AG at each level of EL using the difference operator, we can prove that their interaction must be zero. The interaction is $D_{ij}(T_{ijk}) = D_{ij}(a_{0j0}) + D_{ij}(b_{i00}) + D_{ij}(c_{000}) + D_{ij}(d_{0jk})$. In each of the four arguments on the right, either the i-subscript or the j-subscript is zero (as in the cases of a_{0j0} and b_{i00}, respectively), which means that each stage duration is invariant either over the levels of BF or over the levels of AG, and that taking the difference with respect to the corresponding subscript will produce zero. It follows that both terms in the resulting two-term interaction measure are zero: $D_{ij}(T_{ijk}) = (0, 0)$. (Recall that there are two terms in the interaction measure because AG has three levels.) Two factors are additive if and only if all the terms in their interaction measure are zero. Alice does not know about the component stages, of course, so must discover the additivity of BF and AG by applying D_{ij} to the observed data. When $E_k = E_2$, for example, she finds: $D_{ij}(T_{ij2}) = D_i(T_{i22} - T_{i12}, T_{i32} - T_{i12}) = [(T_{222} - T_{212}) - (T_{122} - T_{112}), (T_{232} - T_{212}) - (T_{132} - T_{112})] = [(62 - 42) - (56 - 36), (76 - 42) - (70 - 36)] = [0, 0]$.

On the other hand, AG and EL are interacting factors, not additive ones; the effect of each of these factors depends on the level of the other, rather than being invariant over the levels of the other. Given a fixed level of bus frequency, for example $BF = BF_1$ (no strike), the T-values in the 2×3 table composed of the first and third rows of boldface values in table 14.2 cannot be expressed additively as in equation 14.5. For example, instead of an equality analogous to equation 14.6, we find that $70 > 29 + (36 - 29) + (49 - 29)$. In figure 14.6, this interaction is indicated by the fact that instead of being parallel, the solid and broken curves joining the circles diverge. Because the direction of the inequality means that raising the level of one factor increases the effect of the other, this is a *positive* (or *overadditive*) *interaction.*

A two-way interaction can be thought of as measuring the extent to which a second factor modulates the effect of a first. Likewise, a three-way interaction can be regarded as measuring the extent to which a third factor modulates the interaction of the first two factors. Again the difference operator is helpful; the three-way interaction is given by $D_k(D_j[D_i(T_{ijk})])$, which we define as $D_{ijk}(T_{ijk})$. For Jim's trip, from equation 14.2 we have

$$D_{ijk}(T_{ijk}) = D_{ijk}(a_{0j0}) + D_{ijk}(b_{i00}) + D_{ijk}(c_{000}) + D_{ijk}(d_{0jk}). \tag{14.7}$$

Because each of the terms on the right has at least one zero as a subscript, at least one of the differences represented by D_{ijk} will be zero; thus for the shopping trip data the three-way interaction is (0, 0). This also follows from the fact that *BF* is additive with both *AG* and *EL*, which means that *BF* does not modulate the two-way *AG* × *EL* interaction. (Like two-way interactions, three-way interactions are symmetric: a zero three-way interaction implies that none of the three two-way interactions is modulated by the remaining factor. Given three factors, *F*, *G*, and *H*, therefore, if *F* does not modulate the *G* × *H* interaction, then the three-way interaction is zero. This in turn means that *G* does not modulate the *F* × *H* interaction, and *H* does not modulate the *F* × *G* interaction.)

14.3.6 Conclusions from Jim and Alice's Story

In our considerations about the shopping trip thus far, we have been deducing the properties of the total duration data *T* from what we know to be the structure of the underlying process—from our specification of the stages and of the relations between factors and stages. That is, we have been developing a set of predictions from a theory. Now let us adopt Alice's perspective; she knows only about the data—the values of *T* in table 14.2—and not directly about the stages or their relations to the factors.

Like any scientist with a set of data, Alice cannot apply a process of deduction to the data to arrive at the correct theory. It is true that if the data are sufficiently reliable, they can be used to deductively *reject* some theories. For example, the fact that *AG* and *EL* interact requires Alice to reject any theory according to which *AG* and *EL* influence no stage in common. (In general, factors that interact must influence at least one stage in common; see principle 5.)

On the other hand, consider what Alice can make of the additivity of *BF* with *AG* and *EL*. Such data *strengthen* the class of theories in which there are two or more stages and *BF* influences no stages in common with *AG* and *EL*, but such data do not *prove* that the correct theory is in that class. The extent to which that class is strengthened by the data depends, for example, on how unlikely it is that if *BF* and *AG* influenced the *same* stage, the effect of *BF* on *T* would be invariant over levels of *AG*. The last word has not been said about this matter, but the consensus among people who have thought about it is that in virtually every instance where it seems as if two factors that influence the same stage have additive effects on its duration, further analysis shows that stage to be decomposable into two or more stages that are selectively influenced by either one or the other of the two factors, but not both. Thus, while the converses of principles 3 and 5 are not valid as deductive relationships, they are useful working hypotheses:

PRINCIPLE 6: IMPLICATION OF ADDITIVITY. Given a process organized in stages, and two factors with additive effects, it is a reasonable working hypothesis that they influence no stage in common.

PRINCIPLE 7: IMPLICATION OF TWO FACTORS INFLUENCING THE SAME STAGE. Given a process organized in stages, and two factors that influence the same stage, it is a reasonable working hypothesis that they interact.

Thus it appears that factors that influence the same stage are unlikely to produce additive effects, whereas factors that influence stages selectively are predicted to have additive effects. It is for these reasons that the observation of additivity strongly favors separate stages with selective influence. The response of *T* to the three factors thus supports the following "theory" that Alice develops about Jim's shopping trip:

1. The trip is composed of at least two stages. (This follows from the additivity of *BF* with the other two factors. If *AG* and *EL* were additive with each other as well as with *BF*, three stages would be indicated.)
2. One stage is influenced only by *BF*, and not by *AG* or *EL*. (This is indicated by the additivity of *BF* with *AG* and with *EL*.)
3. Another stage is influenced by both *AG* and *EL*. (This is indicated by the interaction of *AG* and *EL*, which requires them to influence at least one stage in common.)

Without using more information (such as the linearity or nonlinearity of the effect of *AG* on *T*, and its dependence on the level of *EL*) or collecting information about the effects of new factors (such as the effect of rain, which might lengthen only the bus ride C, or the effect of Jim's losing his eyeglasses, which might lengthen only the process of collecting groceries at the market—A), Alice cannot use the total duration data to test conjectures she might have about the existence of a stage influenced only by *AG*, or about the existence of stage C.

14.4 Stage Models and the Effects of Factors on Mental Operations

14.4.1 Stages: The Modules of a Sequential Process

Having considered the observable stages of a shopping trip, we now turn to the hypothetical stages of a mental process, shown as a flowchart at the

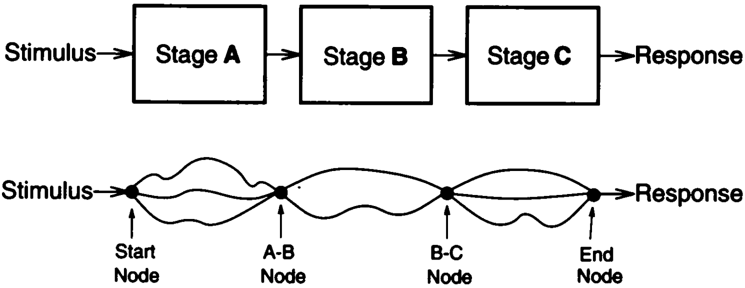


Figure 14.7
Two representations of a stage model of a process. In the box-and-arrow diagram, boxes represent epochs in time during each of which some operation is accomplished, and arrows represent their sequence. In the node-and-path diagram, the process moves through a space in which each point is a state of the information-processing system.

top of figure 14.7. It was in the early days of experimental psychology, in the mid-nineteenth century, that the idea first arose that the time between stimulus and response might be occupied by a series of processing stages, or successive operations so arranged that one operation does not begin until the preceding one has ended. Such a *stage theory* implies that the RT is a *sum*, composed of the durations of the stages in the series, just like the total duration of the shopping trip represented by the flowchart of figure 14.4. Examples of plausible stages in a RT task (such as naming a digit) are the registration of the stimulus, its identification among the set of alternative stimuli, the selection of one of the response alternatives, and finally the organization and execution of the response.

A stage of processing consists of the operation or operations that occur during an epoch in time. For a mechanism with two stages, for example, the stream of events between stimulus and response can be cut at some time point such that the operations before the cut are defined as stage **A** and the operations after the cut as stage **B** and such that the operations carried out in stages **A** and **B** are functionally distinct. Thus, in a mechanism with two stages, functionally distinct operations are carried out in temporally distinct serially arranged nonoverlapping epochs.

In connection with the shopping-trip example, we noted the idea of selective influence of a factor on a stage. Suppose factor F influences only the duration of stage **A** and factor G influences only the duration of stage **B**. Then F acts only during the epoch before the cut, while factor G acts only during the epoch after the cut. Thus, if we consider the occurrence times of all operations whose durations are influenced by factor F, and similarly for factor G, these two sets of occurrence times will be contained in two different serially arranged nonoverlapping epochs.

A warning is in order. Although it is tempting to identify stages (processes) with brain structures (processors), a stage theory says nothing about the pieces of physical/anatomical machinery that carry out the operations in the two stages. For example, whereas the stages are distinct, the pieces of machinery need not be. Thus information "transmitted from one stage to the next" does not necessarily go from one place (in the brain) to another; the phrase is unfortunate because it suggests otherwise. (See section 14.6.1 for discussion of possible relations between processing stages and brain structures.)

Although the idea of processing stages is an old one, it is only during the past three decades that convincing evidence for the existence of such stages became available. Indeed, before then, it was not really apparent what would constitute good evidence. When can we say that a stage theory applies? What properties should hold for the components of RT that correspond to stage durations? How can we determine that an analysis into particular processing stages is a good one? Given a series of stages that have been identified, how can we locate the effects of a new factor in the series?

An alternative way to think about a process with stages is shown at the bottom of figure 14.7 as a series of nodes in a *state space*, connected by alternative paths. Each point in the space corresponds to a different state of the information-processing system. During a processing stage, the system moves from one node to the next by traversing one of the alternative paths. For example, during stage **B**, the system moves from the state specified by the A-B node to the state specified by the B-C node. Some paths take longer than others; a change in the level of a factor that influences a stage selectively can be regarded as altering the path taken, but not the nodes. It is important that the duration of a stage not depend directly on the durations of stages that precede it. This is achieved if the state that corresponds to a node is independent of the path used to arrive at that state (although the time to arrive there is determined by the path). Such a system satisfies a property of "independence of path" reminiscent of Markov chains.

The state and path description need be only as complete as is necessary to determine the incorporated stage durations. Consider, for example, the process described in figure 14.5. Factor *AG* determines what Jim carries with him during stages **B**, **C**, and **D**. However, as *AG* does not influence the durations of these stages, neither the paths after the A-B node nor any of the nodes need reflect the *AG* level. For purposes of determining stage durations, *AG* and *BF* influence only the paths (and times) to arrive at the A-B node and to move from the A-B to B-C nodes, respectively.[11]

Figure 14.8
Frans Cornelis Donders (1818–1889) in 1875.

14.4.2 The Subtraction Method of Frans Cornelis Donders (1818–1889)

The first person to apply a stage theory to mental processes was F. C. Donders, a Dutch scientist also famous for his work in ophthalmology (see figure 14.8).[12] Donders (1868) proposed and demonstrated the *subtraction method* as a way of measuring the times taken by mental operations. To use this method, we construct two different tasks in which RT can be measured, where the second task is thought to require all the stages of the first plus an additional inserted stage. The difference between the $\overline{RT}$s in the two tasks is interpreted as an estimate of the mean duration of the inserted stage. For example, Donders compared a selective-reaction ("go/no-go") task (where the subject had to make a prespecified response to only one of a set of alternative stimuli) to a choice-reaction task (where the subject had to respond differently to each of the stimuli). Donders assumed that in both tasks a stage of stimulus identification was required but that only in the second task was there also a stage of response selection. The difference between $\overline{RT}$s in the two tasks was therefore taken to be the duration of the response-selection stage.

The subtraction method ran into trouble around the turn of the century.[13] Donders' interpretation of his data depends on the validity not only of the stage theory but also of what I have called an "assumption of pure insertion" (Sternberg 1969b). This assumption states that changing from task 1 to task 2 merely inserts a new processing stage, without altering the other stages. Critics of the method argued, however, that the task change also altered the other stages, basing their argument partly on the unreliability of the stage duration estimates produced by the method and partly on introspective evidence. Given failure of the pure-insertion assumption, the difference between $\overline{RT}$s could not be identified as the duration of the inserted stage.[14] As we shall see in section 14.5.8.2, the subtraction method can be regarded as a special case of the AFM. The assumption that a task change causes pure insertion of a stage is a strong version of the assumption that the change in a factor level has selective influence on a stage: insertion (or deletion) is an extreme form of duration change. The purity of the insertion is similar to the selectivity of the influence: both can be tested by determining whether factor effects are additive. (Think of the task 1–task 2 variation as a factor. If changing the level of this task factor does nothing other than insert a stage—pure insertion—then the effects of factors that influence other stages should be invariant over levels of the task factor.)

One modern version of the subtraction method arises in the study of processes with multiple operations of the same kind, as in comparing visual patterns (as discussed in chap. 9, this volume), searching visual displays, or retrieving information from memorized lists. Instead of attempting to insert qualitatively different stages into the process, this work attempts to vary the number of similar stages. This is done by controlling the number of elements to be searched, or the number of relevant features or elements in the patterns to be compared. The hope is that this kind of task difference will be more likely than the ones Donders (1868) used to satisfy the pure-insertion assumption. (For discussion of this modern version of the method, see appendix 2 of chap. 9, this volume, and section 14.5.11.1 on substage equivalence.)

14.4.3 How Plausible Is It for Mental Processes to Be Sequential?

Given that mental processes are carried out by the brain, a system with 10^{12} highly interconnected but relatively slow elements that are able to function concurrently, it seems reasonable to expect mental operations to be carried out in parallel. Nonetheless, we find that some mental processes are carried out sequentially, even when a person is performing a practiced task under time pressure. And it is on this finding that the use of the AFM depends. As discussed in section 9.2.5, some pairs of processes *P*1 and *P*2 are *data-dependent,* in the sense that *P*2 requires information that is produced by *P*1. In this case sequential organization is unsurprising (in terms of the shopping-trip analogy, Jim cannot take his groceries home—stages **B**, **C**, and **D**—until he has finished collecting them at the market).

Perhaps more surprising is the finding of operations that are partially or wholly sequential when there is no apparent data dependence. Examples we shall encounter below are the selection of responses in two independent tasks (section 14.5.9), the recognition of the words in a pair (section 14.5.11), and the sequence of tests carried out during some kinds of visual search (section 14.5.14). The basis for the sequential structure in such cases may be that the system that carries out the set of operations, possibly the same "single" processor, is inherently limited in *capacity.* (If Jim had to retrieve a pair of Alice's shoes from the repair shop as well as collecting groceries at the market, these two operations would have to occur sequentially, because Jim cannot be in two places at the same time.) Another possible basis is that the computational overhead required to coordinate the operations of parallel processors may sometimes make them less efficient than they might appear.

Given what we know about the brain, it has been argued that models of mental operations should make use of "brain-style computation"—parallel distributed processing in which numerous autonomous elements participate concurrently in each computation (see, for example, Anderson, chap. 7, this volume, and Rumelhart 1989). It has to be recognized that there is no inconsistency between a global analysis that reveals sequential operations, and local analyses that show each of these operations to be accomplished by "brain-style computation." Returning to the shopping-trip analogy, our analysis of that process into four stages was at a global level. Within a stage, at a lower, more local level, it is clear that some operations occur in parallel. For example, during stage **A**, Jim does not wheel his cart separately from the market entrance to the location of each item he needs, and if he needs two of something he may reach for them and put them in his cart concurrently, with his two hands. And during stages **B** and **C**, the individual items that Jim purchased are "processed" in parallel.

In the real world we often have to deal with a continuous stream of stimuli, each of which may require some level of analysis and possibly a response. It is appropriate that laboratory tasks be simple relative to real ones; one frequent simplification made by experimental psychologists is to use the *discrete-trials paradigm*, presenting a single test stimulus on each of a series of separated trials, rather than a continuous stream. However, performance in the discrete-trials paradigm cannot reveal the kind of parallel processing called "pipelining" (Harel, 1992; Johnson-Laird, 1983). Here each stimulus S_k in the stream is subjected to a sequence of operations, $\mathbf{A_k}$, $\mathbf{B_k}$, and so on, just as each car in an automobile assembly line is created by a sequence of assembly operations. Under pipelining, however, the **A** process for one stimulus and the **B** process for another can be concurrent. For example, as soon as S_k has advanced from process $\mathbf{A_k}$ to $\mathbf{B_k}$, process $\mathbf{A_{k+1}}$ can be applied to S_{k+1}. That is, whereas different operations are performed sequentially on the same stimulus, they are performed concurrently on different stimuli, just as installation of the engine of one car can be done concurrently with installation of the transmission of another. When a discrete-trials experiment indicates that individual stimuli are processed by a sequence of successive mental operations, this is sometimes taken as evidence against parallel processing by the brain. But the observation of such sequential operations does not preclude the concurrent application of different mental operations to different stimuli, as in an assembly line. (In section 14.5.14, we shall see a possible instance of such parallel processing in relation to visual search.)

Neuroscientists are continuing to revolutionize our understanding of what, exactly, "brain-style computation" is, and some of their findings can be taken as consistent with sequential operations. For example, anatomical and physiological research on the parts of the brain of the macaque monkey that process visual information, probably the best-known complex system in the brain, have revealed thirty-two differently specialized areas organized in "a hierarchy containing ten levels of cortical visual processing plus several additional stages of subcortical processing" (Van Essen, Anderson, and Felleman 1992, 419; see also Young 1992). By "hierarchy" here is meant an ordering—a directional chainlike structure of connected processing units; it appears that there are two separate and parallel anatomical pathways of this kind, both starting at the retina, one for processing motion and the other for processing form and color (Van Essen, Anderson, and Felleman 1992; Van Essen and Maunsell 1983). Machinery with an *anatomical* structure that is both serial and parallel could underlie a combination of *temporally* serial and parallel operations: parallel analysis of motion and color/form features, each accomplished by a set of serially arranged operations that move the information from one processing unit to another, with operations within each unit being carried out in parallel. In another sensory domain, from their studies of the somatosensory system of the brain (the parts that process skin sensations), Pons and others have argued for an evolutionary shift from parallel to serial processing.[15]

14.4.4 An Example: Stages in a Choice Reaction

14.4.4.1 Separate Stages for Stimulus Identification and Response Selection

Section 14.2.1 described some of the data from an experiment in which digits were presented one at a time and subjects named them as fast as possible. Here we consider this experiment in greater detail, using it to exemplify the reasoning associated with the AFM. Which mental operations might underly this performance? Donders (1868) based some of his work on the idea that the processes of identifying the stimulus and selecting the response in the choice-reaction task occur in separate stages. Indeed, it seems plausible that there is an initial set of operations (stage **A**, with mean duration a) that start with the stimulus display and establish the identity of the digit, followed by a second set of operations (stage **B**, with mean duration b) that start with the identity and select and execute the response. A further subdivision of **A** and **B** (into substages) may be possible, but for now we consider decomposing the process into just these two stages. (Because any sequence of stages is itself a stage, it is a reasonable first step to try to decompose a complex process into just two stages.) How might we test this hypothesis? If **A** and **B** are indeed separately modifiable, and if we succeed in finding two factors, one that influences only **A** and another that influences only **B**, then we should be able to construct a test.

Let us start by noting that together with the additive combination property (principle 1, section 14.3.2), our hypothesis implies that the reaction time is the sum of the durations of the two stages: $\overline{RT} = a + b$. Given the hypothesized functions of **A** and **B**, what might we vary that has a chance of influencing a but not b? When I designed the experiment described in section 14.2.1, because it seemed likely that stimulus quality, SQ, as described in that section, might do this, I chose SQ as one factor. When $SQ = SQ_1$, the stimulus digit was intact; when $SQ = SQ_2$, it was degraded.) As shown in figure 14.9, we suppose SQ influences only stage **A**. Let the mean duration of **A** when $SQ = SQ_1$ and SQ_2 be a_1 and a_2, respectively, and assume $a_2 - a_1 = U$ ms. From the direct-expression property (principle 2; section 14.3.3), $\overline{RT}(SQ_2) - \overline{RT}(SQ_1) = a_2 - a_1 = U$ ms. If the assumption of selective influence of SQ on stage **A** is valid, we can reverse the argument, and attribute any change that SQ produces in $\overline{RT}$ to its effect on **A**.

Comment 5: Selective influence and visual sensory latency. One area of research where a similar assumption of selective influence has been important over the years is the study of visual sensory latency using RT measurements (see, for example, Mansfield 1973 and Vuorinen 1989). The effect of the intensity of a visual stimulus on the RT of a

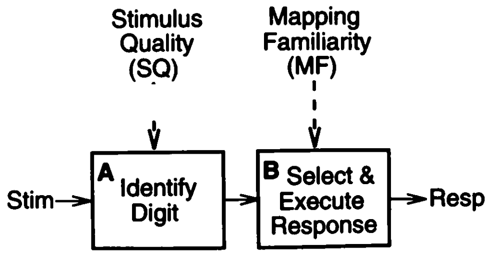


Figure 14.9
Hypothesized stages in digit naming with two factors that might influence them selectively.

"simple reaction" is often attributed entirely to its effect on an early sensory registration stage, **A**. (A "simple reaction" experiment is one where there is a single prespecified response, which the subject is asked to produce whenever the signal is presented.) This interpretation requires us to assume that stimulus intensity has no influence on the duration of later stages, **B**. Support for this assumption is provided by studies that attempt to intercept the output of stage **A** by measuring electrical activity produced at the scalp near the occipital cortex—the part of the cerebral cortex at which visual signals are first registered. When a visual signal occurs, a wavelike pattern of voltage changes can be measured at the scalp, the *visual evoked potential* or *VEP*. Let *VEPT* be the time between the visual stimulus and the occurrence time of a particular feature of the electrical pattern, that is, a measure of the latency of the *VEP*. Consider the difference between $\overline{RT}$ (the sum of a and b) and $\overline{VEPT}$ (which perhaps provides an estimate of a plus a constant). The finding that over a large range of visual intensities this difference is approximately constant supports the assumption that intensity influences a but not b (see Jaskowski, Pruszewicz, and Swidzinski 1990; Vaughan, Costa, and Gilden 1966; and, for a contrasting conclusion, Ulrich and Stapf 1984).

Next, what factor might we vary that has a chance of influencing b but not a? (Because it seemed plausible that the familiarity of the mapping of digits onto names might do this, I chose *MF*, "mapping familiarity," as the second factor. As described in section 14.2.1, when $MF = MF_1$, the mapping was very familiar: "name the digit"; when $MF = MF_2$, the mapping was less familiar: "name the next larger digit".) Let the mean durations of **B** when $MF = MF_1$ and MF_2 be b_1 and b_2, respectively, and let $b_2 - b_1 = V$ ms.

Now consider the four experimental conditions that can be generated by pairing the two levels of *SQ* and *MF* in all possible ways. This gives us the simplest factorial experiment—the "2 × 2 experiment"—whose four conditions are shown in the first two columns of table 14.3. In accordance with principle 3 (section 14.3.4), information in the middle column of table 14.3 shows that the effect of *SQ* is expected to be invariant over the levels of *MF* ($\overline{RT}_{21} - \overline{RT}_{11} = \overline{RT}_{22} - \overline{RT}_{12} = U$), and vice versa. Applications of the difference operator lead to the same conclusions; for example, writing a and b with expanded subscripts, $D_i(\overline{RT}_{ij}) = D_i(a_{i0} + b_{0j}) = D_i(a_{i0}) = a_{20} - a_{10}$, which is independent of j. Recall that this follows from the fact that $D_i(b_{0j}) = b_{0j} - b_{0j} = 0$. And in accordance with principle 4 (section 14.3.4), the invariance of their effects means that factors *SQ* and *MF* are additive, as shown in the middle column of the table.

Now we turn to the actual results for the effects of *SQ* and *MF*, which you have already seen in figure 14.1A. The excellent fit of the parallel lines to the data tell us that *SQ* and *MF* are additive factors. If they had not been, we would have to conclude that the stage theory is wrong for this task, and/or that the factors do not satisfy the assumption of selective influence, and/or that there was an experimental artifact. The result, however, favors the hypothesis of figure 14.9. The four mean data values are given in the last column of table 14.3; the corresponding four fitted values, which define the plotted parallel lines in figure 14.1A, are given in the adjacent column. These fitted values are determined by estimates for $\overline{RT}_{11}$, U, and V of 349.9 ms, 41.5 ms, and 61.1 ms, respectively. For a quantitative measure of the deviation from perfect additivity in the data, we can use the interaction measure:

$$\begin{aligned} D_{ij}(\overline{RT}_{ij}) &= D_i[D_j(\overline{RT}_{ij})] = D_i[\overline{RT}_{i2} - \overline{RT}_{i1}] = D_i(\overline{RT}_{i2}) - D_i(\overline{RT}_{i1}) \\ &= (\overline{RT}_{22} - \overline{RT}_{12}) - (\overline{RT}_{21} - \overline{RT}_{11}) \\ &= (453.1 - 410.5) - (390.9 - 350.5) = 2.2 \text{ ms.} \end{aligned}$$

14.4.4.2 *Why Are Choices Slowed by Signal Uncertainty?*

The data in figure 14.1A are actually the result of averaging over the two levels of a third factor that was also varied in the experiment: the number NA_k of alternative digit-name pairs used during a series of trials, which could be $NA_1 = 2$ or $NA_2 = 8$. As mentioned in section 14.2.1, this is a way of varying the amount of *signal uncertainty* on a trial. For over a century it has been known that choice *RT* is slowed by signal uncertainty, but much of the mystery remains as to how this comes about. In the 1950s the signal-uncertainty effect was one of the phenomena taken to support the view of a person as an information-transmitting channel of limited capacity, partly because $\overline{RT}$ was approximately linear with the logarithm of the the number of stimulus-response pairs, n: $\overline{RT} \approx \alpha + \beta \log n$, and

Table 14.3
Expected $\overline{RT}$s in an experiment with two factors (stimulus quality, *SQ*, and mapping familiarity, *MF*) with two levels per factor and selective influence, and the observed and fitted $\overline{RT}$s shown in figure 14.1A.

Level of SQ_i	Level of MF_j	Expected $\overline{RT}_{ij} = a_i + b_j$	Fitted $\overline{RT}_{ij}$ (ms)	Observed $\overline{RT}_{ij}$ (ms)
SQ_1	MF_1	$\overline{RT}_{11} = a_1 + b_1 = a_1 + b_1 = \overline{RT}_{11}$	349.9	350.5
SQ_1	MF_2	$\overline{RT}_{12} = a_1 + b_2 = a_1 + (b_1 + V) = \overline{RT}_{11} + V$	411.0	410.5
SQ_2	MF_1	$\overline{RT}_{21} = a_2 + b_1 = (a_1 + U) + b_1 = \overline{RT}_{11} + U$	391.4	390.9
SQ_2	MF_2	$\overline{RT}_{22} = a_2 + b_2 = (a_1 + U) + (b_1 + V) = \overline{RT}_{11} + U + V$	452.5	453.1

log n was a measure of the amount of information, in Shannon's sense (1948), that had to be transmitted by the subject in responding correctly to the stimulus. But the information-theoretical view did not provide an acceptable description of the underlying processes that produce the effect.

If SQ and MF could successfully be used to isolate two stages in generating a choice response, then this might permit us to discover which of these two stages, if either, contributes to the uncertainty effect. Such an answer could then guide the development of more refined explanations of the effect. This was one reason for including NA in the experiment. If the effect of NA is localized in the process of identifying the stimulus (stage **A**), we would expect to find that NA modulates the effect of SQ, but that NA and MF are additive. If the effect of NA is localized in the process of selecting the response (stage **B**), we would expect to find that NA modulates the effect of MF, but NA and SQ are additive. If signal uncertainty has its effect by slowing a stage distinct from those influenced by SQ and MF, we should find NA to be additive with both of them, and we will have decomposed the process into three stages rather than two.

My second reason for including the uncertainty factor in the experiment was to permit a more rigorous test of the stages hypothesis. If two factors (SQ_i and MF_j) selectively influence different stages, then their additivity should hold up whatever the level of a third factor (NA_k) and whatever stages that third factor influences. To see this, suppose NA influences both **A** and **B** as well as a third stage **C**, while SQ influences only **A** and MF influences only **B**. Then $\overline{RT}_{ijk} = a_{i0k} + b_{0jk} + c_{00k}$. Now use the difference operator to determine the $SQ \times MF$ interaction: $D_{ij}(\overline{RT}_{ijk}) = D_{ij}(a_{i0k}) + D_{ij}(b_{0jk}) + D_{ij}(c_{00k})$. Because either the i-subscript or the j-subscript or both are zero for each of the three stage durations, the result of applying D_{ij} is zero for each term, so the two-way $SQ \times MF$ interaction is zero for both $k = 1$ and $k = 2$: $D_{ij}(\overline{RT}_{ij1}) = D_{ij}(\overline{RT}_{ij2}) = 0$.[16] Furthermore, the equality of these two two-way interactions means that the three-way interaction, $D_{ijk}(\overline{RT}_{ijk})$, should be zero.

> *Comment 6: Meaning of the three-way interaction.* Recall that the three-way interaction measures the extent to which one of three factors modulates the two-way interaction of the two others. To express this precisely requires us to work with *simple* two-way interactions. A simple two-way interaction is the two-way interaction at particular levels of the remaining factors or factor, for example, $D_{ij}(\overline{RT}_{ij1})$. By expanding $D_{ijk}(\overline{RT}_{ijk})$, it can easily be shown that in any $2 \times 2 \times 2$ factorial experiment the three-way interaction is given by the difference between the simple two-way interactions associated with each of the three pairs of factors, and that these differences must all be equal: $D_{ijk}(\overline{RT}_{ijk}) = D_{ij}(\overline{RT}_{ij2}) - D_{ij}(\overline{RT}_{ij1}) = D_{ik}(\overline{RT}_{i2k}) - D_{ik}(\overline{RT}_{i1k}) = D_{jk}(\overline{RT}_{2jk}) - D_{jk}(\overline{RT}_{1jk})$.

In general, principles 3–7 of section 14.3 imply certain kinds of consistency among patterns of factor effects in an experiment with three or more factors. Tests of these predicted relations are therefore empirical tests of the principles. If the relation between a pair of factors—whether they interact or are additive—depends on whether or not they influence a stage in common, then we expect this relation to remain the same as the levels of other factors are varied. In a three-factor experiment, therefore, the consistency of the relation between the factors in each of the three pairs is tested by variation in the level of the remaining factor. Suppose that at one level of *SQ*, *NA* interacts with *MF*. Using principle 5, we infer that *NA* and *MF* influence at least one stage in common. Principle 7 tells us that they should interact at each level of *SQ*. If $D_{jk}(\overline{RT}_{1jk}) \neq 0$, we therefore expect that $D_{jk}(\overline{RT}_{2jk}) \neq 0$. Similarly, if we observe that *SQ* and *MF* are additive at one level of *NA*, principle 6 tells us that they influence no stage in common. Principles 3–4 tell us that they should be additive at each level of *NA*. If $D_{ij}(\overline{RT}_{ij1}) \approx 0$, we therefore expect that $D_{ij}(\overline{RT}_{ij2}) \approx 0$. Such tests of the principles that underlie the additive-factor method are not provided by the data from a 2 × 2 experiment. However, a two-factor experiment with one or both factors at multiple levels rather than only two levels does provide other such tests, described in section 14.6.7.2.

I therefore ran a 2 × 2 × 2 factorial experiment with eight conditions rather than the four shown in table 14.3; the data in that table are averages over the NA_k levels. What is plotted in figure 14.1A can therefore be described as $\overline{RT}_{ij\cdot\cdot}$. Similarly, the data in figure 14.1B have been averaged over the two levels of stimulus quality, *SQ*, and can be described as $\overline{RT}_{\cdot jk}$. Mean *RT*s for the eight conditions in the experiment are given for each of the five subjects in table 14.4; the means over subjects are given at the bottom of the table and also plotted in figure 14.10.

At the top of the figure are results for $NA_2 = 8$, and at the bottom, for $NA_1 = 2$. Parallel lines have been fitted to each subset of data, corresponding to additivity of *SQ* and *MF* at each level of *NA*. The additivity is nearly perfect, the mean absolute deviation of the data from the parallel lines being less than 1 ms in both cases. The figure also shows that signal uncertainty interacted with both of the other factors. The interaction with the mapping factor is the stronger; the effect of *MF* is much greater with $NA_2 = 8$ than with $NA_1 = 2$. That is, the top two lines are much steeper than the bottom two. The interaction with stimulus quality is weaker, but clearly present: the top two lines are further separated than the bottom two.

The important features of the data are summarized quantitatively in table 14.5, for each subject and for the data averaged over subjects. The last row of the table provides the standard error (s.e.) of the corresponding mean.[17] Consider, for example, the main effect of stimulus quality, given in the second column. The mean of this effect is 42 ms, and the

Table 14.4
Values of $\overline{RT}_{ijk}$ for five subjects and their mean in a 2 × 2 × 2 choice-reaction experiment with digits as stimuli and digit names as responses.

		MF_j:	Familiar		Unfamiliar		
Subject	NA_k	SQ_i:	Intact	Degraded	Intact	Degraded	Mean
BN	2		300	314	300	326	354.1
	8		330	368	425	470	
DH	2		302	332	311	342	363.8
	8		339	396	415	473	
SS	2		329	354	353	369	382.0
	8		353	401	427	470	
AP	2		354	383	384	423	436.5
	8		399	468	500	581	
PM	2		363	405	396	439	469.9
	8		436	488	594	638	
Mean	2		**329.6**	**357.6**	**348.8**	**379.8**	401.2
	8		**371.4**	**424.2**	**472.2**	**526.4**	

effect for individual subjects ranges from 31 to 54 ms. The s.e. reflects the variability of the effect over subjects. Insofar as the values for individual subjects are close to each other, the s.e. is small, and we can have more confidence that the mean for this "sample" of five subjects is close to the mean over all the potential subjects in the experiment—the "true value." Roughly speaking, if a mean based on five observations differs from zero by more than about three times its standard error, we can be reasonably confident that the true value differs from zero, and we would say that "the mean differs reliably from zero." On this basis, all three main effects, and two of the four interactions—$NA \times SQ$ and $NA \times MF$—are reliable. However, consistent with the goodness of fit of the additive models shown in figure 14.10, the $SQ \times MF$ interaction based on the data averaged over levels of NA does not differ reliably from zero, and neither does the three-way interaction. The absence of any three-way interaction reflects the fact that the magnitude of the simple $SQ \times MF$ interaction, $D_{ij}(\overline{RT}_{ijk})$, whose average over k is effectively zero, is not modulated by the level of NA: it is approximately true that $D_{ij}(\overline{RT}_{ij1}) = D_{ij}(\overline{RT}_{ij2}) = 0$.

The interpretation of these results is shown in figure 14.11, an elaboration of figure 14.9. Above the horizontal line are shown the relations found among the three factors. Below the line are shown the inferred processing stages and the relations between factors and stages. The additivity of stimulus quality and mapping familiarity supports the proposal first made by

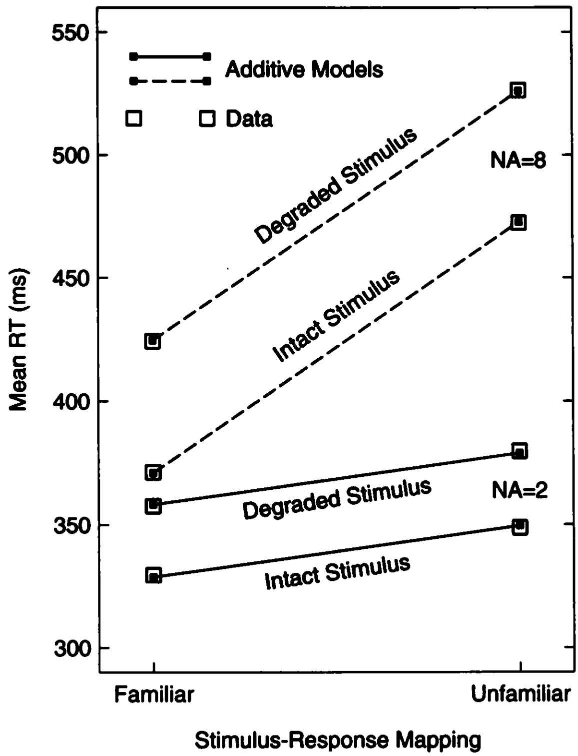


Figure 14.10
Results of experiment with digits as stimuli and names of digits as responses, also shown in figure 14.1. The experiment had eight conditions, generated by combining two levels of each of three factors: mapping familiarity (*MF*), stimulus quality (*SQ*), and number of stimulus-response alternatives (*NA*). Here the mean *RT*s from all eight conditions are shown as open squares. The connected filled squares represent additive models fitted separately to the data for $NA = 2$ (solid lines) and $NA = 8$ (broken lines). The excellence of the fit of the model indicates that at each level of *NA*, the effect of *MF* is invariant over levels of *SQ* (and vice versa). The figure also shows that an increase in the level of *NA* increases the effects of *SQ* (broken lines are more separated than solid lines) and of *MF* (broken lines are steeper than solid lines). Data from table 14.4.

Table 14.5
Main effects and interactions of factors SQ_i, MF_j, and NA_k in the data of table 14.4, for five subjects, with means and standard errors (s.e.).

	Main effects			Interactions			
Subject	$D_i(\overline{RT}_{i..})$	$D_j(\overline{RT}_{.j.})$	$D_k(\overline{RT}_{..k})$	$D_{ij}(\overline{RT}_{ij.})$	$D_{ik}(\overline{RT}_{i.k})$	$D_{jk}(\overline{RT}_{.jk})$	$D_{ijk}(\overline{RT}_{ijk})$
BN	31	52	88	9.5	22	92	−5.0
DH	44	43	84	1.0	27	67	0.0
SS	33	46	62	−7.0	25	52	4.0
AP	54	71	101	11.0	41	72	2.0
PM	45	94	138	−3.5	6	120	−9.0
Mean	**42**	**61**	**95**	**2.2**	**24**	**81**	**−1.6**
s.e.	4	10	13	3.5	6	12	2.4

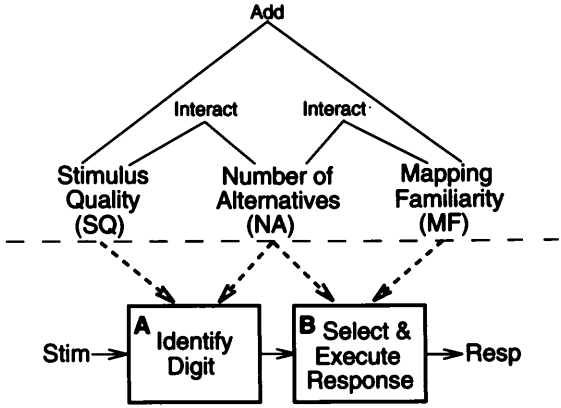


Figure 14.11
Method of additive factors. In the top section of the figure is shown the pattern of factor effects and interactions found in the experiment on digit naming. In the bottom section are shown the inferred stages and factor-stage relations.

Donders that stimulus identification and response selection are accomplished in separate stages. The finding that signal uncertainty (controlled by the level of *NA*) modulates the effects of both *SQ* and *MF* indicates that this factor influences *both* of these two stages. Knowing that the effect of signal uncertainty is due to more than one process does not provide a deep understanding of it, but there is little point in generating and testing more detailed theories that do not acknowledge this.

Comment 7: Stage-specific factor effects. Based on these results, what can we say quantitatively about the effects of the three factors on the separate stage durations *a* and *b*? For factors *SQ* and *MF*, whose effects are selective, the effect on the relevant individual stage is equal to the effect on the full *RT*. For example, $D_i(\overline{a_{i01}})$, the mean effect of *SQ* on stage **A** when $NA = NA_1$, is given by $D_i(\overline{RT}_{i\cdot 1}) = \overline{RT}_{2\cdot 1} - \overline{RT}_{1\cdot 1} = 369 - 339 = 30$ ms. The problem of determining the stage-specific effects of a factor like signal uncertainty (*NA*), whose effect on *RT* is shared by two stages, is much more difficult. (As we shall see in sections 14.5.12 and 14.5.13, it can be solved if an experimenter can arrange to vary the number of repetitions of one of the stages.) Because *NA* modulates the effect of *SQ* so much less than the effect of *MF*, it is tempting to believe that its effect on **A** is smaller than its effect on **B**, but I know of no way to justify this inference. However, if we make the reasonable assumption that raising the level of signal uncertainty cannot decrease the time required to either identify the stimulus or select the response (that is, that

$D_k(a_{i0k}) \geq 0$ and $D_k(b_{0jk}) \geq 0$), then the data do imply bounds on the stage-specific effects of signal uncertainty. Thus the effect of NA on a can be no greater than its effect on the full RT when $MF = MF_1$, or $D_k(\overline{RT}_{i1k})$, which is 42 ms and 67 ms for $SQ = SQ_1$ and $SQ = SQ_2$, respectively. Now consider the effect of NA on b. Because MF influences **B** selectively, this stage is also the locus of the $NA \times MF$ interaction. This interaction of 81 ms = $D_{jk}(\overline{RT}_{\cdot jk}) = D_{jk}(b_{0jk})$ is the increase in the effect of NA on b caused by raising the level of MF. The effect of NA on b when MF is at its high level must be at least as great as this increase: $D_k(b_{02k}) \geq D_k(b_{02k}) - D_k(b_{01k}) = D_{jk}(b_{0jk})$. The effect of NA on response selection when $MF = MF_2$ is therefore greater than its effect on stimulus identification when SQ is at either of its levels, a conclusion you will find also obtains for each of the five subjects individually. This conclusion depends, of course, on the particular levels of SQ and MF used in the experiment. For a more general conclusion (such as "signal uncertainty influences **A** less than **B**"), it is interesting to consider how to adjust such statements for any difference in the relative potency of SQ and MF.

14.4.5 The Method of Additive Factors

In our discussion of the shopping-trip analogy and of the processes that underly digit naming, we have used two sorts of reasoning, summarized in principles 1–7 (sections 14.3.2–14.3.4 and 14.3.6). One direction of inference is from hypothesized stages and factor-stage relations (that is, a theory) to the expected pattern of factor effects (that is, predictions from the theory). In terms of figure 14.11, the reasoning is from the arrangement below the horizontal line to the pattern above it. Where two factors influence no stage in common (like AG and BF in figure 14.4 or SQ and MF in figure 14.11), we expect their effects to be additive (principles 3 and 4). Where two factors influence at least one stage in common (like AG and EL in figure 14.4 or NA and MF in figure 14.11), there is no reason to expect additivity; the most likely relation is some sort of interaction, where the effect of one of the factors on $\overline{RT}$ is modulated by the level of the other (principle 7).

