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# An Interactive Race Model of Divided Attention

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Two classes of models have been proposed to explain how redundant information extracted from separate sources comes to activate a single response. Each provides a fundamentally different account of why responses to redundant signals are typically faster than those to either signal alone (the *redundant-signals effect*). Independent race models assume that a race occurs between perceptual codes on independent channels and that only the winner activates the response. Coactivation models assume that there is some form of energy or activation-strength summation, with information being pooled across channels prior to decision. An intermediate class of models is introduced and a specific exemplar, the interactive race model, is tested in a series of redundant-target detection experiments. In particular, we examine the effects on performance of two types of contingency that have previously been overlooked as sources of task-relevant information. The results reveal that response times are significantly influenced by both interstimulus and stimulus–response contingencies. The interactive race model provides a natural account of these findings as well as several otherwise puzzling results in the divided-attention literature.

There is considerable evidence that subjects are capable of dividing their attention among several separate sources of information (e.g., Duncan, 1984; Pashler, 1989; Shaw, 1978; van der Heijden, 1975; van der Heijden, La Heij, & Boer, 1983). However, the manner in which the extracted information is combined or integrated to activate the appropriate response has been a matter of some debate (Green & Swets, 1966; Grice, Canham, & Boroughs, 1984; Kinchla & Collyer, 1974; Miller, 1982b; Shaw, 1982; van der Heijden, Schreuder, Maris, & Neerincx, 1984). Both of these issues—parallel processing and the integration of information—have recently been studied with the redundant-target detection paradigm. This task requires subjects to press a response key when a predesignated visual target (e.g., the letter *X*) appears in a display consisting of one or two stimuli. When targets appear in both locations (the redundant-target condition), response times are typically faster than when only one target is present (e.g., Holmgren, Juola, & Atkinson, 1974; van der Heijden, 1975), a result known as a *redundancy gain* or *redundant-signals effect.*

Two classes of models have been advanced as explanations for the redundant-signals effect (see Miller, 1982b, for a review). The first account, called the *independent race model* (Figure 1, top panel), holds that each display element is encoded by one of several parallel channels and that a positive response is triggered as soon as a target-present decision is made by any one channel (in Figure 1, the finishing line for this race is denoted by *OR*). This model specifically asserts that the information extracted from separate sources is kept strictly independent. The issue of information integration is thereby sidestepped, because it is argued that information is never actually combined.

The independent race model explains the redundant-signals effect in terms of probability summation or "statistical facilitation" (Raab, 1962). If the distribution of finishing times for each of the two single targets overlap, then the mean finishing time of the winning decision process on redundant-target trials will be faster than either of the two single-target means.

According to the second account, the separate decision mechanisms of the independent race model are replaced by a single decision mechanism (Figure 1, bottom panel), and the information from multiple sources is integrated (denoted by $\Sigma$ in Figure 1) prior to this point. Such activation-strength summation was referred to by Miller (1982b) as *coactivation*, and models incorporating coactivation are the primary alternatives to the independent race model (see also Green & Swets, 1966; Shaw, 1982).

The way in which coactivation models explain the redundant-signals effect is quite different from the statistical facilitation of the independent race model. Positive (target-present) activations do not race against each other but are pooled across channels into a single value. Total activation builds faster when there are two targets present than when there is only one, resulting in faster responses to redundant-target displays (for an explicit model of this type, see Schwarz, 1989).

These two classes of models—independent race and coactivation—are both consistent with the redundant-signals effect, so the presence of a redundancy gain cannot by itself be used to discriminate between them. However, the two models make different predictions concerning the relationships among the entire distributions of response times to single- and redundant-target displays (Miller, 1982b, 1986; Mulligan & Shaw, 1980; Shaw, 1982; see also Colonius, 1986, 1988,

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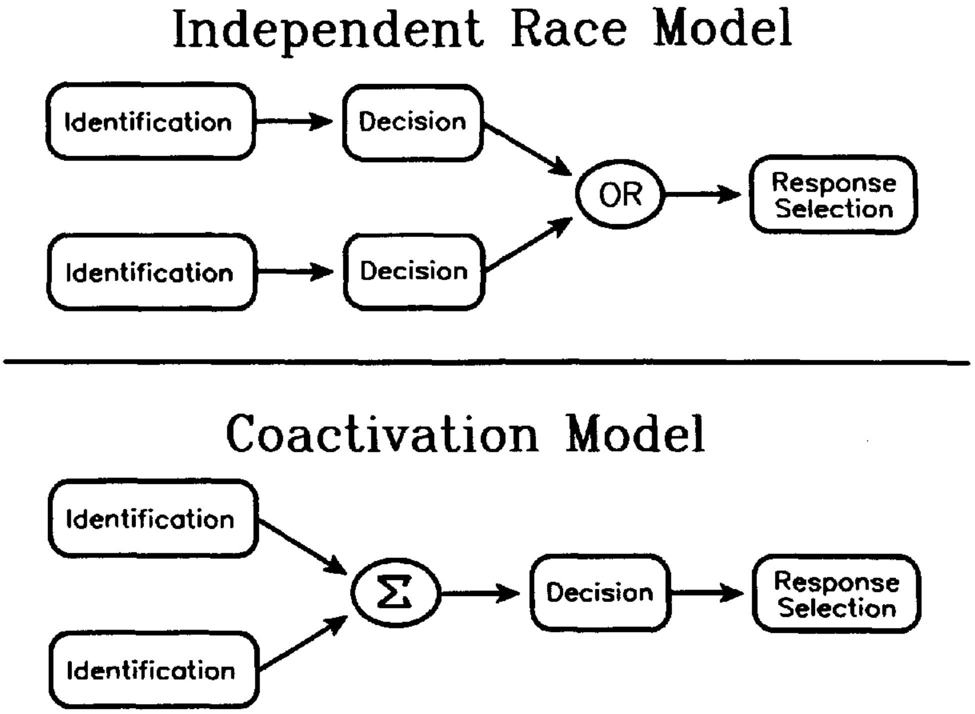

*Figure 1.* Schematic diagrams of the independent race model and the coactivation model. (See text for details.)

1990; Ulrich & Giray, 1986). For example, the independent race model cannot produce a redundancy gain that is larger than that predicted by statistical facilitation. Thus, if the probability of a target in Channel 1 activating a response by time $t$ is $p_1$, and the probability of a target in Channel 2 activating a response by time $t$ is $p_2$, then the probability of a response by time $t$ given redundant targets must be less than or equal to $p_1 + p_2$. More generally, Miller (1982b) showed that the independent race model must satisfy the following inequality:

$$P(\mathrm{RT} < t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)}) \leq P(\mathrm{RT} < t \mid \mathrm{T}^{(1)}) + P(\mathrm{RT} < t \mid \mathrm{T}^{(2)}), \quad (1)$$

where RT is response time, $t$ is time since stimulus onset, and T is a target in the superscripted spatial location. Thus, for example, $P(\mathrm{RT} < t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)})$ is the cumulative distribution function (CDF) for redundant targets, which is defined as the probability that a response occurred before time $t$ given targets in both Locations 1 and 2.

Inequality 1, also known as the *race-model inequality*, must be satisfied by all models that assume separate activations, parallel decisions, and a race to activate a response (on redundant-target trials). Thus, the independent race model must obey this rule, but coactivation models need not obey it. Using this logic, Miller (1982b) conducted a series of redundant-target detection experiments and tested for violations of Inequality 1. Many violations were observed, prompting Miller to reject the independent race model in favor of coactivation.

Coactivation is not the only alternative to the independent race model, however. Other models that retain the separate activations or race aspect of the independent race model but relax or abandon the assumption of channel independence are also viable in the face of violations of the race-model inequality. It is important to consider these alternatives closely, especially given the strong evidence of separate decisions presented by Shaw (1982; Mulligan & Shaw, 1980) and the considerable success that has been achieved in modeling human performance by using race models (e.g., Bundesen, 1987, 1990; Gardner, 1973; Meijers & Eijkman, 1977; Osman, Kornblum, & Meyer, 1986).

## Interactive Race Model

In this article, we examine a model that shares some features in common with both the independent race and coactivation models but is distinct from each. This conceptual compromise, called the *interactive race model* (Figure 2), assumes separate decisions and a race between parallel processes on redundant-target trials, but it also includes some crucial revisions that distinguish it from the independent race model. According to the interactive race model, perceptual channels may exchange information. This differs from the requirement of the independent race model that the channels be independent. However, only specific forms of information (and not general activation) may be exchanged, and a separate decision is made by each channel. This distinguishes it from the coactivation model, which assumes that the activations from redundant signals are pooled prior to decision.

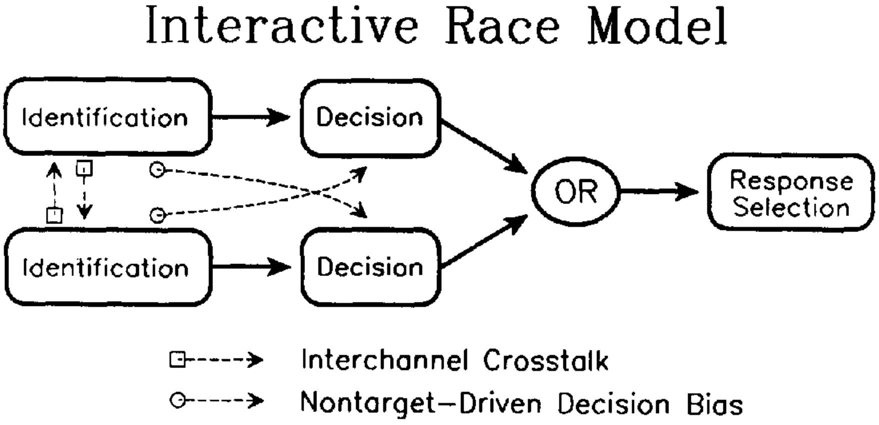

*Figure 2.* Schematic diagram of the interactive race model. (See text for details.)

We propose two mechanisms for information exchange, each operating in parallel with those processes that identify and act on stimuli: *interchannel crosstalk* and *nontarget-driven decision bias.* These mechanisms make the interactive race model consistent with observed violations of Inequality 1 but also place certain constraints on when such violations should be observed.

