Response Time and Accuracy Revisited:
Converging Support for the Interactive Race Model

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The interactive race model embodies 2 central claims: that divided attention is best described as a race between separately processed codes and that the 2 types of design contingency to which the model is sensitive affect different processing stages. Previous support for the model has come from a series of redundant-target tasks examining reaction time (RT) (J. T. Mordkoff & S. Yantis, 1991).

We tested both central claims using near-threshold, accuracy tasks. This approach capitalizes on a known difference between RT and accuracy measures: that (in simple tasks) accuracy is sensitive only to perceptual manipulations, whereas RT is affected by both perceptual and postperceptual factors (J. L. Santee & H. E. Egeth, 1982). The results from 3 experiments provide converging support for the proposed loci of the 2 contingency-sensitive mechanisms within the interactive race model, as well as additional evidence concerning the differential sensitivities of RT and accuracy measures.

A large body of data has shown that people are capable of dividing their attention and processing information from more than one source simultaneously (for three very different approaches, see Bonnel, Possamaï, & Schmitt, 1987; Pashler, 1989; Shaw, 1982). Some of the strongest evidence for this ability comes from research using the redundant-target detection task (e.g., Egeth & Mordkoff, 1991; Grice, Canham, & Boroughs, 1984; Holmgren, Joula, & Atkinson, 1974; Miller, 1982b; Mordkoff & Yantis, 1991; van der Heijden, 1975; van der Heijden, La Heij, & Boer, 1983; van der Heijden, Schreuder, Maris, & Neerincx, 1984). The version of this task used in many previous studies involves speeded go/no-go responding with display size fixed at two letters: Subjects press a response key (i.e., make a go response) as quickly as possible when they detect at least one target and withhold their response (no go) when no targets are present. These experiments have shown that if subjects are presented with two targets—such that the second target is redundant with the first—responses are faster than if only one target is presented. This phenomenon, known as the redundant-signals effect or a redundancy gain, is usually interpreted as resulting from the parallel encoding and simultaneous analysis of both target stimuli on redundant-target trials.

There are two classes of the parallel-processing model that are capable of explaining the redundant-signals effect: race models (e.g., Raab, 1962) and coactivation models (e.g., Miller, 1982b). According to race models, each stimulus is processed separately and the advantage in reaction time (RT) for redundant-target trials comes from there being two chances for a target to be identified quickly. According to coactivation models, all information concerning the presence of at least one target is "pooled" prior to decision, and the redundant-signals effect results from criterion being reached more rapidly when more than one target is contributing activation.

Because both model classes can explain the redundancy gains that are observed in mean RT, more sophisticated tests must be used to discriminate between them. One such test is the race-model inequality (Miller, 1978, 1982b). This inequality assumes a race model and sets an upper bound for the cumulative probability of a response by any time \( t \) given redundant targets:

\[
P(\text{RT} < t | T^u \text{ and } T^l) \leq P(\text{RT} < t | T^u) + P(\text{RT} < t | T^l),
\]

where \( P(\text{RT} < t | T^u \text{ and } T^l) \) is the cumulative probability of a target-present response given targets in both the upper and lower display locations, and \( P(\text{RT} < t | T^u) \) is the cumulative probability of a response given a target in the upper display location and a nontarget (or nothing) in the lower location, and so forth. (In this and all other equations, subscripts denote spatial location.) All race models must obey the race-model inequality, but coactivation models need not. Early results from work using the race-model inequality provided support for both race and coactivation models; in some experiments the rule was obeyed (e.g., van der Heijden et al., 1984), whereas in others it was significantly violated (e.g., Miller, 1982b).

Interactive Race Model

Mordkoff and Yantis (1991) have recently presented a model of divided attention that may explain when and why
violations of the race-model inequality are observed. Their interactive race model can be seen as a compromise between an independent race and coactivation. Under the interactive race model, each target is processed separately and only one target directly activates a go response (on both single- and redundant-target trials); in this way, the model is a member of the race-model class. However, the interactive race model also includes two sets of mechanism that allow some kinds of information to be exchanged between separate processing channels; in this way, the model may be said to involve coactivation because activations are combined under some conditions. It is important to note, though, that the mechanisms that exchange information between channels depend on the presence of certain types of biased contingency within the experimental design. If biased contingencies are absent (i.e., if contingencies are balanced across conditions), then the interactive race model behaves the same as the independent race model.1