The additive-factor method, which can also be called the "method of invariant factor effects," employs the reverse direction of inference: from an observed pattern of factor effects on $\overline{RT}$ to a set of hypothesized stages and factor-stage relations that underlie that pattern. In terms of figure 14.11, the reasoning is from the pattern above the horizontal line to the arrangement below it. (This is the method that Alice had to use in her attempt to understand the variations in the total duration of Jim's shopping trip because all she had were her records of total duration as three factors varied.) In using the method to discover stages of processing, we search for factors, like stimulus quality (*SQ*) and mapping familiarity (*MF*), that have additive effects on $\overline{RT}$. Whenever such additive factors are found, and given no stronger arguments to the contrary, it is reasonable to believe that there exists a corresponding pair of stages, **A** and **B**, between stimulus and response (principle 6). If a third factor is found, for example, to interact with *MF* but not with *SQ*, this is taken to imply that this factor influences RT at least in part because of its effect on **B**, but not because of any effect on **A** (principles 5 and 6). (See section 14.6.3 for further discussion of the nature of the reasoning associated with the AFM.)

Analyses of the kind shown in figure 14.11 have to be tentative, of course. As new instances of additivity are found, the method leads to the postulation of new stages. And as new instances of interaction are found, the functions of the stages already identified become better known.

14.5 Some Applications of the Additive-Factor Method

In previous sections I have used the analysis of digit naming, described in detail, to introduce the AFM. In the present section I discuss a further set of fourteen applications (in correspondingly numbered subsections 14.5.1–14.5.14), selected to address a wide range of issues in human cognition. The applications include problems in perception (1, 9, 12, 13), categorization (3, 6, 9, 13), preparation for action (2), working memory (3, 4, 13), learning (4, 10), word recognition (10, 11), visual search (14), control of multiple tasks (6, 9), informational overload (9), brain-behavior relations (5, 6), and the control of action sequences in speech production (7). Factors include variables that can change from trial to trial (such as stimulus characteristics), as well as relatively enduring states (such as sleep deprivation and localized brain damage; 1, 6). Many of the applications are concerned with whether factor effects are invariant or not, while others make use of the direction of an interaction (3, 6, 9, 10, 14) or its quantitative form (11–13). The subtraction method, treated as a special case of the AFM, plays an important role in one application (8). While the question of the "architecture" of cognitive processes is present in every application, the issue of parallel versus serial organization of two operations (9, 11) or of multiple similar operations (12, 13) is also explicitly considered. Most of the applications are discussed briefly, but three that raise especially interesting issues of both method and substance require more of the reader (6, 8, 9). While many of the applications involve processing stages that are data-dependent, some do not (3, 9, 11–14). Most of these applications sections can be read independently, but there are exceptions.

Application 13 depends on application 3, and applications 12 and 13 depend on ideas about substage equivalence developed in application 11.

In treating these examples, I make numerous quantitative claims about the data, each of which should be accompanied by a statistical defense. Such a defense would include at least a statement about the precision (the sampling error) of the critical quantitative relations, and an explanation of how this statement is derived from measures of variability in the data.[18] I have omitted statistical defenses because including them would make the chapter ungainly, because you, the reader of this volume, are not assumed to know about statistics, and because I would like you to think of these examples as illustrations of the nature of the arguments, rather than as final statements.

14.5.1 What Else Do We Lose When We Lose Sleep?

The most commonly employed strategy for investigating the cognitive effects of sleep deprivation is to use the *task-comparison method*. The effects are measured in a range of contrasting tasks—tasks that presumably differ in the kinds of cognitive demands they make. Each such case requires a task analysis—a theory of the mental operations the task calls for. If the task analyses are valid, then differences in performance deficit from task to task may help us to infer which mental operations are especially sensitive to sleep loss and which are relatively insensitive. If we learned this, not only might we know something about why we need sleep (still a mystery), but we might be able to design jobs less sensitive to sleep deprivation, which is pervasive in today's society—not merely an affliction of undergraduates. The same strategy is also often employed to investigate other "state" variables, such as drug effects and localized brain damage.

In using the task-comparison method to search for selective effects of a factor (such as sleep-deprived versus normal) on a particular mental operation, we need a pair of tasks: an experimental task that calls for that mental operation plus others, and a control task that calls for only the others. Let us call this claim about a pair of tasks a "task-difference hypothesis." Except that the mental operations in each task need not be arranged as stages, it should be evident that this method requires the same pure insertion assumption on which Donders' subtraction method (section 14.4.2) depends. That is, it requires validity of the task-difference hypothesis, which in turn depends on a valid analysis of each task. If proper precautions (see, for example, Shallice 1988) are taken (especially to demonstrate sensitivity of the measure of control-task performance) this strategy can produce persuasive results when it leads to discovery of a *dissociation*—an effect of the factor under study on the experimental task but not the control task. Such a finding is persuasive because it supports aspects of the task analyses as well as informing us about the factor under study. Failure to find a dissociation, however, tells us little; among various possibilities, one is that the task-difference hypothesis, often difficult to test independently, is wrong.

One conclusion that has been drawn about sleep deprivation from the task-comparison approach is that it has a global effect: there is "a generalized effect of sleepiness on all cognitive functioning" (Dinges and Kribbs 1991, 117).

Sanders, Wijnen, and Van Arkel (1982, experiment 1) used an alternative strategy, in which they applied the AFM to examine the effects of experimentally induced sleep deprivation on different mental operations *within the same task*. You are already familiar with a task close to the one they used, the digit-naming procedure discussed in sections 14.2.1 and 14.4.4. The digits were presented as dot patterns; degradation ($SQ_i = SQ_2$) consisted of adding "noise" in the form of other dots. The possible stimuli were "2," "3," "4," and "5." In the familiar-mapping condition ($MF_j = MF_1$), the correct response to a stimulus was to pronounce its name; in the unfamiliar-mapping condition ($MF_j = MF_2$), the associated responses were "three," "four," "five," and "two," respectively.

These manipulations, then, created two levels each of stimulus quality (SQ_i) and mapping familiarity (MF_j). The third principal factor was the subject's sleep state (SLP_k), which also had two levels: normal (data taken during the day after a normal night's sleep; $SLP = SLP_1$) and sleep-deprived (data taken during the day after a night awake in the lab; $SLP = SLP_2$).[19] Given the conclusion from the task-comparison approach that sleep deprivation has a global effect, interfering with the performance of all cognitive operations, what would we expect here? If SQ and MF have additive effects on $\overline{RT}$ (as in the data discussed in section 14.4.4) we would conclude that they selectively influence different stages. If each stage is also influenced by sleep deprivation in accordance with the global-effect hypothesis, we would expect SLP to interact with both SQ and MF. Put another way, raising the levels of SQ and MF adds to the difficulty of different cognitive processes: identifying the stimulus and selecting the response, respectively. If sleep loss interferes with "all cognitive functioning," it should exacerbate both kinds of difficulty. (Suppose this expectation were not borne out. It is interesting to speculate about which kind of difficulty would be more likely to increase with sleep deprivation, considering exactly what is made more difficult by raising the levels of SQ and MF. For SQ, it is perhaps the process of extracting contours from the stimulus pattern that is made more difficult. For MF, the subject must inhibit a well-learned association to produce a conflicting response.)

In considering the data, we should first ask whether they support the conclusion (section 14.4.4.1) that stimulus identification and response selection are carried out in separate stages. The main effects of SQ and MF are substantial: for stimulus quality, $D_i(T_{i..}) = 139$ ms; and for mapping familiarity, $D_j(T_{.j.}) = 137$ ms. Nonetheless, in each sleep state SQ and MF are close to being perfectly additive; mean absolute deviations between the data and fitted additive models are 2.3 ms (sleep state normal; $D_{ij}(\overline{RT}_{ij1})$ negligible) and 4.3 ms (sleep state deprived; $D_{ij}(\overline{RT}_{ij2})$ negligible). The earlier stages analysis is thus confirmed (moreover, in a Dutch laboratory, and with a different way of controlling SQ) and, more interesting, extended to a condition of sleep deprivation. This permits us to ask the crucial question: are both of these stages influenced by sleep state?

What is striking in the results, shown in figure 14.12, is the almost perfect invariance of the effect of mapping familiarity across sleep states, which obtains for each level of SQ. Separate additive models have been fitted to the data for $SQ = SQ_1$ and $SQ = SQ_2$. In each case, the mean absolute deviation is 1.0 ms; both $D_{jk}(\overline{RT}_{1jk})$ and $D_{jk}(\overline{RT}_{2jk})$ are negligible. The effect of sleep deprivation is thus not modulated by mapping familiarity. On the other hand, it is strongly modulated by stimulus quality: the average effect of deprivation with intact stimuli is only $D_k(T_{1\cdot k}) = 17$ ms, but the effect with degraded stimuli is about six times as great: $D_k(T_{2\cdot k}) = 98$ ms. (Compare the separations of the lower two and upper two lines in the figure.) Given the substantial interaction of sleep state with stimulus quality, the absence of an interaction of sleep state with mapping is especially persuasive, and contrasts strongly with the conclusion of Dinges and Kribbs (1991). In terms of the model described in figure 14.11, sleep state influences stage **A** but not stage **B**.

Comment 8: The problem of omitted responses. A frequently reported consequence of sleep deprivation in experiments where a subject must respond to a sequence of stimuli is a high frequency of omitted responses, and the experiment of Sanders, Wijnen, and Van Arkel (1982) was no exception. For example, in the trial blocks with the most difficult condition—degraded stimuli, unfamiliar mapping—sleep-deprived subjects failed to respond on 26 percent of the trials. (Interestingly, the error rate when they did respond was not affected by sleep loss.) The problem of such high rates of nonresponse is that, as a consequence, the RT data might be biased. (What would the RTs have been on those missed trials?) The remarkable orderliness of the results suggests that any such bias was probably small, but this is an issue that needs to be kept in mind. (For a discussion of how error rates bear on the interpretation of RT data, see appendix 2 of chap. 9, this volume.)

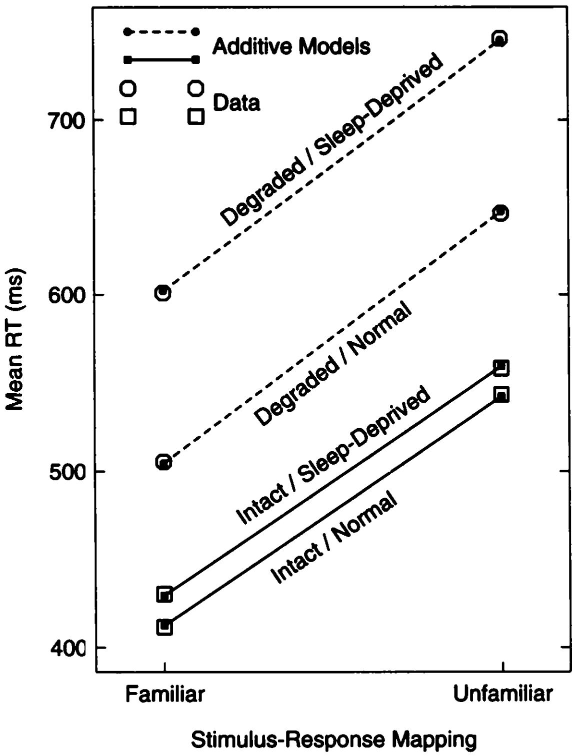


Figure 14.12
Effects of sleep deprivation on performance in task where digits are stimuli and names of digits are responses. Sleep state (*SLP*) interacts with stimulus quality (*SQ*): the difference between $\overline{RT}$s for intact versus degraded stimuli is substantially greater when subjects are sleep-deprived. But *SLP* does not modulate the effect of mapping familiarity (*MF*) for either intact stimuli (the additive model fitted to the lower pairs of points fits well) or degraded stimuli (the additive model fitted to the upper pairs of points fits well). Data from Sanders, Wijnen, and Van Arkel 1982, experiment 1.

14.5.2 How We Prepare to Choose

We have seen that decreasing signal uncertainty by reducing the number of alternative stimulus-response pairs permits subjects to respond faster. (In section 14.2.1 I suggested that this effect occurs because the reduction permits subjects to prepare better for the alternatives that remain.) A second kind of uncertainty that slows the choice response is *time uncertainty*—uncertainty not about *which* signal will occur, but about *when* it will occur. To reduce time uncertainty in choice-reaction experiments so as to minimize the mean and variability of the *RT*, a warning signal is often employed, presented shortly before the reaction signal. This permits the subject to estimate relatively precisely when the signal will occur, just as "ready, set" allows you to estimate when you will hear "go." The optimal "foreperiod" from warning signal to choice-reaction signal is thought to be between 0.5 and 1.0 sec. Longer foreperiods or ones that vary randomly from trial to trial are used if the high time uncertainty of many real-world situations is desired (imagine a dog dashing into the street ahead of your car). A long foreperiod produces more time uncertainty because the precision of our estimate of a time interval decreases as the duration of that interval grows.

Why is *RT* increased by uncertainty about when the stimulus will be presented? In particular, why is the response slowed by a long or variable foreperiod? If all aspects of being prepared to make a choice could be maintained over time, then time uncertainty should not hurt. The fact that it does impair performance indicates that some or all aspects of preparation can be maintained only briefly. An increase in time uncertainty would then either discourage preparation or reduce the chance of being prepared at the moment the signal arrives. Suppose the advantage of a smaller number of signal alternatives is due to a state of preparedness that is short-lived. This advantage—and, in general, the effect of signal uncertainty—should then be lessened by an increase in time uncertainty. That is, the two kinds of uncertainty should interact; any stage that is sensitive to signal uncertainty should be sensitive to time uncertainty, and vice versa.

Consider this idea in relation to the conclusions embodied in figure 14.11. It leads to the expectation that in addition to stimulus quality and mapping familiarity, a third factor that will interact with signal uncertainty is time uncertainty. Furthermore, because *NA* influences both stages **A** and **B** in figure 14.11, *FP* (foreperiod) should, also, and should therefore also interact with *SQ* and *MF*.

Alegria and Bertelson (1970, experiment 2) varied the two kinds of uncertainty in a 2 × 2 factorial experiment, with the results shown in figure 14.13. They used digits as stimuli, with each digit corresponding to a distinct key assigned to a distinct finger; the response was to press the corresponding key as fast as possible when the digit appeared. The two levels of time uncertainty were created by using foreperiods of $FP_1 = 0.5$ sec and $FP_2 = 5.0$ sec. The two levels of signal uncertainty were created by using some series of trials with $NA_1 = 2$ digit-key pairs, and others with $NA_2 = 8$ pairs.

In relating these findings to those plotted in figure 14.10, it is important to keep in mind that while the stimuli are similar in the two experiments, the responses are not.[20] As a working hypothesis, however, it is

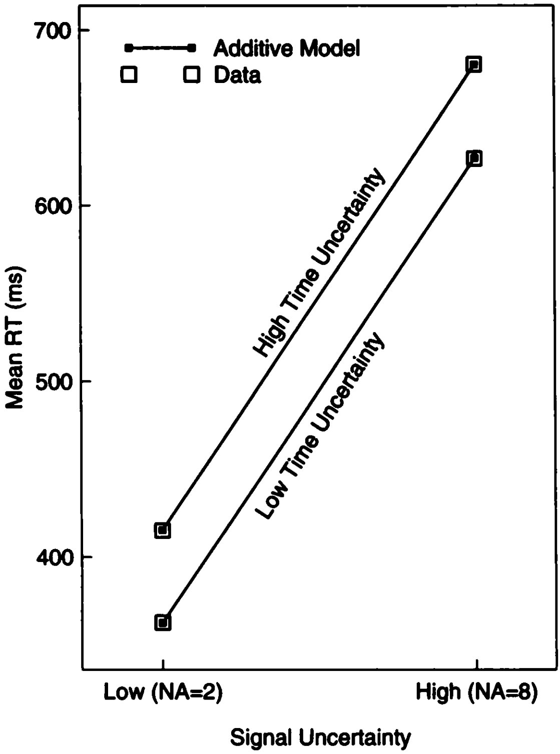


Figure 14.13
Additive effects of signal uncertainty and time uncertainty. Data from Alegria and Bertelson 1970, experiment 2.

reasonable to assume that similar processing stages are used to perform the two different tasks, so that a stage structure like that indicated in figure 14.11 would apply here, also. Contrary to the expectations above, and as shown in figure 14.13, the fit of the additive model is excellent; the mean absolute deviation is only 0.2 ms. The increase in time uncertainty had the same 53 ms effect for each level of signal uncertainty. Because *NA* influences both the encoding (A) and translation (B) stages, the additivity suggests the existence of a third stage, C, influenced by time uncertainty but not signal uncertainty (see figure 14.14). Because *FP* does not influence stages A and B, the preparation associated with the set of alternative signals, which influences those stages, must be persistent rather than short-lived. On the other hand, because C is influenced by *FP*, there must be a short-lived preparatory process associated with that stage.

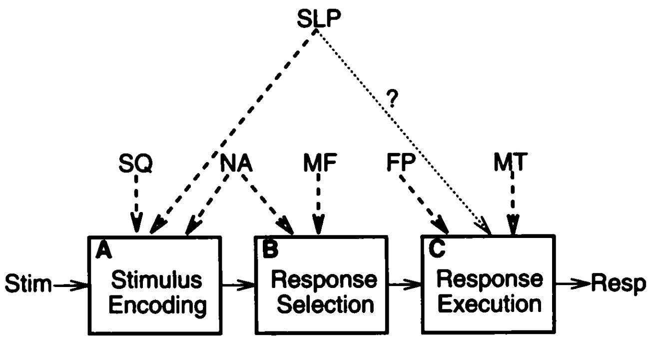


Figure 14.14
Three inferred stages in making a choice, and factor-stage relations for six factors that influence them: stimulus quality (*SQ*), signal uncertainty (number of alternatives, *NA*), mapping familiarity (*MF*), time uncertainty (foreperiod, *FP*), muscle tension (*MT*), and sleep state (*SLP*).

Because stimulus quality (*SQ*) and mapping familiarity (*MF*), which influence **A** and **B**, respectively, might also influence **C**, our interpretation does not require that *FP* be additive with *SQ* and *MF*. But the inference that there is a third stage influenced by time uncertainty would be strengthened if such additivity were found. In a $2 \times 2 \times 2$ experiment with the factors *FP*, *SQ*, and *MF*, Frowein and Sanders (1978) found pairwise additivity of all three factors, strengthening the inference.[21]

What is the function of stage **C**? One possibility is that it has to do with execution of the response, with time uncertainty influencing a general readiness to respond. One way to lend credence to such a possibility would be to discover a response-related factor that interacts with time uncertainty. Sanders (1980a) used instructed muscle tension as such a factor, training subjects with biofeedback to relax the responding arm during the foreperiod in the "relaxed" condition, and comparing that to a "tense" condition in which subjects could tense their muscles in a way they regarded as optimal. Muscle tension (*MT*) and *FP* interacted, supporting the conjecture that **C** is related to response execution: the advantage of a short foreperiod was greater when subjects were permitted to tense their muscles. A related experiment showed that *MT* was additive with both *SQ* and *MF*, further supporting the existence of the three stages and the selective influence of *MT* on stage **C**.

Figure 14.14 provides a summary of the inferences of sections 14.4.4 and 14.5.1 together with those of the present section. We have reviewed some of the effects of five factors, *SQ*, *NA*, *MF*, *FP*, and *MT*. Ten pairs of factors can be constructed from these five, and in one or more of the five experiments mentioned, nine of these pairs have been examined, leading to the inferred structure in the figure. Given that structure, we would expect the unexamined pair (*NA* and *MT*) to be additive. With respect to sleep state (*SLP*) considered in section 14.5.1, we have already concluded that it influences **A** (leading us to expect that it will interact with *NA*), but not **B**. Unless we are to increase the number of stages influenced by this set of factors, the way the effects of *SLP* combine with those of *FP* and *MT*, yet to be determined, will tell us whether it influences **C**.

14.5.3 Retrieving Item versus Context Information from Memory

What is the basis for the dramatic interaction (figure 14.3) of list length and the task difference between item recognition and context recall? As the task is changed from requiring the recognition of item information to requiring the recall of contextual information, the effect of list length increases markedly, becoming about three times as great (the slope of the fitted linear function increased from 38 ms per digit to 113 ms per digit). Context recall calls for position as well as item information. According to a theory about the item task, this implies that a search process in the context task should be slower (Sternberg 1969b, section 11); I ran the context recall experiment to test that prediction, which was pleasantly confirmed by the results.

Unfortunately, there was an obvious alternative explanation possible, which attributes the interaction to the difference between recognition and recall. Because recognition of information in "long-term memory" is believed to require less detailed information and less extensive memory search than recall, why not also for information that has just been memorized and is held in working memory? One indicator of the difference between recall and recognition in the amount of information required is the amount of response uncertainty: in item recognition there are only two alternative responses regardless of list length, whereas in context recall the number of alternative responses ($k - 1$) increases with list length.

To distinguish between kind of information (item versus context) and amount of information (recognition versus recall), I ran the context-recognition experiment. A plausible starting assumption is that all three tasks involve stages of encoding a test stimulus, comparing it in some way to a memory representation of the list, and then selecting and executing a response. It is plausible that list length influences at least the second of these three stages. The interaction of list length with the difference between item recognition and context recall then tells us that this task difference influences the memory-interrogation stage. The question is, what aspect of the task difference is responsible—is it the difference in kind of information or in amount?

If we define a context task factor with levels corresponding to context recognition and context recall, then because these tasks differ in both test stimuli and responses, it is plausible that both the encoding and response stages are affected by this factor. The question is whether context task influences the memory interrogation stage as well. If it does, we would expect an interaction of context task and list length. In particular, we would expect the effect of list length to be greater in the recall task, which presumably requires that more information be extracted from the list. The additivity of context task and list length that we find suggests, instead, that the memory-interrogation processes in the two context tasks are identical. At least in this special case, the search process required for recall appears to be no different from that required for recognition. Taken together, the results therefore indicate that the kind of information required (item versus context) strongly affects the time per item for memory interrogation, whereas the amount of information (recognition versus recall) does not.[22]

The example of additive factors in figure 14.3 differs in an important respect from the examples considered thus far, in which each factor has two levels. In the relevant part of figure 14.3, the list length factor has four levels, so that we have a 4 × 2 design. In general, experiments using factors at more than two levels provide more sensitive tests of additivity because they enable us to see whether the deviations from additivity are *systematic* (are orderly, have a pattern), a property not reflected in the mean absolute deviation of points from fitted curves. In practice, the most common such pattern to be concerned about are deviations that increase or decrease monotonically with one of the factors (see figures 14.29A and 14.39B for examples). If deviations from additivity are systematic, we are more likely to believe there is a true interaction, rather than to attribute the apparent interaction to the natural variability of experimental data (that is, to sampling error, discussed in chap. 12, this volume). Because the additivity of factor effects has powerful implications, the conclusion of additivity must always be evaluated with skepticism. For example, suppose the separation between the data points for context recall and context recognition grew larger with each increase in list length from $k = 3$ to $k = 6$. Then we might believe that there was actually a small interaction of the two factors. In fact, the separations neither increase nor decrease monotonically: their values are 113, 107, 90, and 110 ms, respectively, for $k = 3, 4, 5,$ and 6.[23] Factors examined at only two levels afford neither the possibility of finding that deviations are erratic nor the sensitivity to their systematic patterning. Unfortunately, the use of just two levels tends to be quite common, perhaps because bigger experiments are harder to design and more costly to run.[24]

14.5.4 When We Improve with Practice, What Improves?

A central question in the study of human and animal learning is exactly what has been learned as the result of a given training procedure. The most common approach to answering this question is probably the experiment on *transfer of training*. After a training procedure is completed, the subject is asked to perform a transfer task, which, for example, places some demands on the subject that are shared with the training task, and other demands that are not. (For a modern example of the use of transfer of training see Lindemann and Wright, chap. 11, this volume.) This is a version of the task-comparison approach discussed in section 14.5.1 in connection with sleep deprivation. The interpretation of such experiments depends on—but also contributes to—valid task analyses of the training and transfer tasks, that is, theories of the mental operations they call for.

A great deal of learning goes on in *RT* experiments. Often some of the largest effects are learning effects (usually called "practice effects") in which *RT*s become shorter with no loss of accuracy. Which mental operations are being influenced by practice? One way to answer this question—an alternative to the transfer of training method—is to determine which factors have effects that are invariant over levels of practice, and which factors have effects that change. In other words, if we think of practice as a factor, then the stages it influences can be identified by the other factors with which it interacts.

One task in which the effects of training have been explicitly investigated is the item-recognition task discussed in sections 14.2.2 and 14.5.3. We turn now to the results of two such experiments: experiment 1, which I did at the University of Pennsylvania (Sternberg 1967); and experiment 2, which Kristofferson (1972) did at McMaster University. Both experiments measured performance with lists of length 1, 2, and 4. Instead of changing from one trial to the next, as described in section 14.2.2, the digit list was fixed for about fifty trials in both experiments, but the lists were changed from session to session. What was being practiced from session to session, therefore, was not the retrieval of item information from a particular digit list, but rather the retrieval of item information from digit lists in general. Experiment 1 involved two sessions of 432 trials, separated by about a week, while experiment 2 had a series of thirty sessions of 146 trials on consecutive weekdays, with *RT*s reported as means over sets of five sessions.

$\overline{RT}$s from both experiments are shown in figure 14.15. To avoid clutter, only the data from the first and last sets of five sessions are shown for experiment 2. Both factors have substantial effects: the mean list-length effect (length 4 versus length 1) is 110 ms, and the mean practice effect (experiment 1, session 1, versus experiment 2, sessions 26–30) is 72 ms.

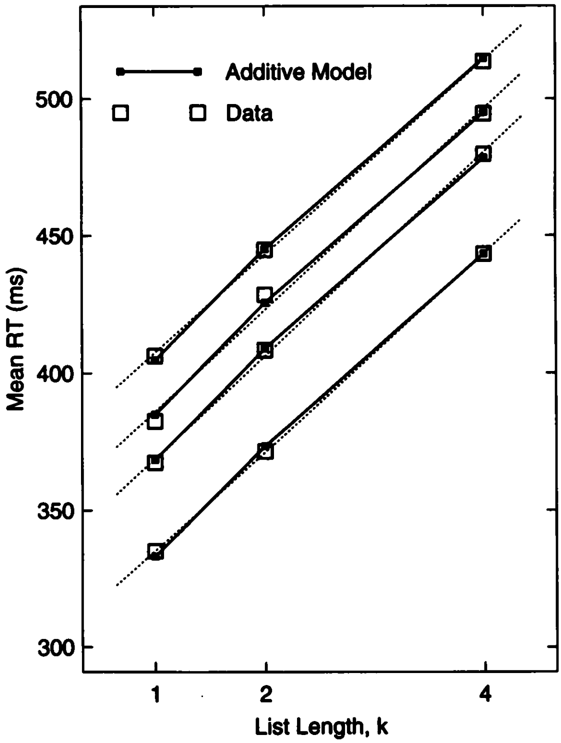


Figure 14.15
Effects of training on item recognition in data drawn from two experiments (experiment 1 from Sternberg 1967; experiment 2 from Kristofferson 1972). The four sets of data (open squares which show means of the $\overline{RT}$s for positive and negative responses) were generated after increasing amounts of training, from top to bottom. Sources of the data are as follows: Top curve: experiment 1, session 1. Second curve: experiment 2, sessions 1–5. Third curve: experiment 1, session 2. Bottom curve: experiment 2, sessions 26–30. The average numbers of training trials when the data were collected, starting with the top curve, are 215, 365, 645, and 4,015. An additive model (filled squares connected by solid lines) was fitted to the four data sets, and fits well, indicating that the effect of list length (k) is invariant over levels of training. Dotted lines are linear functions fitted separately to each data set. From the top function down, their equations are $371.7 + 35.6k$, $349.4 + 36.8k$, $331.7 + 37.2k$, and $299.2 + 36.0k$.

What is striking is not only the similarity of the data from different groups of subjects in different laboratories, but the remarkable invariance of the list-length effect across levels of practice. The filled squares and solid lines are the parallel curves that best fit the four sets of data, and they fit very well, with a mean absolute deviation of only 1.3 ms. Another way to describe the invariance is in terms of the slopes of the fitted linear functions (dotted lines), which, in order of increasing practice, are 35.6, 36.8, 37.2, and 36.0 ms per item, remarkably similar, and showing no systematic trend.[25]

What should we make of the invariance of the list-length effect across levels of training? These results provide a clear answer—albeit a negative one—to the question "What is learned?" as it applies to the item-recognition task. The reduction in $\overline{RT}$ is apparently not the result of an increasingly efficient memory-interrogation process. It remains to be determined which process or processes are responsible for the improvement with practice. The absence of a practice effect on memory search bears on the question of the "ecological validity" of the task. If a laboratory task is "artificial," in the sense of calling on skills not often practiced outside the laboratory (hence "ecologically invalid"), we expect relatively large effects of laboratory practice. Seen in this light, the invariance suggests that the memory-interrogation process called on in the item-recognition task is one that is also used in "everyday life."

14.5.5 Transfer of Information between the Cerebral Hemispheres

The two cerebral hemispheres of the brain appear to have different specialities. For some functions they are differentially efficient; other functions may be localized in only one hemisphere. Reaction-time methods have been applied in interesting ways to analyze these differences, and to measure the transmission of information between the hemispheres. By presenting a stimulus to one side or the other of the visual field, the input can be made to arrive at the contralateral (opposite side) hemisphere. To arrange for the response to be controlled by a particular hemisphere, the experimenter can require it to be expressed by movements of the fingers of the contralateral hand, or by spoken responses (controlled by the left hemisphere in most people).

Consider an experiment in which a stimulus is delivered to either the left hemisphere (S_L) or the right (S_R), but the response is always controlled by the right hemisphere (R_R). When S_L is used, the information must presumably cross to the other hemisphere at some point, in order to control the response. On the other hand, when S_R is used, transmission from one hemisphere to the other is not needed. Based on this analysis, it seems as if the difference between the two RTs, $RT(S_L, R_R) - RT(S_R, R_R)$, or alternatively, $RT(S_R, R_L) - RT(S_L, R_L)$, might provide an estimate of

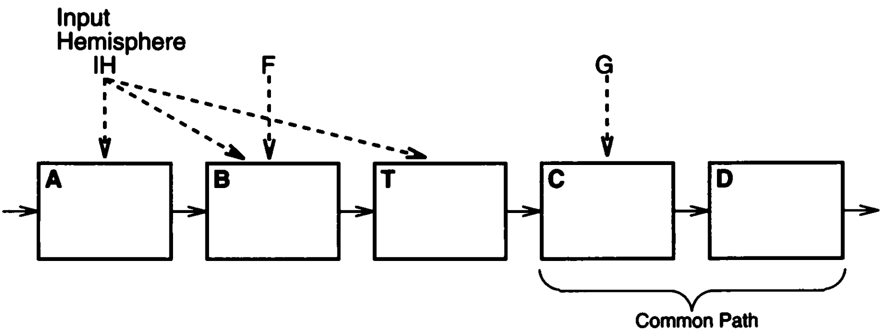


Figure 14.16
A model of information processing in experiments in which stimulus is delivered to either left or right cerebral hemisphere and response is controlled by a particular hemisphere. Stage T is the transmission of information from input to output hemisphere, required if they differ.

the time it takes for information to be transmitted from one hemisphere to the other. An analysis in terms of hypothetical processing stages should make it clear why this simple-sounding approach has met with difficulty and controversy (Bashore 1981; Brown, Larson, and Jeeves 1994).

The model implicit in some of the thinking in this area is shown in figure 14.16. By controlling the hemisphere to which the stimulus is presented (an *input-hemisphere* factor, *IH*), we determine the hemisphere in which all the stages up to stage **T** (transmission from one hemisphere to the other) are carried out. *IH* does not change which hemisphere carries out the stages (**C** and **D**) that follow **T**. Consider the effect of *IH* on $\overline{RT}$. If input and output hemispheres are the same ($IH = IH_1$), stage **T** is null, and takes zero time. If input and output hemispheres differ ($IH = IH_2$), information is transmitted from one to the other during **T**. Can the effect of *IH* be used as an estimate of transmission time? Yes, if the shift in the hemisphere that carries out the early stages (**A** and **B**) has no effect on their durations—that is, if the assumption of pure insertion applies to the transmission process. Otherwise the difference also includes any effects of *IH* on the durations of these early stages (that is, effects of which hemisphere carries them out). Even though transmission time may not be easy to estimate, much can be learned from such experiments.

A simple starting view is that a stage such as **C**, whose hemisphere is fixed across the two levels of *IH*, is not influenced in any way by *IH*. The effect of a factor, *G*, that influences **C** selectively, is therefore expected to be invariant over levels of *IH*. On the other hand, a stage such as **B**, whose hemisphere is determined by the level of *IH*, is likely to function differently as *IH* is varied.[26] In consequence, a factor, *F*, that influences **B** selectively, is likely to interact with *IH*. Similar reasoning applies, of course, if the output hemisphere (OH) is varied, where the quantity examined is $RT(S_L, R_R) - RT(S_L, R_L)$ or $RT(S_R, R_L) - RT(S_R, R_R)$.

This approach would enable us to discover which stages have their hemispheres switched, and which do not, by determining which factors interact with input and output hemispheres, and which do not. One by-product of such investigations is the information they would provide about the ordering of the inferred stages. If the model depicted in figure 14.16 has merit, then a stage, such as **B**, that is influenced by IH must precede a stage, such as **C**, that is not. Consistency among the findings from variation of IH and OH can be used to test the model.

14.5.6 Task Switching and the Frontal Lobes: Inference about Stages from Localized Brain Damage

Suppose that, in the performance of some task, additivity of the effects of factors F and G has been used to identify two processing stages called **A** and **B** and influenced by F and G, respectively. It is possible that the activity during one of these stages, say **A**, depends on or is carried out in a particular brain region, α, that is specialized for that activity. Suppose this region of Jim's brain was functionally impaired. If the impairment was moderate rather than severe, such that Jim could still perform the task and did so without a fundamental change in strategy (that is, stages **A** and **B** still occurred), then the impairment should influence **A** but not **B**. The presence versus absence of impairment in region α (a new, third factor) should then interact with F but not with G.

Such a finding would not only provide useful insights about brain function but could also help support (or deny) conclusions about mental operations arrived at in other ways. If localized impairment of function had selective effects that could be associated with a particular hypothesized stage, this would constitute evidence favoring the existence of that stage. Of course, there is no requirement that the functions of different processing stages be carried out by different processors, anatomically distinct or otherwise, but it is a possibility. (As mentioned above and discussed in section 14.6.1, a flowchart is not necessarily a circuit diagram.)

Although methods for reversible localized impairment of human brain function are being developed (see, for example, Beckers and Homberg 1991), they are not yet well established. For humans, the most widely practiced approach of this sort is therefore one that capitalizes on naturally occurring localized damage, such as certain traumatic head injuries and strokes.[27] Because this approach presents many obstacles, the results must be interpreted with caution.

Comment 9: Difficulties in making inferences from brain damage. Some of the obstacles are as follows: (1) There is typically no performance measure before the damage, so different levels of the new factor, *BD* (brain damage), can be studied only by comparing damaged individuals to *different* undamaged ones. Because individual differences tend to be large and ubiquitous even among people with no obvious brain damage ("normals"), clear-cut inferences are therefore difficult. (2) Traumatic head injuries tend to produce widespread minor damage, as well as localized major damage. The victims of strokes often have widespread cerebrovascular disease. This may be why damage that appears to be localized seems often to produce at least small effects on many functions. (3) Even where functionally distinct brain regions are spatially distinct, there is no reason to expect that the region of damage due to a stroke (which is determined by the brain's vascular organization) corresponds, so as to be functionally specific. Indeed, the localized effects of a stroke may be to damage nerve tracts that project to many brain regions. (4) It may be difficult to find undamaged control subjects with overall levels of performance low enough to be comparable. One approach is to increase the difficulty of the task for these subjects, but such increases may themselves have differential effects on different aspects of performance. (5) After losing tissue that contributes to some function, the brain can sometimes develop a different way to carry out the same function. (6) The requirement that despite brain damage the subject can still perform in the task introduces subject-selection biases with unknown implications.

Despite the obstacles, intriguing findings are beginning to emerge from studies using the AFM where one of the factors is the presence versus absence of damage in a particular region of the brain. For an example of what is possible, let us consider findings from work on the role of the frontal lobes of the brain in switching from one task to another (Rubinstein, Meyer, and Evans forthcoming; Rubinstein, Evans, and Meyer forthcoming) using normal and brain-damaged subjects.

In the task used, four target patterns are displayed, and on each of a series of trials a test pattern is presented. On each trial the subject must decide which target pattern is matched by the test pattern. Each pattern consists of one or more geometric shapes, which vary in (a) number, (b) shape, (c) size, and (d) shading; each of these attributes can have one of four values. In a condition with a low-complexity matching rule, each of the four targets matches the test pattern on a different single attribute, and the subject is instructed to base the match on a particular critical attribute. For example, if the test pattern contained (a) four (b) triangles of (c) size 2 with (d) shading level 3, and the critical attribute was shading level, then the matching target might contain (a) two (b) stars of (c) size 3 with (d) shading level 3. By merely instructing the subject to switch the critical attribute in this kind of experiment, the experimenter can change the task without altering the test or target patterns, so that such switching can be carried out from trial to trial. And the complexity of the judgment can be increased by requiring the matches to be based, for example, on two critical attributes rather than one.[28]

Three hypotheses guide this research. First, an executive cognitive system oversees the coordination of tasks and some aspects of their performance. Second, this system is called into action when a person has to switch between one task and another. And third, the prefrontal region of the cerebral cortex is the part of the brain that carries out the executive function.

The primary factor manipulated in these studies to investigate executive functioning is the requirement of switching between tasks: a *task-sequence* factor, *TS*. In the *pure-task* condition, the critical attribute remains the same from one test pattern to the next. For example, in one pair of pure-task conditions the critical attribute was shape in one series of trials and number in another series. In the corresponding *alternating-task* condition, the critical attribute alternated predictably from one test pattern to the next, so that it was shape for stimuli 1, 3, 5, ... and number for stimuli 2, 4, 6, ... Because there is nothing in the stimulus sequence that indicates which attribute is critical, the subject must remember it (or them) in both pure and alternating conditions. The effect of *TS* is the difference between $\overline{RT}$ for an alternating-task condition and the average $\overline{RT}$ for the corresponding pair of pure-task conditions. Rubinstein, Meyer, and Evans suggest that before determining the match in the alternating condition, the subject must engage in executive processes: shifting to the new goal by storing a command (for example, "Match on number") in short-term memory, and then retrieving from long-term memory the detailed procedures used to carry out that command.

Figure 14.17A shows findings of Rubinstein and colleagues that favor the idea that the executive process and task performance are carried out in distinct stages. In this experiment, normal subjects sorted a deck of cards containing the test patterns into one of four piles, depending on which of the displayed targets was matched. Cards were held face down in one hand and sorted with the other. The $\overline{RT}$ was determined by dividing the total time to sort the deck by the number of cards. One factor that influences task performance is the discriminability of the critical attribute, *AD* (attribute discriminability); how long it takes to discover which of the four targets is matched by a test pattern varies systematically with the attribute. In this experiment, the attributes number and shape were discriminated more rapidly than shading and size. The figure shows that *AD* has an effect that is virtually invariant over the two levels of *TS*, which supports the hypothesis of two stages, executive processes (**A**) and task performance (**B**), with *TS* influencing **A** selectively, and *AD* influencing **B** selectively.

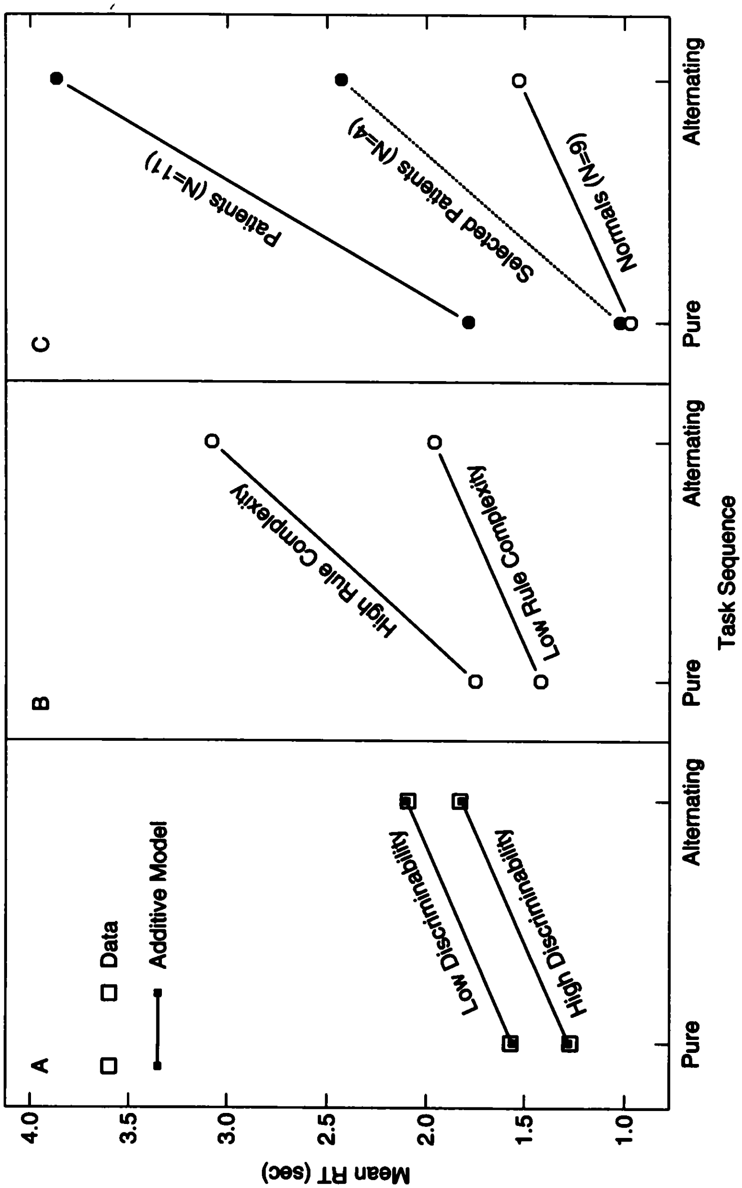
A second factor manipulated by Rubinstein and colleagues is the complexity of the matching rule (*RC*, rule complexity). A task like the example above (with a single critical attribute) is defined as having low *RC*, while a task in which a subject had to base the matching decision on a conjunction of two attributes (like shading and size) is defined as having high *RC*. (In the alternating condition with high *RC*, two pairs of attributes were specified, which alternated from trial to trial. For example, on odd trials the subject might have to choose the target that matched the test stimulus in both shading and size; on even trials in both number and shape.) It seems reasonable that

1. Rule complexity should influence the executive stage **A**, because *activating* a more complex rule should take more time, and
2. Rule complexity should also influence the task-performance stage **B**, because *implementing* a more complex rule should take more time.

The data in figure 14.17B support assertion 1 because *RC* interacts positively with the task-sequence factor, which has been shown (panel A) to influence a distinct stage, **A**. For support of assertion 2 in the spirit of the AFM, we might test whether *RC* interacts with a factor that influences **B** selectively. But the only such factor currently known is discriminability, which is difficult to vary independently of *RC*.