### *Interchannel Crosstalk*

Information exchange by spatially parallel identification channels is here referred to as *interchannel crosstalk* (in Figure 2, this is denoted by the small dashed arrows with open square origins). Crosstalk occurs when any information about the identity of the display element being processed by one perceptual channel influences the identification of the element in the opposite channel. The transfer of such information has two prerequisites: (a) information concerning the identity of the display element in one channel and (b) knowledge about the correlations between this element and the display elements that could occur in the other channel.[1]

As an illustration, consider the following hypothetical experiment: A subject is to press a key whenever an *X* appears in either or both of two spatial locations (the nontargets are *I* and *O*). Assume also that the subject has learned—through experience with the task or explicit instruction—that the probability of an *X* appearing in one display location is very high when an *O* is present in the other location. If, on a given trial involving *X* and *O*, the *O* is identified before the *X* (e.g., because of stochastic fluctuations), then in combination with the known conditional stimulus probabilities, the subject might be less uncertain about the contents of the second channel. Loosely speaking, the channel containing the *O* could "tell" the other channel that it had a high probability of containing an *X* (because, in some sense, the *O* in the first channel constitutes evidence for an *X* in the second). This transfer of contingency-based information between channels is interchannel crosstalk. In this example, the use of such information would aid speeded performance.

In general, however, the information transferred between channels need not always facilitate the identification of a stimulus. For example, if the nontarget *I* almost never appears in the same display as an *X*, then the presence of an *I* in one channel is probabilistic evidence that the other channel does not contain an *X*. Under these conditions, the information passed by a channel containing an *I* would inhibit the identification of an *X* (the target) by the other channel. This would tend to slow responses on those rare trials that included both *I* and *X*. Obviously, then, the sign of the correlation between nontarget and target presence is important.

It is also important to recognize the distinction between the interactions of interchannel crosstalk and the notion of *correlated separate activations* (van der Heijden et al., 1984). Crosstalk is the exchange of information derived from (possibly implicit) knowledge of the contingencies among stimuli; it does not arise as a consequence of variations in motivational state or allocations of a limited perceptual resource, which may cause positive or negative correlations in the finishing times of separate channels, respectively.[2] Furthermore, our use of the term *crosstalk* should not be mistaken for those denoting a form of processing error due to featural confusions (e.g., Broadbent, 1982; Estes, 1982) or migrations (e.g., Mozer, 1983; Pollatsek & Digman, 1977; Wolford, 1975).

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[1] Although it is convenient to refer to a fully identified display element as the first ingredient of interchannel crosstalk, this need not be the case in general. The mechanisms responsible for crosstalk could actually begin exchanging information as soon as any channel had partially identified a display element (cf. McClelland, 1979; Meyer, Yantis, Osman, & Smith, 1985; Miller, 1982a, 1988; Yantis & Meyer, 1988).

[2] This distinction between correlated separate channels and the interactive race model is important for mathematical reasons as well: In a series of simulated experiments, Miller (1982b) showed that the correlation between channels must be extremely large and negative for a correlated separate activations model to violate Inequality 1. Thus, the model presented by van der Heijden, Schreuder, Maris, and Neerincx (1984) should be seen as a member of the independent race-model class in that it is only consistent with violations under extreme conditions. For more detailed analyses, see Colonius (1986, 1990) and Ulrich and Giray (1986).

## *Interstimulus Contingencies*

Of course, the possibility of information exchange by interchannel crosstalk is only relevant to the analysis of performance if the corresponding experimental design includes correlations among the possible display elements. In what follows, we derive quantitative expressions that can reveal whether the necessary contingencies are present. In particular, we examine the conditions under which redundant-target trials enjoy some contingency-based advantage as compared with single-target trials.

We begin with the assumption that each perceptual channel has a set of baseline activations with respect to each of the possible display elements (in our experiments, these are *X*, *O*, and *I*) and that these baseline values are proportional to the frequency of that element's occurrence. If we denote a target in location $a$ as $T^{(a)}$ and nontarget $i$ in location $a$ as $N_i^{(a)}$, then the baseline activation of the perceptual channel assigned to Display Location 1 toward deciding it contains a target would be proportional to the probability of a target appearing in Location 1, or $P(T^{(1)})$. One way to envision this (suggested by Miller, 1982b) is to assume that each identification channel has its own set of logogens (Morton, 1969)—one logogen for each of the items that are possible display elements (e.g., *X*, *O*, and *I*)—and that the baseline activation of each logogen is proportional to the overall probability of that element appearing in the indicated display location.

Now consider the nontarget $N_i$ that is sometimes present in the same display as a target. The conditional probability of a target appearing in Position 1 when $N_i$ appears in Position 2 is given by $P(T^{(1)} \mid N_i^{(2)})$. If this value is greater than the baseline probability of a target appearing in Location 1 (i.e., $P[T^{(1)} \mid N_i^{(2)}] > P[T^{(1)}]$), then crosstalk from Channel 2 to Channel 1 would facilitate the identification of a target. Alternatively, if the conditional probability of Channel 1 containing a target is lower than the baseline probability (i.e., $P[T^{(1)} \mid N_i^{(2)}] < P[T^{(1)}]$), then the crosstalk from Channel 2 to Channel 1 would inhibit target detection. These ideas may be represented quantitatively as

$$\mathrm{ISC}(N_i \Rightarrow T) = P(T^{(a)} \mid N_i^{(b)}) - P(T^{(a)}), \qquad (2)$$

where ISC refers to interstimulus contingency, $N_i$ represents a specific nontarget element, and T represents the target. If $\mathrm{ISC}(N_i \Rightarrow T)$ is positive, then the crosstalk from a channel containing $N_i$ would facilitate a channel in the process of identifying T. If ISC is negative, then crosstalk would inhibit identification.[3]

Of course, there can also be contingencies between channels that both contain targets. The corresponding equation is

$$\mathrm{ISC}(T \Rightarrow T) = P(T^{(a)} \mid T^{(b)}) - P(T^{(a)}), \qquad (3)$$

where $P(T^{(a)} \mid T^{(b)})$ is the probability that a target will appear in location $a$ given a target in location $b$. If the value of $\mathrm{ISC}(T \Rightarrow T)$ is positive, then the identification of a target in one channel could provide information (by way of crosstalk) that would facilitate the processing of a second target by the opposite channel. If $\mathrm{ISC}(T \Rightarrow T)$ is negative, then mutual inhibition of target identification could occur on redundant-target trials.

Now, to the extent that $\mathrm{ISC}(T \Rightarrow T)$ is greater than $\mathrm{ISC}(N_i \Rightarrow T)$ for a given experimental design, the interactive race model would predict redundant-target trials to yield a performance advantage (as compared with single-target trials including $N_i$) over the advantage predicted by statistical facilitation alone. This is because the information passed by a channel containing a target would facilitate another channel in the process of identifying a target more than the information passed by a channel that had identified the nontarget $N_i$.

We can thus define the benefit due to interstimulus contingencies that favors redundant-target trials as the difference between the two values of ISC. Specifically,

$$\begin{aligned}\mathrm{ISCB}(N_i) &= \mathrm{ISC}(T \Rightarrow T) - \mathrm{ISC}(N_i \Rightarrow T)\\ &= P(T^{(a)} \mid T^{(b)}) - P(T^{(a)} \mid N_i^{(b)}), \qquad (4)\end{aligned}$$

where $\mathrm{ISCB}(N_i)$ is the interstimulus contingency benefit in favor of redundant-target trials over single-target trials that include nontarget $N_i$. The second line of Equation 4 follows directly from Equations 2 and 3. Like ISC, ISCB must be calculated separately for each nontarget that may accompany single targets. In addition, ISCB is a signed value; it is positive when the benefit favors redundant-target trials over single-target trials that include the specified nontarget. Finally, it is important to note that Equations 2–4 (as well as Equations 5 and 6 that follow) are statements about experimental designs. They specify what potentially useful information is present in the contingencies among stimulus events (cf. internal constraint, Garner, 1962; see also Miller, 1987). Only through the analysis of data collected through the use of various designs can we discover whether this information is, in fact, used by subjects.

## *Nontarget-Driven Decision Bias*

Interchannel crosstalk concerns the effects that identity information extracted by one channel may have on the identification of the contents of another perceptual channel. A second mechanism incorporated into the interactive race model concerns the possible effects that a nontarget being processed by one identification channel may have on the decision mechanism assigned to the opposite channel (in Figure 2, this is denoted by the curved dashed arrows with open circle origins). Both the independent race and coactivation models assume that nontargets have no bearing on target-present response times, either because they are not involved in the race (independent race model) or because activation from channels containing nontargets cannot be pooled with activation from channels containing targets (coactivation model). The interactive race model, however, allows an iden-

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[3] These equations have been simplified by assuming that all baseline and conditional probabilities are symmetrical across the two display locations. For Equation 2, it has been assumed that $P(T^{(1)}) = P(T^{(2)})$ and that $P(T^{(1)} \mid N_i^{(2)}) = P(T^{(2)} \mid N_i^{(1)})$ for all $N_i$. This is consistent with all of the experimental designs in this article and permits us to use the variable location superscripts (a) and (b) rather than (1) and (2). This constraint is not required in general and manipulations of the contingencies with respect to spatial location could prove fruitful in future work.

tification channel that contains a nontarget to provide contingency information to the decision mechanism assigned to the opposite channel. This information could be used to bias the level of criterion, alternately speeding or slowing responses to those displays that contain one target and one nontarget (cf. Estes, 1982).

It must be emphasized that we are again referring to the use of information derived from knowledge about the contingencies within an experimental design. This is what distinguishes nontarget-driven decision bias from certain forms of response competition (e.g., B. A. Eriksen & C. W. Eriksen, 1974). The interactive race model assumes that only those nontargets that are correlated with the absence of targets will slow responding (cf. van der Heijden et al., 1984). This point is analogous to that made by Miller (1987) in his examination of correlational cuing in focused-attention tasks.