Biased Contingencies

Mordkoff and Yantis (1991) identified two different types of biased contingency that might be included within an experimental design (see also Garner, 1962, for a discussion of contingencies as internal constraints in a wide variety of information-processing tasks). The first type involves correlations between what stimuli are present in each of the two display locations. For example, if the letter X in the upper display location is more often accompanied by the letter X in the lower location than by the letter I, there is said to be a (positive) interstimulus contingency between the presence of two Xs. If the letter X is the target, this contingency could be highly important. The second type of contingency involves correlations between the presence of certain nontargets and whether a go response should be made. These are called nontarget-response contingencies. Because redundant-target displays include no nontargets (just the two targets), these contingencies could also be important because single-target trials would be affected and redundant-target trials would not.2

As an illustration, consider the three experimental designs shown in Tables 1–3. In these designs, the letter X is the target and the letters I and O are nontargets. The design given in Table 1 includes neither type of biased contingency. In particular, the probability of an X appearing in the upper location given an X in the lower location is .50, which is the same as the probability of an X in the upper location given an I in the lower location (.50). Put loosely, this means that an X or an I in the lower location can provide the same information concerning whether an X is in the upper location. Therefore, the design includes no biased interstimulus contingencies: It would be "fair" to compare redundant-target RTs to single-target RTs. Note that we have compared only redundant-X trials with trials with one X and one I. We did not examine the contingencies involving the letter O because there are no target-present trials that include an O (therefore, subjects should never respond when an O is present, so there are no correct-response RTs).

For a comparison, consider the design given in Table 2. This design does include biased interstimulus contingencies. In particular, the probability of an X in the upper location given an X in the lower location is .75, whereas the probability of an X in the upper location given an I in the lower location is only .50. Thus, there is an interstimulus contingency benefit (ISCB) of 0.25 favoring redundant-target trials. As noted earlier, we need to examine only the nontarget I because the nontarget O never appears with a target.

Neither of the designs shown in Tables 1 and 2 includes a biased nontarget-response contingency. To establish this, first note that for both designs, the overall probability that a go response should be made is .50 (i.e., the overall probability that at least one X is present is .50). Next, note that in Tables 1 and 2, the probability that at least one X is present given that an I is present is also .50. Thus, knowing that an I is present (in either location) provides no information as to whether a response should be made on this trial. This means that there is no nontarget-response contingency benefit (NRCB) involving the nontarget I.

By contrast, under the design shown in Table 3, the probability that a go response is correct given the presence of the nontarget I is only .25, whereas the overall probability that a go response is correct is .50. Thus, knowing that an I is present is also information that a response is probably not

1 We focus here on biased contingencies because what matters is whether the contingency-sensitive mechanisms within the interactive race model will affect one type of trial more than another (and hence bias the results). All experimental designs included contingencies, which were merely conditional probabilities: the question was whether the contingencies differed between experimental conditions.

2 The specific equations used to calculate whether biased contingencies exist within a design are given in the Experimental Designs section. Here, we only present the concepts in verbal form.

Table 1
Display Frequencies (per Subblock of 18 Trials): Experiment 1

<table>
<thead>
<tr>
<th>Lower position</th>
<th>X</th>
<th>I</th>
<th>O</th>
</tr>
</thead>
<tbody>
<tr>
<td>X</td>
<td>3</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>I</td>
<td>3</td>
<td>0</td>
<td>3</td>
</tr>
<tr>
<td>O</td>
<td>0</td>
<td>3</td>
<td>3</td>
</tr>
</tbody>
</table>

Table 2
Display Frequencies (per Subblock of 20 Trials): Experiment 2

<table>
<thead>
<tr>
<th>Lower position</th>
<th>X</th>
<th>I</th>
<th>O</th>
</tr>
</thead>
<tbody>
<tr>
<td>X</td>
<td>6</td>
<td>2</td>
<td>0</td>
</tr>
<tr>
<td>I</td>
<td>2</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>O</td>
<td>0</td>
<td>2</td>
<td>6</td>
</tr>
</tbody>
</table>
required on this trial. Because no Is appear in redundant-target displays, this constitutes a bias against single-target trials that include one X and one I (i.e., NRCB = .25). Finally, note that the design shown in Table 3 does include some trials with both an X and an O. These trials are required so that this design would include no biased interstimulus contingencies for the comparison of redundant-X trials with trials involving one X and one I. If the purpose of an experiment using the Table 3 design is to examine the effect of nontarget-response contingencies (only), then the RTs from trials involving the nontarget O should not be analyzed.