Is there any other argument for assertion 2? Suppose the executive stage **A** *vanishes* when task switching is not required, without altering any of the other processing stages. If so, the evidence in panel B that there is an *RC* effect under pure-task conditions, when task switching is not called for, would support assertion 2. (Without the assumption that **A** vanishes, the effect of *RC* under both alternating-task and pure-task conditions could be explained as due to stage **A**.) How might we determine that a change from alternating-task to pure-task conditions causes deletion of **A**, rather than merely reduces its duration? If there is a factor that influences only stage **A**, and **A** is deleted, then we expect to see *no* effect of that factor on $\overline{RT}$. Thus the complete elimination of an effect of a factor when *TS* is changed from alternating-task to pure-task is a special kind of interaction that could be taken as evidence favoring deletion rather than merely shortening of the executive stage. As we shall see, frontal lobe damage may influence only stage **A**, and may thus be such a factor.

Comment 10: The meaning of "pure insertion." The relation we are considering between task switching and the presence of stage **A** is an instance of the assumption of "pure insertion" required by Donders'


A
Data
Additive Model
Low Discriminability
High Discriminability
B
High Rule Complexity
Low Rule Complexity
C
Patients (N=11)
Selected Patients (N=4)
Normals (N=9)
Pure
Alternating
Pure
Alternating
Pure
Alternating
Task Sequence
Mean RT (sec)
4.0
3.5
3.0
2.5
2.0
1.5
1.0

subtraction method, and has two parts. The first part ("pure") asserts that the manipulation should have no influence on other stages. In accordance with the AFM, this part can be tested by determining whether factors believed to influence these other stages have effects on $\overline{RT}$ that are invariant with and without the insertion of A. Suppose the increase in *TS* level inserts the executive stage. Then invariance of the attribute-discriminability (*AD*) effect (assumed to be due to the task-performance stage) over levels of the *TS* factor provides such a test. The second part of "pure insertion" ("insertion"), asserts that the experimental manipulation (say, an increase in level of *TS*) should insert a stage (say, A), rather than merely prolong a stage that is already present. This is the part that would be favored if a factor that influences $\overline{RT}$ under alternating conditions had zero effect under pure-task conditions.

To acquire the data shown in figure 14.17C, Rubinstein, Evans, and Meyer (forthcoming) ran several groups of brain-damaged patients in a pattern-matching experiment, along with a group of matched normals. The task was similar to the low-complexity condition used earlier, but instead of sorting cards the subjects responded with key presses to video displays; it is perhaps because of the procedure change that the $\overline{RT}$s for normals are shorter in this figure. The data shown are from the normals and from the largest group of patients, those with damage in the left prefrontal cortex. In almost all cases the damage was due to a stroke. In thinking about such data, we need to realize that such damage is a crude manipulation: even given precise localization of executive function in the brain, with the neurons in a particular region responsible for it and nothing else, the likelihood is small that the damage due to a stroke would be limited to that region, and hence to that function.

Figure 14.17
Effects of four factors in a categorizing task. *TS*: task sequence pure (matching criterion stays the same from test stimulus to test stimulus) versus alternating (criterion alternates); *AD*: high versus low discriminability of the criterial attribute when rule complexity is low; *RC*: complexity of the classification rule low (one criterial matching attribute) versus high (two criterial attributes); and *BD*: normal versus damaged left prefrontal cortex. Panel A shows additive effects of *TS* and *AD* in normal subjects: the effect of task sequence is invariant over levels of discriminability. Panel B shows the interaction of *TS* and *RC* in normal subjects: the effect of task sequence is modulated by rule complexity. Panel C shows an interaction of *TS* and *BD*: the effect of task sequence is greater in patients than normals. Data from the selected subset of patients (dotted line) show that prefrontal damage that degrades performance when tasks alternate can leave pure-task performance unimpaired. Data in panels A and B from Rubinstein, Meyer, and Evans (forthcoming), experiment 1; data in panel C from Rubinstein, Evans, and Meyer (forthcoming), experiment 1.

Seen in this light, the data in figure 14.17C are impressive. The top pair of points are the means from the full set of eleven patients, and the bottom pair from the normals. The effect of *BD* is substantially greater when task switching is required. Whereas the introduction of task switching produces a mean increase in *RT* of 57 percent for normals, the mean increase is 122 percent for patients. Even more interesting, in four of the eleven patients, their $\overline{RT}$s in the pure-task condition fall within the range of $\overline{RT}$s for that condition in the nine normals. The mean $\overline{RT}$ for those four selected patients is shown by the middle pair of points. It is no doubt premature to draw firm conclusions from such limited data, especially when the selection criterion is a potential source of bias.[29] Suppose, however, that the data pattern for the selected group were confirmed. This would indicate that it is possible for the brain to be damaged in such a way as to leave the task-performance stage unimpaired, while prolonging the executive stage considerably (the mean increase in *RT* due to task switching for this selected group is 140 percent). Furthermore, the absence of an effect of *BD* on pure-task performance (for these patients) is consistent with the idea that the executive stage is deleted when task switching is not required and the task is sufficiently simple, rather than merely shortened.

These tentative conclusions are illustrated by the flowcharts in figure 14.18, which are intended to apply to normals and to the selected group of patients for whom *BD* has no effect on **B**. Panel A contains the sort of diagram you have seen before. But in this case there is evidence that the effect of *TS* on the executive process is special, in the sense that at the pure-task level ($TS = TS_1$) this stage is deleted. The result is that *BD* interacts with *TS* in a special way, such that the *BD* effect disappears when $TS = TS_1$ instead of merely shrinking. This feature is spelled out in figure 14.18B. A symbolic representation of the conclusions is also helpful. Let levels of TS_i (task sequence), BD_j (brain damage), RC_k (rule complexity), and AD_m (attribute discriminability) be indexed as shown, by *i*, *j*, *k*, and *m*, respectively. The model of figure 14.18B can then be expressed as $\overline{RT}_{ijkm} = a_{ijk0} + b_{00km}$, with the condition $a_{1jk0} = 0$ added, to capture the idea that **A** is deleted in the pure-task condition, $TS = TS_1$.

How might these conclusions be strengthened? This could be done by confirming implications of the model. First, we need evidence that the same processing stages are used by brain-damaged subjects as by normals, as reflected in the model. Thus, for example, it is important that *TS* and *AD* have additive effects in brain-damaged as well as normal subjects, that is, that $D_{im}(\overline{RT}_{ijkm}) = D_{im}(a_{ijk0}) + D_{im}(b_{00km}) = 0$ for both $BD_j = BD_1$ and $BD_j = BD_2$. Second, because brain damage is assumed to have no effect on **B** in the selected group of patients, the effect of *AD* should be the same for that group as for normals. That is, $D_m(\overline{RT}_{i1km}) = D_m(\overline{RT}_{i2km}) = D_m(b_{00km})$. And third, whereas *RC* might have a larger effect in brain-

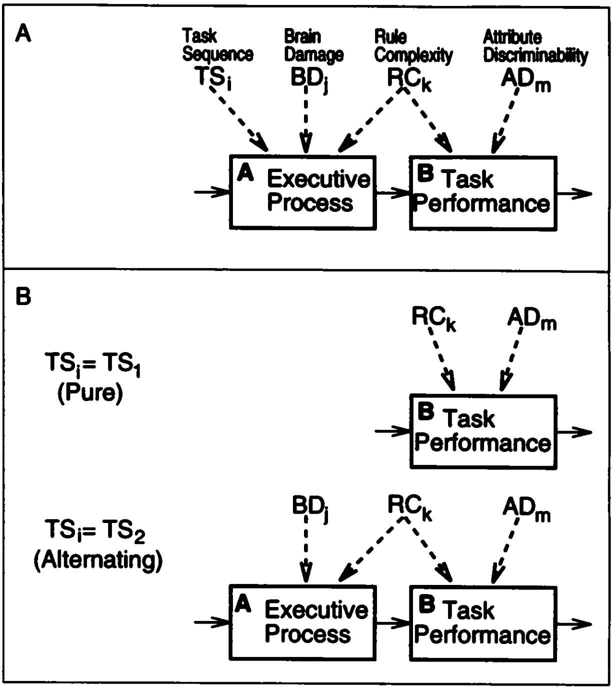
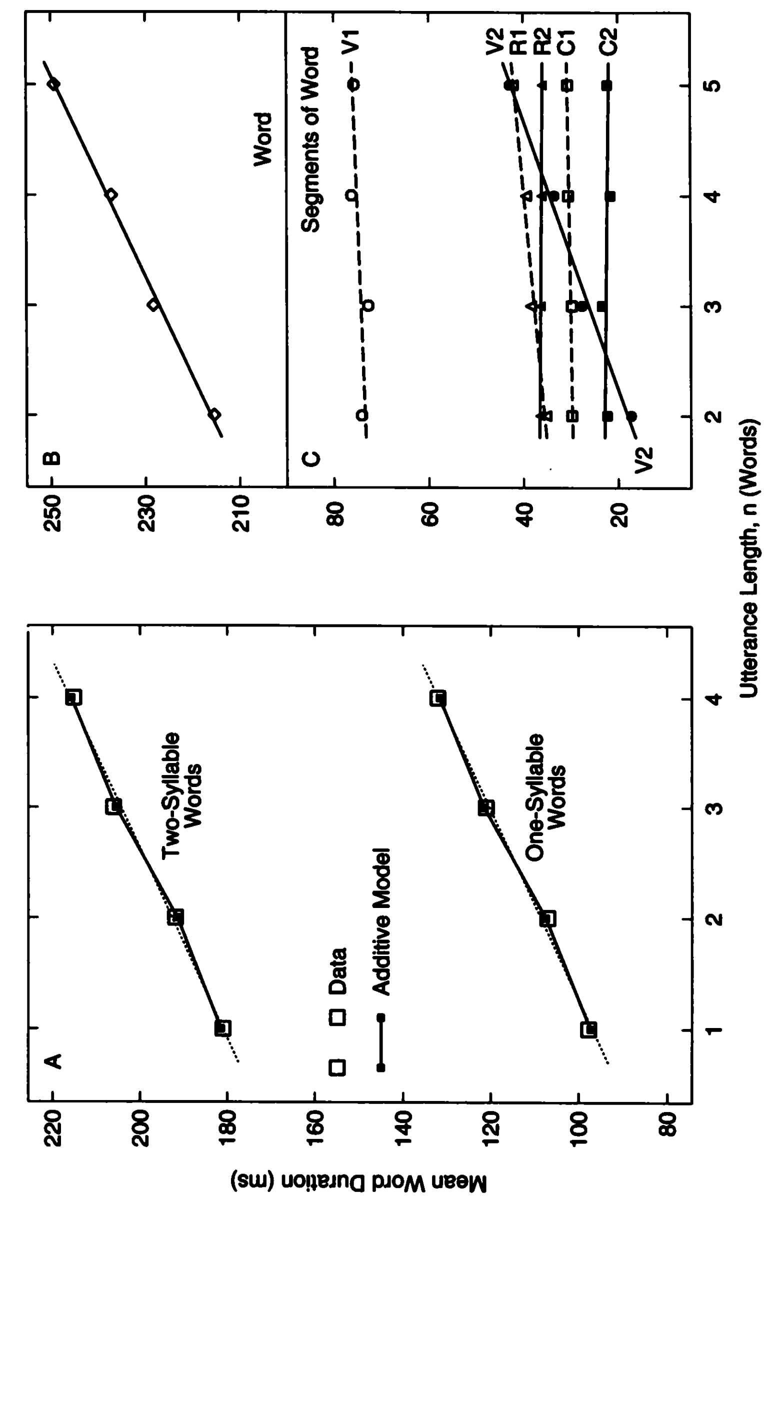


Figure 14.18
Two-stage model of classification performance for normals and for selected set of patients in which brain damage has no effect on pure-task performance. In panel A the model is expressed as a typical flowchart. The diagram in panel B makes explicit the deletion of stage A under pure-task conditions.

damaged subjects than normals in the alternating condition, this should not be true in the pure-task condition, where the *RC* effects for the two groups should be equal. That is, whereas $D_k(\overline{RT}_{22km}) - D_k(\overline{RT}_{21km}) = D_k(a_{22k0}) - D_k(a_{21k0})$ may be positive, $D_k(\overline{RT}_{12km}) - D_k(\overline{RT}_{11km}) = D_k(b_{00km}) - D_k(b_{00km})$ should be zero. If it is to apply to the entire group of patients, for whom *BD* influences stage **B** as well as A, the model, and hence the second and third implication, would have to be modified.

14.5.7 Stages in Speech Production: Real-Time Evidence of a Stage-Specific Effect

Try to say "one, two, three, four, five" as fast as you can. It is not surprising that the shortest time per word that you can achieve depends on the properties of the words, such as the number of syllables each one contains. More unexpected is the finding that the time per word in utterances


A
Data
Additive Model
Two-Syllable Words
One-Syllable Words
Mean Word Duration (ms)
B
Word
C
Segments of Word
V1
V2
R1
R2
C1
C2
Utterance Length, n (Words)

planned in advance also depends on the number of words in the utterance. For utterances of up to about six words, even when they are as simple as "one, two, three," the more words they contain, the more time elapses between the beginning of one word and the beginning of the next (Sternberg, et al. 1988). For example, the time from the beginning of "one" to the beginning of "two" is about 20 ms longer in "one, two, three, four, five" than it is in "one, two, three." That a property of the whole utterance (its length) influences the production of each element is consistent with the idea that the production of a planned utterance is controlled by a representation of the entire sequence—an *utterance program.*

The discovery of how the effect of utterance *length* (number of word units, *n*) combines with the effect of unit *size* (number of syllables per unit), provided a clue about the basis of these effects.[30] Results of the experiment are shown in figure 14.19A, where the mean word duration in milliseconds per word is plotted as a function of the length of the utterance, for utterances of one-syllable words (like "bay, rum, limb") and of two-syllable words (like "baby, rumble, limit").[31] The excellent fit of the additive model in figure 14.19A suggests that two stages are associated with the production of each unit in the utterance, one influenced by utterance length but not unit size, and the other influenced by size but not length. This is the direction my colleagues and I took in developing a model of the rapid production of planned speech, sketched below.

We assume that after seeing a visual display of the utterance to be produced, but before the signal to speak, the subject prepares an utterance program, which is stored in a "motor-program buffer." The program consists of a set of *subprograms*, one for each *unit* in the utterance. (For the experiment whose results are shown in figure 14.19, the number of units equals the number of words, although production-unit boundaries are not necessarily the same as word boundaries.) An alternating sequence of *selection* and *command* stages uses the information in the program. Before the

Figure 14.19
Effects of utterance length on word and segment durations. Panel A shows effects of utterance length (number of words, *n*) and word size (number of syllables per word) on mean word-duration. An additive model (filled squares connected by solid lines) has been fitted to the data (open squares) and fits well, indicating that the effect of utterance length on word duration is invariant over word size (and vice versa). The fitted linear functions (dotted lines), constrained to have the same slope, are $85 + 12n$ ms (one-syllable words) and $169 + 12n$ ms (two-syllable words). Panel B shows mean word duration for utterances composed of two-syllable words selected to facilitate segmentation based on the acoustic signal. The fitted linear function is $194 + 11n$ ms. Panel C (under panel B) shows mean segment durations for the same utterances. The fitted linear functions are, in ms, C1: $29.0 + 0.3n$; R1: $31.2 + 2.2n$; V1: $71.8 + 0.9n$; C2: $23.3 - 0.3n$; R2: $37.0 - 0.2n$; and V2: $1.7 + 8.2n$. Data from Sternberg et al. 1988, sections 2.2 and 4.3.

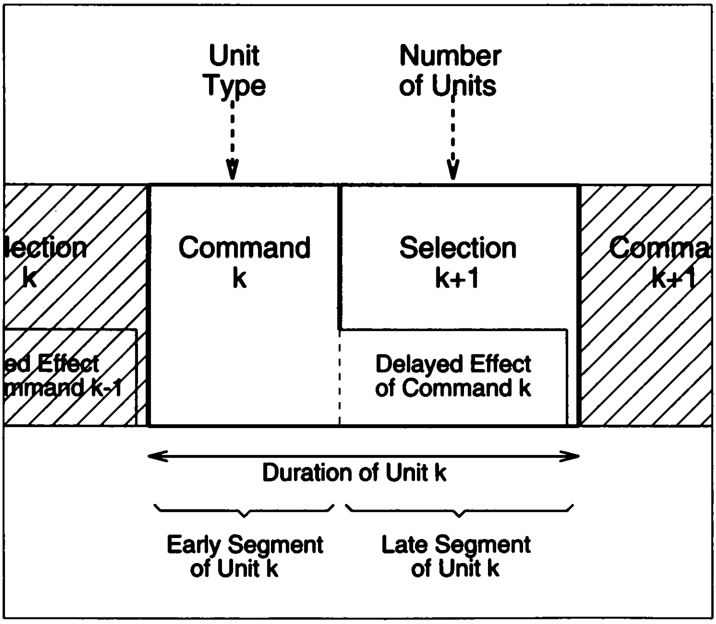


Figure 14.20
Inferred stages associated with typical unit in rapidly produced planned utterance of sequence of such units. Horizontal arrows between stages are omitted to clarify time relations.

production of a unit can be initiated, the relevant subprogram must be selected from the buffer; the command stage then causes it to be "executed." We assume that the duration of the selection stage is influenced by the number of subprograms but not by the length of the subprogram selected or the size of the corresponding unit. In particular, the duration of the selection stage increases linearly with the number of units. (This would not be unreasonable if selection were accomplished by a sequential search through a set of directory entries. However, many find implausible the idea that any search for the next unit should be required.) We also assume that the duration of each command stage is influenced by the size of the subprogram being executed (and thus the size of the unit), but not by the number of units in the utterance. As indicated in figure 14.20, the time from the beginning of one unit to the beginning of the next (the duration of the unit) is thus occupied by the command stage for the unit now being produced, followed by the selection stage for the unit that will follow. The duration of the unit is then the sum of the durations of these command and selection stages.

From the viewpoint of the AFM, what is interesting about this application is that during the inferred stages of processing not only are mental (and brain) events occurring, as in the other applications discussed, but also overt behavior (speech) is being produced. This provides us with a new way of testing the stage model. We have inferred that utterance length influences the duration of only the selection stage. The influence of length is therefore *intermittent*. We should be able to define a series of epochs during the utterance, such that the even epochs are influenced by utterance length, while the odd epochs are not.[32]

This suggests that the series of articulatory events in the produced speech might reflect the underlying stages, which would thus be externalized, and thus separately measurable. Suppose the command stage directly controls an initial subsequence of the sequence of articulatory gestures that makes up a unit. Because the command stage is not influenced by utterance length, the articulation rate within the initial subsequence should also be independent of length. It is thus possible that, within the production of each unit, the extra time required when we lengthen the utterance is *localized*, perhaps toward the *end* of the unit, when the selection stage for the next unit is taking place. An alternative to localization of the effect of utterance length on the maximum speech rate is that it is a global effect, distributed over all the articulatory events, as it might be if maintenance of a longer utterance program in memory made more use of a limited resource shared by the speech production process.

> *Comment 11: More on the model and its predictions.* Because rapid speech does not contain regular periods of silence as long as the duration required by the selection stage, the command stage must have delayed effects as well as direct effects, as indicated in figure 14.20. This permits control of the subsequent articulation of the end of the unit, articulation that occurs during the ensuing selection stage. But to explain the additivity we observed, any such delayed effects must end before the next command stage begins, or must give way to it. Also, because the boundaries of the production unit are not necessarily the boundaries of the word, it does not follow from the model that the segments influenced by utterance length will be those at the end of the word. Finally, because there is supposedly one selection stage per unit, there should be as many such sets of contiguous rate-varying segments as there are units in the utterance.

The prediction based on the additive effects of length and size about the externalized stage structure also depends on a sufficient degree of moment-to-moment coupling between the underlying processing operations and the speech-production process itself. Although we had no evidence about this coupling, the prediction seemed worth testing. We used two-syllable words like "copper" and "token," with stress on the initial syllable and with each syllable starting with an articulatory closure (during which no sound is emitted), to facilitate segmentation based on the acoustic signal. Mean word duration is shown as a function of utterance length in figure 14.19B. We decomposed each word into six segments, *C1*, *R1*, *V1*, *C2*, *R2*, and *V2*, which correspond roughly to the consonant closure, the consonant release, and the vowel, for each syllable, and we measured the durations of each segment. The mean segment durations, in figure 14.19C, show that the prediction is confirmed. Although *V1* is the longest segment (a feature of two-syllable words with initial stress), its duration is virtually unaffected by utterance length. In contrast, most of the effect of length on word duration (74 percent) is localized in the vowel of the second syllable, *V2*, which, for two-word utterances, consumes only 8 percent of the duration of a word. Rather than pervading the unit, the effect of utterance length is indeed intermittent in the speech itself, just as it is in the stages shown by the AFM to underlie its production.

14.5.8 Rotation and Magnification of Mental Images

Students of psychology have been captivated by the claim that people can form a mental image, store it, compare it to a new stimulus, and transform it in various ways without changing its shape, such as mentally rotating it in the plane or scaling its size up or down. Rotation and size scaling are especially interesting because their duration patterns suggest they are similar to corresponding manipulations in the physical world.

When the orientation of a physical object is changed from upright (0°) to upside-down (180°), for example, it must pass through the full set of intervening orientations, $0° \leq \theta \leq 180°$. And when the projection on the retina of a real object 100 cm from you shrinks because it moves to 200 cm, it must pass through the intervening sizes. The similarity to mental operations arises in tasks that subjects supposedly accomplish by using mental images. In some experiments, when two patterns are presented successively and subjects must decide whether they have the same shape despite possible disparities in orientation (or size), the time to respond increases systematically with the magnitude of the disparity. One explanation is that to align the mental image of one stimulus (S_1) with the percept of the other (S_2), the image of S_1 is gradually transformed into the orientation (or size) of S_2, passing through the intervening orientations (or sizes). Such an effect is similar to what would happen if the transformation were carried out by "analog computation"—by manipulating a representation of the physical object that captures its spatial properties relatively directly, rather than a more abstract representation of the object. Why might the alignment of image and percept be necessary? One possible reason is that they are compared by a process of template matching: position-by-position point correlation.[33]

14.5.8.1 Combining Two Image Transformations

It is tempting to think that there might be separate special-purpose processes for mental rotation and mental size-scaling. How might we test this idea? On a simple view of the image-transformation process, the time to rotate an image from orientation θ_1 to θ_2 should not depend on the size of the object, and the time to scale the size of an image from s_1 to s_2 should not depend on its orientation. One approach to testing for separate processes is to require subjects to compare patterns that differ in *both* orientation and size. Do subjects implement separate transformations of each? If they do, the rotation process (and hence the effect of disparity in orientation on it) should be invariant over changes in size ratio. And similarly, the size-scaling process (and hence the effect of size ratio on it) should be invariant over changes in orientation disparity. If, in addition, these two transformations are accomplished in different processing stages, the AFM provides the desired tests of such invariance.

On each trial in their experiment, Bundesen, Larsen, and Farrell (1981) asked subjects to view two successive stimulus presentations of the same character (the characters they used were "3," "4," "7," "J," "P," and "R," all asymmetric both vertically and horizontally). The subject first saw the "target" (S_1), for 0.5 sec, and then after 1.1 sec, the "probe" (S_2), which remained visible until the response was made. The target and probe could differ in size, orientation, or both, to varying extents. In addition, each stimulus could be either normal or mirror-reversed. If both target and probe were normal, or if both were reversed, the correct response was to press the "same" button; otherwise, the correct response was to press the "different" button. Each character had a bar superimposed on it, from its center to its top, to facilitate discrimination of its orientation.

The effects of the orientation and size disparities between the target and the probe, averaged over response type ("same" and "different") and over six subjects, are shown in figure 14.21. The orientation disparity was 0°, 30°, 60°, or 90°, either clockwise or counterclockwise; data are averaged over direction. The ratio of linear sizes, size of probe / size of target, was either r or r^{-1}, where r could be 1.0, 1.5, 2.67, or 4.0. Data are averaged over r and r^{-1} for each r. Shown with the data is a fitted additive model, which fits very well; the mean absolute deviation is only 2 ms. The deviations appear nonsystematic and are small, especially in relation to the range of over 200 ms covered by the means. The effect of orientation disparity is invariant over differences in size, and the effect of size ratio is invariant over differences in orientation. We thus have strong support for the idea that mental rotation and mental size-scaling are accomplished in separate stages. Introspective reports from two of the subjects might lead to a minor modification of this idea; they reported experiencing

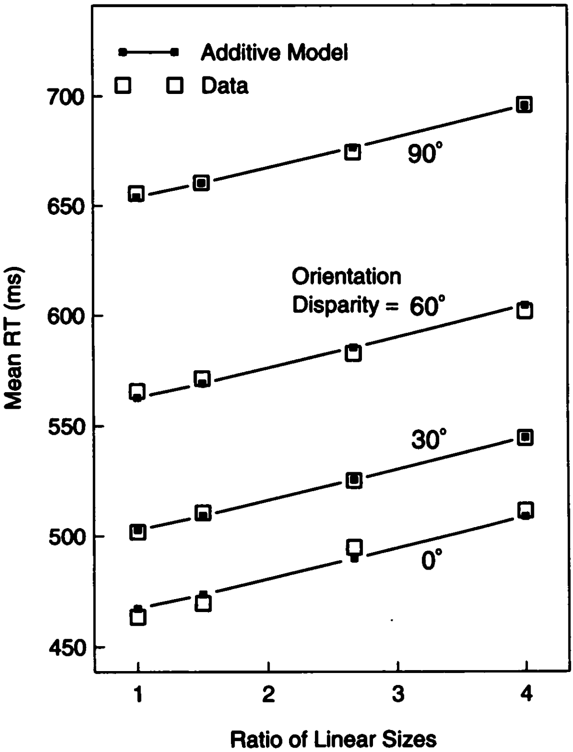


Figure 14.21
Effects of orientation and size disparities between target and probe in same-different experiment. Mean *RT* averaged over "same" and "different" responses (open squares) is shown as a function of the ratio of linear sizes of target and probe, for each of four orientation disparities indicated in degrees. An additive model (filled squares connected by lines) has been fitted to the data, and fits well, indicating that the effect of each kind of disparity is invariant over levels of the other. Data from Bundesen, Larsen, and Farrell 1981.

changes in the size and orientation of their images as concurrent rather than sequential. How might this be reconciled with the support for an arrangement in stages? Bundesen, Larsen, and Farrell suggest that the rotation and size-scaling operations may each be composed of a sequence of partial transformations, so that at any given time only one such transformation is occurring, and that these two kinds of partial transformation are interleaved.[34] That is, instead of two stages there are two processes that alternate, with orientation disparity influencing one process and size ratio influencing the other. Another possibility is that introspective reports on these rapid processes may not be accurate reflections of the underlying operations, and that in fact one transformation is completed before the other begins. The AFM alone cannot discriminate between these possibilities.

Any evidence of other factors that influence one of the two transformations and not the other would strengthen the conclusion of separate processing modules. Between-subject differences fortuitously provide such an effect in this study. The subjects fell into two groups, based on their overall mean *RT*s: for four of the subjects these values ranged from 508 to 548 ms; for the other two they were 643 and 665 ms. The effects of the orientation and size factors are shown separately for the two groups in figure 14.22. Panel A shows that orientation effects differ dramatically: for the fast group, the effect is approximately linear with a range of 119 ms, whereas for the slow group, the effect is distinctly nonlinear with a range of 267 ms, more than twice as great. In contrast, panel B shows remarkable similarity between the size effects in the two groups. Both are approximately linear, and the fitted linear functions have similar slopes (13 and 16 ms per unit ratio). Apparently the mental rotation mechanisms differ markedly between groups, while the mental size-scaling mechanisms are similar. That is, the factor *SUB* (subjects) interacts with orientation disparity but virtually not at all with size ratio. Such a close approximation to the selective influence of *SUB* on mental image rotation is further evidence that rotation and size scaling of mental images are carried out by functionally distinct processes operating in separate stages.

14.5.8.2 Mental Rotation in Detail: Application of the Subtraction Method

By detailed analysis of trials for the four fast subjects on which the target (S_1) and probe (S_2) could differ only in orientation and were equal in size, Bundesen, Larsen, and Farrell (1981) tested the stagewise structure of the image-rotation process and, using a modern version of the subtraction method, produced convincing evidence for the idea that when a mental image is transformed from orientation θ_1 to orientation θ_2, it traverses intermediate orientations. To explain how they did this, I provide a few more details of their experiment. Figure 14.23 shows the set of twelve possible orientations of the target and probe, denoted by θ_t and θ_p, respectively, in degrees counterclockwise from vertical. The target (S_1) could have any one of these twelve orientations, and the probe (S_2) could be separated from the target by $|\theta_p - \theta_t| = 0°, 30°, 60°$, or 90° clockwise or counterclockwise. Bounded by each pair of adjacent orientations are two directed sectors, one directed away from the vertical and one toward it, denoted by upper- and lowercase letters, respectively. For example, associated with 120° and 150° are sectors *e* (directed toward 0°) and *E* (directed away from 0°). The need for directed sectors will become clear below. With twelve θ_t values and seven θ_p values for each θ_t, the full data set of $\overline{RT}(\theta_t, \theta_p)$ has 84 values. By averaging over θ_t values and

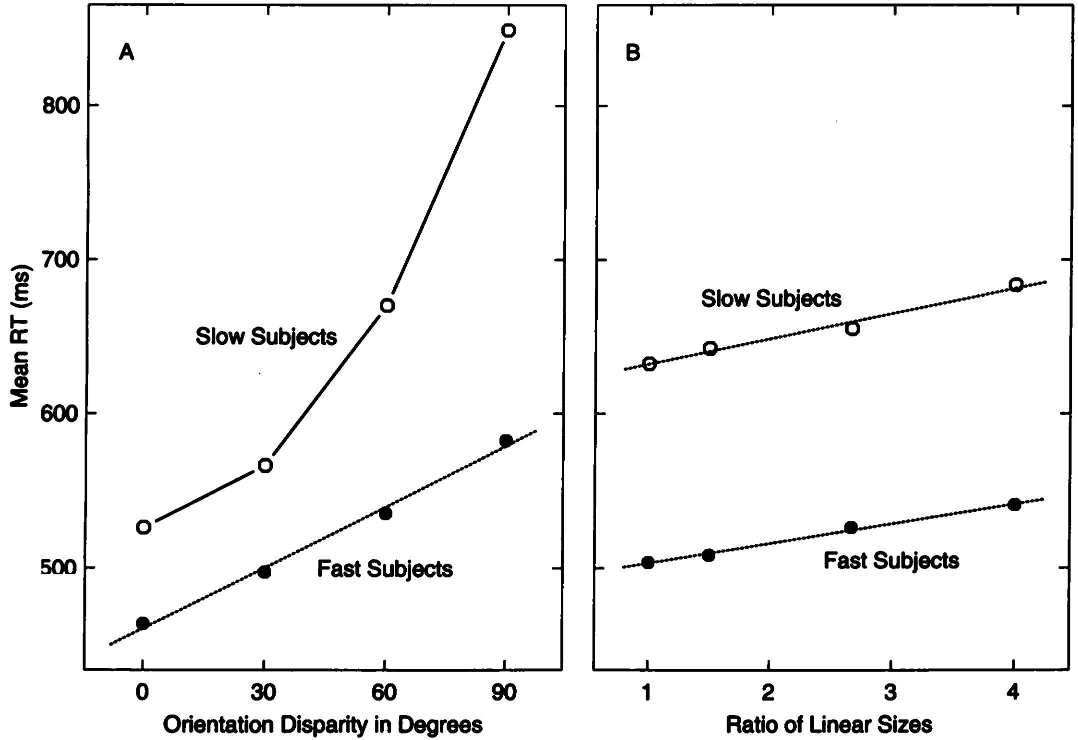


Figure 14.22
Comparison of effects of orientation and size disparity for four fast subjects (filled circles) versus two slow subjects (open circles). Panel A shows effect of orientation disparity. A linear function (dotted line; slope 1.3 ms/deg) has been fitted to the data from the fast subjects. Panel B shows effect of size disparity. Linear functions (dotted lines) have been fitted to each set of data; the slopes for fast and slow subjects, respectively, are 12.8 and 16.4 ms per doubling of size. Data from Bundesen, Larsen, and Farrell 1981.

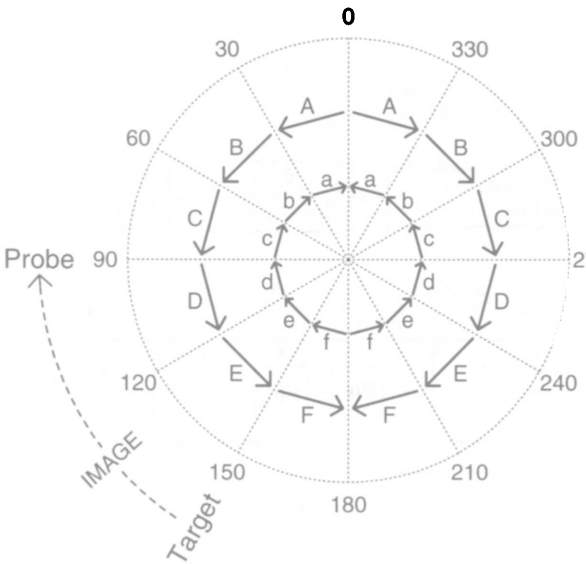


Figure 14.23
The twelve possible orientations of target and probe in experiment of Bundesen, Larsen, and Farrell 1981, with directed sectors denoted by letters. Orientations on a typical trial are indicated by the locations of the words "target" ($\theta_t = 150°$) and "probe" ($\theta_p = 90°$). In the model discussed in the text, the image starts at the orientation of the target and is mentally rotated to the orientation of the probe.

direction of the orientation disparity, the data set is reduced to four values of $\overline{\overline{RT}}(|\theta_p - \theta_t|)$, exemplified by figure 14.22A, where there is also averaging over size difference. (Note the use of $\overline{\overline{RT}}$ to indicate the further averaging.)

Suppose target and probe have orientations $\theta_t = 150°$ and $\theta_p = 90°$, respectively, as indicated in figure 14.23. One model of the process, diagrammed in figure 14.24A,B, is as follows: The image starts at $\theta_t = 150°$, the orientation of the target. After the probe is presented and its orientation determined by an initial encoding process, the image is rotated clockwise through $|\theta_p - \theta_t| = |90° - 150°| = 60°$ to $\theta_p = 90°$, traversing sectors *e* and *d*. Finally, the rotated image and the probe are compared, a same-different decision is made, and the response is initiated. Let $\overline{RT}_{ed}$ denote the mean RT for such a trial.

As suggested in the figure, the image-rotation process in the model is influenced by twelve factors, $H_A, H_B, \ldots, H_a$, one for each sector that might be traversed. (The rotation process is of course regarded as continuous; the cuts that define discrete substages are inserted at 30° intervals only to reflect the manipulations in this particular experiment.) Each of these factors

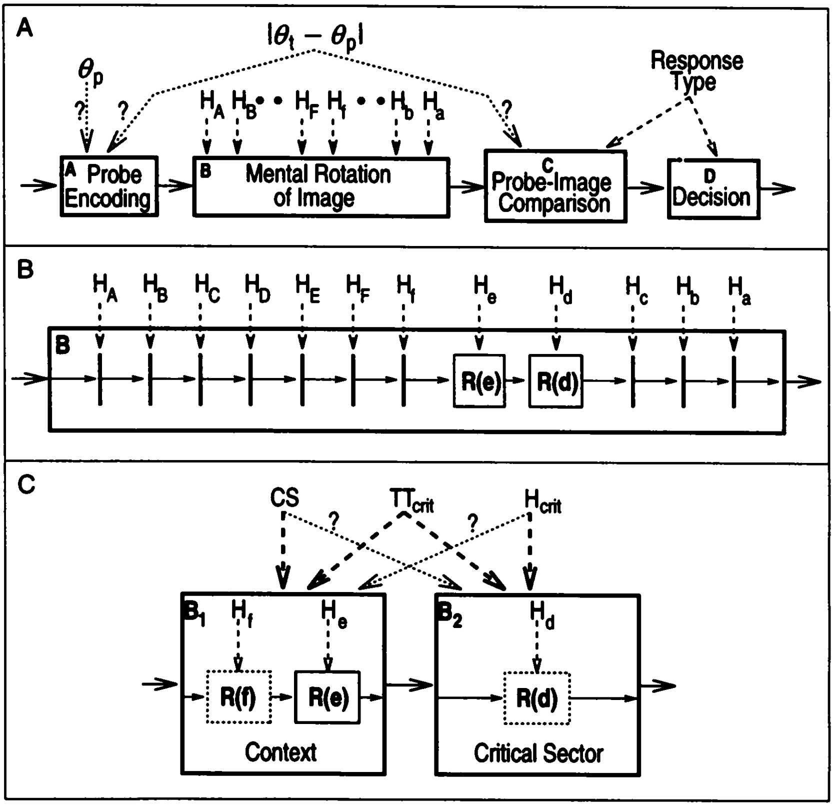


Figure 14.24
A model for the mental rotation task. Panel A shows the four hypothesized stages. Encoding the probe is likely be influenced by its orientation, θ_p. As discussed below, probe encoding might also be influenced by priming of the probe by the target (presented earlier), to an extent that decreased with the difference between their orientations, $|\theta_t - \theta_p|$. The rotation stage is influenced by which sectors must be traversed by the rotation process. For example, factor H_A is elevated if sector A must be traversed. The response type ("same" or "different") is likely to influence the comparison and decision stages. As discussed below, if mental rotation is not distortion-free and the distortion increases with the amount of rotation, the comparison stage might also be influenced by the orientation difference between target and probe. Panel B shows the substages of the mental-rotation stage on a trial when only sectors *e* and *d* must be traversed, as in figure 14.23. In this case, the levels of factors H_e and H_d are elevated, and substages **R**(**e**) and **R**(**d**) have nonzero durations. In panel C (discussed below) stage **B** has been segmented into $\mathbf{B}_1$ (for traversing the context sectors) and $\mathbf{B}_2$ (for traversing a critical sector). The factors are the size of the context rotation (*CS*), the presence of the critical sector (H_{crit}), and the time to traverse the critical sector (TT_{crit}). The order of $\mathbf{B}_1$ and $\mathbf{B}_2$ depends on the particular critical and context sectors.

has two levels, "present" and "absent." Only when a factor's level is elevated (i.e., "present") does the rotation include traversal of the corresponding sector. A change in factor level therefore inserts or deletes a (sub)stage of the image rotation process, and the effect of that factor is the duration of that substage—the time taken to traverse the corresponding sector. (For example, if the level of H_d is "present," but not otherwise, the rotation includes stage $R(d)$, during which the image traverses sector d. Figure 14.24B shows the sequence of twelve stages of image rotation for the trial described above, for which $(\theta_t, \theta_p) = (150°, 90°)$. Only the levels of factors H_e and H_d are elevated; the stages corresponding to the other sectors are all shown as consuming zero time.[35]

To test the idea that the image passes through intermediate orientations, Bundesen, Larsen, and Farrell (1981) strengthened the image rotation model with an additional property that they would also test. They assumed that not only must the image be rotated through all the sectors between θ_t and θ_p (the *intervening-sectors property*), but also that the time to traverse any particular sector is the same, regardless of the other sectors with which its traversal is concatenated (the *sector-duration invariance property*). This strong second property, which excludes all context effects on rotation rate, makes possible a quantitative test of the first property.

If sector-duration invariance could be confirmed, we would have another instance of the invariance of factor effects. For example, the invariance of the time to traverse sector f relative to the presence or absence of sector d in the rotation can be described as the invariance of the effect of H_f with respect to the level of H_d. This analysis illustrates the fact that the subtraction method (section 14.4.2) is a special case of the AFM where the critical factors all have two levels and a change in factor level inserts or deletes a stage, thus providing an estimate of its duration. The pure-insertion assumption then implies (and is tested by) the invariance of the effects of such factors. This view of the subtraction method makes it clear that an additivity test provides one way to assess its validity. For example, one test of sector-duration invariance is provided by asking whether the effects of factors H_d and H_f are additive. One estimate of t_d, the time to traverse sector d, is provided by the effect of H_d when sector f is also traversed, $\overline{RT}_{fed} - \overline{RT}_{fe}$. Another estimate is provided by the effect of H_d when sector f is not traversed, $\overline{RT}_{ed} - \overline{RT}_e$. If the effects of H_d and H_f are additive, then these two estimates of t_d are equal, and $\overline{RT}_{fed} - \overline{RT}_{fe} = \overline{RT}_{ed} - \overline{RT}_e$ (such comparisons are discussed below). Equality would tell us that insertion of stage $R(d)$ has the same effect on $\overline{RT}$ whether or not stage $R(f)$ is present (and vice versa), thus supporting the pure-insertion assumption.

By evaluating averages of such estimates, Bundesen, Larsen, and Farrell (1981) discovered that the traversal times for different sectors differ radically, and also that they depend on the direction of traversal. In general, rotation is slower when the image is farther from upright (sector f in figure 14.23 is slower than sector b), and when the direction of image motion is away from upright rather than toward it (sector B is slower than sector b). However, the bilateral symmetry indicated by the repeated letters in the figure seems to obtain (for example, sector A has the same duration whether clockwise or counterclockwise).

Comment 12: Sector differences and their implications. One set of estimated traversal times for individual sectors $a, \ldots, f$ are 26, 32, 33, 54, 34, and 67 ms, respectively, and for $A, \ldots, F$ are 32, 54, 56, 70, 94, and 53 ms, respectively; these twelve values (which also estimate the effect sizes of the factors shown in figure 14.24B) cover a range of almost four to one. Each of these values was obtained by averaging four estimates. For example, $\hat{t}_a$ is the mean of $\overline{RT}_{cba} - \overline{RT}_{cb}$, $\overline{RT}_{ba} - \overline{RT}_{b}$, $RT_{aAB} - \overline{RT}_{AB}$, and $\overline{RT}_{aA} - \overline{RT}_{A}$. Note that if there is an effect of probe orientation θ_p on an initial stage in which the probe (S_2) is encoded, then the first two of these estimates would include this effect and would thus be biased, because the probe orientations differ between the two terms in these estimators. (In the example, the traversals of *cba* and *cb* end at $\theta_p = 0°$ and $\theta_p = 30°$, respectively.) This is not a problem for the second two estimators, however. (In the example, the traversals of *aA* and *A* both end at $\theta_p = 30°$.) You may wonder how the large differences among the sector durations can be consistent with the approximate linearity of the function that relates $\overline{\overline{RT}}$ to $|\theta_p - \theta_t|$ (figure 14.22A, fast subjects). These differences are irrelevant because each plotted value is a mean of rotations that start equally often at each of the twelve orientations and, for $|\theta_t - \theta_p| > 0$, are directed equally often clockwise and counterclockwise. Hence, given the intervening-sectors property, each plotted value reflects each sector with equal frequency (twice each for the set of rotations that are averaged for 60°, for example). Given sector-duration invariance, the same twelve sector durations are thus included with equal frequencies in each plotted $\overline{\overline{RT}}$ value for which $|\theta_p - \theta_t| > 0$. In particular, the $\overline{\overline{RT}}$s for 30°, 60°, and 90° are means over trials on which rotation durations are, for 30°, $t_a, t_b, \ldots, t_F$; for 60°, $(t_b + t_a), (t_c + t_b), \ldots, (t_E + t_F)$; and for 90°, $(t_c + t_b + t_a), (t_d + t_c + t_b), \ldots, (t_D + t_E + t_F)$. Thus, while linearity of the effect on $\overline{\overline{RT}}$ of $|\theta_p - \theta_t|$ is consistent with a uniform rotation rate, it by no means demonstrates it. To see this in a more dramatic case, suppose only one sector takes nonzero time (for example, suppose the interval from θ_t to θ_p adds k ms to the RT if it includes sector F, and zero ms otherwise). The mean effect of orientation disparity on $\overline{\overline{RT}}$ would still be linear. Furthermore, while consistent with the conjunction of the properties of intervening sectors and sector-duration invariance, such linearity would also be found if the duration of each sector increased or decreased with the number of sectors traversed, if the increase or decrease satisfied certain constraints.