### *Nontarget-Response Contingencies*

As in the derivation of ISC (Equations 2 and 3), we must here consider the difference between a baseline probability and a conditional probability. We start, then, with the assumption that baseline decision criterion is determined by the proportion of target-present ("Go") trials in the experimental design, which is designated $P(+)$. Again, this may be conceptualized in terms of a response logogen (e.g., Keele, 1973) with a baseline level of activation proportional to $P(+)$. If we now denote the conditional probability that a response should be made given a specific nontarget by $P(+ \mid \mathrm{N}_i)$, where $\mathrm{N}_i$ specifies which nontarget, then we have the two values needed to calculate whether the correlation between $\mathrm{N}_i$ and target presence is positive or negative:

$$\mathrm{NRC}(\mathrm{N}_i) = P(+ \mid \mathrm{N}_i) - P(+), \tag{5}$$

where NRC is nontarget-response contingency and $\mathrm{N}_i$ represents a specific nontarget. (We have assumed, as before, that conditional probabilities are symmetrical across display locations and have therefore omitted positional superscripts; see Footnote 3.) Note that we only have to examine the conditional probabilities given the presence of specified nontargets. Targets would not alter decision criterion; they would only drive activation toward criterion (cf. Grice, 1968; Grice, Nullmeyer, & Spiker, 1977).

The value of NRC will be positive if the presence of the specified nontarget is correlated with the correct response being target present ("Go") and negative if correlated with target absent ("No-Go"). The interactive race model assumes that when $\mathrm{NRC}(\mathrm{N}_i)$ differs from zero, responses to displays that include nontarget $\mathrm{N}_i$ will be either facilitated or inhibited (given a positive or negative value, respectively).

To characterize the potential effect that nontarget-response contingencies could have on the size of the observed redundancy gain, we must reverse the sign of Equation 5 so that a negative correlation between the presence of a nontarget and the target-present response represents a positive benefit for redundant-target trials. Thus,

$$\mathrm{NRCB}(\mathrm{N}_i) = P(+) - P(+ \mid \mathrm{N}), \tag{6}$$

where $\mathrm{NRCB}(\mathrm{N}_i)$ is the nontarget-response contingency benefit in favor of redundant-target trials over single-target trials that include $\mathrm{N}_i$. To the extent that this value is positive, the negative contingency between $\mathrm{N}_i$ and a target-present response provides a potentially inhibitory source of information on single-target trials. This could result in an increase of the redundant-signals effect and possibly cause violations of the race-model inequality.

### *Overview*

The interactive race model stands as an alternative to both the independent race and coactivation models. It relaxes some of the independent race model's assumptions while retaining the notion of separate decisions and a race to activate a response on redundant-target trials. Like the coactivation model, the interactive race model allows information extracted from multiple sources to be jointly involved in activating a single response, but the manner in which this integration occurs is quite distinct from that of coactivation. Activations are not simply summed at some stage prior to decision (as under coactivation); instead, only contingency information may be exchanged.

The two mechanisms of information exchange that have been incorporated into the interactive race model not only differentiate it from the other two models but also make it consistent with violations of the race-model inequality (Inequality 1). However, unlike the coactivation model, the interactive race model is only consistent with violations under certain circumstances: when the experimental design contains contingencies that favor redundant-target trials. That the interactive race model places this constraint on when violations may be observed distinguishes it from the coactivation model; that it is ever consistent with violations distinguishes it from the independent race model.

Observed violations of the race-model inequality have already ruled out the independent race model (e.g., Grice, Canham, & Boroughs, 1984; Miller, 1982b). However, both the coactivation and interactive race models are consistent with these results. To provide evidence concerning the viability of these two models, we now describe several redundant-target detection experiments in which we manipulated the two types of contingency described previously (in particular, ISCB and NRCB, Equations 4 and 6). The interactive race model predicts that performance should vary systematically under these condition; the coactivation model does not.

In Experiments 1–3, we test for the effects of interstimulus and nontarget-response contingencies while maintaining display size at two under all conditions. In Experiments 4 and 5, we expand the analysis to those situations in which single-target trials do not include a nontarget element (i.e., single targets appear alone). We begin by introducing the methods used in all five experiments.

## General Method

### *Subjects*

All subjects had normal or corrected-to-normal vision. Most were undergraduate students at the Johns Hopkins University. No subject participated in more than one of the present experiments, nor had any participated in a similar experiment previously. Subjects received between $12 and $15 (see the following) for participating in three 1-hr sessions. Ten subjects participated in each of the five experiments reported in the following; 24 were male, 26 were female, and all but two were right-handed.

### *Apparatus and Stimuli*

The stimuli were presented on a Princeton Graphics SR-12 color monitor controlled by an IBM AT microcomputer equipped with a Sigma Design Color-400 EGA board. Each display consisted of either one or two white letters presented against a black background. The letters (*X*, *O*, and *I*) were chosen for their low confusability and bilateral symmetry. The two display positions were centered 1.2 cm above and below fixation. The letters were placed above and below fixation, rather than to the left and right, to minimize potentially confounding effects of stimulus–response compatibility (Craft & Simon, 1970; Fitts & Seeger, 1953; for further details, see the General Discussion section). Each letter was 1.1 cm high and 0.7 cm wide. From a viewing distance of 45 cm, the eccentricity of the display locations from fixation was 1.53° and the letters subtended 1.40° × 0.89° visual angle. The fixation cross was 0.5 cm × 0.5 cm (0.64° × 0.64°). Subjects responded by pressing a button on a custom response box with the index finger of their dominant hand.

### *Design*

Each block was approximately 50 trials long, depending on the specific experimental design and the number of errors made by the subject. Half of all trials included at least one target (*X*) and required a response; the other half of the trials did not include a target and required that the subject not respond. The proportion of trials including one or two targets, as well as the number of trials including one or two display elements, was varied between experiments (see *Method* sections that follow).

Four contingencies remained constant across all designs. First, the overall probability that a response was required (i.e., $P[+]$, the probability that at least one *X* appeared in the display) was .5 for all experiments. Second, $P(+)$ was independent of whether the elements in two-letter displays were identical; that is, same–different was uncorrelated with target presence. Third, $P(+)$ was independent of whether one or two elements were presented; thus, display size was uncorrelated with target presence. Finally, all probabilities were symmetrical across display locations. For example, the probability of a target appearing in the upper position was equal to the probability of one occurring in the lower position, and so forth (see Footnote 3).

### *Procedure*

Each experiment involved three sessions conducted at similar times on consecutive days. On Day 1, subjects first completed a series of orientation blocks in which they were introduced to the general task and the method of performance feedback. For example, because subjects were paid cash bonuses depending on they accuracy and speed, one such block provided feedback after each trial concerning the number of bonus points earned. In total, Day 1 included 10 blocks of about 50 trials; these practice data were excluded from all analyses.

Days 2 and 3 were identical. They both began with a short block of 20 practice trials, followed by 14 blocks of testing trials. Unknown to the subjects, the first two full blocks on each day were also considered practice, as were the first five trials of each subsequent block and all trials immediately following an error. Data from the warm-up and recovery trials were not included in the analysis. Each block required 3 to 4 min to complete and included a short break at the midpoint. An entire session required approximately 50 min, including an enforced 5-min break after the seventh block.

Each trial began with the presentation of a fixation cross at the center of the screen for 350 ms. After a 400-ms blank interval, the test display appeared, consisting of one or two letters above and/or below fixation. When a response was made (whether correct or a false alarm), the stimulus was immediately removed. If no response occurred within 1,000 ms, the stimulus was removed and the trial was considered a "No-Go." The intertrial interval was 2,000 ms.

A 700-Hz tone sounded for 200 ms following both miss and false-alarm errors; three such tones were sounded if the subject responded before the test stimulus appeared (i.e., on anticipations).[4] Feedback was displayed after each block, including accuracy, mean reaction time, and the number of bonus points earned. The payoff matrix used to determine bonus points was designed to encourage rapid responses while keeping errors to a minimum. About 35 points were awarded on correct "Go" trials—slightly more points were awarded for a relatively rapid response and fewer points for a relatively slow response. Subjects were also given 15 points for correctly withholding a response when no target was displayed. To discourage errors, 350 points were subtracted when a false alarm, miss, or anticipation error was made.

### *Data Analysis*

For each experiment, two different analyses were conducted. The first determined whether redundant-target displays were responded to more quickly than single-target displays. This test compared (within subjects) the mean response time (RT) from redundant-target trials with the mean from single-target trials.

The second analysis tested whether the race-model inequality (Inequality 1) was violated. To conduct these tests, two cumulative distribution functions (CDFs) are required. The first, which corresponds to the left side of Inequality 1, is the CDF for the redundant-target condition. The second, corresponding to the right side of Inequality 1, is the sum of the two single-target CDFs. A violation of Inequality 1 is defined as any quantile at which the RT for the redundant-target condition is less than that for the corresponding bin in the sum of the single-target CDFs.

Specific values for all CDFs were calculated at 18 points corresponding to the 10th through the 95th percentiles at 5% intervals (the 5th percentile was never calculated because this would sometimes require extrapolation beyond the observed data). For example, the 18 values obtained for the redundant-target CDF are the 18 values of RT for which the probability of responding at or before this time were .10, .15, and so on. When necessary, linear interpolation was used to find these values. The 18 values for the sum of the two single-target CDFs are the values of RT for which the sum of the two probabilities was .10, .15, and so forth.

For each subject, two pairs of CDFs were calculated for each session: One pair for Blocks 3–8 and another pair for Blocks 9–14. This served to reduce the influence of practice effects on the overall shape of each distribution. Each testing session thus produced four 18-bin CDFs per subject: two CDFs for the redundant-target condition and two CDFs that were sums of the single-target conditions. To create one final pair of CDFs for a given subject, the four CDFs from each condition (2 days × 2 pairs/day) were averaged at each quantile. This procedure, known as Vincentizing, preserves the overall shape of all constituent distributions (Ratcliff, 1979; Thomas & Ross, 1980).

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[4] Anticipation responses were extremely rare (less than 0.05% overall) and occurred equally often in all conditions. For these reasons, they will not be discussed.

To test the significance of any observed violations of Inequality 1, paired $t$ tests were conducted across subjects at each of the quantiles that showed a mean violation. Note in advance that violations are only ever possible at the lower quantiles (i.e., when $P[\mathrm{RT} < t] < .75$), because the right side of Inequality 1 asymptotes at 2.00, whereas the left side asympotes at 1.00. To produce graphical representations of the group data, the two sets of CDFs were Vincentized across subjects.

## Experiments 1–3

The first three experiments were designed to test separately for the effects of interstimulus contingencies (ISC, Equations 2 and 3) and nontarget-response contingencies (NRC, Equation 5), These tests were conducted by using the divided-attention redundant-target detection paradigm, so the actual independent variables manipulated were interstimulus contingency benefit (ISCB, Equation 4) and nontarget-response contingency benefit (NRCB, Equation 6), respectively. Three groups of 10 subjects were each assigned to perform the target-detection task under one of three different sets of trial probabilities. In all three experiments, displays always included two elements (i.e., nontargets appeared in the opposite location on single-target trials).