### Contingency-Sensitive Mechanisms

Matching the two types of contingency that may be present within an experimental design, Mordkoff and Yantis (1991) proposed two sets of contingency-sensitive mechanisms. The first set, which is said to cause interchannel crosstalk, relies on interstimulus contingencies. Interchannel crosstalk occurs between the processes responsible for identifying the two stimuli. For example, if the conditional probability of an X in the upper location is high given an X in the lower location, then crosstalk would bias the upper channel in favor of identifying an X (as soon as the lower channel had any evidence that it contained an X). Likewise, if the conditional probability of an X in the upper location is low given an O in the lower location, then crosstalk would bias the upper channel against identifying an X. This is exactly the state of affairs under the design given in Table 2. By contrast, under the design given in Table 1, crosstalk would have the same effect on single- and redundant-target trials, so it becomes irrelevant to the test for a redundancy gain or violations of the race-model inequality.

The second set of contingency-sensitive mechanisms causes nontarget-driven decision bias. These effects are assumed to affect a much later stage in the processing sequence than interchannel crosstalk. In this case, the information is not exchanged between identification processes but is one way between identification and decision processes. For example, if the conditional probability that a response should be made is low given the presence of a certain nontarget, then information that this nontarget is present (in either location) is also information that a response should not be made. Nontarget-driven decision bias acts to raise or lower the decision criterion depending on the direction of the decision contingencies. In the case of the design given in Table 3, the presence of an I would cause the decision threshold to be raised via nontarget-driven decision bias because the conditional probability that a response should be made (given an I) is lower than the overall or baseline probability. Note that nontarget-driven decision bias can occur only on single-target trials because redundant-target displays include no nontargets.

### Evidence Supporting the Interactive Race Model

Neither the independent race model nor the coactivation model includes any mechanism sensitive to biased contingencies. Thus, the independent race model predicts that the race-model inequality (Equation 1, above) will always be obeyed, regardless of whether biased contingencies are present. Similarly, the coactivation model does not make differential predictions depending on contingencies, although this model is always consistent with violations of the race-model inequality. By contrast, the interactive race model makes differential predictions: If no biased contingencies are present (as under the design shown in Table 1), then the race-model inequality should always be obeyed; if contingencies are biased in favor of redundant-target trials (see Tables 2 and 3), then violations may be observed.

Support for the interactive race model was first provided by a series of experiments in which the two types of contingency were systematically manipulated (Mordkoff & Yantis, 1991). In the experiments that included no biased contingencies (Experiments 1 and 4), no violations of the race-model inequality were observed. In the experiments that included either an interstimulus contingency (Experiments 2 and 5) or a nontarget-response contingency (Experiment 3) that favored redundant-target trials, significant violations were observed. An analysis of the designs of previous experiments also showed that biased contingencies were included in all experiments that showed significant violations of the race-model inequality (see Mordkoff & Yantis, 1991, Table 11, p. 532). Thus, the interactive race model can also account for the previous discrepancies in the literature.

### Overview

The interactive race model embodies two central claims: (a) that divided attention is best described as an independent race between processing channels when no biased contingencies are present in the experimental design and (b) that the two types of biased contingency—which may appear to be highly similar because both depend on changes to the experimental design—actually affect different processes. In this study we tested both of these claims.

Previous work concerning the interactive race model has examined only RT; here we examined the accuracy of near-threshold detections. With regard to the first claim, we tested whether near-threshold detections would also be independent in the absence of biased contingencies. With regard to the second claim, we capitalized on the differences between the dependent measures of RT and accuracy to test whether the two sets of contingency-sensitive mechanisms are truly distinct. Recall that the mechanisms that cause interchannel crosstalk have been assumed to have their effect on stimulus...
identification, which is an aspect of perception. In contrast, the mechanisms that cause nontarget-driven decision bias have been assumed to affect decision. Because RT and accuracy are differentially sensitive to perceptual and decision-related manipulations (the evidence of this is reviewed shortly), we tested whether RT and accuracy would be differentially affected by interstimulus and nontarget-response contingencies.

The remainder of the article is organized as follows: First, the differences between RT and accuracy measures are briefly reviewed. Next, an accuracy test for independent decisions (similar to one used for RT data) is presented and predictions for accuracy tasks are derived from the interactive race model. The predictions are then tested in three new experiments. Finally, we integrate our findings with those from previous studies of divided attention and comment on the contingency-sensitive mechanisms that the interactive race model includes.