In an important insight, Bundesen, Larsen, and Farrell (1981) recognized that the systematic differences among the traversal times for sectors a, b, ..., f, A, B, ..., F permits a strong test of the property of sector-duration invariance, and thereby the intervening-sectors property. To see what sector differences can contribute to such a test, suppose instead that traversal times are equal for all sectors and directions. The entire pattern of durations of the rotation process, for all combinations of θ_t and θ_p—including additivity of the sector factors—could then be described by the single assertion that the mean duration, t_{mr}, of the mental-rotation stage increases linearly with $|\theta_p - \theta_t|$. An alternative model of the effect of orientation disparity in which the effect was not due to the duration of an image-rotation process would have only to approximate such a linear increase.

Comment 13: Alternative explanations of the orientation-disparity effect. One such alternative is that there is no transformation of the image. Instead, corresponding features in the image and probe are compared sequentially. However, locating a feature in the probe that corresponds to one in the image requires search, and if the orientation disparity is greater, more search (and hence more time) is needed. A second alternative is that the image is rotated to correspond to the probe, but the average time to perform the rotation is fixed, rather than increasing with orientation disparity. The rotated image is a distorted version of the target, however, with an amount of distortion that increases with $|\theta_p - \theta_t|$. The resulting shape discrepancies between the rotated image and the probe slow the comparison process by an amount that increases with orientation disparity. In a third alternative, discussed below, the encoding of the probe is primed by presentation of the target to an extent that decreases with disparity. Any of these alternative mechanisms could produce an increase in $\overline{\overline{RT}}$ with $|\theta_p - \theta_t|$, as required. (To adjust such mechanisms so that the increase is linear might make them less plausible, but an image-rotation mechanism in which mean rotation rate is independent of the size of the rotation is also implausible, and the truth is sometimes implausible. Furthermore, the data are only approximately linear.) The three alternative mechanisms differ in an important respect from the proposed image-rotation explanation:

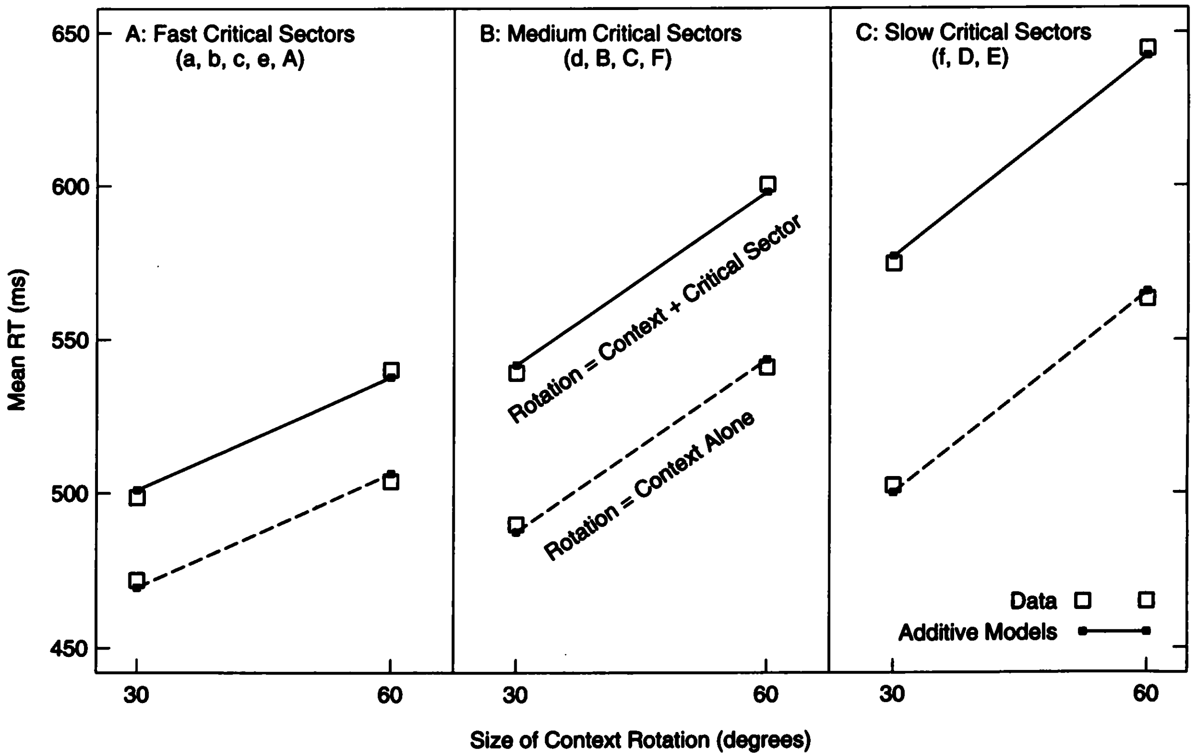
for neither of them is the process for a small orientation disparity nested within the more prolonged process required when the disparity is greater.

One way to think about the implications of the differences of traversal time across sectors is that each sector has a distinctive durational "signature," expressed as an additive contribution to t_{mr} when that sector is traversed. According to the model, that signature should stay the same as we vary the other sectors that must be traversed. Notice that more than one such kind of invariance can be examined. For example, we could ask whether the traversal time for a sector is the same whether it begins or ends a rotation (one kind of invariance). Or we could ask whether it is the same regardless of the durations of the sectors with which it is concatenated (a second kind of invariance). The test of a third kind of invariance is shown in figure 14.25. Here we ask whether the traversal time for a "critical" 30° sector is the same whether it is concatenated with another 30° rotation (the low level of a "context size" factor; $CS = CS_{30}$) or with a 60° rotation (the high level of context size; $CS = CS_{60}$). Each sector can be regarded as the critical one, and we can ask whether the corresponding sector factor interacts with context size. Suppose d is the critical sector and thus $H_{crit} = H_d$ the sector factor. One way to answer this question is to compare two effects. The first is the effect of concatenating **R(d)** (the traversal of sector d) with traversal of one other sector (the effect of H_d when $CS = CS_{30}$) and is estimated by the mean of $\overline{RT}_{ed} - \overline{RT}_e$ and $\overline{RT}_{dc} - \overline{RT}_c$. (In each of these differences the $\overline{RT}$ for context alone is subtracted from the $\overline{RT}$ for context plus critical sector.) The second is the effect of concatenating **R(d)** with traversal of two other sectors (the effect of H_d when $CS = CS_{60}$) and is estimated by the mean of $\overline{RT}_{fed} - \overline{RT}_{fe}$ and $\overline{RT}_{dcb} - \overline{RT}_{cb}$. Given the invariance property, these two means should be equal.[36] Instead of making this comparison separately for each of the twelve sectors, I combined them into three sets according to their mean traversal times, to simplify the test and improve reliability. Membership of the critical sector in a set can be regarded as the level of a factor that specifies its traversal time, TT_{crit}. Mean traversal times for the fast, medium, and slow sets are 32, 54, and 77 ms, respectively, and are reflected by differences among the vertical separations between the fitted parallel lines in the three panels of figure 14.25; the separations increase from left to right.[37] Within a panel, in figure 14.25A, for example, the mean effects of concatenating a fast sector (TT_{crit} short) with 30° and 60° rotations are shown, respectively, by the distances between the left-hand and right-hand pair of data points.

The data for context-alone rotations (bottom pair of data points in each panel) differ across panels A, B, and C because the sets of context sectors differ for the three levels of TT_{crit}. Context and critical sectors must be adjacent or almost adjacent; also for most rotations they are traversed in the same direction relative to vertical. As a result, the traversal times of critical sectors and their associated context sectors are positively correlated. This correlation is reflected by the slopes of the fitted lines in figure 14.25, which are correlated with their separations, increasing with TT_{crit}.

To help in thinking about this analysis, it is useful to consider the model of figure 14.24 as restated in panel C. Here the rotation process (stage **B**) is segmented into two stages, $\mathbf{B}_1$ for traversing the context sectors, and $\mathbf{B}_2$ for traversing the critical sector. The factors to consider are context size (CS), presence or absence of the critical sector (H_{crit}), and time to traverse the critical sector (TT_{crit}, short, medium, or long). The context-size factor determines the levels of the two sector factors associated with stage $\mathbf{B}_1$: For the sectors depicted, $CS = CS_{30^\circ}$ means that H_e is elevated, but not H_f; $CS = CS_{60^\circ}$ means that both are elevated. The figure also indicates relations between these factors and stages $\mathbf{B}_1$ and $\mathbf{B}_2$. The heavy broken arrows between factors and stages represent the model with sector-duration invariance: context size (CS) influences only $\mathbf{B}_1$; presence or absence of the critical sector (H_{crit}) influences only $\mathbf{B}_2$. Traversal time of the critical sector (TT_{crit}) influences both $\mathbf{B}_2$ (a direct effect) and $\mathbf{B}_1$ (an indirect effect, due to the correlation of traversal times for critical sector and context). Given this model, the effect of inserting the critical sector would be invariant over levels of context size.

Figure 14.25 shows that the model with sector-duration invariance (sector factor additive with context size for each of the three sector groups) fits reasonably well: the mean absolute deviation is 4.8 ms, only 9 percent of the mean traversal time of 54 ms. On the other hand, the deviations from the fit (which represent a failure of the subtraction method's pure insertion assumption) must be taken seriously: first, because they are consistent in direction (for each of the three groups, the duration added by traversing a sector is greater with 60° context than with 30° context); and second, because they are quantitatively consistent, being very close to the same magnitude (for short, medium, and long levels of TT_{crit}, respectively, the increase in the effect of their being concatenated with two other sectors rather than one is 9.6, 10.0, and 9.3 ms). This interaction of CS with H_{crit} implies that rather than being a precisely linear function of $|\theta_t - \theta_p|$, $\overline{\overline{RT}}$ should accelerate: $\overline{\overline{RT}}(90^\circ) - \overline{\overline{RT}}(60^\circ)$ should exceed $\overline{\overline{RT}}(60^\circ) - \overline{\overline{RT}}(30^\circ)$. Looking back at figure 14.22A, note that the function (for fast subjects) does accelerate.[38] Once we take seriously the nonlinearity for the fast subjects, it is tempting to regard slow and fast subjects as differing quantitatively rather than qualitatively and to explain their behavior with the same model, differing only in its parameter values.


A: Fast Critical Sectors
(a, b, c, e, A)
B: Medium Critical Sectors
(d, B, C, F)
C: Slow Critical Sectors
(f, D, E)
Rotation = Context + Critical Sector
Rotation = Context Alone
Data
Additive Models
Mean RT (ms)
650
600
550
500
450
30
60
30
60
30
60
Size of Context Rotation (degrees)

How can we explain the small but systematic violation of the model—the interaction of CS and H_{crit}? One way is for CS also to influence $\mathbf{B}_2$, or for H_{crit} also to influence $\mathbf{B}_1$, or both, as indicated by the dotted arrows in figure 14.24C. This pattern of influences can produce the required two-way interaction. However, because all three factors then influence at least one stage in common, we also expect a three-way interaction. That is, if TT_{crit} influences a stage that produces the $CS \times H_{crit}$ interaction, then the magnitude of that interaction should be modulated by the level of TT_{crit}. Does this occur? Consider the data pattern further. Let T_{ijk} denote the mean RT for the ith level of CS, the jth level of H_{crit}, and the kth level of TT_{crit}. Now, using the notation of section 14.3.5, we evaluate the three-way interaction: $D_{ijk}(T_{ijk}) = D_k[D_{ij}(T_{ijk})]$. Recall that $D_{ij}(T_{ijk})$ is the two-way interaction, $(T_{22k} - T_{12k}) - (T_{21k} - T_{11k})$, which takes on the values 9.6, 10.0, and 9.3 ms for $k = 1, 2, 3$, respectively. Applying D_k to these values, we get $(10.0 - 9.6, 9.3 - 9.6) = (0.4, -0.3)$ ms, a negligible three-way interaction, evidence against the influences indicated by the dotted arrows in figure 14.24C, and favoring sector-duration invariance. In short, if we explain the two-way interaction as resulting from the image-rotation process (stage **B**), we are led to expect a three-way interaction as well, which is strikingly absent.

With the model so close to working well, it is tempting to try to resolve this paradox, and explain the $CS \times H_{crit}$ interaction as a result of their common influence on a stage not also influenced by TT_{crit}, that is, a stage not a part of the image-rotation process itself. If so, the property of sector-duration invariance would apply to image rotation. Candidates include the probe-encoding and comparison stages (figure 14.24A), which presumably precede and follow the image-rotation process. One way to phrase the problem is that we must explain how a three-sector rotation can be too slow relative to a two-sector one, but by an amount that is independent of traversal time.

Figure 14.25
Mean RT in mental rotation task for small (30°) and large (60°) context rotations alone, or concatenated with "critical" 30° sector. Panels A, B, and C provide such data for three different sets of critical sectors, distinguished by their estimated traversal times. In panel A, for example, the bottom pair of values (open squares) are the $\overline{RT}$s produced when just contexts of size (CS level) 30° and 60° must be traversed, while the top pair of values are the $\overline{RT}$s produced when the required rotation includes the critical sector concatenated with those contexts. Insofar as the traversal time for the critical sector (the effect of H_{crit}) is invariant over levels of CS, the additive model (filled squares connected by lines) would fit well. The model does fit well, but the consistency of the pattern of deviations over sets of fast, medium, and slow critical sectors (panels A, B, and C) requires us to take these deviations seriously. Data from Bundesen and Larsen 1996.

One possibility is that the number of sectors in the required rotation influences the process of encoding the probe, a process that must be completed before any image-rotation operations begin. To appreciate the possibility of such an effect on encoding the probe, recall that the orientation disparity $|\theta_p - \theta_t|$ is what determines the number of sectors to be traversed. If the prior presentation of the target (S_1) facilitates encoding of the probe (S_2), as suggested by studies of "priming," then it is possible that the amount of priming depends on the similarity of the orientations of probe and target.[39] That is, the larger the orientation disparity, the less the priming and the greater the duration of the encoding stage. If this effect were appropriately nonlinear, the 10 ms interactions could be generated, and we would not have to complicate the image-rotation process itself. A conjecture like this would need empirical verification, of course, probably in an experiment in which the priming effect was measured as directly as possible.

A second possible locus of the $CS \times H_{crit}$ interaction is the comparison stage. If the image suffered increasing distortion as it was rotated, then this could slow its comparison to the probe by an amount that increased with context size. Because such an effect might differ between "same" trials and "different" trials, one test would be to search for modulation of the two-way $CS \times H_{crit}$ interaction by response type ("same" or "different")—that is, a three-way interaction of CS, H_{crit}, and response type, a factor that is likely to influence comparison but not encoding.

14.5.9 Doing Two Things at Once: Why Are We Slower?

In everyday life we often seem able to do two things at once (like conversing and driving a car) without interference, but this may be an illusion. Laboratory measurements show that even simple tasks can interfere drastically (see, for example, Pashler, 1994a). This section describes the overlapping-tasks procedure that has been used to study dual-task interference. To explain many of the findings, psychologists have developed a theory of a "bottleneck" (or "single channel") that prevents concurrent operation of two processes and causes a brief interruption in the processing stream for one of the tasks. The AFM has been extended, initially by Pashler (1984), to test this theory of *deferred processing* and to discover the locus of the hypothesized bottleneck, that is, to determine which operations in the two tasks cannot proceed concurrently. The arguments associated with this problem are more complicated than others in the chapter. Work on the problem is of special interest in relation to the AFM, however, because it provides some of the strongest available evidence for mental operations arranged in stages even when the operations are not data-dependent—that is, even when the second operation does not depend on information furnished by the first.

14.5.9.1 The Overlapping-Tasks Paradigm

On each trial in a typical experiment, the subject performs two different choice-reaction tasks (tasks 1 and 2), each with its own sets of alternative stimuli and responses, sets S_1 and R_1 for task 1, and sets S_2 and R_2 for task 2. To simplify notation, I use RT' and RT'' to denote the reaction times in tasks 1 and 2, respectively. The stimuli are presented in the order S_1 followed by S_2, separated by a short time Δt, often called "stimulus onset asynchrony," which varies unpredictably from trial to trial. It is the variation of Δt that is the diagnostic tool at the heart of this paradigm. Typical values of Δt might be 50, 150, 300, and 800 ms. With a value of Δt as long as 800 ms, R_1 will have occurred well before S_2 is presented. But as Δt is shortened, the tasks overlap increasingly. The subject is instructed to complete task 1 as fast as possible (that is, to minimize $\overline{RT'}$) and is often expected to produce R_1 before R_2, and, given that these goals are met, to minimize $\overline{RT''}$. Dual-task interference is indicated by the finding that $\overline{RT''}$ is prolonged as we reduce Δt and thus force the two tasks to overlap more in time. In idealized data from the overlapping tasks paradigm (figure 14.26), $\overline{RT'}$ is not influenced by Δt, and when the tasks are widely separated (large Δt), neither is $\overline{RT''}$. But once Δt is reduced sufficiently, further reduction produces a corresponding increase in $\overline{RT''}$. Such effects are shown for actual data in figures 14.30 and 14.31, by the increase of 250–350 ms in $\overline{RT''}$ as Δt is reduced.

14.5.9.2 The Bottleneck/Deferred-Processing Model

Before I discuss any data, it will be helpful to consider the model that motivates many of the experiments, diagrammed in figure 14.27. Unlike most other flowcharts in this chapter, the widths of the boxes and other horizontal distances in this figure are intended to be proportional to the durations of the operations they represent. For this reason, the horizontal arrows that usually indicate the temporal ordering of stages have been omitted. The emphasized leftmost edge in each flowchart represents the time of stimulus presentation, and the emphasized rightmost edge represents the time of response occurrence. Thus it is the distance between these two edges that represents RT' or RT''.

Suppose task 1 is accomplished by stages **A**, **B**, and **C**, and task 2 by stages **U**, **V**, and **W**. The model is based on the assumption that with one exception, all between-task stage pairs—such as the pair **C** and **V**—can be carried out concurrently and without influencing each other. The exception is the pair of stages **B** and **V**, which, according to the model,

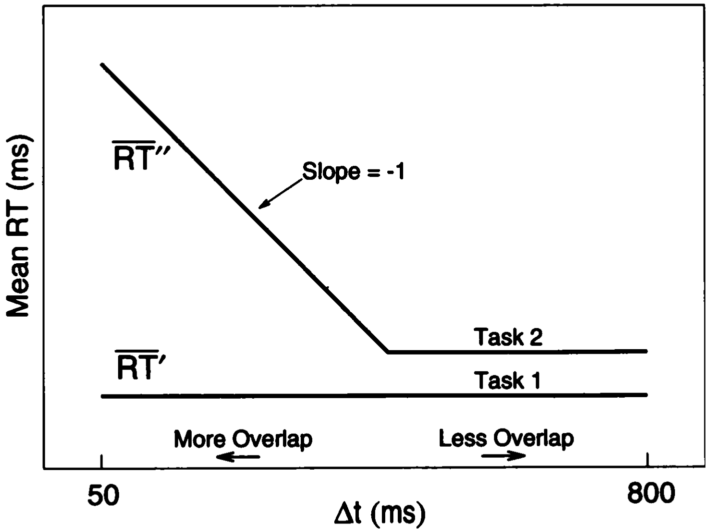


Figure 14.26
Idealized data from an overlapping-tasks experiment. Mean reaction-times for task 1 ($\overline{RT}'$) and task 2 ($\overline{RT}''$) are plotted as functions of Δt, the time elapsing between the stimulus for task 1 (S_1) and the stimulus for task 2 (S_2).

cannot be carried out concurrently, possibly because each of them requires the full capabilities of the same processor. Instead, **V** must be delayed until **B** is completed. At the outset, the functions of these interfering stages are unknown; one of the goals of the research is to discover what they are. The boxes that represent **B** and **V** in figure 14.27 have been drawn taller than the others to help convey the idea that they cannot overlap. If Δt is small (panel B), stage **U** ends before stage **B** does, so the bottleneck is engaged, task 2 operations are interrupted, and the start of **V** must await the completion of **B**. **GP** will denote the waiting period—the gap between the end of **U** and the start of **V** (sometimes called the "slack"); its duration is gp. **GP** is an additional epoch, concatenated with **U**, **V**, and **W**, whose duration contributes, along with theirs, to $\overline{RT}''$. Thus $\overline{RT}''$ is the sum of four mean durations:

$$\overline{RT}'' = u + gp + v + w. \quad (14.8)$$

It is because an increase in Δt causes gp to shrink (until it reaches zero) that it also causes $\overline{RT}''$ to shrink. To make the notation clearer, let us define a factor $F_{\Delta t}$ associated with Δt; as indicated by the downward arrow in panel B, $F_{\Delta t}$ must then also influence **GP**. Note that because of our convention that identifies "higher" levels of a factor with the levels that pro-

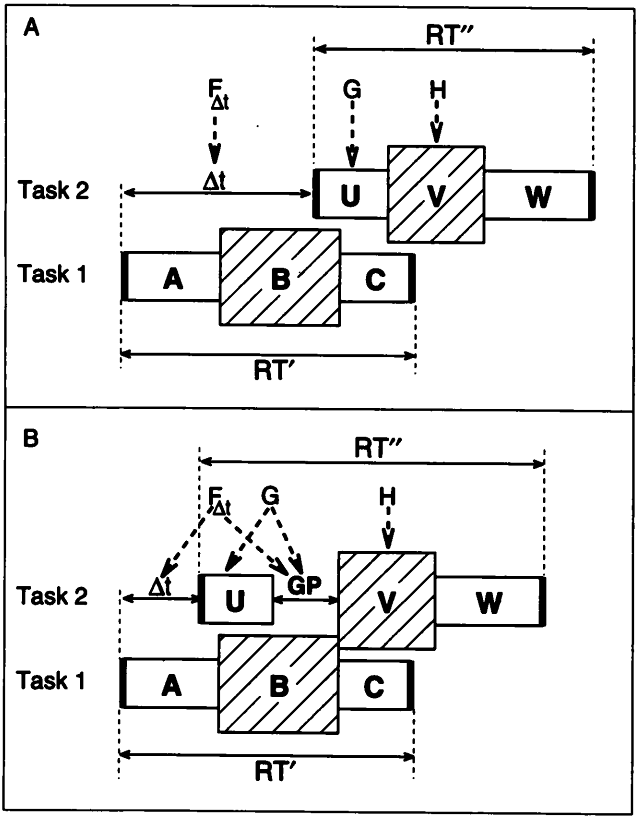


Figure 14.27
Bottleneck/deferred-processing model. Each task is assumed to be accomplished in three stages, **A**, **B**, and **C**, for task 1, and **U**, **V**, and **W**, for task 2. The effect of the bottleneck is to prevent **B** in task 1 and **V** in task 2 from overlapping. Panel A shows that when Δt is sufficiently long ($F_{\Delta t}$ at a low level), **U** ends late enough that the bottleneck is not engaged, and $\overline{RT}''$ is the sum of the durations of **U**, **V**, and **W**. Panel B shows that when Δt is sufficiently short ($F_{\Delta t}$ at a high level), **U** ends before **B** does. The bottleneck is engaged, and delays the start of **V** until **B** ends, and $\overline{RT}''$ is prolonged by the duration, *gp*, of the resulting gap.

duce longer $\overline{RT}''$s, a *higher* level of $F_{\Delta t}$ (and greater task overlap) is associated with a *shorter* Δt. In panel A, Δt is sufficiently long so that **U** does not end until after **B** is completed; thus there is no delay of **V**, and $gp = 0$.

Consider the effects of two other factors, G and H, which influence **U** (before the bottleneck) and **V** (after the bottleneck), respectively. The crux of the model's predictions, and the power it has to illuminate the organization of mental processes, resides in the way in which the effects of G and H behave as $F_{\Delta t}$ is varied. We shall see that the effect of a factor that influences a stage (such as **V**) that is at the bottleneck or after it is unaffected by $F_{\Delta t}$, whereas the effect on $\overline{RT}''$ of a factor that influences a stage (such as **U**) earlier than the bottleneck is modulated by $F_{\Delta t}$; the modulation is strong enough so that at sufficiently high levels of $F_{\Delta t}$ (small values of Δt) the effect of G on $\overline{RT}''$ can vanish altogether.

Because the gap is the source of the $F_{\Delta t} \times G$ interaction, it is helpful to consider exactly how gp varies with these two factors. To be precise here we should consider the durations of **A**, **B**, and **U** on particular trials, instead of their means, a, b, and u. Let a_n, b_n, and u_n represent such individual trial values. (Think of the subscript n as a trial number.) By examining panel B you should be able to see that on any particular trial, gp_n depends on the relation between $a_n + b_n$ and $\Delta t + u_n$:

$$gp_{nij} = \begin{cases} a_n + b_n - \Delta t_i - u_{nj}, & \text{if } a_n + b_n > \Delta t_i + u_{nj}; \\ 0, & \text{if } a_n + b_n \leq \Delta t_i + u_{nj}. \end{cases} \tag{14.9}$$

Here, u_{nj} represents the duration of stage **U** on trial n when G is at level j. When Δt and/or u_{nj} are sufficiently long, gp_{nij} is zero.

Comment 14: Deterministic approximation. I have written thus far as if the duration of a processing stage, given particular factor levels, was a constant, having no inherent variability, despite the fact that variable process durations are more realistic. As discussed in comment 10 (section 9.2.6), this deterministic approximation works very well in accounting for mean durations of sequential processes, where the durations that are variable contribute as terms in a sum. This is because $\text{mean}(x_n + y_n) = \text{mean}(x_n) + \text{mean}(y_n) = x + y$. That is, averaging over trials *after* summing of stage durations (as when we average RTs) gives the same result as averaging *before* summing (as when we make predictions from mean stage durations). Thus, for sequential processes, we can ignore the variability of stage durations in making predictions about $\overline{RT}$. In the overlapping-tasks paradigm, however, some processes occur in parallel and, as a consequence, the deterministic approximation fails in certain respects. Consider equation 14.9, for example. Here Δt is a fixed quantity, determined by the experimenter. But because RT is observed to vary across identical trials, some or all of a, b, and u_j must be inherently variable, and it is plausible that all three are. This means that if $a + b$ is sufficiently close to $\Delta t_i + u_j$, we will have a mixture of trials, on some of which $a_n + b_n$ is large relative to $\Delta t_i + u_{nj}$, engaging the bottleneck and producing a gap, and on some of which $a_n + b_n$ is small and the bottleneck irrelevant. The result is that the deterministic approximation is just that—an approximation. Another way to see this is to express equation 14.9 as $gp_{nij} = \max\{0, a_n + b_n - \Delta t_i - u_{nj}\}$. In general, $\text{mean}(\max\{x_n, y_n\}) \neq \max\{\text{mean}(x_n), \text{mean}(y_n)\} = \max\{x, y\}$. That is, the average of the maxima over trials (which determines mean gap duration) can differ from the maximum of the average (used in the predictions pictured in figure 14.28). Thus the mean of gp_{nij} cannot in general be determined from just the means of the constituents, a_n, b_n, Δt_i, and u_{nj}. However, in much of the remainder of section 14.5.9, we will assume that variability is relatively small, so that the deterministic approximation is a good one. In comment 15, I explain one way the approximation fails.

14.5.9.3 Some Implications of the Model

Implication 1: Propagation through the bottleneck of effects in task 1. Suppose a factor P that influences **A** (before the bottleneck) and/or **B** (at the bottleneck) but none of the other stages in either task. Equation 14.9 shows that if Δt is small (gp is large), then gp will fully express the effect of P, and thus, given equation 14.8, so will $\overline{RT}''$. Because such an effect is also fully expressed by $\overline{RT}' = a + b + c$, one implication of the model is that when Δt is small, P will have the same effect on $\overline{RT}''$ as it has on $\overline{RT}'$. This phenomenon can be described as propagation through the bottleneck of task 1 effects onto task 2 (an example is discussed below and shown in figure 14.29). Figure 14.27B should make it clear why the equality of the effects of P on $\overline{RT}'$ and $\overline{RT}''$ requires that P can have no influence on **C** in task 1 or on **V** or **W** in task 2. Furthermore, full propagation can occur only with values of Δt such that on all trials the bottleneck is engaged and the gap is nonzero. The figure should also help you see that if a factor influences only a stage in task 1 that follows the bottleneck (exemplified by **C**), it can have no effect on $\overline{RT}''$.

Implication 2: Effects in task 2 that are invariant relative to $F_{\Delta t}$. Of the four components of $\overline{RT}''$ (equation 14.8), only v is influenced by factor H. It should be clear from figure 14.27 that whatever the value of gp, any effect of H on the duration of **V** is fully expressed in RT''. That is, the effect of H on RT'' is invariant over values of gp, and hence over levels of $F_{\Delta t}$ and G: H is additive with both $F_{\Delta t}$ and G. Similarly, there would be no modulation by $F_{\Delta t}$ of the effect of a factor influencing a stage beyond the bottleneck, such as **W**. In short, if a factor influences only stages of task 2 at or after the bottleneck, the effect of that factor is not modulated by $F_{\Delta t}$.

Implication 3: Effects in task 2 that are modulated by $F_{\Delta t}$. On the other hand, $F_{\Delta t}$ does modulate the effect of a factor like G that influences a stage in task 2 that precedes the bottleneck. To understand this we have to consider further how *gp* depends on $F_{\Delta t}$ and G. Figure 14.28 illustrates the behavior of *gp* (also described by equation 14.9) and the consequences for the sum $u + gp$, and hence for RT''.

The heavy curves in each of the three panels of this figure show a duration as a function of Δt, for $G = G_1$ (broken curve) and $G = G_2$ (solid curve). Panel A shows that u depends on the level of G, but not on Δt. Panel B represents the effect of G on *gp* as the vertical separation between the two heavy curves. Within region 1, an increase in Δt produces a corresponding decrease in *gp*, and the effect of G on *gp* is invariant over Δt values. Because the effect of G on *gp* (panel B) is equal in magnitude to its effect on u (panel A), but opposite in direction, its effect on u is hidden; the sum $u + gp$ of the two durations, shown in panel C, shows no net effect of G. It is this sum that is expressed in $\overline{RT}''$.

Within region 2, however, the effect of G on *gp* declines as Δt increases, and in region 3 it is zero. As a consequence, the effect of G on u emerges from hiding in region 2, and in region 3 is fully expressed in the sum $u + gp$ and therefore in $\overline{RT}''$.

Examination of figure 14.27B may also help clarify the behavior of *gp*. The arrows show **GP** as being influenced by G as well as by $F_{\Delta t}$. This is because, like increasing Δt, raising the level of G so as to prolong **U** also causes *gp* to decrease (until it reaches zero). Thus, if *gp* is sufficiently large, an effect of G on **U** is hidden by the gap (or "absorbed into slack"), and is not expressed in RT''. On the other hand, if *gp* is zero (figure 14.27A), an effect of G on **U** is fully expressed in RT''. The effect of G on RT'' is therefore modulated by the level of $F_{\Delta t}$: factors $F_{\Delta t}$ and G interact. Furthermore, the nature of the interaction is *underadditivity* (the combination of the two effects is *less* than their sum): a change in $F_{\Delta t}$ that *increases* RT'' (a reduction in Δt that increases *gp*) will *reduce* the effect of G on RT''. Similarly, $F_{\Delta t}$ would interact in this way with a factor that influenced any stage before **V**, the stage subject to the bottleneck.

It follows from implications 2 and 3 that determination of which factors interact with $F_{\Delta t}$ and how they interact can not only test the deferred-processing model but can also inform us about the locus of a bottleneck within the stages of task 2, if there is one. A factor like G, which influences a stage that precedes the bottleneck, must interact (underadditively) with $F_{\Delta t}$ (an example is discussed below and shown by the effect of stimulus contrast in figure 14.31). On the other hand, the effect on $\overline{RT}''$ of a

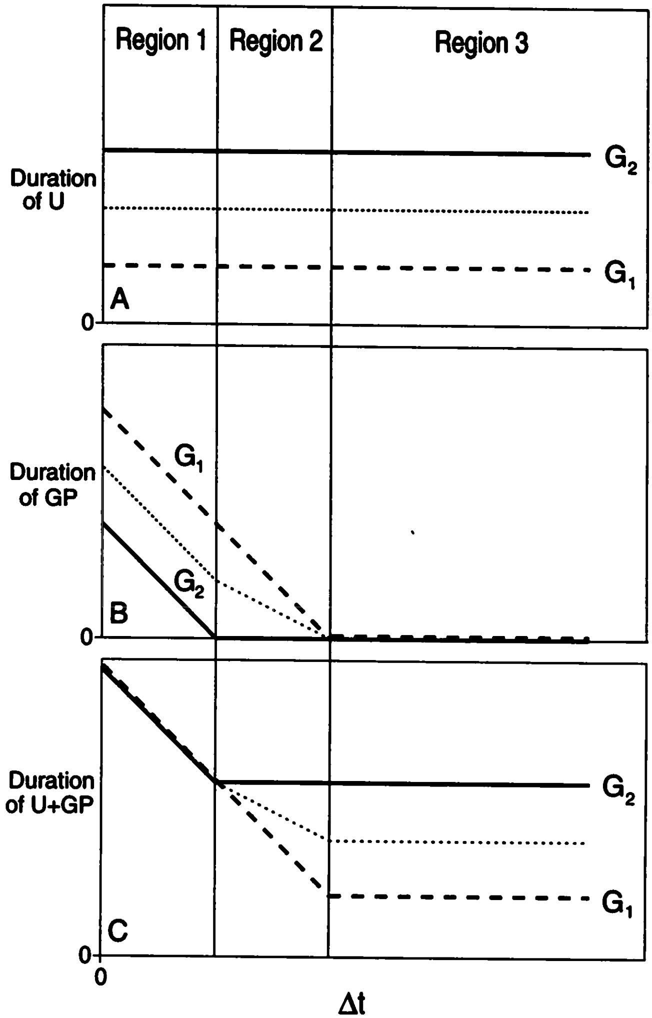


Figure 14.28
Durations of U (panel A) and GP (panel B), and their sum (panel C) as function of Δt, at two levels of G, according to deferred-processing model. Slopes of line segments assume that a time unit corresponds to equal distances on the x and y axes. Durations associated with low and high levels of G are represented by broken and solid lines, respectively. The dotted line represents their average, as described in comment 15.

factor like H, which influences a stage that is at the bottleneck or follows it, must be invariant over levels of $F_{\Delta t}$ (an example is discussed below and shown by the effect of mapping difficulty in figure 14.30). Because stages before the bottleneck must precede stages that follow the bottleneck, such an analysis also provides information about the ordering of stages, something we normally cannot learn from patterns of factor additivity and interaction without information or assumptions that go beyond the AFM itself. Implication 1 can be used in a similar way, to determine the locus among the stages of task 1 of a bottleneck, if there is one. If the effect of a factor on task 1 is propagated to task 2, the stage it influences in task 1 must precede or be at the bottleneck; if a factor's effect is not propagated, the stage it influences must follow the bottleneck.

Further implications. Consider the data we might obtain with $F_{\Delta t}$ at two levels, one with Δt small enough to produce a gap, and the other with no gap, and with G and H also each at two levels. If we let i, j, and k index the levels of $F_{\Delta t}$, G, and H, respectively, and use the notation introduced in section 14.3.2, equation 14.8 becomes

$$\overline{RT}''_{ijk} = u_{0j0} + gp_{ij0} + v_{00k} + w_{000}. \tag{14.10}$$

One implication is that $D_{ij}(\overline{RT}''_{ijk}) = D_{ij}(gp_{ij0}) \neq 0$. This interaction between $F_{\Delta t}$ and G is negative, with $D_{ij}(gp_{ij0}) < 0$, corresponding to the underadditivity explained above. Another implication is that the effects of G and H and of $F_{\Delta t}$ and H are additive, when averaged over levels of the respective third factor, and also at each individual level of that third factor. That is, we expect that $D_{ik}(\overline{RT}''_{i \cdot k}) = D_{jk}(\overline{RT}''_{\cdot jk}) = 0$, and also that $D_{ik}(\overline{RT}''_{ijk}) = 0$ for all values of j, and $D_{jk}(\overline{RT}''_{ijk}) = 0$ for all values of i. Finally, because none of the stage durations in equation 14.10 depends on the levels of all three factors, the three-way interaction is expected to be zero: $D_{ijk}(\overline{RT}''_{ijk}) = 0$. The deferred-processing model is thus remarkably rich in testable consequences; tests and elaborations of the model are being energetically pursued.

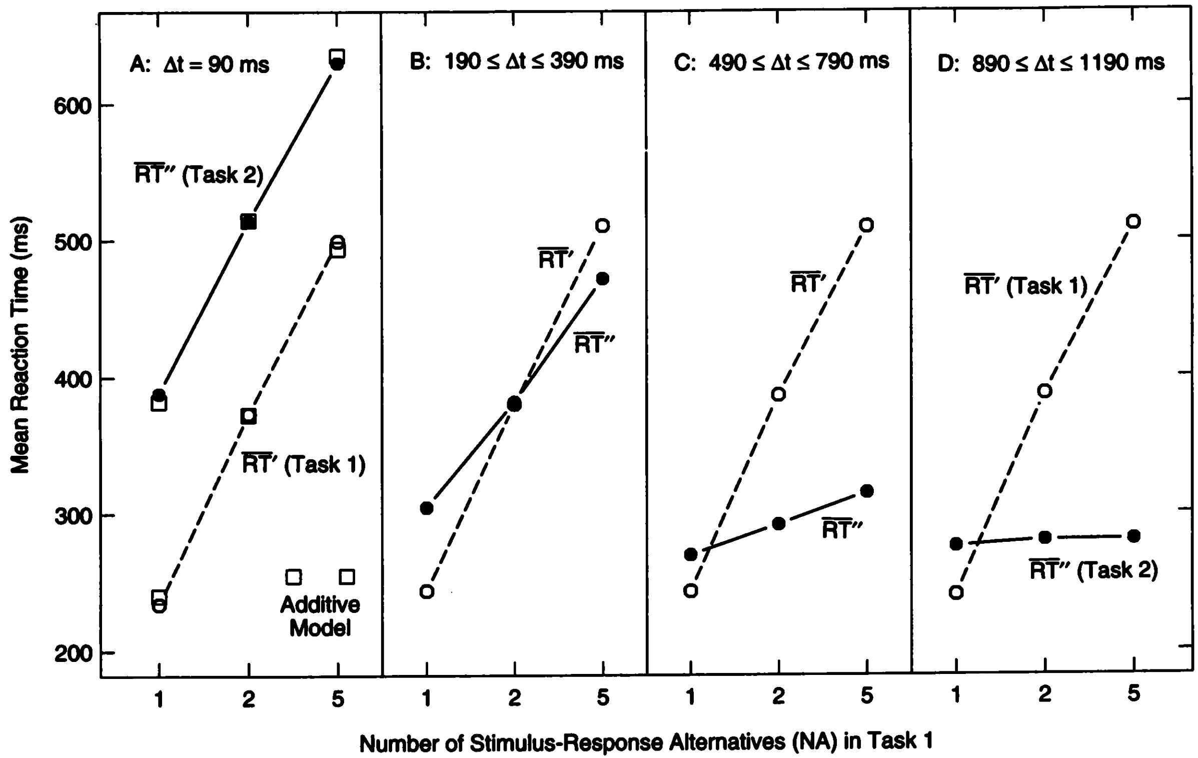
14.5.9.4 *Tests of Three Implications of the Model*

Test 1: Propagation of task 1 effects through the bottleneck. The first of three tests of the deferred-processing model is from a classic study by Karlin and Kestenbaum (1968), who varied a task 1 factor at twelve values of Δt ranging from 90 ms to 1190 ms, and observed the resulting effects on both task 1 and task 2. In task 1, the subject had to press a key with a left-hand finger in response to a visually presented digit. The factor was the number of digit-finger pairs, NA; in different blocks of trials NA was 1, 2, or 5. Task 2 was a two-choice pitch discrimination, involving two tones and two right-hand keys, one for "high" and one for "low." The data for four sets of values of Δt, averaged over the $\overline{RT}$s in each set, are shown in figure 14.29. The effect of NA on $\overline{RT}'$ (task 1) is substantial (about 270 ms from $NA = 1$ to $NA = 5$) and essentially independent of Δt. When Δt is long (panel D), $\overline{RT}''$ is about 280 ms, with virtually no effect of the task 1 factor. As the two tasks are brought together (moving to panels C and B) $\overline{RT}''$ increases dramatically, and the effect of the number, NA, of stimulus-response pairs in task 1 has an increasing effect on performance in task 2. When $\Delta t = 90$ ms, the shortest value in the experiment (panel A), $\overline{RT}''$ is about 140 ms longer than $\overline{RT}'$, but most important, the effect of NA on $\overline{RT}''$ is almost as great as its effect on $\overline{RT}'$. This can be seen by how well the additive model fits; considering the large size of the effects of both task and NA, the deviations are small (the mean absolute deviation is only 4 ms), though systematic.

Test 2: Additive effects on $\overline{RT}''$ of $F_{\Delta t}$ and classification difficulty. In an attempt to discover the locus of the bottleneck in a classification task, McCann and Johnston (1989) used stimuli in task 2 that varied in the difficulty with which they were mapped onto their responses. The stimuli S_2 were four outline rectangles of different widths, denoted as 1 (smallest), 2, 3, and 4 (largest). Widths 1 and 2 required a button press with the left hand, widths 3 and 4 with the right hand. Because they were further from the response criterion, extreme widths 1 and 4 elicited faster responses than moderate widths 2 and 3. Thus the moderate widths are associated with the "difficult" level of a *mapping difficulty* (MD) factor, the extreme widths with the "easy" level. McCann and Johnston's hypothesis was that MD is a factor like H in figure 14.27, influencing a stage at or after the bottleneck. Task 1 was a pitch-discrimination task: S_1 was a high or a low tone and R_1 was a spoken response, either "high" or "low."

$\overline{RT}''$s for easy and difficult mappings (factor MD) are shown as a function of Δt (factor $F_{\Delta t}$) in figure 14.30, along with the best-fitting additive model. The excellent fit of model to data tells us that the effect of MD is almost perfectly invariant (about 60 ms) over levels of $F_{\Delta t}$; the mean absolute deviation is 1.0 ms, and $D_{ik}(\overline{RT}''_{ik}) = (1, -3, 4)$ ms, a good approximation to (0, 0, 0). This is what we would expect from the bottleneck model if the stage influenced by the difference between stimulus size and response criterion was at or after the bottleneck (was either **V** or **W** in figure 14.27).