Subjects in Experiment 1 participated under a design in which both ISCB and NRCB were zero. This experiment provides a direct test of the independent race model and the interactive race model (which both predict no violations of Inequality 1 under these conditions); it also forms baseline condition against which the other two experiments may be compared. Coactivation models may (but need not) yield violations of Inequality 1 under these conditions.

Experiment 2 used a design with positive ISCB but no NRCB. This allowed us to test whether interstimulus correlations affect performance without the potentially confounding influences of stimulus–response contingencies. The interactive race model predicts larger redundancy gains under these conditions than under those of Experiment 1; here it is also consistent with violations of Inequality 1. In contrast, the independent race and coactivation models make the same predictions for Experiment 2 as they do for Experiment 1.

Experiment 3 involved positive NRCB but no ISCB and tested for the effects of nontarget-response correlations. Again, the interactive race model predicts increased redundancy gains and violations of Inequality 1 as compared with Experiment 1, whereas the independent race and coactivation models predict the same results.

### Method

As stated before, the design of Experiment 1 included no contingencies among stimuli or between stimuli and responses, so both ISCB and NRCB were zero. This was accomplished by selecting trial probabilities that satisfy Equations 7 and 8 (which set ISCB and NRCB, respectively, to zero):

$$P(\mathrm{T}^{(a)} \mid \mathrm{T}^{(b)}) = P(\mathrm{T}^{(a)} \mid \mathrm{N}_i^{(b)}) \tag{7}$$

and

$$P(+ \mid \mathrm{N}_i) = P(+). \tag{8}$$

Table 1
*Trial Frequencies (Per Block of 42), Experiments 1 and 4*

| Lower position | Upper position<br>Target | <br>Nontarget 1 | <br>Nontarget 2 |
|---|---|---|---|
| Target | 7 | 7 | 0 |
| Nontarget 1 | 7 | 0 | 7 |
| Nontarget 2 | 0 | 7 | 7 |

$$\mathrm{ISCB}(\mathrm{N}_1) = P(\mathrm{T}^{(a)}|\mathrm{T}^{(b)}) - P(\mathrm{T}^{(a)}|\mathrm{N}_1^{(b)})$$
$$= .5 - .5 = 0$$
$$\mathrm{NRCB}(\mathrm{N}_1) = P(+) - P(+|\mathrm{N}_1) = .5 - .5 = 0$$

The specific trial frequencies for a design that will satisfy these requirements are shown in Table 1. Recall that ISCB and NRCB are calculated separately for each nontarget. For Nontarget 1, $P(\mathrm{T}^{(a)} \mid \mathrm{T}^{(b)}) = P(\mathrm{T}^{(a)} \mid \mathrm{N}_1^{(b)}) = .5$ (thus ISCB is zero) and $P(+ \mid \mathrm{N}_1) = P(+) = .5$ (thus NRCB is zero). No targets ever appeared in the same display as Nontarget 2, so no calculations are needed (because no data were collected in this condition).

The design used in Experiment 2 yields a positive value for ISCB (see Table 2). For Nontarget 1, $P(\mathrm{T}^{(a)} \mid \mathrm{T}^{(b)}) = .75$ and $P(\mathrm{T}^{(a)} \mid \mathrm{N}_1^{(b)}) = .5$, so ISCB = .25 (Equation 4). At the same time, $P(+ \mid \mathrm{N}_i) = P(+) = .5$, so NRCB = 0 (Equation 6). Again, no targets ever appeared with Nontarget 2.

Experiment 3 includes two types of single-target trials (see Table 3). For Nontarget 1, NRCB = .25. However, single-target trials with Nontarget 2 were also necessary so that ISCB (for Nontarget 1) could be held at zero. These data were discarded, so ISCB and NRCB for Nontarget 2 are not shown. For all of these experiments, then, only trials involving Nontarget 1 are relevant to the present analysis.

### Results

*Redundancy gains.* All three experiments revealed a significant effect of target redundancy (see Table 4). For Experiment 1, the mean RT on redundant-target trials was 22 ± 3 ms faster than the mean RT on single-target trials, $t(9) = 8.73$, $p < .001$.[5] For Experiment 2, the mean redundancy gain was 33 ± 3 ms, $t(9) = 10.38$, $p < .001$, and for Experiment 3, it was 23 ± 2 ms, $t(9) = 9.50$, $p < .001$. Between-experiment tests revealed that the redundancy gain for Experiment 2 was greater than that for either Experiment 1 or Experiment 3, $t(18) = 2.73$ and 2.45, respectively, both < .05. Redundancy gains for Experiments 1 and 3 did not differ significantly, $t(18) = 0.45$, $p > .25$.

*Tests of the race-model inequality.* Figure 3 shows the group CDFs for the redundant-target condition and the sum of the functions for the single-target conditions from Experiments 1–3 (top, middle, and bottom panels, respectively). Although all three experiments revealed at least one point at which the race-model inequality was ordinally violated (as evidenced by the redundant-target function being above and to the left of the single-target function), significant violations of Inequality 1 occurred only in Experiments 2 and 3. For

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[5] Estimates of the redundant-signals effect are given as the $M \pm SE$, each rounded to the nearest millesecond. Some $t$ statistics in the text differ from $M/SE$ because of rounding.

Table 2
*Trial Frequencies (Per Block of 40), Experiments 2 and 5*

| Lower position | Upper position | | |
|---|---|---|---|
| | Target | Nontarget 1 | Nontarget 2 |
| Target | 12 | 4 | 0 |
| Nontarget 1 | 4 | 0 | 4 |
| Nontarget 2 | 0 | 4 | 12 |

$$\mathrm{ISCB}(\mathrm{N}_1) = P(\mathrm{T}^{(a)}|\mathrm{T}^{(b)}) - P(\mathrm{T}^{(a)}|\mathrm{N}_1^{(b)})$$

$$= .75 - .5 = .25$$

$$\mathrm{NRCB}(\mathrm{N}_1) = P(+) - P(+|\mathrm{N}_1) = .5 - .5 = 0$$

Experiment 2, the race-model inequality was significantly violated from the 15th to the 65th percentile ($ps < .05$, one-tailed). For Experiment 3, the race-model inequality was significantly violated at the 20th, 25th, 30th, and 35th percentiles ($ps < .05$, one-tailed). Thus, responses to redundant-target trials were too rapid to be consistent with the independent race model, but only under the two experimental designs that included contingencies favoring redundant-target trials. Tables 5–7 summarize the results from Experiments 1, 2, and 3, respectively.

*Error rates.* In all three experiments, subjects produced more false-alarm responses when the two nontargets were identical than when two different nontargets were presented (see Table 4). We have no ready explanation for this; same-different was uncorrelated with the correct response. However, the finding that false alarms were reliably faster than correct "Go" responses ($p < .05$ for all comparisons) suggests that these responses may actually have been "fast guesses" (Ollman, 1966; Yellott, 1967, 1971). Because of this, we applied the distribution-function correction procedure of Grice et al. (1977), according to which "twins" of each false-alarm RT are removed from the correct target-present response distributions (see C. W. Eriksen, 1988, for rationale). We then repeated the tests of the race-model inequality. The results in each case were qualitatively identical to those depicted in Figure 3.

### *Discussion*

Experiments 1–3 demonstrate that contingencies included in the design of a divided-attention target-detection task can have significant effects on performance. These results reveal the existence of cognitive mechanisms sensitive to subtle forms of task-relevant information and suggest that violations of the race-model inequality may rely on the presence of contingencies that benefit redundant-target trials. When no such contingencies were included, no significant violations were observed (Experiment 1). When contingencies among stimuli (Experiment 2) or between stimuli and responses (Experiment 3) were such that redundant-target trials enjoyed some benefit, then significant violations were observed.

Table 3
*Trial Frequencies (Per Block of 42), Experiment 3*

| Lower position | Upper position | | |
|---|---|---|---|
| | Target | Nontarget 1 | Nontarget 2 |
| Target | 3 | 3 | 6 |
| Nontarget 1 | 3 | 0 | 9 |
| Nontarget 2 | 6 | 9 | 3 |

$$\mathrm{ISCB}(\mathrm{N}_1) = P(\mathrm{T}^{(a)}|\mathrm{T}^{(b)}) - P(\mathrm{T}^{(a)}|\mathrm{N}_1^{(b)})$$

$$= .25 - .25 = 0$$

$$\mathrm{NRCB}(\mathrm{N}_1) = P(+) - P(+|\mathrm{N}_1) = .5 - .25 = .25$$

Table 4
*Mean Response Times (RTs, in Milliseconds) and Error Rates (in %), Experiments 1–3*

| Condition | Exp. 1 | Exp. 2 | Exp. 3 |
|---|---|---|---|
| Mean RT | | | |
| Single target | 297 | 300 | 281 |
| Redundant targets | 275 | 267 | 258 |
| Redundancy gain | 22 | 33 | 23 |
| Miss errors | | | |
| Single target | 0.1 | 0.0 | 0.0 |
| Redundant targets | 0.0 | 0.0 | 0.1 |
| False-alarm errors | | | |
| Two different nontargets | 2.4 | 1.4 | 1.7 |
| Two identical nontargets | 3.3 | 2.1 | 4.3 |

Like the previous studies using this paradigm (e.g., Miller, 1982b), the present results rule out the independent race model. However, when considered together, Experiments 1–3 also raise questions about models which assume that activation strengths are pooled; being insensitive to contingencies, coactivation models do not predict any differences among these three experiments. In contrast, the interactive race model provides a natural account of these results: Violations are observed only under those conditions that include contingencies favoring redundant-target trials.