Response Time Versus Accuracy

Everyday introspection suggests that different processes are taxed when people try to respond accurately and perception is difficult (e.g., if viewing time is restricted or the display is degraded) as opposed to when they try to respond quickly and perception is easy (e.g., if viewing time is unlimited and the objects are distinct). For example, when asked a question concerning the inaccuracy of an unseeded response that is based on visual input, people typically respond in ways that implicate early perceptual mechanisms (e.g., "It was too dark" or "It happened too fast"). In contrast, when asked questions that relate to how quickly they performed some act that depended on visual input, people may cite perceptual, decision-related, or motor processes in their replies (e.g., "My fingers weren't on the buttons" or "I wasn't ready to move"). This difference is most salient in the context of simple tasks (e.g., go/no-go, redundant-target detection) under which stimulus-response mappings are easy and people do not have to remember more than one or two stimuli on any given trial; when difficult mappings or large displays must be remembered, memory probably plays a major role in both speeded and unseeded tasks.

Santee and Egeth (1982) provided evidence to support this intuitive distinction between the dependent measures of RT and accuracy. Their work concerned two simple tasks that both required subjects to respond to the letter in a specific location. One was a postcued, single-item-report task (e.g., Bjork & Murray, 1977; Egeth & Santee, 1981); the other was a selective-attention, flankers task (e.g., Eriksen & Schultz, 1979). Previous postcued tasks always restricted viewing time (under 25 ms) and used accuracy as the dependent measure. Previous flankers-task experiments examined RT and the display remained visible until a response was made. Santee and Egeth (1982) first noted that the results from the restricted-viewing accuracy tasks appeared to conflict with the results from the speeded RT tasks. In the former, "data-limited" situation (Norman & Bobrow, 1975), responses were less accurate when displays contained multiple exemplars of a specific letter. By contrast, under the "resource-limited" situation (Norman & Bobrow, 1975) used for the flankers task, responses were faster with repeated letters. Herein lies the conflict: Less accurate implies lower performance, whereas faster implies higher performance. Thus, the effect of repeating a stimulus letter depends on whether the experiment examines data-limited accuracy or resource-limited RT (see Tables 1 and 2 in Santee & Egeth, 1982, for an illustration). Of more general importance and contrary to a common assumption of that time (explicitly stated by Eriksen & Eriksen, 1979, pp. 196–197, 203–204; see also Lappin, 1978; Smith & Spoehr, 1974), the two dependent measures are not always equivalent (see also Pashler, 1989).

The apparent "repeated-letter paradox" may be explained in terms of two sets of cognitive processes, referred to here as perception and decision (cf. Egeth, Pomerantz, & Shwartz, 1977; Miller, 1982a, 1982c; Pashler, 1989; Sanders, 1980; see also Pashler & Badgio, 1985). According to this view, performance accuracy depends mostly on perception (i.e., what really matters is whether the critical stimulus was perceived), whereas RT depends on both perception and decision, with greater emphasis on the latter. Thus, the lability of the effect of repeating letters within a display is explained by positioning that repeated letters encounter difficulty in perception but also enjoy an advantage in decision. Under the assumption that the latter effect is stronger than the former, the entire pattern of results is explained (for detailed discussions of the repeated-letter paradox, see Kanwisher, 1987, 1991; Mordkoff, 1991).

Santee and Egeth (1982) summarized the implications of their study with the suggestion "that the convergence of reaction time and accuracy within the context of a specific information processing model should be demonstrated empirically rather than assumed a priori" (p. 489). This article presents our attempt to establish such convergence within the context of divided attention and redundancy gains.

Testing for Independent Decisions Using Accuracy Data

It is clearly impossible to use the race-model inequality (Equation 1) on accuracy data because this test requires the analysis of RT distributions. However, it is still possible to test the predictions of independent-decision (race) models using accuracy data (for detailed methods, see the Appendix; Mulligan & Shaw, 1980; Shaw, 1982, see also Duncan, 1980). The way that we tested whether the two target detections on redundant-target trials would be independent events was to use what we call the independent-opportunities equality. This test is similar to the strict test for independent decisions (see Mordkoff & Yantis, 1991, pp. 535–536) in that it focuses on the probability that subjects fail to respond given the presence of at least one target (i.e., the probability of a miss error). Under the assumption that each stimulus is processed independently, one may predict the miss rate on redundant-target trials using the miss rates observed on single-target trials:

\[
P(\epsilon | T^u \text{ and } T^c) = P(\epsilon | T^u)P(\epsilon | T^c),
\]

(2)
where \( P(\epsilon | T^u) \) and \( T^u \) is the probability of making a miss error given targets in both display locations and \( P(\epsilon | T^o) \) is the probability of making a miss error given a target in the upper display location (only).