Note that the role of mapping difficulty, and hence the function of the stage or stages it influences, is ambiguous. For example, suppose there are separate width-encoding and response-selection processes. Which of them is influenced by moderate width (difficult mapping) versus extreme width (easy mapping)? The answer probably depends on details of what the width-encoding process does. If it produces an analog representation


A: Δt = 90 ms
B: 190 ≤ Δt ≤ 390 ms
C: 490 ≤ Δt ≤ 790 ms
D: 890 ≤ Δt ≤ 1190 ms
RT″ (Task 2)
RT′ (Task 1)
RT′
RT″
Additive Model
Mean Reaction Time (ms)
200
300
400
500
600
1
2
5
Number of Stimulus-Response Alternatives (NA) in Task 1

of the stimulus width, then the decision whether the response should correspond to "large" or "small," determined by a response-selection process, could be influenced by *MD*. But if the width-encoding process produces a categorization ("one of the large pair" or "one of the small pair"), then it is hard to see how it could matter to a subsequent response-selection process whether the stimulus was moderate or extreme within its category. This is a relatively obvious instance of a general problem: learning that a hypothetical stage is influenced by a particular factor (which in this case is distinguished by being at or after the bottleneck) provides only the beginning of an understanding of what function is performed during that stage. Within the context of the AFM, additional factors must be studied. Outside that context, more detailed models of the functionally distinct processes that have been discovered must be developed and tested.

Test 3: Underadditive effects on $\overline{RT}''$ *of* $F_{\Delta t}$ *and stimulus quality.* Pashler and Johnston (1989, experiment 1) varied the contrast of visually presented letters, which were either white (high contrast) or gray (low contrast) on a black background. Their hypothesis was that contrast (factor *SQ*) would influence an early stage such as **U** in figure 14.27, likely to precede the bottleneck. In task 1, subjects pressed a key with one of two left-hand fingers, depending on whether the pitch of a tone was high or low. In task 2, they identified a letter as A, B, or C, responding with one of three right-hand fingers; the letter followed the tone by $\Delta t = 50$, 100, or 400 ms. Also, in some trial blocks subjects performed task 2 alone; this condition can be regarded as equivalent to a very large Δt. In figure 14.31, the rightmost pair of points represents the $\overline{RT}''$ values for this condition and reveals a 53 ms effect of *SQ*. When task 1 is introduced and the tone-letter interval Δt shortened to 50 ms, the mean *SQ* effect shrinks to 7 ms. The existence of the interaction, and its direction (underadditive), are consistent with their hypothesis.

> *Comment 15: Cautionary remarks: Implications of the variability of stage durations.* One way in which the deterministic approximation (comment 14) fails is in predicting the shape of the function that relates

Figure 14.29
Propagation of effect in task 1 through bottleneck to task 2. Number of stimulus-response alternatives in task 1 (*NA*) has approximately the same effect on $\overline{RT}'$ (open circles connected by broken lines) regardless of the time interval Δt between stimuli for the two tasks. (Compare these data across the four panels.) For long Δt (panel D) *NA* has no effect on $\overline{RT}''$ (filled circles connected by solid lines). As Δt is shortened (moving to panels C, B, and finally A), $\overline{RT}''$ grows longer and reveals an increasing effect of *NA*. When $\Delta t = 90$ ms, the shortest value examined, the effect in task 2 ($\overline{RT}''$) is almost as great as in task 1 ($\overline{RT}'$). This is shown by the closeness of the data to the fitted additive model (represented in this figure by open squares). Data from Karlin and Kestenbaum 1968.

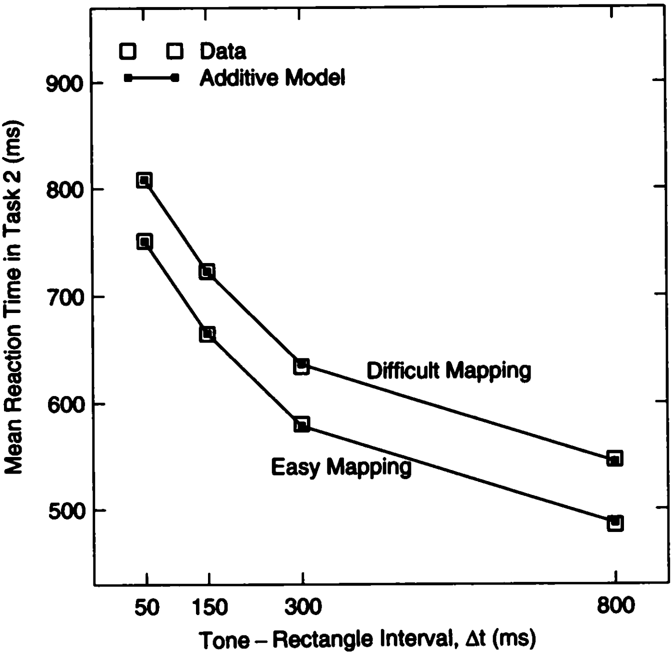


Figure 14.30
Mean reaction times in task 2 from overlapping-tasks experiment in which task 1 was discrimination of pitch of tone, and task 2 was classification of size of rectangle, achieved by either easy or difficult size mapping. $\overline{RT}''$ (open squares) for each level of mapping difficulty (*MD*) is plotted as a function of the interval Δt between tone and rectangle (factor $F_{\Delta t}$); as the interval shrinks (moving leftward in the figure) it can be seen to increase. The excellent fit of the additive model (filled squares connected by lines) shows that the effect of *MD* is invariant over levels of $F_{\Delta t}$. This finding as well as the increase in $\overline{RT}''$ as Δt shrinks would be expected from the deferred-processing model if *MD* influences a stage at or after the bottleneck, such as V or W in figure 14.27. The impression that the curves diverge is a visual illusion. Data from McCann and Johnston 1989.

the mean gap duration, *gp* to Δt (which determines the shape of the function that relates $\overline{RT}''$ to Δt). Equation 14.9 and figure 14.28 indicate that insofar as *gp* is at all responsive to Δt, a decrease of τ ms in the latter causes an increase of τ ms in the former: the plot of *gp* versus Δt has a slope of -1, until *gp* reaches zero. Variation in *a*, *b*, and *u* would have the effect on figure 14.28B of producing a set of vertically separated curves, like the two in the figure, as if the level of factor *G* was subject to variation. The mean gap duration for a given Δt would then be the average of the points on that set of curves. (To educate your intuition, imagine there are just two such curves—the two heavy curves in the figure—and that each applies

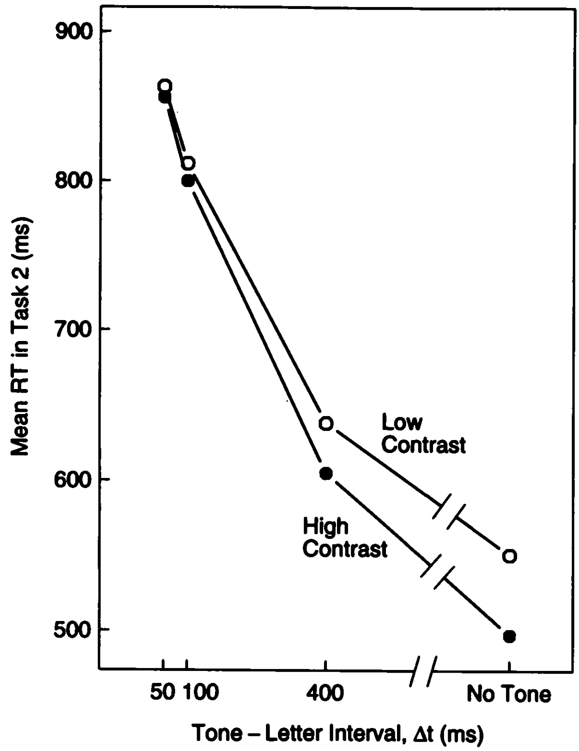


Figure 14.31
Mean reaction times in task 2 from overlapping-tasks experiment in which task 1 was discrimination of pitch of tone and task 2 was letter identification. $\overline{RT}''$ for high-contrast letters (filled circles) and low-contrast letters (open circles) is plotted for intervals Δt between tone and letter of 50, 100, and 400 ms, and for a condition in which no tone was presented (effectively a very large value of Δt). As Δt shrinks (moving leftward in the figure), $\overline{RT}''$ can be seen to increase, and the effect of contrast to decrease, an underadditive interaction of the contrast and $F_{\Delta t}$ factors. This pattern of data would be expected from the deferred-processing model if contrast influences a stage before the bottleneck, such as U in figure 14.27. Data from Pashler and Johnston 1989, experiment 1.

on 50 percent of the trials. The resulting *gp* would then be the average of the two curves, indicated by the dotted curve in the figure. In region 2 the slope is $-.5$ instead of -1.) Thus, whereas the slope for small Δt may be -1, we expect it to change gradually to zero as Δt increases. It may be such variability that causes the descending functions in figures 14.30 and 14.31 to be curved rather than linear, and to have slopes less extreme than -1. But without knowing the distributions of the relevant durations, we cannot make detailed quantitative predictions about such curves.

14.5.10 How Do Repetition and Familiarity Speed Word Recognition?

Much has been learned about how words are recognized by measuring the RT to name a word, or to classify it semantically (e.g., is it the name of a living thing?), or to decide whether a letter string is a word, that is, to make a *word/nonword decision* ("lexical decision"). On each trial in a typical word/nonword task, the subject sees a string of letters (e.g., STEP or STIP) and, consistent with accuracy, must press one of two buttons as quickly as possible, one for "word," the other for "nonword."[40]

One of the most surprising findings to have emerged from such research is the long duration of the aftereffect of having seen and responded to a word. As shown by Scarborough, Cortese, and Scarborough (1977), one consequence is a reduction in the $\overline{RT}$ for a subsequent word/nonword decision about the same word by as much as 100 ms. This facilitation effect ("repetition priming") declines by only 15 percent as more trials ("lags" from 0 to 31 trials) intervene between the first and second presentations; it even persists (albeit attenuated) when the second presentation occurs in a subsequent session as much as three days later.

The long persistence of the effect is especially remarkable when we consider what happens if instead of making a word/nonword decision, the subject makes an *old/new* decision ("old" if the word was presented previously, "new" otherwise). In this kind of experiment the aftereffect of a presentation declines rapidly (that is, there is rapid forgetting): As the lag increases from 0 to 31 trials, the error rate increases to about 25 percent, consistent with forgetting of half of the words. And for the correct responses, $\overline{RT}_{\mathrm{old}}$ slows by about 100 ms. The effect of repetition on the word/nonword decision may thus reflect a different kind of memory mechanism from that on which the old/new decision depends.

Scarborough, Cortese, and Scarborough (1977) wanted to know which of the processes that underlie the word/nonword decision are responsible for the remarkable repetition effect. The way they approached this question was to vary factors assumed to selectively influence each of a sequence of three hypothesized stages in the word/nonword task, along with presentation number, PN (first versus second), and to determine which, if any, of these factors interacts with PN. (The effect of PN—the reduction in $\overline{RT}$ from first to second presentation of a word or nonword—is the repetition effect.) The hypothesized stages (any of which might be susceptible to repetition effects) and the factors assumed to influence them selectively can be described as follows:

Stage A: Stimulus encoding
Factor: Same-case repetition versus other-case repetition (CR)
Words were printed in all uppercase or all lowercase. The idea behind using the case repetition factor CR was that if repetition facilitates the encoding process, then the effect should be reduced if the case is changed such that the second presentation is physically different from the first. This factor is defined just for $PN = PN_2$, of course.
Stage B: Search of lexical memory
Factor: Usage frequency in English (FRQ)
Usage frequency has a substantial effect on the $\overline{RT}$ for many tasks, including word/nonword decisions. Thus the $\overline{RT}$ difference between rare and frequent words (1 versus 1,000 occurrences per million) is as much as 150 ms, a large effect, especially given that the total $\overline{RT}$ for lexical decisions is only about 600 ms. It has to be kept in mind that naturally occurring variation in usage frequency is a complicated factor because orthographic, phonemic, and semantic features of words, some hard to control, are correlated with it.
Stage C: Decision and response
Factor: Proportion of trials calling for the "word" response (WB for word bias)
This proportion could be either .57 or .78, and was expected to affect decision and response biases for "word" versus "nonword." One possibility for the repetition effect is that as a result of the first presentation, the subject associates the word/nonword response with the stimulus, and that on the second presentation the decision process is then altered, as it now makes use of the stored association. Such a change in the basis of the decision is likely to interact with a change in decision bias.

Processes with these three functions are clearly necessary for performance in the word/nonword task. But this does not mean that they are organized as stages or that the factors listed have the appropriate selectivity of influence. In the spirit of the AFM, a full test requires demonstrating that each of the three two-way interactions among the three factors is zero, along with the three-way interaction (because of the definition of CR, such a test has to be restricted to the RTs for second presentations).

Scarborough, Cortese, and Scarborough (1977) evaluated these interactions, but because the main effect of *CR* and the effect of *FRQ* on second presentations were small, the tests were not sensitive. To some extent, they relied on indirect evidence to support the stages and selective-influence assumptions, for example, the finding in other experiments that the effects of stimulus quality and *FRQ* are additive in the word/nonword task. For decisions about words, they found that the effect of *FRQ* was considerably reduced by repetition (*PN* = 2); indeed, it was halved in one experiment and eliminated altogether in another (see figure 14.33B).[41] This argues that the *PN* effect is due at least in part to its influence on **B**. They also found that the effect of *WB*—26 ms—was additive with the effect of *PN*; this argues against **C** as a locus of the repetition effect (and, like any instance of additivity, favors the stages hypothesis). Finally, the effect of *PN* was slightly greater for same-case repetition than for different-case repetition; this argues that **A** is another locus of the repetition effect. Taken together, the findings suggest that, for words, repetition influences both the encoding stage (**A**) and the memory-search stage (**B**). One conjecture is that the presentation of a word has two effects. First, it produces an *episodic memory* on which remembering of the presentation is based. And second, it causes a change in the stored representation of that word in a *lexical memory*, which facilitates the search for that representation. (Indeed, the cumulative effects of such changes in lexical memory induced by repeated presentations may be an important component of the *FRQ* effect.)

That the repetition of a nonword facilitates the nonword decision requires us to elaborate this story. If nonwords have no representations in lexical memory, the mechanism for repetition priming of nonwords must differ from the mechanism for words. Consistent with this idea is a difference between words and nonwords in the degree to which the *PN* effect is modulated by lag. As shown in figure 14.32, the aftereffect of presenting a word is surprisingly persistent (the separation between the unbroken lines is reduced very little as lag increases). In contrast, for nonwords the aftereffect is relatively short-lived (the separation between the broken lines declines rapidly with lag).

To test further the idea that the repetition effect for words is due mainly to a search of lexical memory (stage **B**), Scarborough, Cortese, and Scarborough (1977) tried to devise a task that would be performed without this stage. They compared the vocal $\overline{RT}$s of subjects asked to *pronounce* the letter string to the (manual) $\overline{RT}$s for word/nonword decisions. By mixing words with (pronounceable) nonwords, and not requiring correct pronunciation of the words, they hoped to induce subjects not to use their lexical memories. Figure 14.33A shows, for nonwords, the joint effects of this *TASK* factor and *PN*. Invariance of the repetition effect

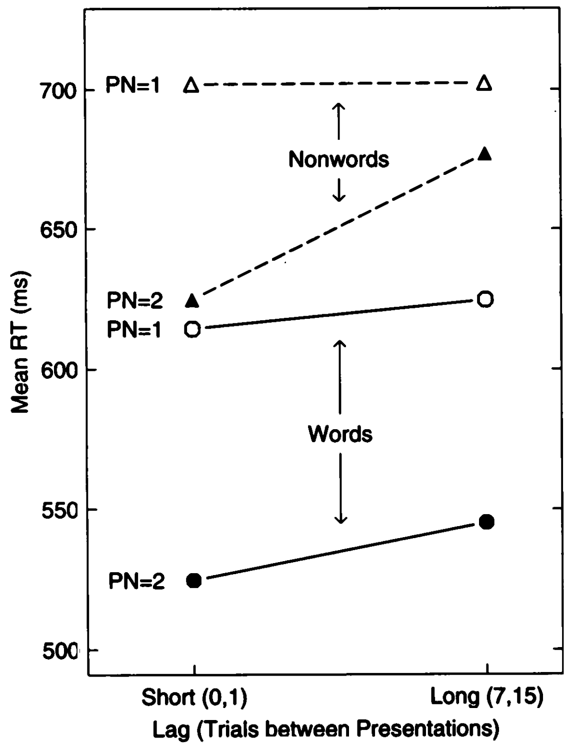
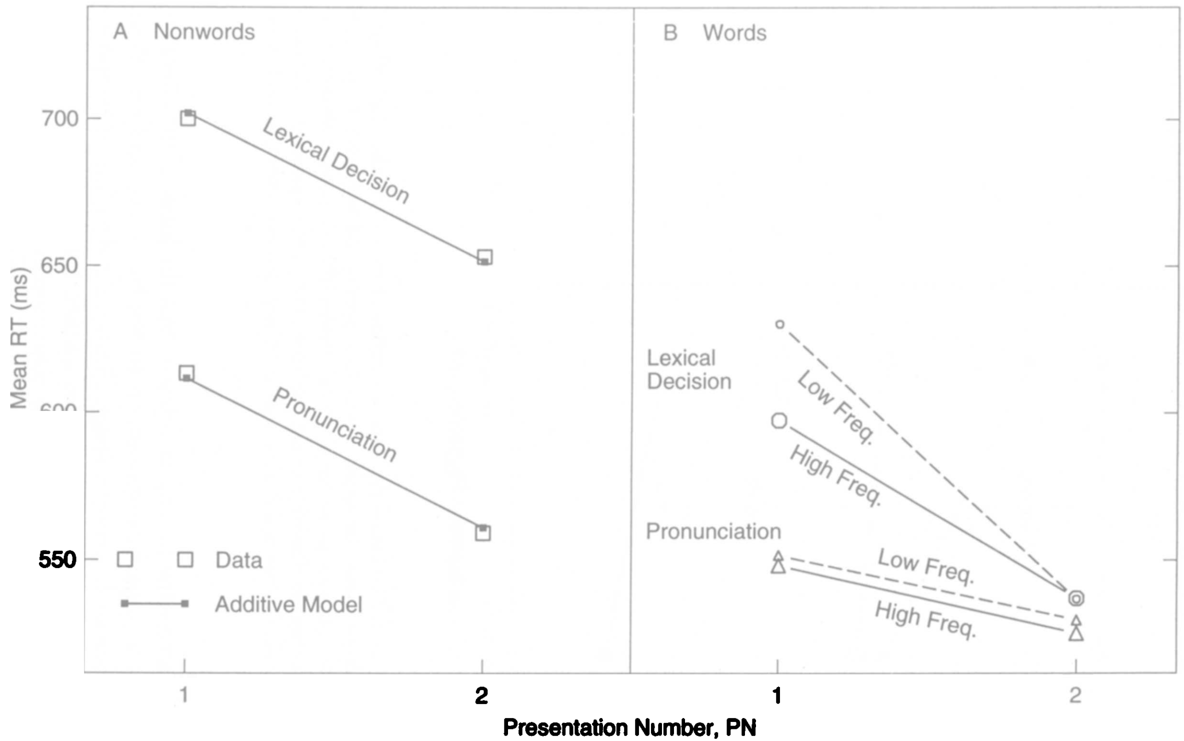


Figure 14.32
Effect of repetition on $\overline{RT}$ for lexical decisions as function of number of trials (lag) between first and second presentation, for words (lower four points) and nonwords (upper four points). Separate $\overline{RT}$ values are shown for first presentations ($PN = 1$, open symbols) for different lags, because they are derived from different trials. Data from experiment 1 (figures 1 and 2) of Scarborough, Cortese, and Scarborough 1977, averaged over uppercase and lowercase presentations.

for nonwords across the two tasks indicates that the locus of the effect for nonwords is in a stage that is common to the two tasks, and that there exists at least one such common stage. Clearly, the decision and response stages must differ between tasks. If we assume that the memory-search stage has indeed been eliminated in the pronunciation task (an assumption I comment on below), we are left with the encoding stage A as the common one, and with the conclusion that, for nonwords, A is the locus of the repetition effect. Together with the observation that the repetition effect for nonwords declines rapidly with intervening trials, we infer that whereas the effect of repetition on memory search is persistent, its effect on encoding is short-lived.


A Nonwords
B Words
Mean RT (ms)
700
650
600
550
Lexical Decision
Pronunciation
Data
Additive Model
Lexical
Decision
Low Freq.
High Freq.
Pronunciation
Low Freq.
High Freq.
1
2
1
2
Presentation Number, PN

For words, modulation by *TASK* of the effects of usage frequency (*FRQ*) and repetition (*PN*) is shown in figure 14.33B. In the pronunciation task the effect of usage frequency almost vanishes, evidence favoring the assumption mentioned above that the memory-search stage has been eliminated.[42] However, the repetition effect, although attenuated, is still present, indicating that part of the effect for words is due to the encoding stage. Given that the effect of repetition on encoding is short-lived, the substantial persistence of the combined effect for words tells us that for them the component due to the encoding stage cannot be large, consistent with the small size of the effect of repetition on the pronunciation of words.

14.5.11 Do Readers Recognize One Word at a Time?

One approach to answering this question would be to devise an experiment where, to respond correctly, the subject had to recognize all members of a set of simultaneously displayed words. Suppose $\overline{RT}$ increased linearly with the number of words in the set. It would then be tempting to conclude that the words were recognized one at a time, that is, sequentially. We should be skeptical of this conclusion, however. While such data would suggest that *some* component of the processing of words occurs sequentially, those data by themselves would not tell us which component. One possibility is indeed the recognition process—the process during which an internal representation of the stimulus makes contact with the stored representation of the corresponding word in lexical memory. But other possibilities include the sequential component being limited to the encoding process that forms the stimulus representation, for example, or to the process that combines the decisions about individual words once recognition has occurred, or prepares the response. Instead of manipulating a factor like the number of words, sharp inference requires us to use a factor to which the recognition process itself is known to be sensitive.

Figure 14.33
$\overline{RT}$s in lexical-decision and pronunciation tasks for nonwords (panel A) and words (panel B). Data are averaged over lag. For nonwords, panel A shows the observed effects of *PN* and *TASK* (open squares). An additive model (filled squares connected by lines) fits well, indicating that the effect of *PN* (repetition) is invariant over tasks, suggesting that it is mediated by a stage that is common to the two tasks. For words, panel B shows, in contrast to nonwords, that the repetition effect is attenuated in pronunciation relative to lexical decision, but still present. Panel B also shows that the frequency effect is virtually absent in pronunciation. Although the frequency effect in lexical decision has been found to decline with repetition in other studies, elimination of the effect after a single presentation is atypical, and should probably be attributed to sampling error. Data from Scarborough, Cortese, and Scarborough 1977, experiments 1 (lexical decision; data also shown in figure 14.32) and 3 (pronunciation).

Scarborough and Landauer (1981) realized that the study of repetition effects described in section 14.5.10 showed that repetition priming was a factor that met the requirements, because most of its effect on lexical decisions was associated with the recognition stage—the search of lexical memory. Also, the long persistence and large size of the effect of repetition priming on recognition helped lead to a feasible experimental design. On each test trial in their experiment a subject saw a pair of letter strings side by side, and had to decide whether or not both strings were words. One or the other or both strings might be pronounceable nonwords (like "feeb" or "govify"), calling for a "no" response; only if both strings were words was a "yes" response correct. The two strings contained the same number of letters and, when both were words, they were of the same form class (nouns or verbs), but were otherwise unrelated. Thus, given accurate performance, a "yes" response could be made only after the subject determined, for both strings, that they were words.

Before the series of test trials subjects had a series of priming trials on which they made lexical decisions about individual letter strings that might later be repeated in the test series. Among the series of test trials that followed, those in which both strings were words were of four types: the left-hand and right-hand words, w^L and w^R, could each be primed (P) or unprimed (U), yielding the patterns *UU*, *PU*, *UP*, and *PP*. We shall discuss only these "yes" trials.

It is helpful to define priming factors P_i^L and P_j^R associated with w^L and w^R, respectively, each with levels 1 (primed) and 2 (unprimed). The combinations are shown in table 14.6. Also shown is the variable n_{upr}, which represents the number of unprimed words (0, 1, or 2), that is, the number of the factors P^L and P^R that are at elevated levels.

Let $\mathbf{B^L}$ and $\mathbf{B^R}$, with durations b_{i0}^L and b_{0j}^R, be those processes associated with w^L and w^R, respectively, that are influenced by repetition priming. It is reasonable to believe that the two priming factors P_i^L and P_j^R have selective effects on the two sets of processes. That is, P_i^L influences $\mathbf{B^L}$ but not $\mathbf{B^R}$, and vice versa. Now consider the implications of the temporal organization of $\mathbf{B^L}$ and $\mathbf{B^R}$. First, suppose they can proceed in parallel.

Table 14.6
Factor levels for two-word test trials in the repetition-priming experiment of Scarborough and Landauer (1981).

Trial type	P_i^L	P_j^R	n_{upr}
PP	P_1^L	P_1^R	0
PU	P_1^L	P_2^R	1
UP	P_2^L	P_1^R	1
UU	P_2^L	P_2^R	2

The RT would then depend on the slower of these processes for the two strings. Priming just one of the two words would help relatively little, but priming both would have a big effect.

Comment 16: Implications of parallel word-recognition processes. How would the effects of P_i^L and P_j^R combine if the two recognition processes proceeded in parallel? This is most easily seen by starting with the simplifying assumption that the durations, b_{i0}^L and b_{0j}^R of the two processes take on fixed values b_1 if primed and b_2 if unprimed, with $b_1 < b_2$. Because initiation of the "yes" response requires that both letter strings be classified as words, it must await the completion of both processes. The contribution to $\overline{RT}$ would then be the slowest of the two durations: $\max\{b_2, b_2\} = b_2$ for $\overline{RT}_{UU}$, $\max\{b_1, b_2\} = b_2$ for $\overline{RT}_{PU}$, $\max\{b_2, b_1\} = b_2$ for $\overline{RT}_{UP}$, and $\max\{b_1, b_1\} = b_1$ for $\overline{RT}_{PP}$. However, it is reasonable to expect variation from word to word of both the process durations and the magnitudes of the effect of priming on those durations. One consequence, for example, is that, on average, $\max\{b_2, b_2\}$ is expected to be somewhat greater than $\max\{b_1, b_2\}$. The longest of two variable long durations is likely to be longer than the longest of a long and a short duration. (For related discussion see section 9.3.2.3.)

In contrast, suppose $\mathbf{B^L}$ and $\mathbf{B^R}$ are arranged as substages of a stage $\mathbf{B^*}$. Then their contribution to $\overline{RT}$ will be the sum of their mean durations, so that

$$\overline{RT}_{ij} = a_{00} + b_{i0}^L + b_{0j}^R. \tag{14.11}$$

Equation 14.11 reflects the additivity of the two priming factors P_i^L and P_j^R, but we can say more than this because of the *equivalence* of $\mathbf{B^L}$ and $\mathbf{B^R}$ and of the corresponding priming factors. Balancing of the test words in the experiment guaranteed that w^L and w^R were not systematically different in any way. This suggests that the effects of priming on the two words should be equal, on average: $D_i(b_{i0}^L) = D_j(b_{0j}^R) = \beta$. It follows from the effects being equal as well as additive that $\overline{RT}_{ij} = a_{00} + \beta n_{upr}$. Thus $\overline{RT}_{PU}$ and $\overline{RT}_{UP}$ should be equal and $\overline{RT}$ should increase linearly with n_{upr}.

Results from the word/word trials in Scarborough and Landauer's second experiment (1981), in which the size and spacing of the words approximated those encountered in normal reading, are shown in figure 14.34.[43] The linear function fits remarkably well, and $\overline{RT}_{PU} = \overline{RT}_{UP}$ is nicely approximated, confirming the seriality of word recognition under these conditions, as well as the substage-equivalence hypothesis. More specifically, the results argue for the seriality of $\mathbf{B^L}$ and $\mathbf{B^R}$; they do not speak to the temporal organization of any parts of the processing of the

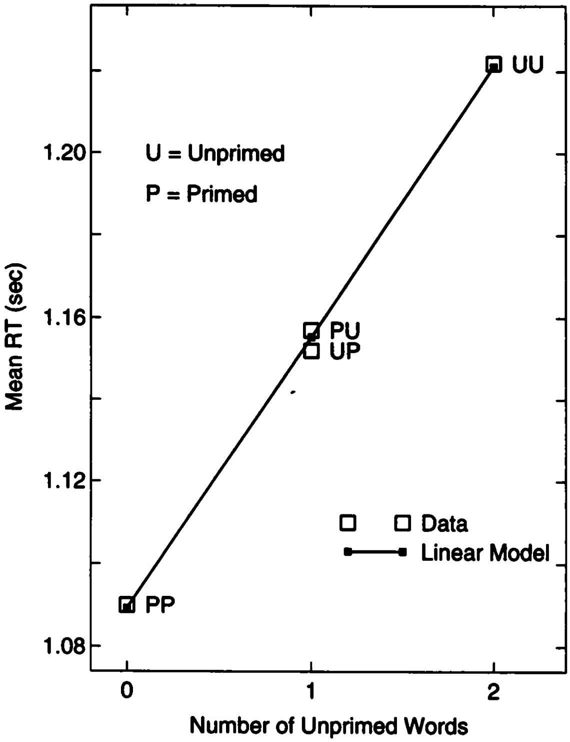


Figure 14.34
Results from "yes" trials in two-string lexical decision task in which subjects were to respond "yes" only if both strings were words. Some of the word and nonword strings had been primed by being presented in a previous one-string lexical decision task. $\overline{RT}$s (open squares) are shown as a function of the number of unprimed words in the stimulus pair. Also shown is a fitted linear function (filled squares connected by lines). This function expresses a model with two properties. First, the effect of priming the left-hand word is invariant over primed and unprimed right-hand words (and vice versa), that is, the effects of priming are additive. Second, the effects of priming the left-hand and right-hand words are equal. Data from Scarborough and Landauer 1981, experiment 2.

two words not influenced by priming. As Scarborough and Landauer (1981, 8) point out, however, it has to be recognized

> that repetition affects many important parts of the processes required to recognize a word. Repetition can remove most of the effects of word familiarity, which in turn can produce variations of over 100 ms [in lexical-decision *RT*s for single strings] and also affects visual encoding processes (Kolers 1975) that are not influenced by word frequency.... The additivity of repetition effects in our experiments is therefore indirect evidence that much, perhaps

all, of the encoding and retrieval processes for the two items of a pair occurs sequentially.

14.5.11.1 Equivalent Substages

The analyses of the present section and the two that follow depend on the idea of equivalent substages. It is plausible that people accomplish many tasks by employing sets of operations that are similar. For example, in some kinds of visual search you must test whether each displayed element is a target. Likewise, identification of a string of k characters or search of a memorized set of k items may be accomplished by a set of k similar operations, which carry out the same function and are influenced by similar factors. Given an experimental design in which the words, characters, or other elements are appropriately balanced or randomized, it is then reasonable to suppose such substages are equivalent, in the sense of having the same mean duration and being influenced to the same extent when the level of a factor that influences them (a "substage factor") is elevated; "equivalent" can then also be applied to the corresponding factors. In general, equivalence leads to linearity of the combined effect of the number of substage factors at elevated levels, a property that implies, but is not implied by, the additivity of factor effects.[44] In the example of the present section and the two that follow, the hypothesis of equivalence is plausible. Not only can the hypothesis be tested, but if it is valid the implications of these operations being arranged in stages are stronger than the implications of stage theory discussed thus far (because of added quantitative predictions) and increase the power of tests for the existence of stages. As we shall see, they also permit us to infer the stage-specific effects of a factor even when it influences more than one stage, which is normally not possible (see comment 7).

In the application of the present section, the number of substages is fixed (at two, $\mathbf{B}^L$ and $\mathbf{B}^R$). Because the substage factors P^L and P^R are varied in a standard factorial design (see table 14.6), this method of testing for the seriality of equivalent operations can be called the "factorial diagnostic." In the work described in the next two sections, all substage factors are set at either their low levels or their high levels, while the number of substages is varied. Because the durations of all substages are thus either short or long, this method of testing for seriality can be called the "homogeneous diagnostic."

14.5.12 Are Characters Encoded in Parallel, Sequentially, or Both?

An alternative approach for exploring the temporal organization of equivalent operations is to use the homogeneous diagnostic mentioned in section 14.5.11. Here we vary the number, k, of operations required, rather than fixing that number as in the example of that section, where it was $k = 2$. And we also vary the level of a factor believed to influence the durations of all of the k required operations. (To apply this diagnostic to word recognition, we would vary the number of displayed letter strings and arrange for either all of them or none of them to be primed.)

Consider, for example, a task used to study the temporal organization of the encoding of an array of digits by Pashler and Badgio (1985, experiments 3–5).[45] Subjects had to speak the name of the largest digit in an array of k random digits. This task has the virtue that, whereas an increasing number of digits must participate in encoding and decision processes as k increases, the complexity of the response remains the same. Let AS_k (array size) be the factor whose level is k. The most obvious measurement is of the effect of AS_k on $\overline{RT}$. The interpretive difficulty is that for a task of even modest complexity, we do not know which of the component operations might be responsible for such an effect. To name the largest digit, not only must the k digits each be encoded, but time is probably also consumed by magnitude extraction and comparison operations and, as the comparisons proceed, by storage and retrieval operations as well. Pashler and Badgio made the reasonable assumption that to perform in this task, all the displayed digits had to be encoded to the point of identification.[46] They wanted to decide between two simple alternatives: are the digits encoded in series or in parallel? They recognized that determining how $\overline{RT}$ grew with AS_k would be of little help; the effect of AS_k could indeed be due to a sequential encoding process, but it could also be due to other sequential operations that follow a parallel encoding process, for example.

> *Comment 17: Implications of linearity.* If $\overline{RT}$ were found to increase *linearly* with k, it would be tempting to argue that there is a set of operations carried out sequentially, with one for each digit in the array, and with each operation having a fixed mean duration, regardless of the number of other such operations. This is of course a description of the modern version of the subtraction method that has been applied in the studies of same-different judgments discussed in chapter 9. (See comment 12 in that chapter for an alternative explanation of linearity.) A linear effect is a special kind of invariant factor effect: the effect of an increment factor (adding one item) is invariant over levels of a size factor (the number of items present). For the task of naming the largest digit, however, it is a matter of controversy whether the effect of AS_k on $\overline{RT}$ is linear; in the data set to be presented below, $\overline{RT}$ increases in an accelerating fashion with k.

Inferential leverage is provided by another factor, used with AS_k in a factorial experiment, a factor that can be assumed to selectively influence

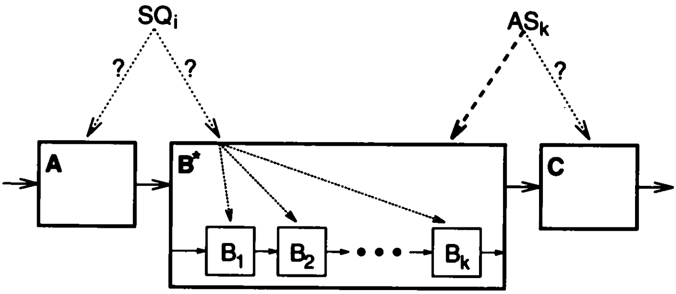


Figure 14.35
A model for encoding array of characters in which stage **B*** consists of sequence of equivalent substages, $\mathbf{B}_1, \mathbf{B}_2, \ldots, \mathbf{B}_k$, whose number is determined by array size AS_k, and whose mean durations (which are equal) may be influenced (equally) by stimulus quality (SQ_i).

the encoding process. Figure 14.35 should help make clear the implications of a sequential encoding process for the interaction of the two factors.

The figure shows that one of the encoding operations, stage **B***, is hypothesized to consist of k equivalent substages, $\mathbf{B}_1, \mathbf{B}_2, \ldots, \mathbf{B}_k$, one for each displayed digit, with the number of displayed digits given by the level of factor AS_k.[47] Factor SQ_i (stimulus quality) is a factor assumed to influence the encoding process (SQ_i might correspond to the level of visual contrast of the displayed digits, or to the presence or absence of a superimposed masking pattern, for example). Because all displayed items are treated homogeneously (unlike the words in the example of section 14.5.11), SQ_i is shown as influencing each of the k substages of **B***. The possibility that SQ_i may influence another stage, as well as **B*** (for example, a stage during which the displayed digits are processed in parallel), is indicated by its potential influence on **A**. And the possibility that AS_k may influence another stage as well as **B*** is indicated by its potential influence on **C**; for example, **C** might correspond to a process of magnitude comparison of the encoded digits, a process whose duration might increase nonlinearly with k. The model can be expressed by

$$\overline{RT}_{ik} = a_{i0} + b^*_{ik} + c_{0k}. \qquad (14.12)$$

The hypothesis of substage equivalence (section 14.5.11) asserts that the durations of the substages of **B*** are equal, on average, and that on average their durations are equally influenced by SQ_i.[48] Thus

$$b^*_{ik} = k\beta_{i0}, \qquad (14.13)$$

where β_{10} and β_{20} are the substage durations under the two levels of SQ_i. The subscripting of β expresses the assumption that AS_k influences the number of substages but not their durations.

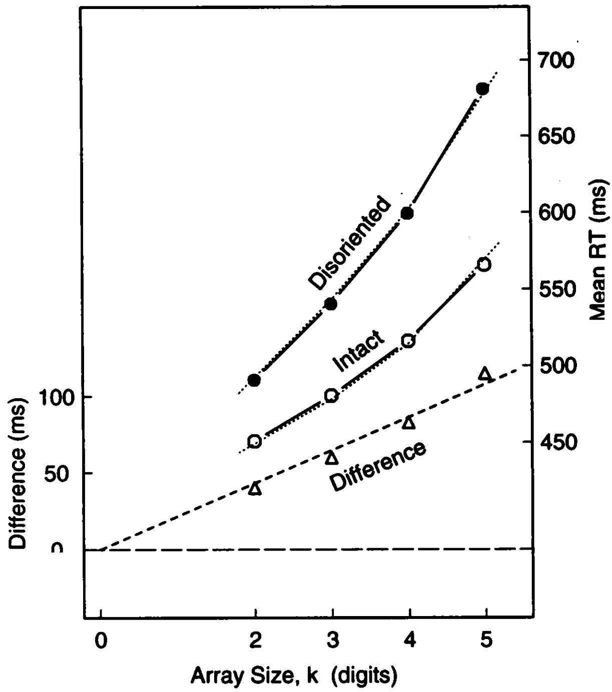


Figure 14.36
Mean RTs (right-hand ordinate, circles) for naming largest member of array of from 2 to 5 digits, for intact stimuli (open circles, $SQ = SQ_1$) and disoriented stimuli (filled circles, $SQ = SQ_2$). Also shown (left-hand ordinate, triangles) is the effect of stimulus quality, SQ, which is well approximated by a linear function passing through the origin: $D_i(\overline{RT}_{ik}) = 21.6k$ ms (broken line). The dotted curves have been fitted to the $\overline{RT}$s, subject to the constraint that their difference is specified by that fitted linear function. Data from Pantzer and Sternberg 1992.

Before considering the implications of equations 14.12 and 14.13, let us look briefly at the results of a relevant experiment by Pantzer and Sternberg (1992). Subjects had to name the largest digit in a row of $k = 2, 3, 4,$ or 5 digits. Factor SQ_i controlled digit legibility by determining the orientation of the digits, which could be either normal or disoriented by both reflection about the vertical axis and rotation through 90° in the plane. Results are shown in figure 14.36.

As can be seen in the figure, the effect of array size is nonlinear for both normal and disoriented rows, consistent with a nonlinear effect of AS_k on c. Also, the effect of disorientation grows with array size. How should it grow? Assuming two levels of SQ_i, and using the operator notation introduced in section 14.3.3, it follows from equations 14.12 and 14.13

that the effect of SQ_i on $\overline{RT}_{ik}$ (the difference between the functions for disoriented and intact digits) should be linear in k:

$$D_i(\overline{RT}_{ik}) = (a_{20} - a_{10}) + k(\beta_{20} - \beta_{10}) = \Delta a + k\Delta\beta, \tag{14.14}$$

and that the interaction of SQ_i ($i = 1, 2$) and AS_k ($k = 2, 3, 4, 5$) has a special form: it is a *multiplicative interaction*:

$$D_{ik}(\overline{RT}_{ik}) = (\Delta\beta,\ 2\Delta\beta,\ 3\Delta\beta). \tag{14.15}$$

If we think of $D_i(\overline{RT}_{ik})$ as a function of k, its slope is given by the mean effect of SQ_i on the substage duration, and its zero intercept is given by the effect of SQ on a_{i0}. If there is no effect of SQ_i on a (or if there is no stage A), then the zero intercept of the effect of SQ_i on $\overline{RT}$ should be zero, and $D_i(\overline{RT}_{ik})$ should fall on a line through the origin. Such a line (broken line in figure 14.36) has been fitted to the observed effect of disorientation, and fits well.

What is the source of the curvature in the two functions in figure 14.36 that relate $\overline{RT}$ to array size? We have assumed no effect of AS_k on stage A, and have concluded that it has a linear effect on stage **B***. The curvature must then be due to stage C. Because c_{0k} is not influenced by SQ_i, the curvature should be the same for both levels of SQ. For $SQ = SQ_1$, $\overline{RT}_{1k} = a_{10} + b^*_{1k} + c_{0k}$. For $SQ = SQ_2$, $\overline{RT}_{2k} = a_{20} + b^*_{2k} + c_{0k} = a_{20} + b^*_{1k} + k\Delta\beta + c_{0k}$. Like these two functions, the two dotted curves in the figure, which fit the data well, differ by only the multiplicative function $k\Delta\beta$ and thus have the same curvature.

In similar experiments, Pashler and Badgio (1985) used different methods to manipulate legibility (contrast reduction, superimposition of a masking pattern) and obtained different patterns of results, consistent with an effect on a but not on b. Thus there may be at least two stages associated with the encoding of a character, one parallel over characters and influenced by one kind of illegibility (exemplified by reduced contrast and masking), and the other sequential and influenced by another kind (exemplified by disorientation). For additional evidence see Pantzer 1997.

14.5.13 What Do We Search for When We Search Memory?

Suppose you look at a handwritten "3" and want to know whether it is a "5." At some point between presentation of the "3" and your decision, a representation of the stimulus pattern is compared to a memory representation of the target character. What is the nature of the encoded stimulus? How much analysis of the stimulus pattern is carried out in forming its representation?

Two clues suggest that the pattern may be processed to a considerable extent as its representation is formed. First, when engineers have tried to solve the character-recognition problem by machine the pattern is often normalized or subjected to image-filtering operations before being tested; other artificial recognizers carry such "preprocessing" further, incorporating an operation in which features are extracted from the pattern, features to be used in the subsequent test. Second, the neurophysiology of the visual system indicates that the information available at the higher visual centers is not simply a mapping of the retinal image: abstracted features are represented as well.