## Experiments 4 and 5

Miller (1982b), van der Heijden et al. (1984), and Grice, Canham, and Boroughs (1984) all found that violations of the race-model inequality occur only when single-target displays include a nontarget in the second location. When single-target displays do not include nontargets (i.e., when the location not containing a target is left blank), violations have not been observed. This pattern of results has led to an account called the *distraction decrement*, according to which responses to single-target displays that include a nontarget are slowed because the nontarget either saps attentional capacity away from the target (Grice, Canham, & Boroughs, 1984) or produces response competition (van der Heijden et al., 1984). This cannot happen with redundant targets, or course, because by definition there are no nontargets in these displays. According to the distraction decrement account, the absence of nontargets on redundant-target trials is the major source of redundancy gains (Grice, Canham, & Gwynne, 1984; Grice & Gwynne, 1987) and violations of the race-model inequality

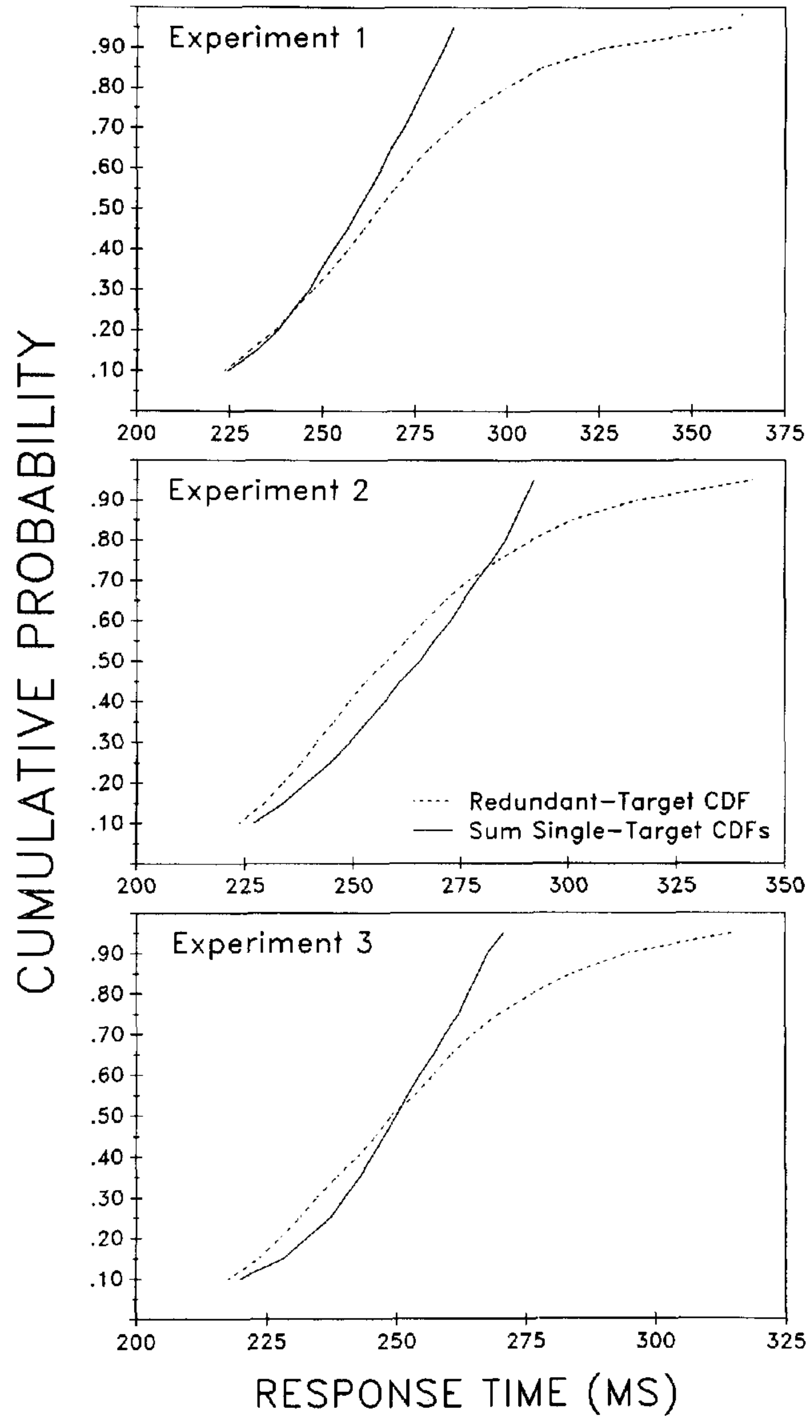

*Figure 3.* Group cumulative distribution functions for Experiments 1–3. (In each panel, the dashed function represents the Vincentized distribution function from the redundant-target condition, and the solid function represents the sum of the Vincentized distribution functions from the two single-target conditions. See Tables 5, 6, and 7, respectively, for a more detailed representation.)

(Grice, Canham, & Boroughs, 1984; van der Heijden et al., 1984).

However, there is an alternative explanation for this pattern of results, one that again focuses on the presence or absence of interstimulus and stimulus–response contingencies. If we assume that the perceptual system is able to recognize an empty display location as a type of nontarget stimulus, then empty locations may be sources of contingency information. More concretely, we may calculate values for ISCB and NRCB for the following condition: $N_i$ = "empty display location."

Table 5
*Tests of the Race-Model Inequality, Experiment 1*

| Percentile | Redundant-target distribution (1) | Sum single-target distribution (2) | Violation of Inequality 1 (2) − (1) | $t(9)$ |
|---|---|---|---|---|
| 10 | 224 | 224 | | |
| 15 | 231 | 232 | 1 | 0.76 |
| 20 | 237 | 238 | 1 | 0.34 |
| 25 | 243 | 242 | | |
| 30 | 248 | 247 | | |
| 35 | 253 | 250 | | |
| 40 | 257 | 253 | | |
| 45 | 261 | 257 | | |
| 50 | 265 | 260 | | |
| 55 | 269 | 263 | | |
| 60 | 274 | 266 | | |
| 65 | 279 | 269 | | |
| 70 | 285 | 272 | | |
| 75 | 291 | 275 | | |
| 80 | 299 | 277 | | |
| 85 | 309 | 280 | | |
| 90 | 326 | 283 | | |
| 95 | 360 | 285 | | |

We conducted such an analysis, examining the experimental designs that have been used in support of the distraction decrement, and found that those experiments that did not include nontargets in single-target displays also either incorporated contingencies that were unfavorable to redundant-target trials or contained no (differential) contingencies at all. At the same time, those experiments that did include nontargets in single-target displays incorporated contingencies that were favorable to redundant-target trials (for details, see the *Discussion* section and Table 11). Thus, the presence or absence of nontargets has previously been confounded with the two types of contingency that we have suggested may be crucial determinants of whether the race-model inequality is violated. It is therefore not yet possible to determine whether the observed pattern of results is due to the simple presence or absence of nontarget stimuli—as suggested by the distraction decrement account—or to the contingencies incorporated in previous experimental designs.

To establish the actual source of observed violations, two additional experiments were conducted in which single targets were always presented without an accompanying nontarget. In Experiment 4, both ISCB and NRCB were set to zero. In Experiment 5, ISCB was positive and NRCB was zero. If it is the use of contingency information that yields violations of the race-model inequality, then violations should not be observed in Experiment 4 (without contingencies) but should

Table 6
*Tests of the Race-Model Inequality, Experiment 2*

| Percentile | Redundant-target distribution (1) | Sum single-target distribution (2) | Violation of Inequality 1 (2) − (1) | $t(9)$ |
|---|---|---|---|---|
| 10 | 224 | 227 | 3 | 1.20 |
| 15 | 229 | 234 | 5 | 2.02* |
| 20 | 234 | 239 | 5 | 2.68* |
| 25 | 238 | 245 | 7 | 2.54* |
| 30 | 242 | 249 | 7 | 3.12* |
| 35 | 246 | 253 | 7 | 3.11* |
| 40 | 249 | 257 | 8 | 3.60* |
| 45 | 253 | 261 | 8 | 3.48* |
| 50 | 258 | 266 | 8 | 2.90* |
| 55 | 262 | 269 | 7 | 2.40* |
| 60 | 267 | 273 | 6 | 2.49* |
| 65 | 271 | 276 | 5 | 2.07* |
| 70 | 277 | 279 | 2 | 0.97 |
| 75 | 284 | 282 | | |
| 80 | 291 | 285 | | |
| 85 | 301 | 288 | | |
| 90 | 316 | 290 | | |
| 95 | 343 | 292 | | |

*Note.* Statistical tests are one-tailed.
* $p < .05$.

Table 7
*Tests of the Race-Model Inequality, Experiment 3*

| Percentile | Redundant-target distribution (1) | Sum single-target distribution (2) | Violation of Inequality 1 (2) − (1) | $t(9)$ |
|---|---|---|---|---|
| 10 | 218 | 220 | 2 | 0.99 |
| 15 | 224 | 228 | 4 | 1.63 |
| 20 | 228 | 233 | 5 | 1.99* |
| 25 | 231 | 237 | 6 | 2.51* |
| 30 | 235 | 240 | 5 | 2.35* |
| 35 | 238 | 243 | 5 | 2.04* |
| 40 | 243 | 245 | 2 | 1.07 |
| 45 | 246 | 248 | 2 | 0.70 |
| 50 | 249 | 250 | 1 | 0.38 |
| 55 | 254 | 252 | | |
| 60 | 257 | 255 | | |
| 65 | 261 | 257 | | |
| 70 | 265 | 259 | | |
| 75 | 270 | 262 | | |
| 80 | 277 | 264 | | |
| 85 | 284 | 266 | | |
| 90 | 295 | 268 | | |
| 95 | 315 | 271 | | |

*Note.* Statistical tests are one-tailed.
* $p < .05$.

appear in Experiment 5 (with contingencies). In contrast, if a distraction decrement is the primary source of race-model inequality violations, then neither of these experiments should yield violations, because neither includes nontargets on single-target trials.

## Method

The design for Experiment 4 was the same as that for Experiment 1 (see Table 1), except that Nontarget 1 was actually an empty display location. This is a direct replication of van der Heijden et al. (1984), in which no significant violations of the race-model inequality were observed. Experiment 5 used the same design as Experiment 2 (see Table 2), except that Nontarget 1 was an empty location. Thus, Experiment 4 provided no contingency-based information, whereas Experiment 5 contained an interstimulus contingency benefit favoring redundant-target trials. We did not conduct an experiment with a nontarget-response contingency benefit and no distractors (i.e., we did not replicate Experiment 3 without distractors) because it is impossible to design such an experiment without introducing a correlation between display size and the correct response.

## Results

Both experiments yielded significant redundancy gains. The values were 19 ± 2 ms, $t(9) = 10.28$, $p < .001$, and 27 ± 4 ms, $t(9) = 7.20$, $p < .001$, for Experiments 4 and 5, respectively (see Table 8). A between-experiment test revealed that the redundancy gain for Experiment 5 was larger than that for Experiment 4, $t(18) = 2.01$, $p < .05$, one-tailed.

Figure 4 displays the group distribution functions for Experiments 4 and 5 (top and bottom panels, respectively). No violations of the race-model inequality occurred in Experiment 4. In Experiment 5, the race-model inequality was significantly violated from the 10th to the 40th percentile (all $ps < .05$, one-tailed). The results are summarized in Tables 9 and 10. The error rates were similar to those observed in Experiments 1–3 (see Table 8), and a correction for fast guessing did not alter the results significantly.

## Discussion

Experiment 4, which contained no differential contingencies, yielded results quite similar to those of van der Heijden et al. (1984) and the present Experiment 1. In particular, the data did not violate the race-model inequality, and the redundancy gain was approximately 20 ms. The former result is predicted by both the interactive race model and the distraction decrement account. The results from Experiment 5, however, were quite different and allow us to discriminate between the two explanations. Following the prediction of the interactive race model, Inequality 1 was violated even when nontargets were omitted from single-target displays. From this we conclude that violations depend on whether the experi-

Table 8
*Mean Response Times (RTs, in Milliseconds) and Error Rates (in %), Experiments 4 and 5*

| Condition | Experiment 4 | Experiment 5 |
|---|---|---|
| | Mean RT | |
| Single target | 297 | 301 |
| Redundant targets | 278 | 274 |
| Redundancy gain | 19 | 27 |
| | Miss errors | |
| Single target | 0.0 | 0.1 |
| Redundant targets | 0.0 | 0.0 |
| | False-alarm errors | |
| One nontarget | 2.6 | 0.7 |
| Two (identical) nontargets | 3.4 | 1.8 |

mental design contains contingency benefits favoring redundant-target trials. That violations that were observed when single-target trials did not include a nontarget rules out explanations based exclusively on distraction decrements.

To provide further evidence for this claim, the pattern of race-model inequality violations reported in the literature was examined as a function of whether the corresponding experimental design contained interstimulus contingency benefits and/or nontarget-response contingency benefits favoring redundant-target trials. Table 11 summarizes all of the experiments that have been analyzed for violations of the race-model inequality and that used Go/No-Go or presence/absence responding. For each set of conditions, the values of ISCB and NRCB are shown; also listed are whether nontargets were included in single-target displays and whether the race-model inequality was significantly violated at any quantile.

An examination of this table reveals that the five experiments conducted by Miller (1982b) included varying contingencies in their designs. It can also be seen that, apparently quite by chance, the experiments with nontargets used designs

Table 9
*Tests of the Race-Model Inequality, Experiment 4*

| Percentile | Redundant-target distribution (1) | Sum single-target distribution (2) |
|---|---|---|
| 10 | 226 | 221 |
| 15 | 233 | 229 |
| 20 | 240 | 235 |
| 25 | 246 | 240 |
| 30 | 252 | 244 |
| 35 | 257 | 248 |
| 40 | 262 | 252 |
| 45 | 267 | 256 |
| 50 | 271 | 259 |
| 55 | 274 | 263 |
| 60 | 279 | 265 |
| 65 | 284 | 268 |
| 70 | 289 | 271 |
| 75 | 295 | 274 |
| 80 | 303 | 276 |
| 85 | 313 | 278 |
| 90 | 327 | 281 |
| 95 | 357 | 284 |

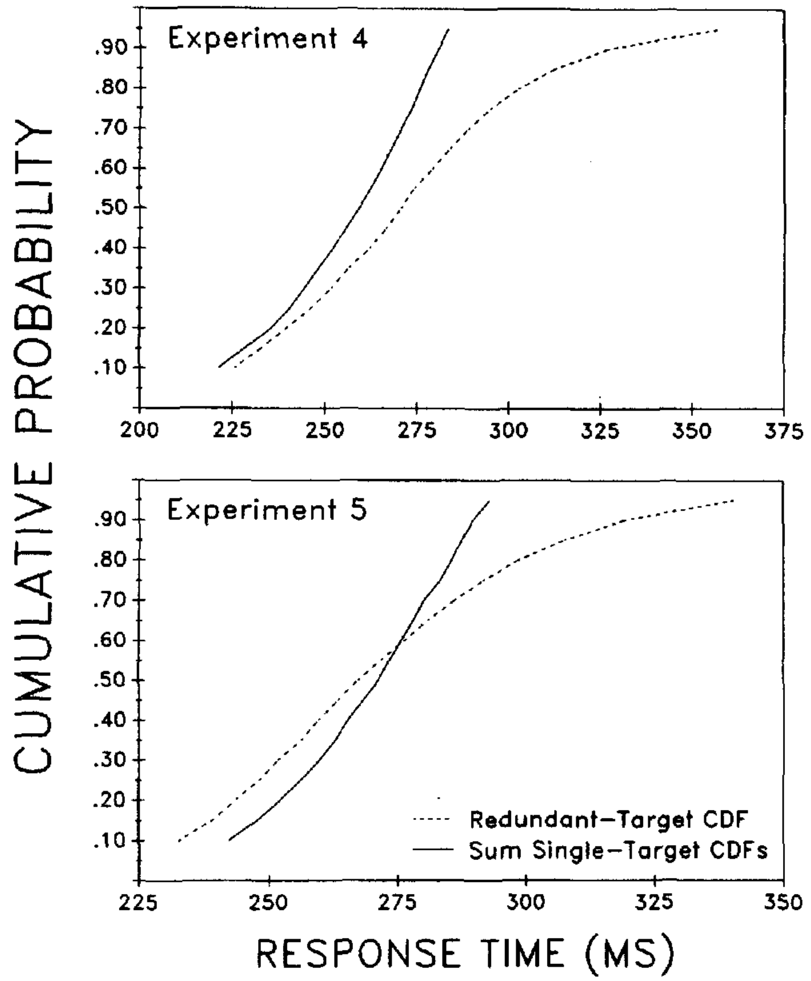

*Figure 4.* Group cumulative distribution functions for Experiments 4 and 5. (In each panel, the dashed function represents the Vincentized distribution function from the redundant-target condition, and the solid function represents the sum of the Vincentized distribution functions from the two single-target conditions. See Tables 9 and 10, respectively, for a more detailed representation.)

Table 10
*Tests of the Race-Model Inequality, Experiment 5*

| Percentile | Redundant-target distribution (1) | Sum single-target distribution (2) | Violation of Inequality 1 (2) − (1) | $t(9)$ |
|---|---|---|---|---|
| 10 | 233 | 242 | 9 | 3.09* |
| 15 | 239 | 248 | 9 | 2.62* |
| 20 | 244 | 252 | 8 | 2.73* |
| 25 | 248 | 256 | 8 | 2.47* |
| 30 | 252 | 260 | 8 | 2.62* |
| 35 | 256 | 263 | 7 | 2.19* |
| 40 | 260 | 265 | 5 | 1.84* |
| 45 | 263 | 268 | 5 | 1.54 |
| 50 | 267 | 271 | 4 | 1.24 |
| 55 | 272 | 273 | 1 | 0.60 |
| 60 | 276 | 276 | | |
| 65 | 281 | 278 | | |
| 70 | 286 | 280 | | |
| 75 | 292 | 283 | | |
| 80 | 299 | 286 | | |
| 85 | 307 | 287 | | |
| 90 | 319 | 290 | | |
| 95 | 340 | 293 | | |

*Note.* Statistical tests are one-tailed.
* $p < .05$.

with either positive ISCB or both ISCB and NRCB. Consistent with the present analysis, these conditions yielded significant violations of the race-model inequality. In contrast, Miller's experiment that included some single-target displays without distractors (Experiment 5, Condition b) used a design with negative ISCB and no NRCB for this condition and yielded no violations of the race-model inequality. Likewise, the experiment by van der Heijden et al. (1984), which did not include distractors and had no contingencies in favor of redundant-target trials, did not violate the race-model inequality either. It is this unfortunate covariation of nontarget/no-nontarget with contingency benefits (or costs) that has led to some confusion concerning the true source of violations. The present experiments show that it is the presence of informative contingencies and not a distraction decrement that determines the observed pattern of results.

## General Discussion

We have reported the results of five experiments in which we systematically manipulated the correlations among stimuli and between stimuli and responses. When no potentially informative contingencies were present in the design (Experiments 1 and 4), violations of the race-model inequality (Inequality 1) were not observed. In contrast, when interstim-

Table 11
*Classification of Divided-Attention Experiments: Nontargets, Contingencies, and Violations of the Race-Model Inequality*

| Experiment | Nontargets[a] | ISCB | NRCB | Violations[b] |
|---|---|---|---|---|
| Miller (1982b) | | | | |
| 1 | No | .000 | +.250 | Yes |
| 2 | No | .000 | +.250 | Yes |
| 3 | Yes | .000 | +.250 | Yes |
| 4 | Yes | +.100 | +.267 | Yes |
| 5, Condition a | Yes | .000 | +.333 | Yes |
| 5, Condition b | No | −.333 | .000 | No |
| van der Heijden et al. (1984) | | | | |
| — | No | .000 | .000 | No |
| Present study | | | | |
| 1 | Yes | .000 | .000 | No |
| 2 | Yes | +.250 | .000 | Yes |
| 3 | Yes | .000 | +.250 | Yes |
| 4 | No | .000 | .000 | No |
| 5 | No | +.250 | .000 | Yes |

*Note.* ISCB denotes interstimulus contingency benefit (Equation 4). NRCB denotes nontarget-response contingency benefit (Equation 6).
[a] Did nontargets accompany targets on single-target trials?
[b] Were significant violations of the race-model inequality (Inequality 1) observed?

ulus contingencies favoring redundant-target trials were introduced (Experiments 2 and 5), robust violations of the race-model inequality were observed. This result was obtained whether or not nontargets were included in single-target displays, undermining accounts that invoke a distraction decrement. Similarly, when the correlation between nontargets and the target-present response was negative (Experiment 3), violations of the race-model inequality again appeared.

From these results we conclude that violations of the race-model inequality—both here and in previous work (see Table 11)—are likely to have been caused by the presence of contingencies that subjects quickly learn and exploit.[6] The interactive race model provides a simple explanation for this pattern of results. Violations of Inequality 1 that are observed under experimental designs including interstimulus or stimulus–response contingencies arise because of the operation of two mechanisms of information exchange that are posited to function in parallel with identification and decision. In the following sections, we discuss several other findings that provide additional evidence for this claim, and we highlight some theoretical implications that follow from it.