One way to address this question behaviorally would be to observe the effects of varying the legibility of the pattern. As a working hypothesis, suppose the part of the process that forms the stimulus representation takes the form of a separate *encoding* stage, **A**, with the process of comparing it to the memory representation together with other operations defining a second stage, **D**. Reducing the legibility of the pattern with a superimposed mask, for example, would be expected to slow the recognition response. If we could determine which stage, **A** or **D**, is influenced by such variation in legibility, this would help answer our question. If the representation formed by the encoding stage was highly processed, then we would expect the effect of the mask to be located primarily in that stage. On the other hand, if the encoding stage did relatively little, and produced a representation of something close to the physical stimulus, then the slowing due to the mask is likely to be localized in the stage that includes the comparison process.

To determine which stage is influenced by legibility, we could find two other factors different from the legibility manipulation, one selectively influencing **A** and the other selectively influencing the comparison process, and search for interactions of these factors with legibility. There are two problems associated with this approach, however. First, it might be difficult to find such factors. Second, while we could learn whether the legibility manipulation influences **A**, **D**, or both, if the answer were "both," we could say little about the stage-specific effects of legibility from the magnitudes of the interactions (see comment 7 about stage-specific factor effects in section 14.4.4.2).

If an alternative approach using equivalent substages (section 14.5.11.1) were available, it would eliminate both problems. First, we would not have to discover factors that selectively influence either **A** or **D**. And second, even if legibility influences both **A** and **D**, we would be able to estimate the stage-specific effects. This approach would work if

1. We could vary the *number* of comparison operations,
2. They are arranged sequentially and can be regarded as equivalent substages, and
3. We were justified in assuming that the encoding process is carried out only once, regardless of the number of comparisons.

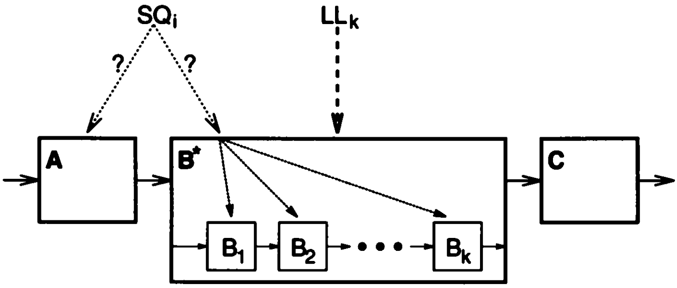


Figure 14.37
Model for searching memorized list of items in which memory-interrogation stage **B*** consists of sequence of equivalent substages, $\mathbf{B_1}$, $\mathbf{B_2}$, ..., $\mathbf{B_k}$. During each substage the representation of a list item is compared to a representation of the test item that was generated during stage **A**. The number of substages is determined by list length (LL_k); their mean durations (which are equal) may be influenced (equally) by stimulus quality (SQ_i).

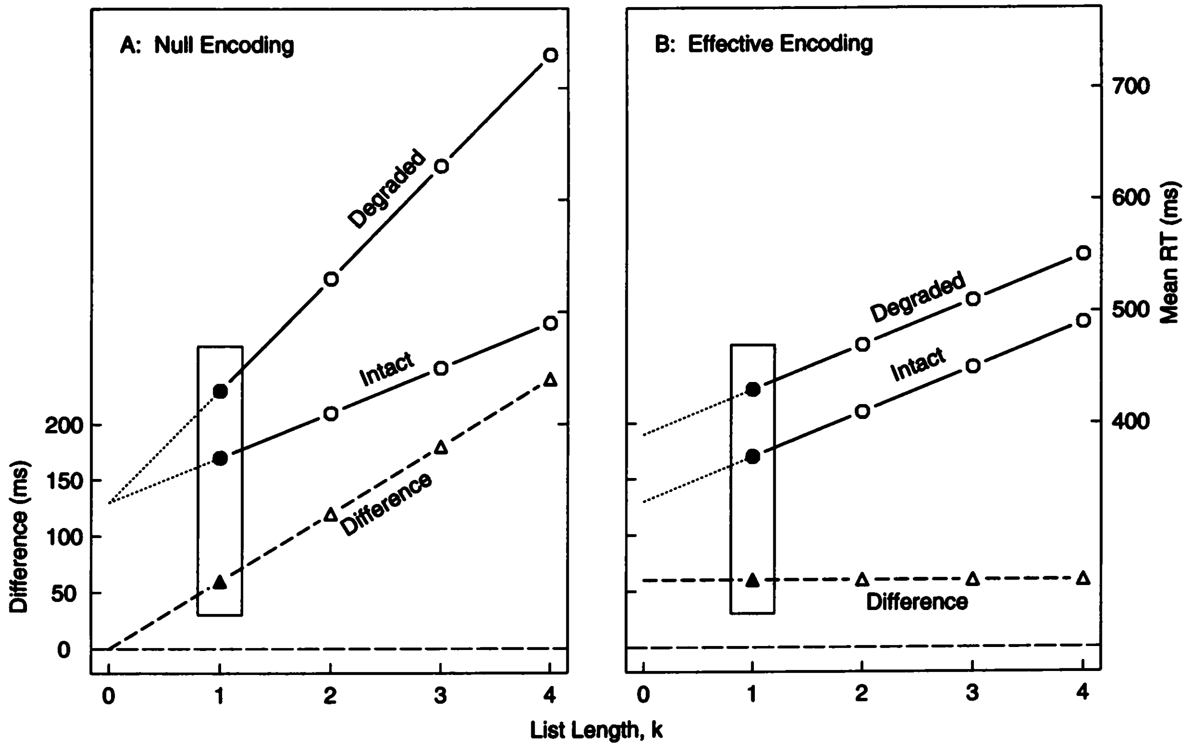
How might we vary the number of comparison operations? Possibly by varying the number of targets. The linearity of the effect of list length (figures 14.3 and 14.15) on $\overline{RT}$ in the item-recognition task invites us to infer a process in which a stimulus representation is compared sequentially with a representation of each item in the memorized list of target items.[49] Consider the model shown in figure 14.37. Here stage **D** has been divided into a sequential-comparison stage, **B***, and a final stage **C** during which the decision is made and the response is selected and executed. The final stage is assumed to be influenced neither by list length (LL_k), which determines the number of comparisons, nor by stimulus quality (SQ_i) (note the contrast with the analysis in section 14.5.12 and figure 14.35, in which stage **C** is influenced by AS_k). Stage **B*** in turn contains a sequence of one or more comparison substages, $\mathbf{B_1}$, $\mathbf{B_2}$, ..., $\mathbf{B_k}$. We have an answer to our question if we can separately evaluate the effects of SQ on stages **A** and **B***. Let the factors be list length, $LL_k = k$, which determines the number of comparisons, and stimulus quality, SQ_i. From the equivalent-substages assumption we have $b^*_{ik} = k\beta_{i0}$, and $\overline{RT}_{ik}$ can be expressed as

$$\overline{RT}_{ik} = a_{i0} + b^*_{ik} + c_{00} = a_{i0} + k\beta_{i0} + c_{00}, \qquad (14.16)$$

a linear function of k. The effect of stimulus quality (assuming two levels) is then

$$D_i(\overline{RT}_{ik}) = (a_{20} - a_{10}) + k(\beta_{20} - \beta_{10}) = \Delta a + k\Delta\beta, \qquad (14.17)$$

also linear in k. The slope of this function is the mean effect of SQ on the duration of one comparison substage, and its zero intercept is the mean effect of SQ on the encoding stage. (More generally, Δa is the effect of SQ



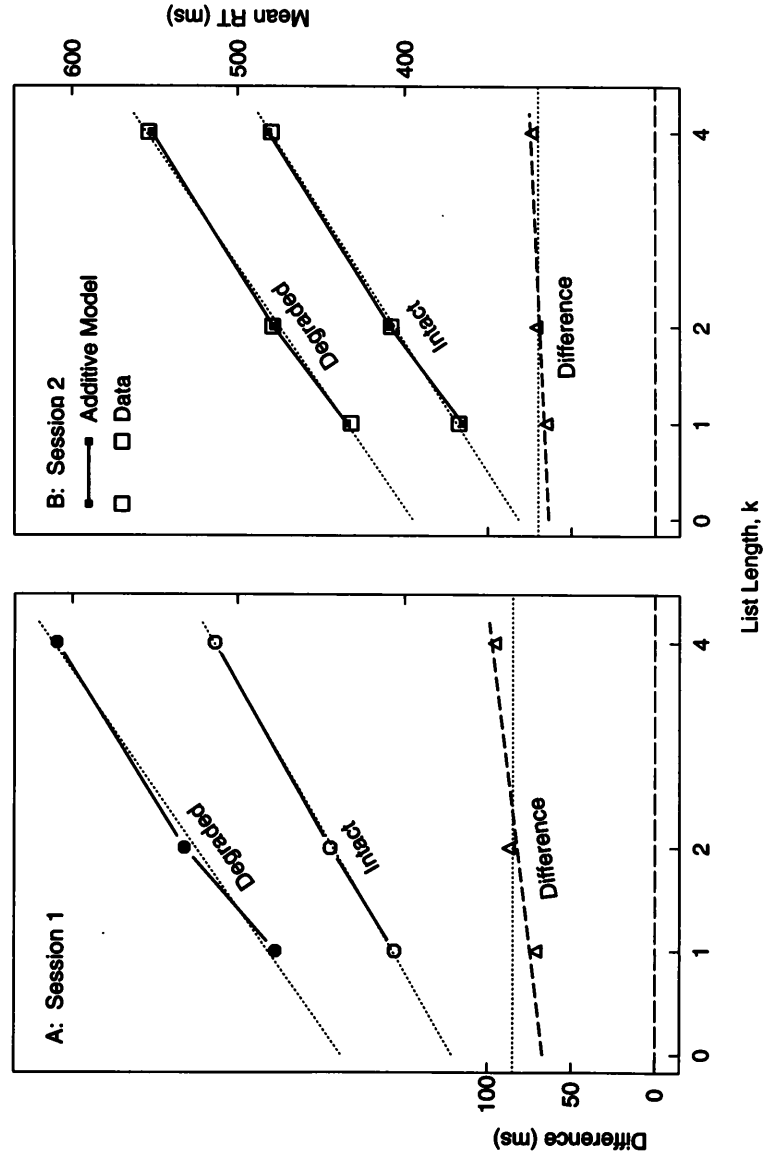
A: Null Encoding
B: Effective Encoding
Degraded
Intact
Difference
Degraded
Intact
Difference
Difference (ms)
0
50
100
150
200
Mean RT (ms)
400
500
600
700
0
1
2
3
4
0
1
2
3
4
List Length, k

on operations that occur just once, while $\Delta\beta$ is its effect on operations that occur k times.) Thus, if the model is supported (and one potential source of support would be linearity of the relation between $\overline{RT}_{ik}$ and k), then even if SQ influences both encoding and comparison durations, we can estimate its stage-specific effects.

Consider the implications for $D_i(\overline{RT}_{ik})$ of two extreme possibilities for the encoding process. Suppose, first, that the encoding process does nothing (*null encoding*), so that a raw image of the test stimulus is used during stage **B**. Then $\Delta\alpha = 0$ and the effect of degrading the test stimulus is due entirely to an increase in the duration of the comparison process, which generates for $D_i(\overline{RT}_{ik})$ the multiplicative function $k\Delta\beta$. Factors SQ and LL interact, and their interaction (expressed by equation 14.15) is multiplicative. In the idealized data of figure 14.38A the filled points (boxed) represent a 60 ms effect of degradation in the simple experiment with a single target, which could be due to prolongation of either stage **A** or stage **B**. The remaining points in panel A show idealized results of elaborating the simple one-target experiment by varying LL, if the effect is due entirely to **B**, the comparison process. That the fitted lines have the same zero intercept, and thus intersect at $k = 0$, corresponds to $\Delta\alpha = 0$.

Suppose, second, that the effect of the mask on the stimulus is completely removed during the encoding stage (*effective encoding*), so that the stimulus representation is the same, whether or not a mask is presented. The comparison process is then protected from any effect of visual degradation of the test stimulus. In this case, $\Delta\beta = 0$ and the entire effect of any degradation must be on the encoding process, which generates for $D_i(\overline{RT}_{ik})$ the effect $\Delta\alpha$, invariant over levels of LL_k. That is, factors SQ and LL are additive, as illustrated by the idealized data in figure 14.38B, where the filled points for the one-target experiment are identical to those in panel A. The constancy of the difference between the two increasing $\overline{RT}$ functions as the number of comparisons increases reflects the absence of any effect of SQ on a comparison operation.

Figure 14.38
Idealized memory-search data, given two extreme possibilities (panels A and B) for encoding process. The equivalent-substages assumption implies that $\overline{RT}$ (right-hand ordinate) will increase linearly with list length in both cases. Also shown as a function of list length is the effect, $D_i(\overline{RT}_{ik})$, of stimulus quality on $\overline{RT}$ (left-hand ordinate), the difference between the functions for degraded and intact test items. Panel A shows the expected data pattern, given *null encoding*, where an unprocessed image of the test item is used during stage **B**, and where the full effect of SQ is on that stage. The effect of SQ (labeled "Difference") increases linearly with list length, according to a function that passes through the origin. Panel B shows the expected data pattern, given *effective encoding*, where the stimulus representation used during stage **B** has no residue of any degradation, and where the full effect of SQ (which is independent of list length) is on stage **A**. As shown by the boxed data in the two panels, limiting the experiment to a single list length does not permit the two possibilities to be distinguished.


A: Session 1
B: Session 2
Additive Model
Data
Degraded
Intact
Difference
Mean RT (ms)
600
500
400
Difference (ms)
100
50
0
List Length, k
0
1
2
4

Thus, because list length determines the number of targets and hence the number of sequential comparisons, variation of list length can be used to create varying numbers of equivalent substages; we can thus discover either that the *SQ* effect with just one target is localized in the encoding stage (A) or that it is localized in the comparison stage ($\mathbf{B}_1$). We can do so not by assessing additivity or interaction of *SQ* with factors that influence those stages selectively (the first approach mentioned) but by finding instead one factor (here, *LL*) that influences the number of comparison stages. If the situation is more complicated than either null or effective encoding, and *SQ* influences *both* the encoding and comparison operations, then the equivalent-substages approach not only replaces the first approach but improves upon it. Both approaches allow us to discover that *SQ* influences both **A** and $\mathbf{B}_1$, but only the former enables us to estimate the magnitudes of the two effects. That is, elaborating the simple one-target (list length $LL_k = 1$; boxed data) experiment by adding one or more conditions with multiple targets (lists of length $LL_k > 1$) enables us to disentangle effects that are otherwise conflated, as shown by the unboxed idealized data in figure 14.38.

Results of a relevant item-recognition experiment (described as "experiment 1" in section 14.5.4) are shown in figure 14.39. During half the trial blocks in that experiment, all the test stimuli (digits) were degraded by a superimposed checkerboard pattern, the same one shown in figure 14.2. In figure 14.39 are shown mean *RT*s for intact and degraded test stimuli for each of the two sessions (the data from trial blocks with intact test stimuli are those also plotted in figure 14.15). The approximation of the four functions to linearity is good enough to be taken as supporting the equivalent-substages assumption and the model described by equation 14.16. The critical data are the difference functions, plotted at the bottom of each panel, which correspond to $D_i(\overline{RT}_{ik})$.

Figure 14.39
Results from two sessions of an item-recognition experiment. For each session $\overline{RT}$ averaged over positive and negative responses (right-hand ordinate) is plotted as a function of list length for intact and degraded test items. Also shown is the effect of *SQ* (left-hand ordinate, triangles) and a linear function fitted to that effect (broken line). Panel A shows that in session 1 the functions for degraded and intact test items (dotted lines) differ in slope as well as intercept. The equations are $\overline{RT} = 371.7 + 35.6k$ (intact) and $\overline{RT} = 438.8 + 43.2k$ (degraded); the broken line fitted to the difference (effect of *SQ*) is $D_i(\overline{RT}_{ik}) = 67.1 + 7.6k$. Panel B shows that in session 2 the observed functions (open squares) for degraded and intact test items differ in intercept, but do not differ reliably in slope; they are well described by an additive model (filled squares connected by solid lines) in which the effect of *SQ* is invariant over levels of *LL*. The equations of the linear functions fitted to the data (dotted lines) are $\overline{RT} = 331.7 + 37.2k$ (intact) and $\overline{RT} = 395.4 + 39.9k$ (degraded); the broken line fitted to the difference (effect of *SQ*) is $D_i(\overline{RT}_{ik}) = 63.7 + 2.7k$. Data from Sternberg 1967.

Let us begin by considering the data in panel B, from the second session, collected after an average of 315 trials with the same fixed degradation pattern. As can be seen, a model in which *SQ* and *LL* have additive effects fits well, with a mean absolute deviation of only 1.6 ms. This demonstration of separate modifiability supports the existence of distinct stages for encoding and memory interrogation. The effect of *SQ* on the zero intercept (an estimate of its effect on the duration of stage **A**) is $\Delta a = 64$ ms. On the other hand, its effect on the slope (an estimate of its effect on the duration of each substage) is an unreliable $\Delta\beta = 3$ ms, which we can take to be zero. That is, the results are very close to the effective-encoding pattern of figure 14.38B.

During the first session, neither of the predictions from the two extreme possibilities is satisfied. While there is a 67 ms effect on the intercept, indicating that the degradation considerably prolongs the encoding stage, there is also an 8 ms effect on each comparison substage. (This may seem like a small effect, but it represents a 22 percent increase in the duration of the substage, when the latter is estimated by the slope of $\overline{RT}_{\text{intact}}$.) If we interpret this straightforwardly, the representation produced by the encoding process must be highly processed, eliminating most but not all of the effect of visual degradation. However, it is still visual (rather than an identity or a name), because only a visual representation can incorporate an effect of visual degradation. That the observed $D_i(\overline{RT}_{ik})$ is well approximated by a linearly increasing function (as in equation 14.17) adds support to the model with *k* equivalent substages.

How should we interpret the difference in the pattern of data for the two sessions? There are at least three possibilities, none of which is entirely satisfactory. One possibility is that with practice there is a basic change in the structure of the process. However, the slopes of the functions relating $\overline{RT}$ to list length for intact stimuli in the two sessions are virtually identical—35.6 and 37.2 ms per digit for sessions 1 and 2, respectively. It therefore seems unlikely that there was a change, with practice, from a visual to a nonvisual representation. Also, this similarity, along with parsimony, argues against **A** and **B** being separate stages in the second session but not in the first.

A second possibility is to analyze the experiment in terms of a stable processing structure, but a third factor in addition to *SQ* and *LL*, namely practice (*PR*) with two levels, corresponding to sessions 1 and 2. Which stages are influenced by *PR*? Because Δa is virtually the same from one session to the next (67 and 64 ms, respectively), it can be argued that *PR* has no effect on **A**. Because *PR* modulates the effect of *SQ* on the $\{\mathbf{B_k}\}$ (that is, it modulates the interaction of *SQ* and *LL*), it must influence $\mathbf{B^*}$. Because *PR* also has an effect (of 42 ms) on the zero intercept averaged over *SQ* levels, an effect that cannot be produced by its influence on $\mathbf{B^*}$, it must also influence stage C. This analysis indicates (surprisingly) that instead of permitting the encoding stage to produce a representation of a degraded stimulus that contains less residual degradation, practice causes the comparison process to be less sensitive to the residual degradation that remains. If *PR* influences stage **B***, however, then we would normally expect its influence to show itself at all levels of other factors; the difficulty is that as mentioned above, we find a $PR \times LL$ interaction only for degraded stimuli ($SQ = SQ_2$).

A third possibility is to depart from equation 14.16 and explain the $SQ \times LL$ interaction in session 1 by an influence of *LL* on the encoding process **A** (as well as on the comparison process **B***), instead of an influence of *SQ* on **B***. (It is conceivable that by consuming some limited mental resource, maintaining the list in memory competes with the encoding process, and that the effect is greater with a longer list.) The effect of practice could then be explained simply by its influence on stage **A**. However, where equation 14.16 specifies the forms of both the effect of *LL*, $D_k(\overline{RT}_{ik})$, and the $SQ \times LL$ interaction, $D_{ik}(\overline{RT}_{ik})$—as linear and multiplicative, respectively—the present alternative does not. To the degree we are confident the interaction is multiplicative (shown by the linearity of the difference functions in figure 14.39) and the effect of *LL* is indeed linear, the model expressed by equation 14.16 would therefore be favored. (A model that predicts one or more quantitative findings is favored over an alternative model that merely accommodates them.) It would be useful to have more data addressing this issue, covering a larger set of list lengths.

14.5.14 How Do We Benefit from Seeing Ahead When We Search a Display?

For more than a century, since the time of Cattell, it has been known that people can identify a series of items more rapidly when they can see ahead. Cattell (1885) allowed subjects naming a series of letters to see one or more new letters before completing the response to the preceding one. The letters were printed on the outside of a cylinder; subjects viewed them one after another through a window while the cylinder revolved at an adjustable rate. Cattell found that the maximum rate that permitted accurate naming increased with the number of letters that could be seen through the window—that is, with the amount of *preview*. Like Cattell's naming experiment, visual search for a complex target is an example of a continuous serial task: rather than waiting for the experimenter to present the next item, subjects can start deciding about it as soon as they are able.[50] In his doctoral dissertation, Chase (1969) showed that preview also speeds visual search. Chase manipulated the size of a window that the subject moved over the row of items being searched.

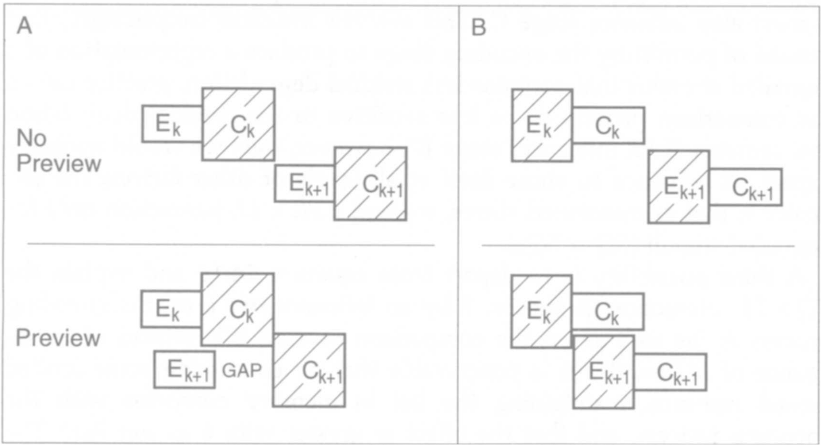


Figure 14.40
Two possible mechanisms for effects of preview on processing of stimuli S_k and S_{k+1}, successively encountered in an array being searched. As in figure 14.27, horizontal arrows are omitted to clarify time relations, and boxes representing processes that cannot overlap are drawn taller than the others. In panel A, comparison stages for different items do not overlap. Top: No preview ($PV = PV_2$). Bottom: Preview ($PV = PV_1$) permits $\mathbf{E}_{k+1}$ to overlap with $\mathbf{E}_k$ and $\mathbf{C}_k$, reducing the effect of *SQ* on total time. In panel B, encoding stages for different stimuli do not overlap. Top: No preview ($PV = PV_2$). Bottom: Preview ($PV = PV_1$) permits $\mathbf{C}_k$ to overlap with $\mathbf{E}_{k+1}$ and $\mathbf{C}_{k+1}$, reducing the effect of *NT* on total time.

Why should preview have this effect? The manipulation is inherently crude and complicated: at a fixed rate the increase in window size also increases the total time for which each item is displayed, and as we do not know where the subject's eyes are pointed, even "preview" may be a misnomer (is "postview" necessarily less apt?). The most interesting hypothesis is that at least some of the mental operations associated with different items can occur in parallel, and that preview permits them to do so. Which such operations occur in parallel, and which do not? Figure 14.40 indicates how patterns of factor effects can help to answer such questions. The logic has some similarities to the reasoning used in thinking about overlapping tasks, as in section 14.5.9. Just as we can ask how the performance of one task slows the performance of another with which it overlaps, so we can ask how preview speeds the decisions about successive items in visual search.

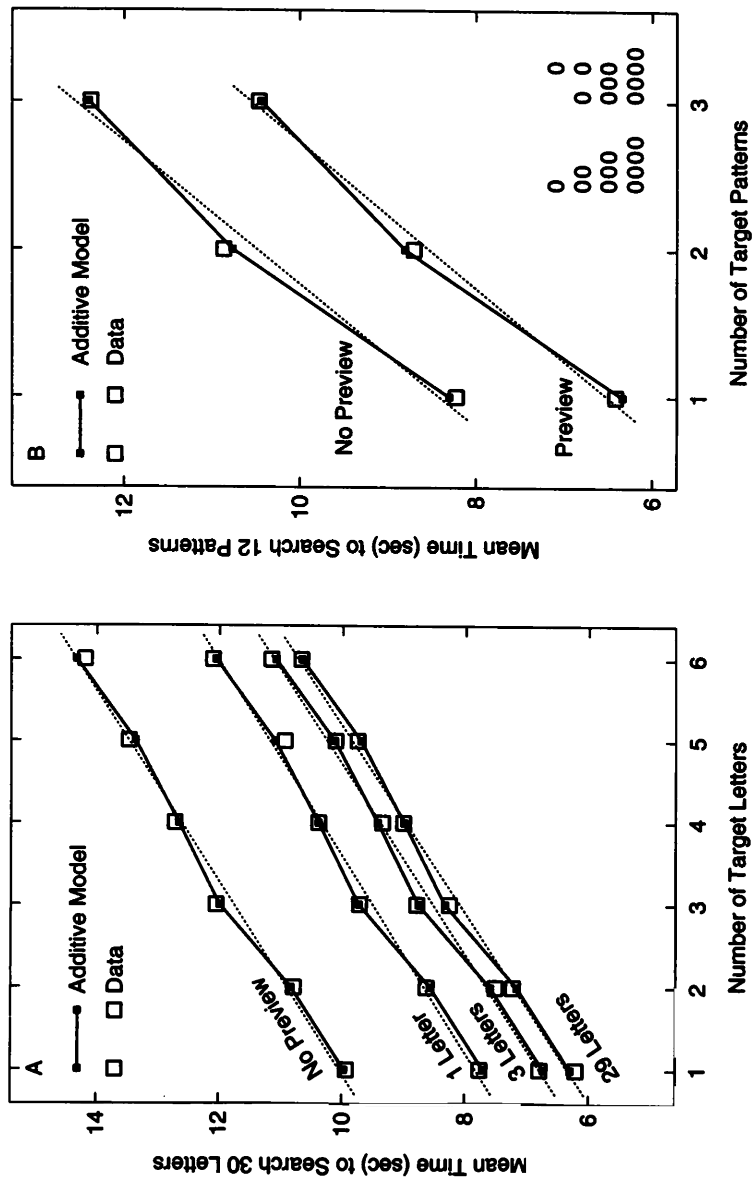
In the analysis of overlapping tasks we considered the *RT* for making a decision based on one stimulus. The measure in Chase's experiments was, in contrast, the total time to search a row of many items.

Comment 18: Reaction time, working rate, and "pipelining." It is tempting to consider the average increment in this total due to an added item—the *working rate*—as equivalent to the average time to process one item, or an average reaction time, but this would be a mistake. Even if the operations performed on any one item are sequential, the processing of multiple items may overlap in time, as in pipelining (section 14.4.3). It follows that the time during a continuous task from the start of processing an item to the decision about it may be substantially greater than the working rate. As an analogy, consider the time from one car to the next exiting an assembly line, or the working rate of the line, versus the time for one car to be assembled, or the time it takes for the line to "process" one car.

Suppose that to process each stimulus, two stages are required, one (**E**) for encoding the stimulus, and another (**C**) for comparing the encoded stimulus to the search targets. Suppose, further, that two factors are varied, stimulus quality (*SQ*) and number of targets (*NT*), presumed to selectively influence **E** and **C**, respectively. Among the two-stage processes that would permit an effect of preview, two are shown in figure 14.40.

One possibility, shown in panel A, is that if preview permits, stages $\mathbf{E}_k$ and $\mathbf{E}_{k+1}$ for different stimuli overlap in time, but stages $\mathbf{C}_k$, $\mathbf{C}_{k+1}$ do not. We assume, as a simple starting hypothesis, that *without* preview (top), stage $\mathbf{E}_{k+1}$ does not begin until stage $\mathbf{C}_k$ is complete, perhaps because the subject does not uncover S_{k+1} until then. *With* preview (bottom), stages $\mathbf{E}_k$ and $\mathbf{E}_{k+1}$ overlap partially or fully. Consider the effects of factors *SQ* and *NT* on the total time, with and without preview. Because the **C** stages do not overlap in either case, the total time must include the full durations of all the **C** stages, with or without preview. The effect of factor *NT* on the total time is therefore unchanged by preview. In contrast, because the **E** stages do overlap with preview, it is only when there is no preview that the total time includes the full durations of all these stages. When preview is provided, the effect of increasing the level of *SQ* on the duration of the **E** stages is partially or fully hidden, in two ways. The prolonging of $\mathbf{E}_{k+1}$, for example, is partially hidden, first, by its overlapping with the prolonged $\mathbf{E}_k$, and second (if $\mathbf{C}_k$ and $\mathbf{E}_{k+1}$ overlap), by the gap created as $\mathbf{E}_{k+1}$ advances to overlap with $\mathbf{E}_k$. As a result, the effect of *SQ* on the total time is reduced by preview. Like $F_{\Delta t}$ in the overlapping-tasks paradigm (section 14.5.9), the preview factor *PV* should therefore be additive with a factor like *NT*, but should interact with a factor like *SQ*.

According to the second possibility, shown in panel B, stages $\mathbf{C}_k$ and $\mathbf{C}_{k+1}$ for different stimuli overlap, as do stages $\mathbf{C}_k$ and $\mathbf{E}_{k+1}$, preview permitting, but stages $\mathbf{E}_k$, $\mathbf{E}_{k+1}$ do not. A similar argument shows that preview should reduce the effect of factor *NT* on the total time, but not the effect of factor *SQ*.


B
Additive Model
Data
No Preview
Preview
Mean Time (sec) to Search 12 Patterns
Number of Target Patterns
A
Additive Model
Data
No Preview
1 Letter
3 Letters
29 Letters
Mean Time (sec) to Search 30 Letters
Number of Target Letters

In general, if stages associated with different stimuli overlap when preview permits, then factors that influence those stages should interact with *PV*, and in such a way that their effects are increased when preview is removed (an "overadditive" interaction). On the other hand, if stages associated with different stimuli do not overlap even with preview, then effects of factors that influence them should be invariant over levels of *PV*. The relation between *PV* and other factors can thus be used as a diagnostic to determine which operations on one stimulus overlap with which operations on another, and which do not. (It is instructive to consider what the full set of possible arrangements is of such two-stage processes, and the data patterns they imply.)

On each trial in Chase's first experiment (1969) subjects searched through a row of thirty test letters for any of one to six target letters, and, as they searched, pressed a button for each target they encountered. The window through which subjects viewed the letters could accommodate one letter (no preview), or two, four, or all thirty letters. Figure 14.41A shows that both the amount of preview (*PV*) and the number of target letters (*NT*) had substantial effects, and it is clear from the excellent fit of the additive model that the effect of *PV* is invariant over levels of *NT* (and vice versa).[51] The stage influenced by *NT* is presumably a process in which test items are compared to memorized targets.[52] Apparently the memory-comparison processes for different test items do not overlap under these conditions. This experiment therefore leaves open the question of what is responsible for the substantial effect of *PV*.

In the experiment that helps to answer this question, Chase (1969, experiment 5) used nonsense patterns rather than letters. On each trial subjects searched for one or more targets in a row of twelve patterns (two sample patterns are shown in figure 14.41B). Instead of responding overtly when they found a target, subjects counted targets covertly and reported their count at the end of the trial. The window they moved along each row to view the patterns could accommodate either one item (no preview) or two (preview of one item). A second factor was stimulus quality (*SQ*): the window was either clear (the patterns intact) or stippled

Figure 14.41
Additive effects on total time of preview (*PV*) and number of targets (*NT*) in visual search, evidence against model shown in figure 14.40B. Data (open squares), additive model (filled squares connected by lines) and fitted linear functions (dotted lines). In panel A, target set contains $1 \leq NT \leq 6$ letters; four levels of *PV*; button press during search for each target found in rows of 30 letters. Note similar deviation from linearity at each level of *PV* (unexplained). Slopes of the fitted linear functions for increasing amounts of preview are 0.87, 0.86, 0.84, and 0.86 sec/target. In panel B, target set contains $1 \leq NT \leq 3$ nonsense patterns (illustrated); two levels of *PV*; count of targets found in rows of 12 patterns reported at end of search. Data from Chase 1969, experiments 1 and 5 for panels A and B, respectively.

(the patterns degraded). And a third factor was the number of target patterns for which the subjects searched (*NT*), which could be one, two, or three.

As in the task with letters, Chase found that the effect of *NT* was approximately invariant over levels of preview (figure 14.41B). He also found that the effect of *NT* was approximately invariant over levels of *SQ* (figure 14.42A), indicating that these two factors influence different stages. (See section 14.5.13 for related discussion.) I have already suggested that *NT* influences a memory-comparison stage; it seems reasonable to use "encoding" to describe the stage influenced by *SQ*. Finally, Chase found that the effect of *SQ* interacts with *PV* (figure 14.42B): preview reduced the effect of degradation by more than 50 percent.[53] These results indicate that preview increases the search rate at least partly by permitting different stimuli to be encoded in parallel. But there is apparently no overlap of the processes by which different encoded stimuli are compared to memorized targets.

14.6 The Additive-Factor Method: Concluding Remarks

14.6.1 Processing Stages and Brain Structures

When introducing the concept of a processing stage (section 14.4.1), I was careful to define it as a function carried out during an epoch in time, not necessarily associated with a distinct neuroanatomical processing device. Thus the figures in this chapter containing boxes (representing processes) and arrows (representing succession in time) are flowcharts, not anatomical circuit diagrams. It is remarkably easy to slip into a mode of thinking in which stages are process*ors* rather than process*es*, actors rather than actions; confusion about what a stage might be finds its way into much writing on the subject, even by experts. Because processing stages correspond to epochs, the nature of time forces them to be strung together end to end; as they are inherently ordered, there is no other alternative. If we now misinterpret such a diagram as describing how processing devices are connected, we have only one simple arrangement of many that are possible, one in which each device operates on just the output of one other device and delivers its output, in turn, to a third; we have arbitrarily excluded numerous alternatives. For example, Neisser (1976, 23) described the result as "a simple flow from the periphery to the mind." Broadbent (1984, 55) described it as "a pipeline ... through which information passes from the senses to the effectors" and suggested that it permitted only "bottom-up" explanations that would preclude, for example, using context and preliminary analysis to guide further sampling of the input in a perception task.[54] Stage models need not be constrained in this way;

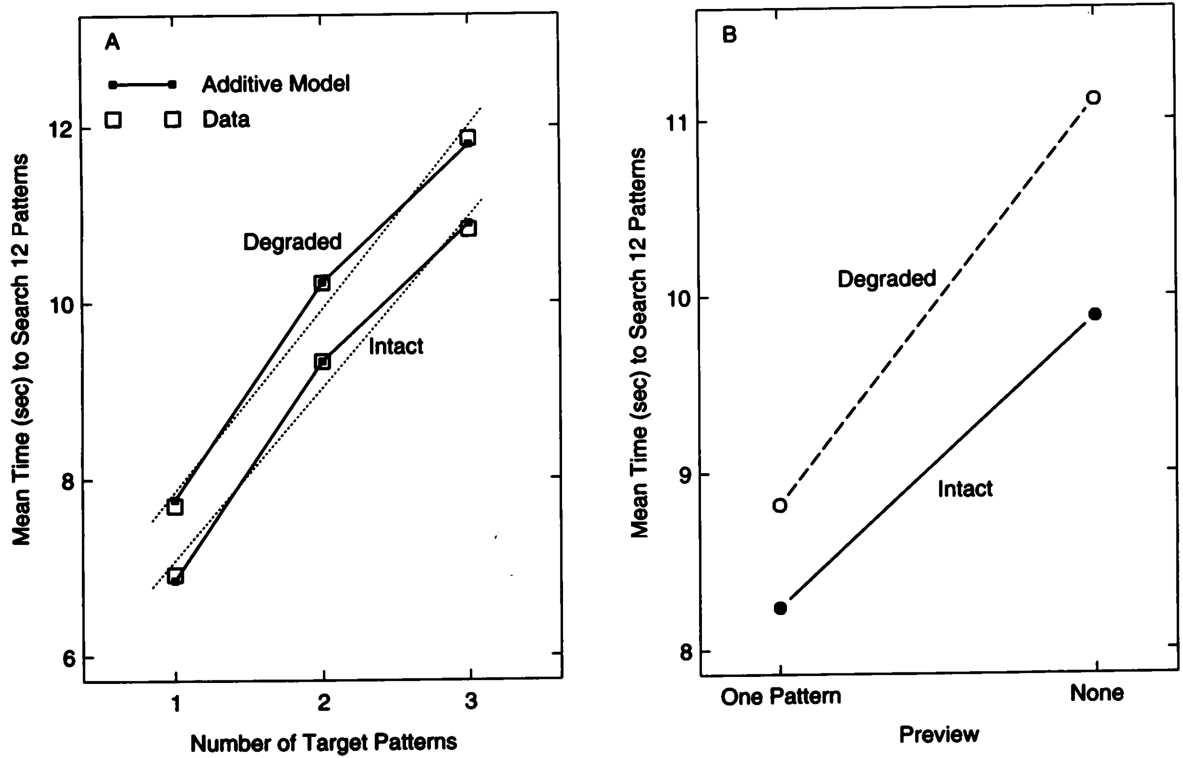


Figure 14.42
Further relations of factor effects in search for nonsense targets. Panel A shows effect of *NT* on times to search (open squares) among intact and degraded test stimuli. Fit of additive model (filled squares connected by lines) indicates that effect of *NT* is approximately invariant over levels of *SQ* (and vice versa). Fitted linear functions (dotted lines) also shown. Panel B shows *PV* × *SQ* interaction. Effect of stimulus quality is reduced by preview, evidence for the model shown in figure 14.40A. Data from Chase 1969, experiment 5.

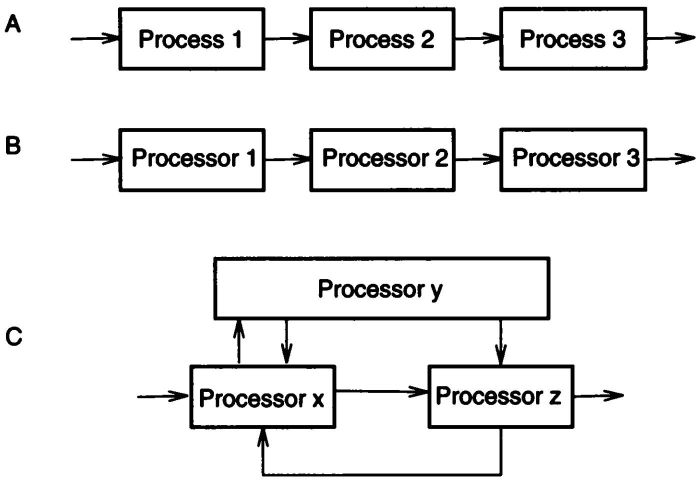


Figure 14.43
Flowchart versus circuit diagram. In panel A (stage diagram or flowchart), arrows represent succession in time of a series of processes. In panel B (processor or circuit diagram that resembles a stage diagram), arrows represent the flow of information from one processor to another, as in Broadbent's "pipeline" and Neisser's "simple flow." Panel C is a processor diagram that does not resemble any stage diagram.

they merely partition processing operations into components that are temporally successive, functionally distinct, and separately modifiable. In explaining the process of recognizing a degraded printed word, for example, there is no obstacle to having a stage during which a preliminary interpretation of the whole word is developed, influenced by context, followed by a stage during which such a preliminary interpretation guides the resampling of letter features. See figure 14.43.

Emphasizing the distinction between process and processor encourages us to ask an important question: Are the processes identified as separate stages in a particular task carried out by anatomically distinct neural structures? That is, is there a circuit diagram whose component processors correspond to the stages of processing in a flowchart? Three arrangements are illustrated in figure 14.44. Three processing stages are defined by sequential nonoverlapping epochs P_1, P_2, and P_3; three brain structures that are processing units, in different locations in the brain, are denoted x, y, and z. Panel A shows the three stages being carried out by the same processor, panel B shows each stage being carried out by a distinct processor, and panel C shows a more complicated arrangement. Let us consider the two extreme arrangements shown in panels A and B. Which of these arrangements is likely to obtain under which circumstances?

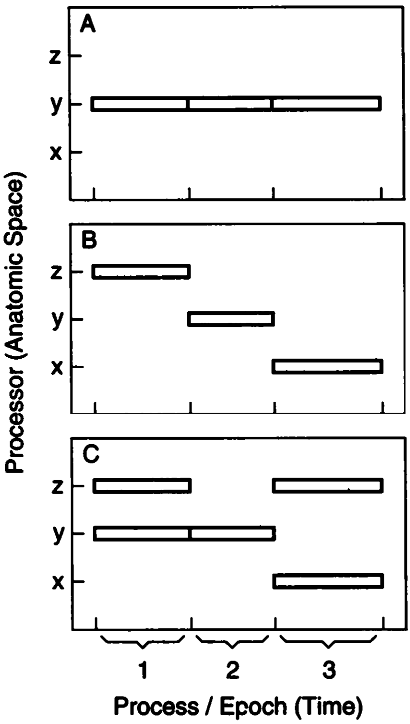


Figure 14.44
Some possible relations between processing stages (carried out during successive epochs, shown on x-axis) and processors (brain structures), shown on y-axis. Panel A shows three stages carried out by one processor. Panel B shows each stage carried out by a different processor. Panel C shows a more complex relation between stages and the processors that carry them out. For example, processors x and y are both involved in carrying out process 1.

As more studies accumulate like the one described in section 14.5.6 on task switching, where attempts are made to determine which stage or stages of a well-understood task are influenced by localized brain damage, they may provide information relevant to this question. For the present, however, what we can say is largely conjectural. It seems plausible that the likelihood of a panel A versus panel B arrangement depends on the relations among the stages. Section 14.4.3 mentioned two alternative reasons why processes P_1 and P_2 might be arranged in stages: P_2 might require data from P_1, or the available capacity might permit only one process at a time. A plausible explanation of seriality without data dependence is that both processes are carried out by the same limited-capacity processor and that the arrangement shown in panel A is the one that applies. One example is provided by results from the overlapping-tasks paradigm (section 14.5.9), which suggest that the response-selection processes in two different, independent tasks cannot overlap in time. A second example is provided by the experiment on deciding whether two letter strings are words (section 14.5.11). In both examples, the two processes found to be sequential are not data-dependent, and the functions they perform are similar, as well, another reason why they might be carried out by the same processor.[55] In contrast, an example of data-dependent processes is provided by the analysis of digit naming (section 14.4.4). Here, because the response-selection process cannot start without data from the stimulus-identification process, we need not invoke a limited-capacity property to explain seriality. Furthermore, the functions carried out by stimulus identification and response selection are different, unlike the examples without data dependence, which adds to the plausibility of distinct processors. For stages that carry out data-dependent processes the arrangement shown in panel B seems more likely.