## *Forced-Choice Responses and Stimulus–Response Compatibility*

Not included in Table 11 are the experiments by Grice, Canham, and Boroughs (1984) that involved redundant targets and tests of the race-model inequality but used two-alternative forced-choice rather than Go/No-Go or presence/absence responding. Under these conditions, it is not possible to manipulate NRCB in any subtle manner (because all trials include at least one target), but interstimulus correlations can still be varied. As it turns out, both NRCB and ISCB were held constant at zero in this study. Nevertheless, significant violations of the race-model inequality were observed in one of the three experiments that used letters as stimuli (their Experiment 3).

Although this may appear inconsistent with the interactive race model, closer consideration of one of their failures to find violations suggests a solution. Unlike in the present work, Grice, Canham, and Boroughs (1984) presented the stimulus letters side by side in their Experiments 3 and 4. Because two different responses were used (one by each hand), this arrangement introduced the effects of stimulus–response compatibility (e.g., Fitts & Seeger, 1953; Wallace, 1971). Specifically, when the target on a single-target trial appeared ipsilateral to the hand associated with the correct response, responding was faster (and more accurate) than when the mapping was reversed (Craft & Simon, 1970). Of course, redundant-target trials always included a target ipsilateral to the hand assigned to the correct response. It was under these conditions that violations of the race-model inequality were found.

For their Experiment 5, Grice, Canham, and Boroughs (1984) altered the displays so that the letters appeared above and below fixation. This arrangement eliminated the influence of stimulus–response compatibility on performance. The experiment was conducted to examine whether the inclusion of compatibility effects within Experiment 3 and 4 was in any way responsible for the observed results. Clearly, they were: The race-model inequality was not significantly violated in Experiment 5. In fact, the results from this experiment were remarkably similar to those from the present Experiment 1. One must recognize, therefore, that the effects of stimulus–response compatibility may also contribute to whether violations are observed. Future work should be aimed at specifying the exact locus of these effects within the framework of an interactive race model.

## *Temporal Dynamics of Channel Interaction*

The present account is consistent with the results of Miller's (1986) examination of the "timecourse of coactivation." Using a bimodal redundant-target detection task with varying stimulus onset asynchronies (SOAs) between auditory and visual signals, Miller examined performance in terms of two new inequalities (each somewhat similar to Inequality 1). Violations of both inequalities were observed, leading to the conclusion that activations from separate stimuli are pooled across modalities by some relatively slow process. However, more than 64% of all trials in this experiment included redundant targets (110 per block of 170), suggesting that interstimulus contingencies may have been a factor.

With 11 different values of SOA, it is difficult to tabulate the design of Miller's (1986) experiment. Instead, the important pattern emerges when two conditional probabilities are graphed as a function of SOA. The upper function in Figure 5 (closed circles) represents the conditional probability that an auditory target will be presented at some point during the trial given that a visual target has already been presented. The lower function (open circles) represents the conditional probability that an auditory target will be presented given that a visual target has not yet been displayed.[7]

These two functions correspond to the values that are necessary to calculate the interstimulus contingency benefit favoring redundant targets at any given moment since the start of a trial. The upper function (closed circles) represents $P(\mathrm{T}^{(a)} \mid \mathrm{T}^{(b)})$ and the lower function (open circles) represents $P(\mathrm{T}^{(a)} \mid \mathrm{N}_i^{(b)})$, where $\mathrm{N}_i$ is no (target) signal. The vertical distance between these curves is therefore equal to ISCB (Equation 4) at each value of SOA. As can be seen, ISCB was always highly positive and tented to increase with SOA. The interactive race model is thus consistent with the violations of the race-model inequality that were observed under these conditions. Our analysis may also explain why Miller found that violations were greatest with values of SOA somewhat different from zero. In summary, whereas Miller (1986) interpreted his results as evidence "that response processes accumulate activation over a significant period of time" (p. 342), we

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[6] This learning need not be conscious or strategic. Indeed, we have informal evidence that conscious awareness of the contingencies is unrelated to their effects: One third of the subjects reported (during debriefing) that they were unaware of the contingencies, but the data from these subjects were nearly identical to those from the two thirds that reported having noticed the correlations.

[7] As in all previous examples, the probabilities were symmetrical across (in this case) modalities. Thus, the words *auditory* and *visual* may be exchanged, and the probabilities remain the same.

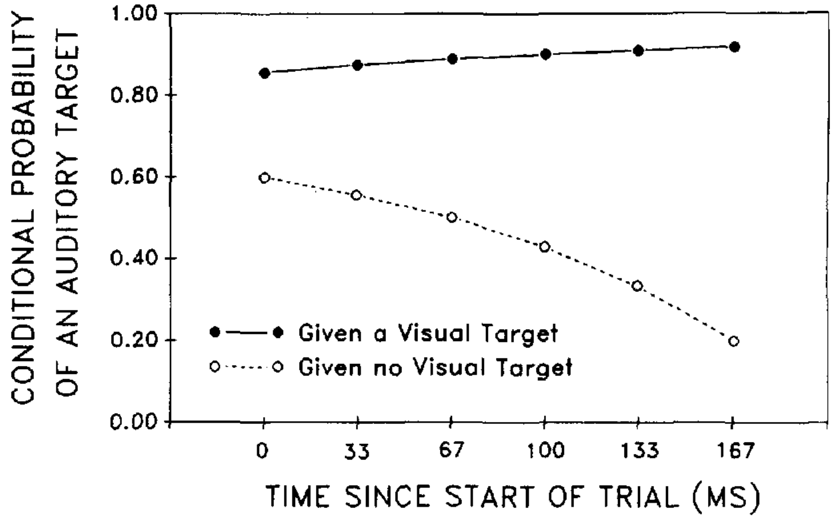

*Figure 5.* Depiction of the experimental design used by Miller (1986). (Conditional probability of a target in the auditory modality given that a visual target has already appeared [closed circles] or has not already appeared [open circles], as a function of time since the start of a trial.)

would argue instead that the mechanisms responsible for interchannel crosstalk are (a) active for a relatively long period and (b) sensitive to temporally defined contingencies like those shown in Figure 5.

### *Trial-Frequency Accounts*

There is an alternative to interchannel crosstalk that might in principle explain the results of Experiments 2 and 5. Note that in the design shared by these two experiments (Table 2), redundant-target trials occurred more frequently than either of the two single-target trials (30% vs. 10% each). If one assumes that either encoding or identification time is inversely proportional to frequency of occurrence (e.g., Logan, 1988), then the redundant-target condition enjoys an advantage under this design that has little to do with interstimulus contingencies.

We have three reasons for preferring the interactive race model over a trial-frequency account. First, the latter class of models cannot explain why Experiment 3 yielded significant violations of the race-model inequality. In the design of Experiment 3 (Table 3), the frequency of each of the relevant single-target displays was equal to that of the redundant-target display, yet violations of the race-model inequality were still observed. The presence of NRCB (a form of contingency-based information) provides a natural account for this result.

Second, the effect of stimulus frequency has typically been demonstrated only in tasks that use single-element displays (e.g., Miller & Pachella, 1973; Theios, Smith, Haviland, Traupman, & Moy, 1973), and it is not clear whether these results will generalize to situations involving divided attention and multielement displays. For example, Miller's (1987) study of correlational cuing within the flanker paradigm found little evidence for a consistent effect of stimulus frequency: In his Experiment 2, responses to some low-frequency displays were faster than those to displays that appeared eight times as often (see his Table 2 and Figure 2). Similar to the present analysis, Miller concluded that correlations among stimulus events were responsible for the observed pattern of response times.

Finally, to support our contention that display frequency was probably not responsible for the race-model inequality violations found in Experiments 2 and 5, we conducted one additional test. It involved calculating the correlation between trial frequency and mean response time for redundant-target trials across all experiments. (Only redundant-target trials were used to avoid possible confounding from the variations in display size across the various types of single-target trials.) The result was positive ($r = .41$); responses to high-frequency displays were actually slower than those to low-frequency displays. This surprising result has since been replicated in a control experiment that included no redundant-target trials: One set of single-target trials occurred twice as often as another, but the high-frequency displays were responded to about 10 ms slower than the low-frequency displays. In light of these findings, trial frequency does not provide a satisfactory account of the present results.

### *Capacity Limits*

The present study has demonstrated that many subtle contingencies included in the design of a divided-attention experiment can affect performance. Nevertheless, this should not be read as an endorsement of unlimited-capacity models of perceptual processing. In light of recent evidence that capacity limitations exist for even the simplest tasks (e.g., Duncan, 1980; Kleiss & Lane, 1986; Prinzmetal & Banks, 1983; Shaw, 1984), such an argument is untenable. In fact, the present analysis can provide additional examples of capacity limitations in relatively easy visual tasks.

Consider the study by Grice, Canham, and Gwynne (1984), which only found a redundancy gain when redundant-target trials were compared to those single-target trials that included a nontarget. When single targets were presented without nontargets, responses were just as fast as those to redundant targets. This result is in direct contrast to many others (e.g., the present Experiments 4 and 5; Miller, 1982b, Experiment 5, Condition b; van der Heijden, 1975; van der Heijden et al., 1983, 1984), all of which found significant redundancy gains without the inclusion of nontargets in single-target displays.

This seeming conflict can be settled by examining the correlations among stimuli. In the experiments by Grice, Canham, and Gwynne (1984), the interstimulus contingency benefit was negative (ISCB = −0.167); under these conditions, interchannel crosstalk on redundant-target trials would be expected to inhibit target identification (in relation to the crosstalk on single-target trials). This may explain why the usual redundancy gain was not observed for all comparisons.

However, interstimulus contingencies cannot be invoked to explain why responses on single-target trials were faster without nontargets than with them, because ISCB was the same for each of these conditions. It seems necessary to add that processing is quicker (or suffers from less mutual interference) when displays include only one element. This can be taken as further support for a limit in perceptual capacity. Whether such effects reflect a "distraction decrement" (Grice, Canham, & Boroughs, 1984) or a "cost of filtering" (Kahneman, Treisman, & Burkell, 1983; Treisman, Kahneman, & Burkell, 1983) has important implications, but they are beyond the scope of the present discussion.

## *Status of Coactivation*

The discussion so far leaves open the status of Miller's (1982b, 1986) coactivation hypothesis. In particular, though the interactive race model is capable of explaining the results from a variety of experiments, no evidence has been presented that directly contradicts the notion that activation strengths are summed prior to decision. For example, models that incorporate both the contingency-sensitive mechanisms of the interactive race model *and* the activation-summing mechanisms of the coactivation model are also compatible with all of the studies we have reviewed. Failure to find violations of the race-model inequality (e.g., Experiments 1 and 4) is not sufficient to rule out coactivation; this test is extremely conservative and coactivation models in no way require that violations occur. Furthermore, evidence supporting separate decisions (e.g., Mulligan & Shaw, 1980) does not contradict models that posit summation at some later state (e.g., in the motor component; see Diederich & Colonius, 1987).

Is there a test that may be conducted to discriminate between the interactive race model and a coactivation model that has been modified to include interchannel crosstalk and nontarget-driven decision bias? One possible such analysis is the strict test of the independent-decisions, capacity-sharing model presented by Mulligan and Shaw (1980; see also Grice, Canham, & Boroughs, 1984). This test concerns the probabilities of *not* responding given a single target or redundant targets at various times after stimulus onset (i.e., the survival functions).

Consider the following argument, which is based on the predictions of a race model: If each channel on a redundant-target trial presents an independent opportunity for the target to be detected, then the probability that a response has not been made by any time $t$ since stimulus onset given redundant targets should be the cross-product of the probabilities that a response has not been made by time $t$ given either single target alone (Ross, 1976, p. 57, Equation 4.1). Formally this may be represented as follows:

$$P(\mathrm{RT} > t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)}) = P(\mathrm{RT} > t \mid \mathrm{T}^{(1)}) \times P(\mathrm{RT} > t \mid \mathrm{T}^{(2)}), \quad (9)$$

where $P(\mathrm{RT} > t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)})$ is the probability that a (target-present) response has not been made by time $t$ given redundant targets, and so forth. By taking the natural logarithm of each side, we obtain

$$\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)}) = \ln P(\mathrm{RT} > t \mid \mathrm{T}^{(1)}) + \ln P(\mathrm{RT} > t \mid \mathrm{T}^{(2)}). \quad (10)$$

Violations of Equation 10 represent evidence against the independent-decisions, capacity-sharing model. A violation may occur under either of two conditions: (a) when the underlying decision mechanisms are not independent (e.g., they involved interactive or pooled activations) or (b) if capacity is not shared between channels (e.g., when all capacity is assigned to one location or when only one element is displayed).

If we apply this analysis to Experiment 1, the interactive race model must predict that Equation 10 will be satisfied. This holds because the design of this particular experiment includes no differential contingencies (satisfying Condition 1) and involves equal capacity loads in the single- and redundant-target conditions because display size is constant at two (satisfying Condition 2). Thus, the interactive race model should behave like an independent-decisions, capacity-sharing model under these conditions. In contrast, most coactivation models (e.g., Kinchla & Collyer, 1974; Schwarz, 1989) do not predict that Equation 10 will be obeyed.

In the top panel of Figure 6, the sum of $\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(1)})$ and $\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(2)})$ is plotted against $\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(1)} \,\&\, \mathrm{T}^{(2)})$ for the nine chosen quantiles of Experiment 1.[8] We have also included a dashed diagonal corresponding to the prediction of an independent-decisions, capacity-sharing model. As can be seen, the agreement between the data and the predictions of the interactive race model is good. For this experiment, only the point corresponding the the 90th percentile deviates significantly ($p < .01$) from Equation 10. However, this violation represents no evidence in favor of coactivation, because it is in the wrong direction: Responding on redun-

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[8] To conduct these tests of Equation 10, the probability of not responding by time $t$ for each of the two single-target conditions (single target in upper location, single target in lower location) was calculated at nine points. The values of $t$ that were used are those corresponding to the 10th–90th percentiles of the redundant-target condition (in 10% intervals). Within-subject $t$ tests were then conducted at each of these quantiles. To create the group plots (Figure 6), the mean value across subjects of $\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(a)} \,\&\, \mathrm{T}^{(b)})$ and $\ln P(\mathrm{RT} > t \mid \mathrm{T}^{(a)}) + \ln P(\mathrm{RT} > t \mid \mathrm{T}^{(b)})$ was obtained at each quantile.

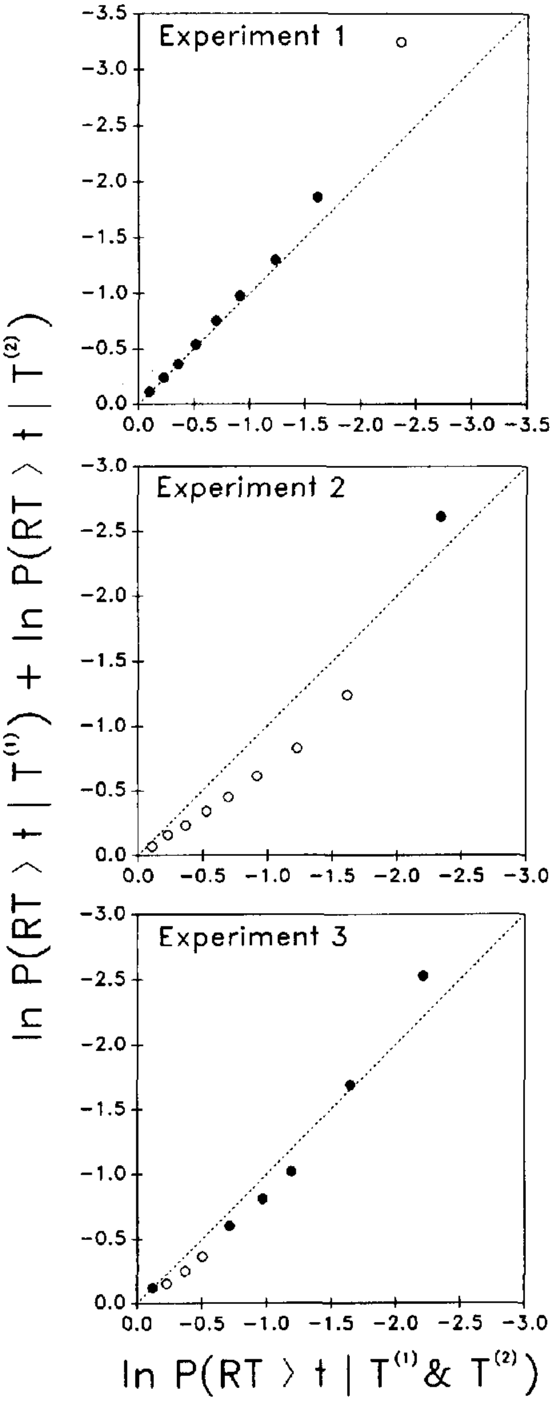

*Figure 6.* Group plots comparing the log survival functions for Experiments 1–3. (In each panel, the dashed diagonal represents the prediction of the independent-channels, capacity-sharing model [Equation 10]. The closed points represent quantiles at which Equation 10 was not significantly violated. The open points represent quantiles that deviate significantly from the diagonal [$p < .05$].)

dant-target trials was too slow to be consistent with the independent-channels, capacity-sharing model. In general, these results are difficult to reconcile with models which assume that activation strengths are summed across channels on redundant-target trials.

Similar analyses were conducted with the data from Experiments 2 and 3 (Figure 6, middle and bottom panels, respectively). In both cases Equation 10 was significantly violated at numerous percentiles: Responses were too fast on redundant-target trials to be consistent with the independent-decisions, capacity-sharing model. These findings complement the analyses that use the race-model inequality (Inequality 1) and reflect a violation of the assumption that processing prior to decision is independent.[9]

### *If There's a Race, Where's the Finishing Line?*

Pashler (1989) recently proposed a model to account for the results from a variety of dual-task and divided-attention experiments. The model highlights the qualitative differences between the operation of attention within each of two sequential modules, here referred to as *perceptual processing* and *response selection*, respectively. Within perceptual processing, attention takes on properties similar to those associated with limited capacity or resources (e.g., Kahneman, 1973; Wickens, 1980, 1984). In contrast, within response selection, attention resembles a bottleneck (Broadbent, 1958), with only one response being programmed at a time.

Pashler's (1989) model also includes a single, distinct process residing between those mechanisms that perceive stimuli and those that act on them. This device serves as a queue under dual-task (multiresponse) conditions; it holds the second of two response-requiring codes until the first response has been selected (Pashler, 1984; Pashler & Johnston, 1989). This, in turn, suggests a speculative answer to the question posed previously: The finishing line for the race between redundant targets is the queue that precedes response selection. The first target whose cognitive code reaches the queue would trigger a response. The loser of the race (i.e., the code from the second target) would be held in the queue until the positive response to the first code had been activated; however, because subjects are instructed to respond only once (in target-detection tasks), this code would have no observable effect.

### *Concluding Remarks*

In closing, we must acknowledge that the interactive race model could be seen as a member of the coactivation class. After all, both interchannel crosstalk and nontarget-driven decision bias involve the combining of information extracted from separate sources, so some sort of information integration

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[9] The tests of Equation 10 that were conducted for Experiments 4 and 5 are not presented because of the complexity involved in interpreting their results. Briefly, responses on redundant-target trials were often too slow to be consistent with the independent-decisions, capacity-sharing model because display size (and hence perceptual load) covaried with the number of targets; they were also sometimes too fast (but only in Experiment 5) because of the inclusion of positive ISCB. These results do, however, support both the notion that perceptual capacity is limited (see the *Capacity Limits* section) and the interactive race model's assumption that contingencies may cause processing times to be interdependent.

must be said to occur. However, the term *coactivation* usually implies that the activation strengths from two distinct channels are directly summed at some predecisional stage (Grice, Canham, & Boroughs, 1984; Miller, 1982b, 1986; Schwarz, 1989), so we have opted against this terminology. A central aspect of the model we propose involves a race between separate perceptual codes (see Figure 2), and it seems least misleading to call it an interactive race model. Furthermore, both forms of information exchange posited by our model allow channels containing targets and nontargets to interact, but in no coherent sense could this information be said to be "pooled" in activating a single representation. To the extent that this is a theoretically substantive issue and not merely one of labeling, additional differences between the interactive race model and the coactivation model should be identified and empirically tested in future work.

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