14.6.2 Perspectives from Other Module-Finding Methods

It is helpful to consider the AFM in the context of some related methods for discovering mental-processing modules. Recall that I defined a processing module as a separately modifiable subprocess of a complex process (section 14.1.1). As we shall see, compared to other methods, the AFM is appropriate for a situation in which (1) we measure time (rather than accuracy, for example), and (2) we measure an entire process (rather than having separate measures of its parts). In other situations, exemplified below, if we want to demonstrate that two subprocesses, **A** and **B**, belong to distinct modules, we must still demonstrate separate modifiability, which we do by finding an instance of selective influence. That is, we have to discover two factors (usually experimental manipulations), F and G, such that F influences **A** but leaves **B** invariant, while G influences **B** but leaves **A** invariant. How do we know we have succeeded in finding selective influence? This depends on what we can measure about the putative modules, and whether we can measure them separately or only in combination.

14.6.2.1 Separate Measures

Direct separate measures. Suppose we have separate measures of each putative module. One example is separate time measures. If a marker signal is transmitted when each part completes its work, then we could directly measure the durations of parts **A** and **B**, the two putative stages. Event-related potentials (chap. 15, this volume) have been used as separate measures; the marker signals are identifiable patterns in electrical changes at the scalp. In speech production, points on the articulatory trajectory have been used as marker signals for completion of part processes (section 14.5.7). A second example is separate measures of amount of brain activity. Suppose two processing modules used in a task are implemented in disjoint regions of the brain, module **A** in region x, and module **B** in region y, and that we can measure the amount of activity in each of those regions. We would then have separate measures of the two modules. Given separate measures (as in either example), we can test the selective influence of factors F and G directly.

Derived separate measures. The examples of separate measures given above depend on our gaining physical access to the separate putative modules. An alternative is for separate measures to be derived from different aspects of the behavior of the process as a whole.[56]

Two such measures derived from a set of responses are, for example, the discrimination acuity (A_z) and the decision criterion (LR_c) of signal detection theory (see sections 13.2 and 13.3). These measures have been shown to be separately modifiable, indicating that they reflect distinct processing modules for discrimination and decision.[57] A second example is Roberts's analysis (chap. 2, this volume) of response-rate data from rats in the peak procedure. The peak procedure is a discrete-trials fixed-interval procedure: the first response after a fixed delay from the start of the trial is rewarded. In this procedure, the instantaneous response rate first rises with time from the start of the trial, reaching a peak in the neighborhood of the fixed delay, and then falls, producing a single-peaked function that describes response rate versus time. One pair of separate measures derived from this response-rate function are its maximum, the *peak rate*, and the time into the trial at which the peak rate occurs, the *peak time*. Roberts and others have shown that peak rate and peak time are separately modifiable (see his section 2.2). A third example is introduced by Osman (section 15.3), where he discusses measurement of the pattern of event-related potentials at the scalp when one aspect of a stimulus tells the subject which hand to move (left versus right) if a response is required, and a second aspect tells whether a response is required (go versus no-go). Two potentials are measured as a function of time from presentation of the stimulus, reflecting activity on the left and right sides of the brain, probably in the motor cortex. One derived time measure is of the onset of the "lateralized readiness potential" or LRP, a difference between the two potentials that reflects preparation of one of the alternative responses. A second derived time measure is of when the LRPs for response and non-response trials begin to diverge (the onset of the "go/no-go difference wave"). Osman and others have shown that these two time measures are separately modifiable.

An example of derived separate measures based on single rather than multiple responses (unlike the three examples just mentioned) is provided by response latency and response velocity. A single manual response can vary in both latency (time to achieve some criterion velocity) and peak velocity. There are hints that these two attributes of the response may depend on distinct modules of the process that generates it (e.g., Abrams and Balota 1991).

14.6.2.2 Composite Measures

We often choose (or are able) to measure only one aspect of the process as a whole. Given only a single measure, it is more difficult than with separate measures (whether direct or derived) to find evidence for decomposition into modules and to learn about which factors influence which modules. The increase in difficulty results from the need to know how the putative modules contribute to the behavior of the process as a whole, that is, a *combination rule* for the effects of the modules on the measure. The issue of the combination rule complicates the problem of discovering modules, but because the system we are studying must often remain intact, we may be restricted to composite measures and they may be one-dimensional, which precludes separate measures. In such cases we need to produce not only evidence for separate modifiability, but also evidence that supports the hypothesized combination rule. Of course, the rule, which tells us about how the modules contribute to the process as a whole, is usually of considerable interest, rather than merely a means to an end. The combination rules that seem easiest to work with are for processes whose modules are arranged in series.

Accuracy as a composite measure. Suppose we could measure the accuracy of the process as a whole, and that the process as a whole performed correctly only if each of its modules performed correctly. (One natural way in which this property would arise is if the modules were carried out in stages; Schweickert 1985). If we assume that the correctness of one module is independent of the correctness of another, and that one error does not undo another, then the process would perform correctly with a probability given by the product of the probabilities that its modules perform correctly: $P = P_a * P_b * P_{\text{other}}$, where P_a and P_b are the probabilities that modules **A** and **B** perform correctly and P_{other} is the probability that all other modules perform correctly. The observed accuracy, P, would be a composite measure, with the combination rule being *multiplication*. Given stages plus selective influence, the effects of F and G on accuracy would be multiplicative because $P(F, G) = P_a(F) * P_b(G) * P_{\text{other}} = f(F) * g(G)$, where f and g are functions of the levels of factors F and G, respectively (note the similarity to equation 14.5). One consequence, for example, is that the ratio $P(F_1, G_1)/P(F_2, G_1)$ would remain invariant when the level of G is changed from G_1 to G_2. If we found multiplicative effects of two factors on P, this would jointly support the multiplicative combination rule and selective influence, which would in turn argue that **A** and **B** are modules.

Response rate as a composite measure: The method of multiplicative factors. Suppose that the modules operate in sequence, and that the output of each module (which is the input to the next) results from applying a weight between zero and one to its input. We can think of the variable transformed by each module as a level of activation. Suppose further that the response rate in an operant situation (such as bar pressing by a rat) is proportional to the level of activation of the final module. The rate is then proportional to the product of the weights, $R = k * W_a * W_b * W_{other}$, and the combination rule is *multiplication.*[58] If selective influence applies, so that factor F influences only W_a and factor G influences only W_b, then $R(F, G) = k * W_a(F) * W_b(G) * W_{other} = f(F) * g(G)$, so that F and G have multiplicative effects on R. The multiplicativity of effects that results from the combination of sequential modules plus selective influence is exploited by the multiplicative-factor method (developed by Roberts 1987; see also his section 2.2.2 in the present volume), an analogue of the AFM that has been found especially useful in the study of animal behavior, where Roberts discovered many instances of invariant ratios of response rates, helpful in guiding theory development.

Time as a composite measure: The method of additive factors. Suppose the modules are carried out in stages, so that the process duration is the sum of the module durations: $T = T_a + T_b + T_{other}$, and suppose we measure the process duration, T. This quantity is then a composite measure, with the combination rule being *summation.* If we add selective influence of F and G on **A** and **B**, respectively, the effects of F and G on T would be additive because $T(F, G) = T_a(F) + T_b(G) + T_{other} = f(F) + g(G)$. Corresponding to the invariant *ratios* of the two examples above, we have invariant *differences* in this case: $T(F_2, G_1) - T(F_1, G_1)$ remains the same when the level of G is changed from G_1 to G_2. Because "effects" are defined as differences, this means we also have invariant factor effects. It is this implication of the combination of organization in stages plus selective influence that is exploited by the AFM. Thus, if we find additive effects of two factors on mean RT, this would jointly support the summation combination rule and separate modifiability, which would, in turn, argue that **A** and **B** are modules.

We would of course like to ask separately about (1) the existence of modules and (2) how they are arranged, but given only a composite measure, we can ask only the combined question. With respect to issue 1, the additivity of factor effects (which depends on the combination rule as well) is not the property of fundamental interest but an indirect test of something else, namely, separate modifiability. Given selective influence of factors *F* and *G* and given summation as the combination rule (appropriate when time is the measure and the modules are arranged in stages), it follows that the effect of *F* must be invariant over levels of *G*, and vice versa, that is, *F* and *G* are additive. Additivity therefore tests jointly the stages model plus the selective influence of factors *F* and *G*. The test of selective influence has to be indirect, and has to invoke other assumptions (such as the stages model) if all we have is a one-dimensional composite measure. The appeal of separate measures is that they permit us to ask about issue 1 without knowing how contributions of the putative modules combine—which we need to know in order to specify the combination rule for composite measures.

> *Comment 19: Separate measures versus double dissociation.* It is helpful to contrast the use of separate measures (of putative modules in performance of the same task) and the *method of double task-dissociation.* Consider the dissociation method when factors *F* and *G* are damage to different parts of the brain, so that the search is for processes carried out by distinct processors. Dissociation means that *F* influences the performance of task *U* but not task *V*, while *G* influences task *V* but not task *U*. Thus patients with one kind of lesion might perform poorly on task *U* but not *V*, while patients with another kind might perform poorly on task *V* but not *U*. Such a finding indicates that the two parts of the brain perform different functions and are separately modifiable, and that tasks *U* and *V* depend on different functions. But double task dissociation is not especially useful for decomposing one complex mental process (used to perform a single task) into its constituent processes. To do so using double dissociation would require complete task analyses (section 14.5.1) of *U* and *V*. Applied with care, the method may show that each task includes one or more processes not included in the other. But for the functions of those processes to be more than conjectural, much additional knowledge is needed.

14.6.3 What is "Additive-Factors Logic"?

This section summarizes, somewhat more formally than sections 14.1.3, 14.3, and 14.4, the logical status of several of the inferences associated with the AFM, including principles 3–7 (introduced in section 14.3). Consider a theory or hypothesis that contains two propositions, H1 (*stages*) and H2 (*selective influence*) about a pair of mental processes. Let us call the theory "(H1 + H2)." Proposition H1 asserts that processes **A** and **B** operate in nonoverlapping epochs, that is, **B** begins only when **A** is complete. The alternative, ~H1, is that processes **A** and **B** are incorporated in the same stage ("stage" is defined such that there is at least one between stimulus and response, which occupies the entire stimulus–response interval). Proposition H2 asserts that factors *F* and *G* influence processes **A** and **B** selectively: *F* influences the duration of **A** but not **B**, whereas *G* influences the duration of **B** but not **A** (the alternative, ~H2, is that *F* and *G* influence at least one process in common). Weak as it is, (H1 + H2) has a powerful implication, denoted here by "*ADD*": the mean effects of *F* and *G* on the time T_{AB} to accomplish processes **A** and **B** will be invariant and hence additive. That is, the change from one level to another of factor *F* will increase T_{AB} by the same amount, regardless of the level of *G*, and vice versa. (This implication follows from the fact that we can write $T_{AB} = T_A + T_B$, when T_{AB} is measured in physical time, where T_A and T_B are the unobserved mean durations of processes **A** and **B**.) The implication from (H1 + H2) to *ADD* is one of entailment, or logical necessity, just as for implications derived from many theories.[59]

Because (H1 + H2) implies *ADD*, it follows that the falsity of *ADD* (denoted ~*ADD* and based on the observation of interacting effects of *F* and *G* on T_{AB}) entails the falsity of (H1 + H2). This in turn implies one of three possibilities: H1 is false (processes **A** and **B** are incorporated in the same stage), H2 is false (factors *F* and *G* influence at least one process in common), or both are false. Whichever of these three possibilities obtains, ~ *ADD* implies that factors *F* and *G* influence at least one stage in common and that this stage is not decomposable into (sub)stages influenced selectively by *F* and *G*; we shall use ~(H1 + H2) to denote this state of affairs.

If observations confirm an implication of a theory, our degree of belief in the theory is increased, but these observations do not logically *entail* the truth of the theory. Roughly speaking, the fewer plausible alternative theories that have the same implication, the greater the increment in our belief strength from its confirmation (see Howson and Urbach 1993). Observation of property *ADD* is thus more potent for confirming theory (H1 + H2) insofar as there are few plausible mechanisms for which the conjunction of ~(H1 + H2) and *ADD* obtains, that is, for which two factors influencing the same nondecomposable stage have additive effects. The following argument supports the implausibility of this conjunction. Consider the one-dimensional interaction contrast $(\overline{RT}_{11} + \overline{RT}_{22}) - (\overline{RT}_{12} + \overline{RT}_{21})$ from a simple 2 × 2 experiment (two factors, each at two levels). This contrast can take on an infinite number of possible values—a continuum from negative to positive. An additive pattern (interaction contrast zero) corresponds to only a single point on this continuum and is thus extremely unlikely without some special reason to expect it; all other patterns are interactive.[60]

Just as the implausibility of the conjunction of $\sim$(H1 + H2) and *ADD* causes observation of *ADD* to give support to (H1 + H2), so it causes a belief in $\sim$(H1 + H2) to lead us to expect $\sim$*ADD*. Neither of these relations are ones of logical necessity, however. Instead, both are governed by the normal practices of inductive inference.

In short, the inference from stages plus selective influence to the additivity of factor effects is deductive inference, just as are many predictions derived from theories. On the other hand, the inference from the observation of additive factor-effects to the conjunction of stages plus selective influence is inductive inference, an instance of confirming a theory by validating one of its predictions. Inductive inference is sensitive to other knowledge we may have. Thus the inference rule should be qualified: we infer (H1 + H2) from *ADD* if there are no stronger reasons to the contrary. The search for factors *F* and *G* with additive effects has as its primary goal the demonstration that processes **A** and **B** are separately modifiable and sequential, and therefore stages. A secondary goal of the search for such factors is to provide information about the processes carried out during these stages.

Because of these relations of implication and confirmation and their elaborations, as exemplified in the applications of section 14.5, patterns of factor effects, especially additive effects, are of considerable interest. Factorial experiments with *RT* measures are especially useful for suggesting and helping to select among relatively weak theories about mental processes. (A weak theory, "A" is strengthened by adding to its defining properties, thus generating theory "B," a special case of theory A. Which properties are added determines which stronger, special case, B_1, B_2, ... of the weak theory is thus generated. A weak theory can account for a larger variety of data than a strong one, and is harder to reject. One goal of research is to arrive at strong theories that are not rejected after thorough testing, despite their strength.) Given its rejection, the weakness of a theory is an asset, because a weak theory corresponds to a large class of strong theories (Broadbent 1958, chapter 12, for example, argues for beginning with weak theories). Given confirmation, a weak theory provides only a starting point, of course, but one that may nonetheless significantly guide and constrain further research.

14.6.4 Extending the Method beyond the Mean

This chapter has been entirely concerned with the analysis of mean reaction-times, but *RT* data are much richer than this. Even within a particular condition in an experiment (that is, when the explicitly controlled factors are held at fixed levels) the *RT* is variable rather than constant. The data from each condition thus consists of a distribution of *RT*s that has not only a mean, but also a spread and shape that may be informative.[61] How can the search for stages make use of this additional information? Consider a model with two stages, **A** and **B**, and selective influence of factors F and G, with levels indexed by i and j, respectively. First, let us review the argument based on means. For this model we can write $RT_{ij} = T_{ai} + T_{bj}$ for an individual trial, and hence, for a set of trials,

$$\overline{RT}_{ij} = \overline{T}_{ai} + \overline{T}_{bj}, \tag{14.18}$$

where the plus sign expresses the fact that the combination rule for means is summation: the unobservable means of the stage durations are summed to determine the mean RT. Because $\overline{T}_{ai}$ and $\overline{T}_{bj}$ depend only on the levels of F and G, respectively, equation 14.18 implies that the observable effects of each factor on $\overline{RT}$ are invariant over levels of the other, and hence additive. Another way of describing the separate modifiability property is to say that the stage durations T_a and T_b are "independent in mean": we can change $\overline{T}_a$ while leaving $\overline{T}_b$ invariant, and vice versa. As it stands, however, this "general stage model" (or *G-stage model*) says nothing about aspects of the data distributions other than their means.

The model is considerably strengthened by adding a further property: assume that stage durations T_a and T_b are *stochastically independent*, as well as being independent in mean, giving us the *SI-stage model*. (Stochastic independence means that knowing the duration of one stage on a trial gives us no information about the duration of the other, and implies that stage durations are uncorrelated.) The SI-stage model has many properties not shared with the G-stage model, of which it is a special case. For example, we can write

$$\text{var}(RT_{ij}) = \text{var}(T_{ai}) + \text{var}(T_{bj}), \tag{14.19}$$

where $\text{var}(x)$ is the variance of x, a measure of the spread of the distribution of x. That is, for the SI-stage model, the combination rule for variances is also summation: the unobservable variances of the stage durations sum to determine the variance of the RT. Because the selective influence property also implies that $\text{var}(T_{ai})$ and $\text{var}(T_{bj})$ depend only on the levels of F and G, respectively, equation 14.19 implies that the observable effects of each factor on $\text{var}(RT)$ are invariant over levels of the other, and hence additive, just as for $\overline{RT}$. That is, when the G-stage model is strengthened to become the SI-stage model, factors that influence stages selectively have additive effects on $\text{var}(RT)$ as well as on $\overline{RT}$. Other properties of the SI-stage model (including properties of entire RT distributions) are discussed in Ashby and Townsend 1980, Roberts and Sternberg 1993, Townsend 1984, and other references cited therein. Recent tests of the SI-stage model have supported it (Roberts and Sternberg 1993), thus also favoring the weaker, more general G-stage model, a fortiori. (Confirmation of a special case, or stronger variant, of a more general—hence weaker—model also confirms the more general model.)

14.6.5 Nonstage Architectures That Produce Additive Effects

As discussed in section 14.6.3, the finding of additive effects of factors on mean *RT* (*ADD*) supports two propositions, namely, H1: processes arranged in stages, and H2: selective influence of factors on different processes, hence separate modifiability (modularity) of those processes. The amount of support generated by *ADD* for H1 depends on how likely are other arrangements of processes (other "mental architectures") within which factor effects are additive.[62] The amount of support generated by *ADD* for H2 depends on how likely it is that additive effects can be produced by two factors that influence the same process.

Suppose two factors influence different processing modules selectively. Are there alternatives to stage architectures composed of such modules that can produce *ADD*? In recent years it has been discovered that there are architectures that appear not to be stage models, but can produce either exact or approximate additivity of factor effects on mean *RT*; two examples are described below. This weakens the support from *ADD* for H1. However, I know of no interesting cases where additive effects are produced by two factors that influence the same nondecomposable process; the support from *ADD* for H2 is not correspondingly weakened. It follows that the AFM is probably a good way to search for processing modules, but that additional evidence may be needed to determine how they are arranged.

14.6.5.1 Alternate Pathways

The alternate-pathways (AP) model (Roberts and Sternberg 1993) is one example of a nonstage architecture that can produce additive factor effects. In this model, the task is accomplished by process **A** (one pathway) on a proportion p of the trials, and by process **B** (the alternate pathway) on the remaining trials. As experimenters we observe the mixture of the two kinds of trials, which we are unable to distinguish reliably. Selective influence takes the form of pathway **A** being influenced by factor F but not G, while pathway **B** is influenced by factor G but not F. Perhaps surprisingly, the model produces additive effects of F and G on $\overline{RT}$.[63] Thus, at the level of mean *RTs*, the AP and G-stage models cannot be distinguished. Given either model, H2 (separate modifiability) is supported by an observation of additivity of factor effects on mean *RT*. But the existence of an alternative to stages that produces additive effects on the mean weakens the support provided for H1 (stages).

Predictions can be derived from the AP model for variances and other properties of the RT distributions, properties about which the G-stage model is mute. However, as we have seen, the SI-stage model, a strengthened G-stage model, does make such predictions. Furthermore, these predictions contrast with those of the AP model, which predicts for example, that effects of factors F and G on var(RT) will be interactive rather than additive. Thus far, tests of this and other predictions have not only permitted rejection of the AP model, but strongly favor the SI-stage model, and hence the G-stage model (Roberts and Sternberg 1993).

14.6.5.2 Overlapping Processes

Partial overlap. Suppose an arrangement of processes P_1 and P_2 where P_1 starts when the stimulus is presented and P_2 starts when a duration T_1 has elapsed after P_1 starts. Suppose further that P_1 continues to operate for some duration τ_1 after P_2 starts. (As an analogy in terms of data-dependent processes, think of a relay race: the first runner does not stop short when passing the baton to the second runner, but what the first runner does after passing the baton is not relevant to winning the race.) Finally, suppose the response is initiated when P_2 has been operating for a duration T_2, where $T_2 > \tau_1$. Processes P_1 and P_2 then overlap (for a duration τ_1), so that, strictly speaking, we do not have a stages architecture. What difference does this make? The RT is still the sum of two durations, T_1 and T_2, each associated with one of the processes. If T_1 and T_2 are selectively influenced by factors F and G, respectively, the arguments we have been making up to now will work. If the processes are not data-dependent, then the portion of process P_1 that overlaps P_2 can have no impact on T_2. However, if P_2 uses data provided by P_1, then it is possible that only partial data would be available when P_2 starts, and that additional data would be provided during the τ_1 interval. This could mean that there is an interval of processing when both F and G have effects, which could result in a failure of additivity.

Complete overlap. A model with overlapping data-dependent processes that has attracted considerable attention is McClelland's "cascade model" (1979), as further developed by Ashby (1982). The model consists of a set of processing units, with the output from unit k serving as the input to unit $k + 1$. Because "activation" is transmitted continuously from one unit to the next, all units actually operate concurrently, and activation begins to rise at the output of the final unit as soon as the stimulus is applied to the input of the first. Thus, although the units are ordered structurally by their input-output relations, in an important sense they are not ordered temporally and can be regarded as operating in parallel. Because of such striking contrasts with a stage model, it is especially noteworthy (and not yet understood) that under certain conditions the cascade model can produce additive effects of factors on both mean and variance, just as the SI-stage model does. One condition is that the units are modules, in the sense of being separately modifiable; thus the factors must selectively influence the activation growth rates (the rates at which output activation responds to input activation) of different units. A second condition is that a factor cannot influence the asymptotic activation of the unit whose growth rate it influences. A third condition is that there is at least one unit whose activation growth rate is low and independent of the factor levels.

Because the cascade model can accommodate additive effects of factors on the *RT* variance, it cannot be tested in the same way as the AP model. But the cascade model also predicts certain relations among the means and variances, and when these have been tested, the model has been rejected (Roberts and Sternberg 1993). Again, that an architecture quite different from a sequence of stages is capable of producing additive effects on $\overline{RT}$ reduces the support for a stages mechanism generated by merely observing such effects; additional tests are needed. And again we have a case where the processes influenced by the additive factors are modules, even if not stages.

14.6.6 Some Strengths and Limitations of the Method

The AFM permits learning about the structure of mental processes from patterns of factor effects on mean reaction-time, while requiring minimal commitment to theories about how the parts of the process work. The method avoids the limitations of Donders' approach by requiring us merely to vary the durations of hypothesized stages rather than to insert or delete them. It also avoids some of the difficulties of approaches that seek understanding by creating complex models with numerous interrelated defining properties—difficulties of discovering what plausible data patterns such models cannot accommodate, and, when such a model does fail, of determining which of its properties is responsible.

Why does the issue of factor additivity seem less crucial in research where the measure of performance is something other than time? One reason is that in most other domains we need a rather detailed theory to justify a preference for one measurement scale over another, and an arbitrary change of scale can turn additive effects into interactions. One of the appeals of the study of reaction time is that, with nothing more than the stages idea, we have specified the preferred measurement scale (physical time, not a transformation of it) and made meaningful the quantitative analysis of data.

> *Comment 20: What is special about time?* Duration, measured in physical time, is appropriate because it is additive: the duration of

stage A concatenated with stage B is the sum of their durations. Compare it, for example, to speed, the reciprocal of time, which is not additive. The effect of a change in T_1 on $(T_1 + T_2)^{-1}$ depends on T_2 rather than being invariant relative to T_2; the principle of full expression (section 14.3.3) fails for speed. Similarly, the mean RT is an appropriate summary measure because it, also, is additive: $\text{mean}(T_1 + T_2) = \text{mean}(T_1) + \text{mean}(T_2)$. Under some conditions the minimum may also be additive, and, indeed, Donders (1868) used it. The median is not additive, in general, and is therefore inappropriate, though in some circumstances the sample median may be better than the sample mean as an estimate of the true mean. (See section 12.7 for a discussion of errors of estimation.)

On the other hand, the method has distinct limitations, which include:

1. Any decomposition into stages that is produced by the AFM is tentative. Each stage is defined by the set of factors that influence it. As new instances of interaction are observed, our ideas about what occurs in the affected stage will change; as new instances of additivity are discovered, additional stages will be inferred.
2. Although instances of additivity support the hypothesis of separate stages, other considerations must generally be used to determine the temporal order of these stages. But such considerations do arise naturally in some applications (see sections 14.5.5 and 14.5.9, for example).
3. Unlike the subtraction method, the AFM does not provide information about overall stage durations, although the duration of a stage is of less interest than whether there is such a stage, what influences it, what it accomplishes, and what its relation is to other stages.
4. The method distinguishes only processes that are arranged in stages and separately modifiable, which excludes interesting cases. Two examples show that although an interaction can lead to the rejection of the idea of separate stages, we cannot use it to reject the more general idea of separate processes.

The first example shows that the processes must be sequential. Suppose two separately modifiable processes occur in parallel. Then, even if they are influenced selectively by a pair of factors, the factors would be expected to interact, and the two processes would be identified as a single stage by the method. (Suppose also that response initiation awaited the completion of both processes. Then some or all of the effect on $\overline{RT}$ of raising the level of F would be hidden by raising the level of G. Raising the levels of both factors would thus have an effect that was less than the sum of their separate effects, sometimes called an "underadditive" interaction.)

The second example shows the importance of the separate modifiability property. Suppose two processes arranged in sequence share a limited capacity that is allocated to different processes in accordance with task demands. Then we might expect that if we require process **A** to accomplish more (by raising the level of a factor that influences that process), this will reduce the capacity available for process **B**. If more is also demanded of **B**, the effect of this demand would be a greater increase in duration than if the available capacity had not been reduced, violating separate modifiability. Raising the levels of both factors would thus have an effect that was *greater* than the sum of their separate effects, sometimes called an "over additive" interaction, and the two processes would be identified as a single stage.

5. In understanding a mental process, decomposing it—determining its parts—is a useful starting point, but only a starting point. A flowchart answers only a crude and initial question, although an essential one. If a process is organized into sequential modules, it is well to discover this and to try to characterize these modules crudely before proceeding to develop more detailed theories.

14.6.7 Design Matters

The simplest factorial experiment is the 2 × 2 design, in which two factors are studied, each at two levels. An observation of additivity (or interaction) of these factors permits us to apply the AFM and draw conclusions about the structure of the underlying process. Unfortunately, there are no other aspects of the data from such an experiment for which the inferred structure has any implications that could be used to test the conclusions. This limitation of the 2 × 2 design can be overcome by using experimental designs in which three or more factors are included in the same experiment, and some or all of the factors are examined at three or more levels. One of the principal benefits of such designs is that, together with the stage model and the inferred relations between factors and stages, the data they provide can be tested for internal consistency, as mentioned in section 14.4.4.2. Let us explore the possibility of consistency tests with a few examples.

14.6.7.1 Multiple Factors

Consider first an experiment with three factors, F, G, and H. Suppose F and G are found to have additive effects at one level of H. With no other arguments to the contrary, we infer that there are two stages, influenced

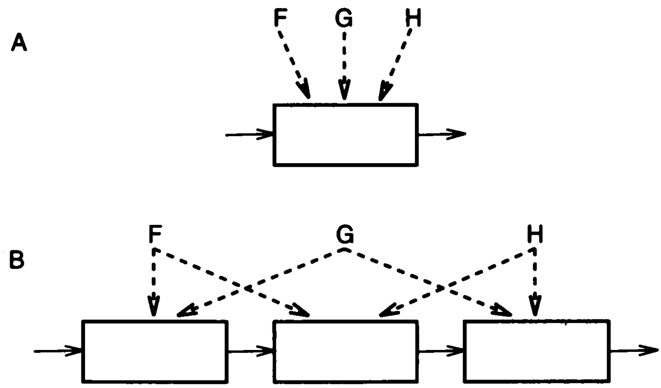


Figure 14.45
Theoretical power of multifactor factorial experiments. Two arrangements of stages and the factors that influence them. They cannot be distinguished by three two-factor experiments (involving *F* and *G*, *F* and *H*, and *G* and *H*, respectively) but can be distinguished by one three-factor experiment (involving *F*, *G*, and *H*). Only the latter provides a measure of the critical three-way $F \times G \times H$ interaction.

selectively by *F* and *G*. If this conclusion about the structure of the process is correct, then because we do not expect a change in the level of *H* to alter the structure, it follows that *F* and *G* should be found to be additive at *each* level of *H*. (This should be true whether or not *H* interacts with *F*, *G*, or both.) Similarly, if *F* and *G* are found to interact at one level of *H*, they should interact at each of its levels. Figures 14.10, 14.12, and 14.25 illustrate such tests. If *H* were not included as a factor in the experiment, we would be unable to test whether such consistency requirements are satisfied.

A three-factor experiment permits us to evaluate the three-way interaction, which we can express as $D_{ijk}(T_{ijk})$ in the notation introduced in section 14.3.5. Such information is not provided even by three separate two-factor experiments, one with each pair of factors. A stage model can produce a nonzero three-way interaction only if all three factors influence at least one stage in common. Suppose the factors in one of the three pairs (say, *F* and *G*) have additive effects. We infer that *F* and *G* influence no stage in common. This means that all three factors influence no stage in common and requires that the three-way interaction be zero. Again, the more elaborate experiment permits us to test a consistency requirement.

Suppose we run three separate experiments, each with two factors: *F* with *G*, *F* with *H*, and *G* with *H*, and in each case we find an interaction. One possibility is that all three factors influence the same stage ($T_{ijk} = a_{ijk}$) as shown in figure 14.45A. The other possibility is that there are three stages, **A** influenced by F and G, **B** influenced by F and H, and **C** influenced by G and H ($T_{ijk} = a_{ij0} + b_{i0k} + c_{0jk}$), as shown in figure 14.45B. Only in the first instance should there be a three-way interaction; a three-factor experiment is required to discriminate between the two possibilities.

Another reason to include additional factors is to incorporate a test of the soundness of the experiment. Given the limited knowledge we have of the relations among task variations, reaction time, and error rate, it is hard to use the data pattern to detect experimental artifacts, such as might be caused by systematic differences across conditions in subject motivation or fatigue, or in the extent to which accuracy is traded for speed.[64] As mentioned in section 14.6.3, additivity of effects can be regarded as just one point (zero) on a continuum of values of amount of interaction. An artifactually produced displacement of a measurement away from its true value is unlikely to generate spurious additivity. An interaction of respectable size is less fragile and less readily obscured than an additive relation between factors. Inclusion within an experiment of a pair of factors that are expected to be additive (because they are expected to selectively influence different stages or have been shown to be additive in other experiments) provides a useful test for artifacts: if the amount of interaction of such factors is measured to be zero, this renders the experiment more trustworthy. In short, an interaction is more credible if it is observed in an experiment that also reveals one or more instances of additivity.

It has to be recognized that there are drawbacks to including additional factors in an experiment. Experiments of this sort are best designed such that each subject is tested in all the conditions. We therefore have to be especially concerned about effects of practice, fatigue, and carryover effects from one condition to the next. If conditions cannot all be arranged to vary randomly from trial to trial, and some or all must instead be run in blocks of trials, then increasing the number of conditions may make it harder to generate a suitably balanced design.

14.6.7.2 Multiple Levels

Let us return to the 2 × 2 experiment. As shown in section 14.3.5, the interaction contrast in this case is given by a single number:

$$D_{ij}(T_{ij}) = D_j[D_i(T_{ij})] = D_j[T_{2j} - T_{1j}] = (T_{22} - T_{12}) - (T_{21} - T_{11}). \quad (14.20)$$

Suppose, instead, each factor is examined at three levels (a 3 × 3 experiment). Recall that the D_i and D_j operators then each produce two values rather than one (equation 14.4). The interaction contrast is then a set of four values:

$$\begin{aligned} D_{ij}(T_{ij}) &= D_j[D_i(T_{ij})] \\ &= D_j[\{T_{2j} - T_{1j}, T_{3j} - T_{1j}\}] \\ &= \{(T_{22} - T_{12}) - (T_{21} - T_{11}), (T_{23} - T_{13}) - (T_{21} - T_{11}), \\ &\quad (T_{32} - T_{12}) - (T_{31} - T_{11}), (T_{33} - T_{13}) - (T_{31} - T_{11})\}. \end{aligned} \tag{14.21}$$

The 3×3 experiment thus provides an opportunity for a test of consistency not available from the 2×2 experiment. Either all four values should be zero (if F and G influence no stage in common), or all four values should be nonzero (if F and G influence the same stage). Furthermore, because the larger experiment (nine conditions in the 3×3 experiment compared to the four conditions in the 2×2 experiment) provides four opportunities rather than only one to test for interaction versus additivity, it provides substantially more useful information per condition.

14.6.8 Statistical Issues

Because readers of this book are not assumed to be well versed in statistics, I have virtually ignored the problem of sampling variability and how to deal with it.[65] This omission should by no means be taken as discounting the importance of the problem, which is critical in applications of the AFM.

The analysis of variance of factorial experiments, introduced in the 1920s by R. A. Fisher, is the appropriate context in which to examine relations between factors. However, the significance tests usually performed in conjunction with the analysis of variance are asymmetric: we are forced to assume that effects are invariant, that is, additive (the "null hypothesis") unless the contrary can be proved. Given the strong implications of invariance, this direction of asymmetry is particularly inappropriate. It encourages the assertion of additivity, together with its implications for the structure of the process under study, merely because the imprecision of an experiment (its insufficient power) prevents an interaction (the modulation of the effect of one factor by another) from reaching "statistical significance"—that is, prevents the hypothesis of additivity from being rejected. The precision of the experiment has to be considered explicitly, along with the magnitudes of the observed interactions. One way to do this is to use confidence intervals associated with measures of interaction. It is well to ask how large an interaction would be needed in order to reject the hypothesis of additivity. And, when possible (which may be seldom), it is helpful to consider how large an interaction would be expected from interesting alternative mechanisms. (In this context, perhaps the most serious consequence of the primitiveness of our theories is that we seldom know what size interaction to expect when we expect one.)

Unfortunately there seems to be no fully satisfactory decision procedure for asserting additivity, or any other null hypothesis. Another defect of the statistical tests often used to assess additivity is that they are no more sensitive to deviations that are systematic (as in figures 14.25 and 14.39A) than to deviations that are not (as in the context data in figure 14.3), even though we regard the former as more serious than the latter (because it is more likely to favor a plausible competing model). As for most issues of data interpretation, the evaluation of fallible data in relation to the additive-factor method is a matter of subtle judgement informed by knowledge of other related studies; there is no recipe.

14.6.9 The Importance of Ronald A. Fisher (1890–1962)

Why were tests of additivity not employed in Donders's time to distinguish valid and invalid applications of his method (1868)? I suggest that it is because it was not until the 1920s—well after the subtraction method's first period of popularity—that Fisher (1926, 1935; see figure 14.46)

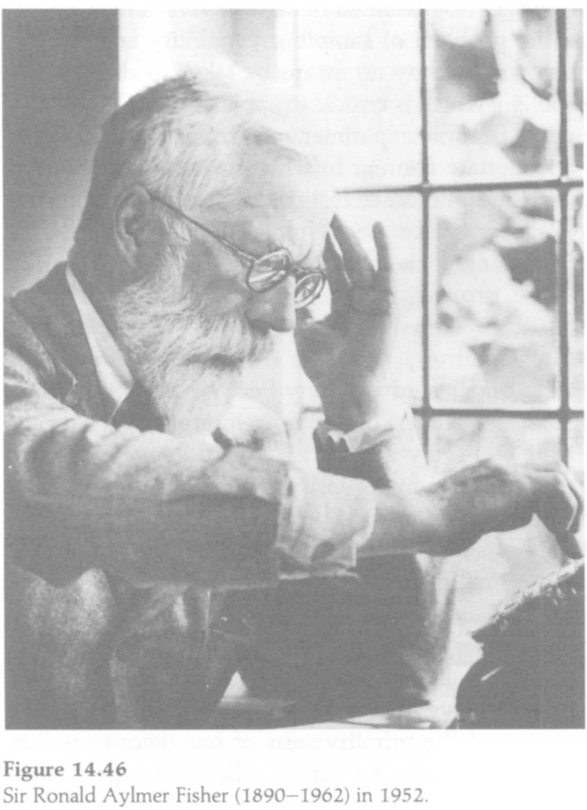


Figure 14.46
Sir Ronald Aylmer Fisher (1890–1962) in 1952.

invented the factorial experiment, demonstrated its efficiency, developed the concepts of main effects and interactions, focused attention on additive models, and provided the tools (especially the analysis of variance) to test them. It is perhaps not surprising that Donders and others of his time, working without this theoretical and methodological apparatus, were not stimulated to perform factorial experiments or examine interactions, and that only in the 1960s did we find researchers hinting at the important role of factor interactions in reaction-time experiments (Bertelson and Barzeele 1965; Broadbent and Gregory 1965) and employing additivity tests in conjunction with the subtraction method (Taylor 1966).

Suggestions for Further Reading

For general readings about the use of *RT* in research on human information-processing and about the subtraction method, see the "Suggestions for Further Reading" in chapter 9.

Section 14.4. In reading about processing stages you will encounter more than one meaning of "stage." Thus, in some discussions the stages described in the present chapter are called "discrete stages" to distinguish them from overlapping processes, which are unfortunately sometimes also called "stages." The paper that introduced the additive-factor method and provided examples of its use is Sternberg 1969a. For discussions of the idea of stages of processing and the AFM, see Miller 1988 (evidence for and against discrete-stage models); Stoffels, Van der Molen, and Keuss 1990 and Mulder, Sanders, and Van Galen 1995 (collections of papers on models, methods, and applications); and Van der Molen et al. 1991 (a review of methods and findings of stages analysis, integrated with studies using electrophysiological measures such as those discussed in chap. 15, this volume). A. F. Sanders (1977, 1980, 1990) has not only used the AFM in numerous individual studies, but has also provided very useful critiques of the method and syntheses of findings from it. For the complexities and pathologies of parallel processing, see, for example, Harel 1992, chapter 10, and Johnson-Laird 1983, chapter 16. The signal-uncertainty effect was probably first reported by Merkel (1885). It is discussed in relation to information theory by Bricker (1955), Hick (1952), Luce (1960), and Hyman (1953). For recent reviews and models see Luce 1986, chapter 10, Schweickert 1993, and Smith 1980. Kornblum and Lee (1995) test a model for stimulus-response compatibility, related to the mapping-familiarity factor.

Section 14.5.1. Consistent with the conclusions of Sanders, Wijnen, and Van Arkel (1982), Humphrey, Kramer, and Stanny (1994) found that, whereas sleep deprivation slows responses, it has no effect on the estimated rates of memory search or visual search. Together with evidence from event-related brain potentials this led them to conclude that sleep state influences an early perceptual process.

Section 14.5.6. Similar findings of additivity of task-sequence and task-performance factors have been obtained by Allport, Styles, and Hsieh (1994), although they argue against a task-switching stage, based partly on their finding of a persisting cost of predictable task switching even when as much as a second elapses between one trial and the next. To argue in favor of a task-switching stage thus requires assuming that much of this stage cannot occur until the stimulus is presented, which seems implausible. But despite its implausibility, Rogers and Monsell (1995) provide evidence that favors such stimulus control of a major component of the switching process.

Section 14.5.7. For these and related experiments on speech production, see Sternberg et al. 1988; Sternberg, Knoll, and Turock 1990, and references cited therein.

Section 14.5.8. Initial interest in the mental transformation of images was based partly on its possible relation to the fundamental question in visual perception of how we recognize patterns despite variations in size and orientation. But it now appears that the way in which we identify a familiar shape despite variations in size and orientation may be different from how we decide whether two patterns of different size, orientation, or both have the same shape (see Farah and Hammond 1988; Larsen and Bundesen 1978; but see also Tarr and Pinker 1989.) In a study related to the present one, Bundesen, Larsen, and Farrell (1983) alternated the same stimulus pairs in an apparent-motion display, measuring the minimum interstimulus time that would produce an illusion of rigid motion. They found that like $\overline{RT}$, this measure also is an additive function of the disparities of size and orientation, suggesting a relation between imagery processes and those underlying apparent motion. For a collection of important studies of the transformation of mental images, see Shepard and Cooper 1982. Like Bundesen, Larsen, and Farrell 1981, Cooper 1976 (reprinted in Shepard and Cooper 1982) provides real-time evidence for the intervening-sectors property, akin to the evidence for stage structure in speech production discussed in section 14.5.7.

Section 14.5.9. For a history of attempts to explain why we are slower when doing two things at once, see Welford 1980; for a modern review, see Pashler 1994a. For models that acknowledge the variability of stage durations, and go beyond the deterministic approximation, see Ollman 1968 and Luce 1986, section 14.5.4. De Jong (1993) has assembled persuasive evidence for the existence of more than one bottleneck. Roberts and Sternberg's further analysis (1993) of the McCann-Johnston (1989) data provides additional support for the deferred-processing model. The extent to which the bottleneck is a fundamental constraint in human information-processing rather than a strategic option is controversial. Meyer and Kieras 1997 (a, b) and Meyer et al. 1995, present a general and powerful framework in which deferred processing is a particular strategy used only under particular conditions, especially the high priority often given to task 1. Among other difficulties that they cite for the idea of a fundamental constraint expressed by the deferred-processing model are factor effects in task 2 that decrease as Δt grows, and changes in the pattern of results induced by training and instructions. Meyer and Kieras (1997b) argue that Pashler's finding (1994c) of deferred processing even when two responses were given equal priority is due to a response-execution bottleneck, engaged only because both responses were manual.

Section 14.5.10. For a comprehensive review of attempts to localize the effect of usage frequency on word recognition, see Monsell 1991; for a review of the repetition priming of words, see Monsell 1985. Recent work on the combined effects on visual word recognition of stimulus quality, usage frequency, and semantic context requires that the conclusions of Scarborough, Cortese, and Scarborough (1977) now be elaborated. The early finding that the effect of degradation on word/nonword decisions is invariant over levels of usage frequency (now upheld by other studies) is consistent with an encoding stage (A) that precedes any use of stored word representations, followed by a memory search stage (B) that is influenced by usage frequency but not by stimulus quality. However, we now also know that recognition is facilitated by having recently seen a word semantically related to the test word and, moreover, that this context effect is sometimes greater with degraded than intact stimuli, depending on the levels of other factors (Meyer, Schvaneveldt, and Ruddy 1975; Stolz and Neely, 1995), as well as being greater with low-frequency than high-frequency words (Becker 1979). The problem is that effects of semantic relatedness, like effects of frequency, must be a reflection of stored word information. An interaction between context and frequency is therefore not surprising, but the interaction between context and stimulus quality is. According to the AFM, we must thus postulate two stages, with stage **A** influenced by quality and stage **B** by frequency, but the context factor must influence both **A** and **B**. Thus both stages must make use of stored information. Borowsky and Besner (1993) develop a theory of word recognition along these lines.

Section 14.5.11. The factorial diagnostic has been used, for example, by Egeth and Dagenbach (1991) as well as Scarborough and Landauer (1981), and the homogeneous diagnostic by Pashler and Badgio (1985); these diagnostics were compared by Pantzer and Sternberg (1992) and Pantzer (1997). For further discussion of the behavior of parallel processes, see Townsend 1974, Townsend 1984, or Schweickert and Townsend 1989.

Section 14.5.12. Other evidence from application of the AFM that favors the existence of more than one stage of encoding is reviewed by Sanders (1980b, 1990) and Pantzer (1997).

Section 14.5.13. In an interesting follow-up study also related to the model of section 14.5.5, O'Boyle and Hellige (1982) compare the effects of test-item degradation on item-recognition performance when the test item is presented to the left versus right cerebral hemisphere.

Section 14.5.14. One of the difficulties in studying visual search is poor stimulus control: we generally do not know the precise time relation between when a subject takes in information from a particular stimulus and when the decision about that stimulus is made. Paradigms that provide better stimulus control have other deficiencies. For example, "simulated search" in which items are presented one after another in one location, paced by the experimenter rather than the subject, may induce different strategies from those in subject-paced search (Sperling et al. 1971; Sternberg and Scarborough 1971). And requiring an overt response to each stimulus, as Cattell (1885) and Pashler (1994b) did, seems qualitatively different from search. Some studies have measured eye position during subject-paced search; see, for example, Gould 1973; and Rayner and Fisher 1987.

Section 14.6.2. The task-dissociation method is discussed by Dunn and Kirsner (1988), Roberts (1993), and Shallice (1988, chapter 10).

Section 14.6.3. Sternberg (1984) and Townsend (1984) consider "additive-factors logic."

Section 14.6.4. For further discussion of extending the AFM beyond the mean, see Ashby and Townsend 1980; Sternberg 1969a; and Townsend 1984.

Section 14.6.5. Miller (1993) has developed and explored one family of models in which overlapping processes are permitted, and found that in general they do not produce additive factor effects. In contrast, Miller, Van der Ham, and Sanders (1995) have explored a family of models similar to the cascade model that can accommodate additive effects. Schweickert (1978, 1983), Schweickert and Townsend (1989), and Townsend and Schweickert (1989) have explored a wide range of mental architectures (of which stage models are a special case) that can be identified by factorial reaction-time experiments.

Section 14.6.6. Luce (1986, section 12.4), Pachella (1974), and Townsend (1984) provide critiques of the method.

Section 14.6.8. Issues of statistical testing related to the AFM are discussed by Sternberg (1969a) and Townsend (1984); and an alternative to the traditional approach is proposed by Rouanet, Lépine, and Holender (1978).

Notes

For comments on the manuscript, I am especially grateful to my student (and teacher) Teresa Pantzer, and also to Janice Hamer, Hazel Murphy, and Jennifer Sternberg. For other guidance, I thank Frank Norman, Seth Roberts, Paul Rosenbaum, and Don Scarborough. For unpublished details of their data, I thank Claus Bundesen, Jeffrey E. Evans, James C. Johnston, Axel Larsen, Robert McCann, David E. Meyer, and Joshua Rubinstein. I thank Peter Lumsdaine (age 12) for persuading me that the story of Alice and Jim had to be told. Preparation of this chapter was facilitated by a Paul C. Williams Term Professorship in Psychology.

1. As we learn more about which brain events are associated with which mental operations, and increase our ability to measure those brain events in humans, such measurements will help overcome this limitation.
2. Similar arguments have also influenced thinking about brain structure. Discussing the primate visual brain, Van Essen, Anderson, and Felleman (1992, 422) have argued: "Complex problems are often best solved by breaking them into smaller components....

Creating separate modules for different subtasks allows neural architecture to be optimized for particular types of computation. It also allows different types of information to be represented explicitly, in ways that can simplify later stages of analysis." Note that Marr's argument (1976) was about the organization of computational *processes*, not of computational *devices*. The same device might carry out more than one of his processing modules. The idea of a processing module presented in this chapter is a unit of processing that is not necessarily implemented by a modular neural structure as defined, for example, by Fodor (1983), Shallice (1988), or Van Essen, Anderson, and Felleman (1992).

3. See comment 1 of chapter 9 (section 9.1.4) on the role of the numbered comments.
4. The mapping factor is traditionally called "stimulus-response (S-R) compatibility." The levels might then be called "high compatibility" (for the more familiar mapping) and "low compatibility" (for the less familiar).
5. When I report the fitting of data by curves or lines, I have used a "least squares" procedure (see chap. 10, this volume). Here the pair of parallel curves is that pair for which the sum of the squared deviations of the fitted values from the data points (measured vertically, on the scale of the dependent variable) is minimized.
6. As you may recall from section 14.1.1, this is a "simple effect," calculated at levels j and k of the other factors.
7. There are several reasonable ways to expand the definition of the effect of a factor; the way I have chosen has properties that are helpful in the present context.
8. As we shall see shortly, however, if AG has a linear effect on one stage but a nonlinear one on another (as it does on d when the elevator is broken), the effect on T is nonlinear. This is because the effect on T is the sum of the effects on the stage durations, and the sum of linear and nonlinear effects is nonlinear.
9. "Additive" is sometimes used in a much weaker sense: two influences are additive if their combined effect is merely greater than either of their individual effects. In this chapter, two influences are said to be "additive" if their combined effect is the *sum* of their individual effects.
10. Given how much Jim can carry, a second bag requires a second ascent of the stairs and ten more minutes, and a third bag slows this second ascent by four minutes.
11. How we describe a process is partly determined by considerations of parsimony and explanatory power. This is illustrated by translating the more complex process of figure 14.4 into paths and nodes, where the description can incorporate an invariant node between each stage and the next by treating AG as a factor that influences stage **D** as well as **A**, but not the stages in between. If, instead, we treated AG as influencing all states of the system after stage **A**, the description would not express the invariance of the durations of stages **B** and **C** across levels of AG.
12. Donders' research in vision helped create the scientific basis for the correction of nearsightedness, farsightedness, and astigmatism.
13. A good brief summary is provided by Woodworth (1938) in his famous text on experimental psychology, in a section on the "discarding of the subtraction method."
14. Helmholtż (1850) had used the subtraction of one RT from another, not for the analysis of mental processes, but to measure the speed of conduction in human sensory nerves. He compared the RTs to stimuli at two different points on the skin, at different distances from the brain. Donders (1868) expressed skepticism about the validity of the pure-insertion assumption in Helmholtz's work, but failed to apply this concern to his own research.
15. According to Pons et al. (1987, 417), "an important aspect of sensory processing in touch is carried out sequentially in the cortex, by transmission of information from lower order to higher order stations, in an arrangement similar to that for sensory processing in vision." Pons, Garraghty, and Mishkin (1992, 518) write: "The results are in line with

accumulating anatomic and electrophysiological evidence pointing to an evolutionary shift in the organization of the somatosensory system from the general mammalian plan, in which tactile information is processed in parallel in SI and SII, to a new organization in higher primates in which the processing of tactile information proceeds serially from SI to SII. The presumed functional advantages of this evolutionary shift are unknown." For the visual system, Poggio (1983, 130) comments that "Even in vision ... serial processing is likely to play an important role quite early on, certainly earlier than most neurobiologists accustomed to the idea of topographic maps and inner-screen would be ready to admit." And Hurlbert and Poggio (1985, 310) suggest that "sequentiality in the brain is not the result of a capricious choice of evolution, but a requirement imposed by the intrinsic nature of visual computations."

16. To be precise in equations such as these, terms like $\overline{RT}_{ijk}$ and $D_{ij}(\overline{RT}_{ijk})$ should be taken to be the expected (or "true") values of these quantities. The corresponding data quantities are then estimates of these expected values, which may deviate from the expected values themselves because of sampling error. (For a discussion of sampling error, see chap. 12, this volume.)
17. The standard error (s.e.) is the standard deviation of the sampling distribution of the mean (discussed in chap. 12, this volume).
18. A modicum of such information is, however, included among the results in tables 14.4 and 14.5 related to application of the AFM in understanding the basis of the signal-uncertainty effect. There, data for individual subjects and standard errors of some of the critical quantities are provided, to enable the reader to consider questions of reliability.
19. Sanders, Wijnen, and Van Arkel (1982) also tested each subject both in the morning and the afternoon; they found a substantially greater effect of sleep deprivation in the afternoon. For example, while the average effect of *SLP* on the $\overline{RT}$ for correct responses was 23 ms in the morning, it was 92 ms, four times as great, in the afternoon. For simplicity, the data have been averaged over the two levels of the time-of-day factor; the conclusions about selectivity of sleep-loss effects are unaffected by this averaging.
20. It may be for this reason that the effect of signal uncertainty is more than twice as great here as in the closest corresponding condition (unfamiliar mapping, intact stimuli) in the data of table 14.4.
21. For the familiar mapping, the stimulus, an arrowhead, pointed to the response button. For the unfamiliar mapping, the response button was located 90° counterclockwise from the direction in which the arrowhead pointed.
22. Because of the differences between test stimuli and responses for the two context tasks, the additivity also suggests that list length has no influence on either stage A or stage C.
23. If we use $j = 1$ and 2 to denote context recall and recognition, respectively, and apply the difference operator introduced in section 14.3, we find $D_{jk}(\overline{RT}_{jk}) = (-6, -23, -3)$ ms, with small and fluctuating components. On the other hand, if we use $j = 1$ and 2 to denote item recognition and context recall, respectively, the separations are $D_j(\overline{RT}_{jk}) =$ 133, 204, 302, and 353 ms for $k = 3$, 4, 5, and 6, respectively, and the interaction is $D_{jk}(\overline{RT}_{jk}) = (71, 169, 220)$ ms, with large and increasing components.
24. The main added difficulty of experiments with more factor levels arises when the trials for each combination of factor levels are presented in a block, rather than being mixed together randomly with the trials for other combinations. In a blocked design, a larger number of different conditions (combinations of factor levels) means greater difficulty, for example, in ensuring that the average amount of practice in the task is the same for each condition. It was relatively easy to study multiple levels of list length in the experiments described above and in section 14.2.2 because this factor could be varied randomly from trial to trial.
25. The issue of invariance of the list-length effect (assessed by goodness of fit of parallel curves with any shape) can be evaluated separately from the issue of its specific functional

form, in this case, linearity (assessed by goodness of fit of linear functions). While the former issue bears on the effect of practice on the memory-interrogation process, the latter issue bears on the nature of that process. Although the deviations from linearity are small (mean of the absolute deviations from the fitted lines is only 1.6 ms), they are all in the same direction (concave down). For the four sets of data separately, from top to bottom, mean absolute deviations from linearity are 1.2, 3.6, 1.4, and 0.2 ms, respectively.

26. Because at least in principle a change in *IH* level could reduce the durations of **A** and **B** while increasing the duration of **T** by requiring interhemisphere transmission, application of the subtraction method could actually produce a negative estimate of the transmission time.
27. A stroke is the interruption of the blood supply to a region of the brain, often because a blood vessel becomes blocked, which kills neurons in that region.
28. This task was modeled on the Wisconsin Card-Sorting Test (which is sometimes used to evaluate the effects of brain damage), in which each card must be placed on one of four piles depending on which target it matches.
29. Insofar as the $\overline{RT}$ for a subject in the pure-task condition is subject to sampling error (see Wickens, chap. 12, this volume), selection of subjects on the basis of short $\overline{RT}$s could mean that their $\overline{RT}$s in that condition are biased low. Thus we might have selected not the patients who have the characteristic of producing normal pure-task performance, but the ones whose *RT*s happened to be short on that particular occasion on which it was measured. Insofar as the $\overline{RT}$ is a reliable measurement of the subject, however, the bias due to selection would be minor. Inspection of the data from a related experiment with the same subjects, in which they were measured twice, suggests that the measure is quite reliable.
30. Other experiments showed that the production unit on which the effect depends is a group of syllables associated with a primary stress (a *stress group*), rather than the word (Sternberg et al. 1988, section 3.2). For example, "one and two and three" contains three units, not five.
31. The mean unit duration is determined as follows: Start with the relation between utterance duration D_n and utterance length n, which has been shown to be an accelerating quadratic function under these rapid-speech conditions: $D_n = \alpha + \beta n + \gamma n(n-1)$, $(\gamma > 0)$. Fit such a function to get the estimates $\hat{\alpha}$, $\hat{\beta}$, and $\hat{\gamma}$, correct D_n for end effects (such as the lengthening of the final syllable of an utterance) by subtracting $\hat{\alpha}$, divide by the number of units n, and add $\hat{\gamma}$, which gives, for the mean unit duration, $\hat{\gamma} + (D_n - \hat{\alpha})/n$. Insofar as the duration D_n is a quadratic function of n, the mean unit duration is then $\beta + \gamma n$, which increases linearly with n, as has been observed in several experiments (Sternberg et al. 1988).
32. The selection stage for the first unit occurs before the utterance begins.
33. Another explanation of why time increases with the disparity in orientation (or size) is that high-level shape features are extracted from each stimulus, matching is based on comparison of feature lists, and the extraction or comparison operations (or both) are slowed by an amount that increases with the disparity. If such other mechanisms are available, comparisons by template matching may not be necessary for all same-different judgments. The task of judging whether a single character is normal or mirror-reversed is more sensitive to orientation than the task of identifying it; this suggests that when the "different" patterns in a same-different task are mirror images (as in the experiment to be described), template matching is especially likely to be used.
34. This could be described as the "multiplexing" of transformations, as in multiplexed signal transmission over one telephone line. Given that invariance of the effect of each factor is observed over levels of the other that include zero disparity (where multiplexing

would not occur because one of the transformations would be null), this claim might require us to believe that there was no "overhead" associated with switching between one kind of transformation and the other.

35. In the model of figure 14.24 we have a set of similar substages that are data-dependent, each substage after the first starting with information (an oriented image) developed during the previous substage. For example, suppose the initial image is at 150°. Stage *R*(*d*), during which sector *d* is traversed, takes as its input an image with orientation 120°, which is not available until the initial image is rotated through sector *e* during stage *R*(*e*). Sections 14.5.14 and 14.5.15 discuss models with sequences of substages that are similar to each other but not data-dependent.
36. This approach to testing invariance, based on data generously provided by Claus Bundesen and Axel Larsen, differs from their approach of fitting a regression model to the full set of data.
37. Mean durations of the fast group (sectors *a, b, c, e, A*) ranged from 26 to 34 ms, of the medium group (sectors *d, B, C, F*) from 53 to 56 ms, and of the slow group (sectors *f, D, E*) from 67 to 94 ms.
38. For the subset of data we have been considering, in which target and probe are equal in size, the values of $\overline{RT}$ for 30°, 60°, and 90° are 485.5, 531.2, and 586.5 ms, respectively. The adjacent differences increase by 9.6 ms, as expected from the magnitude of the interaction.
39. See section 14.5.10 for a study in which repetition priming of words was found to influence an encoding process.
40. Although this task is unfamiliar to subjects as such, it seems to depend on natural-language mechanisms. Good performance requires little laboratory practice, and *RT*s are sensitive to factors known to be important in tasks more like natural language use.
41. If the effect of *FRQ* were always fully eliminated by repetition, this might indicate that repetition caused deletion of stage **B**.
42. The small residual effect may be due to the confounding of other word attributes with *FRQ*, mentioned above.
43. In the first experiment, displays were twice as large, perhaps increasing the chance of contamination from eye movements, and also permitting the argument that subjects were forced by the unusual display size to process strings sequentially. Results were similar to those of experiment 2, but not as pretty, with $\overline{RT} = 1047$, 1118, 1109, and 1157 ms for conditions *PP, UP, PU*, and *UU*, respectively.
44. In section 14.5.8.2 we noted the duration differences among the substages for traversal of individual sectors in mental rotation, which mean that these substages are not equivalent. However, we also saw (in comment 12) how appropriate balancing and averaging can create a situation in which means of substage durations are equal, yielding substage equivalence and linearity.
45. See also section 2.4 of volume 2 of the present series.
46. This assumption can be questioned. Consider a display containing the digits "8" and "3," and suppose the "8" has been identified. To conclude that it is the largest requires determining only that the other digit is not a "9," which may not require it to be identified as a 3.
47. The asterisk is used to distinguish the stage containing a sequence of substages from the substages themselves.
48. If items differ from each other, then achieving substage equivalence requires suitable balancing or randomizing. Ideally, then, the distribution of digits should be the same for each serial position within a display and for displays of each size. One problem associated with the task of naming the largest digit is that this requirement cannot be achieved without permitting repeated digits in the larger displays, which may generate interpretive problems of its own, because of repetition priming, for example.

49. This interpretation (Sternberg 1966, 1967, 1969b, 1975) is now controversial; see, for example, McElree and Dosher 1989, and Greene 1992, chapter 5.
50. See Dosher, chap. 10, this volume, on models of visual search.
51. The mean absolute deviation is .059 sec, only 0.7 percent of the range of search times.
52. A plausible model of the search time is $T = \alpha + n_{test}(\beta + \gamma n_{targ}) = \alpha + \beta n_{test} + \gamma n_{test} n_{targ}$, where n_{test} is the number of test stimuli, n_{targ} the number of targets, and γ the average over matches and mismatches of the time to compare a stimulus to one target. As $n_{test} =$ 30, the slope of the linear function relating T to n_{targ} is 30γ. The mean of the observed slopes in figure 14.41A is 0.86 sec/target. Division by 30 provides an estimate of 29 ms for the comparison time, γ, not far from the time per comparison estimated from the item-recognition data discussed in section 14.5.3.
53. As Chase (1969) pointed out, we should be concerned about a possible artifactual source of this interaction: preview permits the window to be moved more rapidly over the row of patterns. It is possible that the stimulus patterns are degraded less when the stippling "noise" is moved more rapidly over them. However, in some recent experiments on the effects of preview on a nonsearch task, Pashler (1994b) found that manipulating letter legibility by varying visual intensity, which is not subject to this possible artifact, had an effect that interacted with preview, which suggests that Chase's finding is valid.
54. Other examples of statements that could encourage this misinterpretation of stage models include Gopher and Sanders 1984, 231: "Processing stage models are primarily interested in describing the flow of information through the organism ..."; and Rabbitt and Maylor 1991, 277: "'Arrows' indicate the direction of information flow between hypothetical subsystems ('Boxes')...."
55. I challenge the reader to think of any claims of seriality for processes that are not data-dependent and carry out functions that are qualitatively different. I know of none.
56. Roberts (1993) describes this as the "method of independent measures." See also his section 2.2, this volume.
57. The corresponding traditional terms are d' for discrimination acuity and β for the decision criterion.
58. In a variant of this kind of process, the input is a stream of pulses, a weight associated with a module is the probability it will transmit a pulse, and a response is produced for each output pulse or for each occurrence of some fixed number of output pulses.
59. See comment 20 (section 14.6.6) for an explanation of why physical time is the appropriate measure of T_{AB}.
60. This is an ideal description. In the case of real, fallible data, a null interaction is not distinguishable from a small one (how small depends on the precision of the experiment), which weakens the statement.
61. For the idea of a distribution and a discussion of measures of spread such as the variance, see Wickens, chapter 12, this volume.
62. For the idea of a mental-processing architecture, see chapter 6, this volume.
63. Roberts and Sternberg (1993) show that the AP model is equivalent to a special case of the G-stage model, where **A** and **B** are interpreted as stages rather than alternative pathways; this equivalence renders the additivity of factor effects unsurprising. To create the equivalence, they define a hypothetical third factor not controlled by the experimenter that interacts in a special way with both F and G: it nullifies the effect of G on stage **B** on a proportion p of the trials and nullifies the effect of F on stage **A** on the remaining trials. This reinterpretation is helpful as a mathematical trick, but sufficiently implausible to justify our regarding the AP model as a nonstage architecture.
64. For a discussion of the speed-accuracy trade-off, see appendix 1 of chapter 9, this volume.
65. An introduction to the problem of sampling variability and some of the methods of statistics used to cope with it is provided by Wickens, chapter 12, this volume. Wickens

also introduces the confidence interval, the null hypothesis, the power of statistical tests, and the analysis of variance, all mentioned in the present section.

References

Abrams, R. A., and Balota, D. A. (1991). Mental chronometry: Beyond reaction time. *Psychological Science* 2, 153–157.

Alegria, J., and Bertelson, P. (1970). Time uncertainty, number of alternatives and particular signal-response pair as determinants of choice reaction time. In A. F. Sanders (Ed.), *Attention and performance III. Acta Psychologica* 33, 36–44.

Allport, A., Styles, E. A., and Hsieh, S. (1994). Shifting intentional set: Exploring the dynamic control of tasks. In C. Umilta and M. Moscovitch (Eds.), *Attention and performance XV: Conscious and nonconscious information processing*, pp. 421–452. Cambridge, MA: MIT Press.

Ashby, F. G. (1982). Deriving exact predictions from the cascade model. *Psychological Review* 89, 599–607.

Ashby, F. G., and Townsend, J. T. (1980). Decomposing the reaction time distribution: Pure insertion and selective influence revisited. *Journal of Mathematical Psychology* 21, 93–123.

Bashore, T. R. (1981). Vocal and manual reaction time estimates of interhemispheric transmission time. *Psychological Bulletin* 89, 352–368.

Becker, C. A. (1979). Semantic context and word frequency effects in visual word recognition. *Journal of Experimental Psychology: Human Perception and Performance* 5, 252–259.

Beckers, G., and Homberg, V. (1991). Impairment of visual perception and visual short-term memory scanning by transcranial magnetic stimulation of occipital cortex. *Experimental Brain Research* 87, 421–432.

Bertelson, P., and Barzeele, J. (1965). Interaction of time-uncertainty and relative signal frequency in determining choice reaction time. *Journal of Experimental Psychology* 70, 448–451.

Borowsky, R., and Besner, D. (1993). Visual word recognition: A multistage activation model. *Journal of Experimental Psychology: Learning, Memory, and Cognition* 19, 813–840.

Bricker, P. D. (1955). Information measurement and reaction time. In H. Quastler (Ed.), *Information theory in psychology*, pp. 350–359. Glencoe, IL: Free Press.

Broadbent, D. E. (1958). *Perception and communication*. New York: Pergamon Press.

Broadbent, D. E. (1984). The Maltese cross: A new simplistic model for memory. *Behavioral and Brain Sciences* 7, 55–94.

Broadbent, D. E., and Gregory, M. (1965). On the interaction of S-R compatibility with other variables affecting reaction time. *British Journal of Psychology* 56, 61–67.

Brown, W. S., Larson, E. B., and Jeeves, M. A. (1994). Directional asymmetries in interhemispheric transmission time: Evidence from visual evoked potentials. *Neuropsychologia* 32, 439–448.

Bundesen, C., and Larsen, A. (1996). Personal communication, January 8.

Bundesen, C., Larsen, A., and Farrell, J. E. (1981). Mental transformations of size and orientation. In J. Long and A. Baddeley (Eds.), *Attention and performance IX*, pp. 279–294. Hillsdale, NJ: Erlbaum.

Bundesen, C., Larsen, A., and Farrell, J. E. (1983). Visual apparent movement: transformation of size and orientation. *Perception* 12, 549–558.

Cattell, J. M. (1885). Über die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben. *Philosophische Studien* 2, 635–650. Translated as "On the time required for recognizing and naming letters and words, pictures and colors" by R. S. Woodworth. In *James McKeen Cattell: Man of science*, pp. 13–25. Lancaster, PA: Science Press, 1947.

Chase, W. G. (1969). Parameters of visual and memory search. Ph.d. diss., University of Wisconsin.

Cooper, L. A. (1976). Demonstration of a mental analog of an external rotation. *Perception & Psychophysics* 19, 296–302.

De Jong, R. (1993). Multiple bottlenecks in overlapping task performance. *Journal of Experimental Psychology: Human Perception and Performance* 19, 965–980.

Dinges, D. F., and Kribbs, N. B. (1991). Performing while sleepy: Effects of experimentally-induced sleepiness. In T. H. Monk (Ed.), *Sleep, sleepiness and performance*, pp. 97–128. London: Wiley.

Donders, F. C. (1868). Over de snelheid van psychische processen. *Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool* 2, 92–120. Translated as "On the speed of mental processes" by W. G. Koster. In Koster (Ed.), *Attention and performance II. Acta Psychologica* 30 (1969), 412–431.

Dunn, J. C., and Kirsner, K. (1988). Discovering functionally independent mental processes: The principle of reversed association. *Psychological Review* 95, 91–101.

Egeth, H., and Dagenbach, D. (1991). Parallel versus serial processing in visual search: Further evidence from subadditive effects of visual quality. *Journal of Experimental Psychology: Human Perreption and Performance* 17, 551–560.

Farah, M. J., and Hammond, K. M. (1988). Mental rotation and orientation-invariant object recognition: Dissociable processes. *Cognition* 29, 29–46.

Fisher, R. A. (1926). The arrangement of field experiments. *Journal of the Ministry of Agriculture of Great Britain* 33, 503–513.

Fisher, R. A. (1935). *The design of experiments*. Edinburgh: Oliver and Boyd.

Fodor, J. A. (1983). *The modularity of mind*. Cambridge, MA: MIT Press.

Frowein, H. W., and Sanders, A. F. (1978). Effects of visual stimulus degradation, S-R compatibility, and foreperiod duration on choice reaction time and movement time. *Bulletin of the Psychonomic Society* 12, 106–108.

Gould, J. D. (1973). Eye movements during visual search and memory search. *Journal of Experimental Psychology* 98, 184–195.

Gopher, D., and Sanders, A. F. (1984). S-Oh-R: Oh Stages! Oh Resources! In W. Prinz and A. F. Sanders (Eds.), *Cognition and motor processes*, pp. 231–253. Berlin: Springer.

Greene, R. L. (1992). *Human memory: Paradigms and paradoxes*. Hillsdale, NJ: Erlbaum.

Harel, D. (1992). *Algorithmics: The spirit of computing*, 2d ed. Reading, MA: Addison-Wesley.

Helmholtz, H. von (1850). Messungen über den zeitlichen Verlauf der Zuckung animalischer Muskeln und die Fortpflanzungsgeschwindigkeit der Reizung in den Nerven. *Müller's Archiv für Anatomie, Physiologie, und wissenschaftliche Medizin*, 276–364.

Hick, W. E. (1952). On the rate of gain of information. *Quarterly Journal of Experimental Psychology* 4, 11–26.

Howson, C., and Urbach, P. (1993). *Scientific reasoning: The Bayesian approach*. 2d ed. La Salle, IL: Open Court.

Humphrey, D. G., Kramer, A. F., and Stanny, R. R. (1994). Influence of extended wakefulness on automatic and nonautomatic processing. *Human Factors* 36, 652–669.

Hurlbert, A., and Poggio, T. (1985). Spotlight on attention. *Trends in Neurosciences* 8, 309–311.

Hyman, R. (1953). Stimulus information as a determinant of reaction times. *Journal of Experimental Psychology* 45, 188–196.

Jaskowski, P., Pruszewicz, A., and Swidzinski, P. (1990). VEP latency and some properties of simple motor reaction-time distribution. *Psychological Research* 52, 28–34.

Johnson-Laird, P. N. (1983). *Mental models*. Cambridge, MA: Harvard University Press.

Jolicoeur, P. (1988). Mental rotation and the identification of disoriented objects. *Canadian Journal of Psychology* 42, 461–478.

Karlin, L., and Kestenbaum, R. (1968). Effects of number of alternatives on the psychological refractory period. *Quarterly Journal of Experimental Psychology* 20, 167–178.

Kolers, P. (1975). Specificity of operations in sentence recognition. *Cognitive Psychology* 37, 289–306.

Kornblum, S., and Lee, J.-W. (1995). Stimulus-response compatibility with relevant and irrelevant stimulus dimensions that do and do not overlap with the response. *Journal of Experimental Psychology: Human Perception and Performance* 21, 855–875.

Kristofferson, M. W. (1972). Effects of practice on character-classification performance. *Canadian Journal of Psychology* 26, 54–60.

Larsen, A., and Bundesen, C. (1978). Size scaling in visual pattern recognition. *Journal of Experimental Psychology: Human Perception and Performance* 4, 1–20.

Luce, R. D. (1960). The theory of selective information and some of its behavioral applications. In Luce (Ed.), *Developments in mathematical psychology: Information, learning, tracking*, pp. 1–119. Glencoe, IL: Free Press.

Luce, R. D. (1986). *Response times: Their role in inferring elementary mental organization*. New York: Oxford University Press.

Mansfield, R. J. W. (1973). Latency functions in human vision. *Vision Research* 13, 2219–2234.

Marr, D. (1976). Early processing of visual information. *Philosophical Transactions of the Royal Society of London* B275, 483–524.

McCann, R., and Johnston, J. C. (1989). The locus of processing bottlenecks in the overlapping-tasks paradigm. Paper presented at the meeting of the Psychonomic Society, Atlanta.

McClelland, J. L. (1979). On the time relations of mental processes: An examination of systems of processes in cascade. *Psychological Review* 86, 287–330.

McElree, B., and Dosher, B. A. (1989). Serial position and set size in short-term memory: The time course of recognition. *Journal of Experimental Psychology: General* 118, 346–373.

Merkel, J. (1885). Die zeitlichen Verhältnisse der Willensthätigkeit. *Philosophische Studien* 2, 73–127.

Meyer, D. E., and Kieras, D. E. (1997a). A computational theory of executive cognitive processes and multiple-task performance: Part 1. Basic mechanisms. *Psychological Review* 104, 3–65.

Meyer, D. E., and Kieras, D. E. (1997b). A computational theory of executive cognitive processes and multiple-task performance: Part 2. Accounts of psychological refractory-period phenomena. *Psychological Review* 104.

Meyer, D. E., Kieras, D. E., Lauber, E., Schumacher, E. H., Glass, J., Zurbriggen, E., Gmeindl, L., and Apfelblat, D. (1995). Adaptive executive control: Flexible human multiple-task performance without pervasive immutable response-selection bottlenecks. *Acta Psychologica* 90, 163–190.

Meyer, D. E., Schvaneveldt, R. W., and Ruddy, M. G. (1975). Loci of contextual effects on visual word recognition. In P. M. A. Rabbitt and S. Dornic (Eds.), *Attention and performance V*, pp. 98–118. London: Academic Press.

Miller, J. (1988). Discrete and continuous models of human information processing: Theoretical distinctions and empirical results. *Acta Psychologica* 67, 191–257.

Miller, J. O. (1993). A queue-series model for reaction time, with discrete-stage and continuous-flow models as special cases. *Psychological Review* 100, 702–715.

Miller, J., Van der Ham, F., and Sanders, A. F. (1995). Overlapping stage models and reaction time additivity: Effects of the activation equation. *Acta Psychologica* 90, 11–28.

Monsell, S. (1985). Repetition and the lexicon. In A. W. Ellis (Ed.), *Progress in the psychology of language*, vol. 2, pp. 147–195. Hillsdale, NJ: Erlbaum.

Monsell, S. (1991). The nature and locus of word frequency effects in reading. In D. Besner and G. W. Humphreys (Eds.), *Basic processes in reading: Visual word recognition*, pp. 148–197. Hillsdale, NJ: Erlbaum.

Mulder, G., Sanders, A. F., and Van Galen, G. P. (1995). *Discrete and continuous information processing. Acta Psychologica* 90, 1–351.

Neisser, U. (1976). *Cognition and reality*. San Francisco: Freeman.

O'Boyle, M. W., and Hellige, J. B. (1982). Hemispheric asymmetry, early visual processes, and serial memory comparison. *Brain and Cognition* 1, 224–243.

Ollman, R. T. (1968). Central refractoriness in simple reaction time: The deferred processing model. *Journal of Mathematical Psychology* 5, 49–60.

Pachella, R. G. (1974). The interpretation of reaction time in information-processing research. In B. H. Kantowitz (Ed.), *Human information processing: Tutorials in performance and cognition*, pp. 41–82. Hillsdale, NJ: Erlbaum.

Pantzer, T. (1997). Parallel and serial operations in character identification. Ph.d. diss., University of Pennsylvania.

Pantzer, T., and Sternberg, S. (1992). Parallel and serial operations in character identification. Paper presented at the Psychonomic Society Meeting, St. Louis. November.

Pashler, H. (1984). Processing stages in overlapping tasks: Evidence for a central bottleneck. *Journal of Experimental Psychology: Human Perception and Performance* 10, 358–377.

Pashler, H. (1994a). Dual-task interference in simple tasks: Data and theory. *Psychological Bulletin* 116, 220–244.

Pashler, H. (1994b). Overlapping mental operations in serial performance with preview. *Quarterly Journal of Experimental Psychology* 47A, 161–191.

Pashler, H. (1994c). Graded capacity sharing in dual-task interference? *Journal of Experimental Psychology: Human Perception and Performance* 20, 330–342.

Pashler, H., and Badgio, P. C. (1985). Visual attention and stimulus identification. *Journal of Experimental Psychology: Human Perception and Performance*, 11, 105–121.

Pashler, H., and Johnston, J. C. (1989). Chronometric evidence for central postponement in temporally overlapping tasks. *Quarterly Journal of Experimental Psychology* 41A, 19–45.

Poggio, T. (1983). Visual algorithms. In O. J. Braddick and A. C. Sleigh (Eds.), *Physical and biological processing of images*, pp. 128–153. Berlin: Springer-Verlag.

Pons, T. P., Garraghty, P. E., Friedman, D. P., and Mishkin, M. (1987). Physiological evidence for serial processing in somatosensory cortex. *Science* 237, 417–420.

Pons, T. P., Garraghty, P. E., and Mishkin, M. (1992). Serial and parallel processing of tactual information in somatosensory cortex of rhesus monkeys. *Journal of Neurophysiology* 68, 518–527.

Rabbitt, P. M., and Maylor, E. A. (1991). Investigating models of human performance. *British Journal of Psychology* 82, 259–290.

Rayner, K., and Fisher, D. L. (1987). Letter processing during eye fixations in visual search. *Perception & Psychophysics* 42, 87–100.

Roberts, S. (1987). Evidence for distinct serial processes in animals: The multiplicative-factors method. *Animal Learning & Behavior* 15, 135–173.

Roberts, S. (1993). Use of independent and correlated measures to divide a time-discrimination mechanism into parts. In D. E. Meyer and S. Kornblum (Eds.), *Attention and performance XIV: Synergies in experimental psychology, artificial intelligence, and cognitive neuroscience*, pp. 589–610. Cambridge, MA: MIT Press.

Roberts, S., and Sternberg, S. (1993). The meaning of additive reaction-time effects: Tests of three alternatives. In D. E. Meyer and S. Kornblum (Eds.) *Attention and performance*

XIV: Synergies in experimental psychology, artificial intelligence, and cognitive neuroscience, pp. 611–653. Cambridge, MA: MIT Press.

Rogers, R., and Monsell, S. (1995). Costs of a predictable switch between simple cognitive tasks. *Journal of Experimental Psychology: General* 124, 207–231.

Rouanet, H., Lépine, D., and Holender, D. (1978). Model acceptability and the use of Bayes-fiducial methods for validating models. In J. Requin (Ed.), *Attention and performance VII,* pp. 687–701. Hillsdale, NJ: Erlbaum.

Rubinstein, J., Meyer, D. E., and Evans, J. E. (forthcoming). Executive control of cognitive processes in task switching. *Journal of Experimental Psychology: Human Perception and Performance.*

Rubinstein, J., Evans, J. E., and Meyer, D. E. (forthcoming). The control of task switching in patients with prefrontal and parietal cortex damage.

Rumelhart, D. E. (1989). The architecture of mind: A connectionist approach. In M. I. Posner (Ed.), *Foundations of cognitive science,* pp. 133–159. Cambridge, MA: MIT Press.

Sanders, A. F. (1977). Structural and functional aspects of the reaction process. In S. Dornic, (Ed.), *Attention and performance VI,* pp. 3–25. Hillsdale, NJ: Erlbaum.

Sanders, A. F. (1980a). Some effects of instructed muscle tension on choice reaction time and movement time. In R. S. Nickerson (Ed.), *Attention and performance VIII,* pp. 59–74. Hillsdale, NJ: Erlbaum.

Sanders, A. F. (1980b). Stage analysis of reaction processes. In G. E. Stelmach and J. Requin (Eds.), *Tutorials in motor behavior,* pp. 331–354. Amsterdam: North-Holland.

Sanders, A. F. (1990). Issues and trends in the debate on discrete vs. continuous processing of information. *Acta Psychologica* 74, 123–167.

Sanders, A. F., Wijnen, J. L. C., and Van Arkel, A. E. (1982). An additive factor analysis of the effects of sleep loss on reaction processes. *Acta Psychologica* 51, 41–59.

Scarborough, D. L., Cortese, C., and Scarborough, H. S. (1977). Frequency and repetition effects in lexical memory. *Journal of Experimental Psychology: Human Perception and Performance* 3, 1–17.

Scarborough, D. L., and Landauer, T. K. (1981). *Lexical decisions about pairs of adjacent words: A reaction time analysis.* Bell Laboratories Technical Memorandum TM81-11221-5. September.

Schweickert, R. (1978). A critical-path generalization of the additive-factor method: Analysis of a Stroop task. *Journal of Mathematical Psychology* 18, 105–139.

Schweickert, R. (1983). Latency network theory: Scheduling of processes in sentence verification and the Stroop effect. *Journal of Experimental Psychology: Learning, Memory, and Cognition* 9, 353–383.

Schweickert, R. (1985). Separable effects of factors on speed and accuracy: Memory scanning, lexical decision, and choice tasks. *Psychological Bulletin* 97, 530–546.

Schweickert, R. (1993). Information, time, and the structure of mental events: A twenty-five year review. In D. E. Meyer and S. Kornblum (Eds.), *Attention and performance XIV: Synergies in experimental psychology, artificial intelligence, and cognitive neuroscience,* pp. 535–566. Cambridge, MA: MIT Press.

Schweickert, R., and Townsend, J. T. (1989). A trichotomy: Interactions of factors prolonging sequential and concurrent mental processes in stochastic discrete mental (PERT) networks. *Journal of Mathematical Psychology* 33, 328–347.

Shallice, T. (1988). *From neuropsychology to mental structure.* Cambridge: Cambridge University Press.

Shannon, C. (1948). A mathematical theory of communication. *Bell System Technical Journal* 27, 379–423 and 623–656.

Shepard, R. N., and Cooper, L. A. (1982). *Mental images and their transformations.* Cambridge, MA: MIT Press.

Smith, G. A. (1980). Models of choice reaction time. In A. T. Welford (Ed.), *Reaction times*, pp. 173–214. London: Academic Press.

Sperling, G., Budiansky, J., Spivak, J. G., and Johnson, M. C. (1971). Extremely rapid visual search: The maximum rate of scanning letters for the presence of a numeral. *Science* 174, 307–311.

Sternberg, S. (1966). High-speed scanning in human memory. *Science* 153, 652–654.

Sternberg, S. (1967). Two operations in character-recognition: Some evidence from reaction-time measurements. *Perception & Psychophysics* 2, 45–53.

Sternberg, S. (1969a). The discovery of processing stages: Extensions of Donders' method. In W. G. Koster, (Ed.), *Attention and performance II. Acta Psychologica* 30, 276–315.

Sternberg, S. (1969b). Memory-scanning: Mental processes revealed by reaction-time experiments. *American Scientist* 57, 421–457.

Sternberg, S. (1975). Memory scanning: New findings and current controversies. *Quarterly Journal of Experimental Psychology* 27, 1–32.

Sternberg, S. (1984). Stage models of mental processing and the additive-factor method. *Behavioral and Brain Sciences* 7, 82–84.

Sternberg, S., Knoll, R. L., Monsell, S., and Wright, C. E. (1988). Motor programs and hierarchical organization in the control of rapid speech. In Fujimura, O. (Ed.), *Articulatory organization: From phonology to speech signals. Phonetica* 45, 175–197.

Sternberg, S., Knoll, R. L., and Turock, D. L. (1990). Hierarchical control in the execution of action sequences: Tests of two invariance properties. In M. Jeannerod (Ed.), *Attention and performance XIII*, pp. 3–55. Hillsdale, NJ: Erlbaum.

Sternberg S., and Scarborough, D. L. (1971). Parallel testing of stimuli in visual search. In *Visual information processing and control of motor activity*. Sofia: Bulgarian Academy of Sciences, pp. 179–188.

Stoffels, E.-J., Van der Molen, M. W., and Keuss, P. J. G., Eds. (1990). *Stage analysis of the reaction process: Models, methods, and applications. Acta Psychologica* 74, 111–320.

Stolz, J. A., and Neely, J. H. (1995). When target degradation does and does not enhance semantic context effects in word recognition. *Journal of Experimental Psychology: Learning, Memory, and Cognition* 21, 596–611.

Tarr, M. J., and Pinker, S. (1989). Mental rotation and orientation-dependence in shape recognition. *Cognitive Psychology* 21, 233–282.

Taylor, D. H. (1966). Latency components in two-choice responding. *Journal of Experimental Psychology* 72, 481–487.

Townsend, J. T. (1974). Issues and models concerning the processing of a finite number of inputs. In B. H. Kantowitz (Ed.), *Human information processing: Tutorials in performance and cognition*, pp. 133–185. Hillsdale, NJ: Lawrence Erlbaum Associates.

Townsend, J. T. (1984). Uncovering mental processes with factorial experiments. *Journal of Mathematical Psychology* 28, 363–400.

Townsend, J. T., and Schweickert, R. (1989). Toward the trichotomy method of reaction times: Laying the foundation of stochastic mental networks. *Journal of Mathematical Psychology* 33, 309–327.

Ulrich, R., and Stapf, K. H. (1984). A double-response paradigm to study stimulus intensity effects upon the motor system in simple reaction time experiments. *Perception & Psychophysics* 36, 545–558.

Van der Molen, M. W., Bashore, T. R., Halliday, R., and Callaway, E. (1991). Chronopsychophysiology: Mental chronometry augmented by psychophysiological time markers. In J. R. Jennings and M. G. H. Coles (Eds.), *Handbook of cognitive psychophysiology: Central and autonomic nervous system approaches*, pp. 9–178. Chichester: Wiley.

Van Essen, D. C., Anderson, C. H., and Felleman, D. J. (1992). Information processing in the primate visual system: An integrated systems perspective. *Science* 255, 419–423.

Van Essen, D. C., and Maunsell, J. H. R. (1983). Hierarchical organization and functional streams in the visual cortex. *Trends in Neurosciences* 6, 370–375.

Vaughan, H. G., Jr., Costa, L. D., and Gilden, L. (1966). The functional relation of visual evoked response and reaction time to stimulus intensity. *Vision Research* 6, 645–656.

Vuorinen, R. (1989). *Latency determination and temporal filtering of retinal signals in photopic vision. Annales Academiae Scientiarum Fennicae* B235. Helsinki: Suomalainen Tiedeakatemia.

Welford, A. T. (1980). The single-channel hypothesis. In A. T. Welford (Ed.), *Reaction times*, pp. 215–252. London: Academic Press.

Woodworth, R. S. (1938). *Experimental psychology*. New York: Holt.

Young, M. P. (1992). Objective analysis of the topological organization of the primate cortical visual system. *Nature* 358, 152–155.