Violations of Equation 2 represent evidence against independent decisions. In particular, a lower value on the left side of Equation 2 than on the right side would mean that fewer miss errors were made on redundant-target trials than could be explained by independent race models. This result is predicted by coactivation models (Grice et al., 1984; Miller, 1982b; Schwarz, 1989; for a complete introduction, see Shaw, 1982), some nonindependent race models (e.g., van der Heijden et al., 1984), and—but only under certain conditions—the interactive race model. No known model predicts a higher value on the left side of Equation 2.3

**Predictions for Accuracy Tasks**

The interactive race model behaves like an independent-decisions model when no biased contingencies are included in the experimental design. Thus, under a no-biased-contingencies design (e.g., see Table 1), the data from an accuracy task are predicted to obey the independent-opportunities equality.

Given that interstimulus contingencies are posited to have their effect on the parallel channels responsible for identifying stimuli, one must also assume that these contingencies have an effect on perception. Thus, detection accuracy should be affected by the inclusion of interstimulus contingencies within an experimental design. In particular, if interstimulus contingencies are biased in favor of redundant-target trials, the ability of subjects to detect at least one target from a redundant-target display should be increased (relative to single-target trials), causing a violation of the independent-opportunities equality.

By contrast, nontarget-response contingencies are posited to have their effect on decision or response-selection processes, which are postperceptual. Thus, detection accuracy should be insensitive to the presence of these contingencies within an experimental design because the accuracy measure appears to be unaffected by various forms of decision bias (Kim & Kwak, 1990; Santee & Egeth, 1982; cf. Estes, 1982).

**Method**

To afford the desired comparison between accuracy and RT, the methods used in this accuracy study were as similar as possible to those used in the RT study of Mordkoff and Yantis (1991). The same stimulus letters, experimental designs, and response requirements were used. There were only two modifications: (a) The subjects were instructed to be as accurate as possible and not to concern themselves with the speed of their responses and (b) the stimuli were postmasked and presented for brief durations.

Three experiments were conducted, corresponding to Experiments 1–3 of the study by Mordkoff and Yantis (1991). The first included neither type of contingency (see Table 1). The second included only an interstimulus contingency benefit favoring redundant-target trials (see Table 2). The third included only a nontarget-response contingency benefit favoring redundant-target trials (see Table 3).

**Subjects**

Eighteen undergraduates at the University of California, San Diego, served as subjects, 6 in each of three experiments. All reported normal or corrected-to-normal vision. No subject participated in more than one of the experiments, nor did any report having participated in a similar experiment previously. Each subject was individually run in two 50-min sessions at the same hour on consecutive days and received about $10 in compensation.

**Apparatus and Stimuli**

The stimuli were presented on NEC Multisync color monitors controlled by IBM XT-compatible microcomputers equipped with EGA graphics adapters. Each display consisted of two white letters presented against a black background. The two display positions were centered 1.2 cm above and below fixation. The stimuli were the uppercase letters \( X, O, \) and \( I \), and \( X \) was always the target. Each letter was 1.1 cm tall 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 \( 1.40 \times 0.89 \) visual angle. The postmasks were each \( 1.4 \times 0.9 \) cm \((1.78 \times 1.15)\) and were made by filling a random 30% of the available pixels. A different set of masks was generated on each trial, and the two masks on a given trial were different from each other. The fixation cross was 0.5 cm \( \times \) 0.5 cm \((0.64 \times 0.64)\). Subjects responded by pressing a button on a custom response box with the index finger of their dominant hand.

**Experimental Designs**

Each block was 33–45 trials long and included five warm-ups and two subblocks of testing trials (block is here defined in terms of when subjects received feedback and were given long rests). 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 varied between experiments.

The three experimental designs are shown in Tables 1–3. The first design (see Table 1) includes neither an ISCB nor an NRCB. In particular, in this design, the probability of an \( X \) appearing in a display location given an \( X \) in the opposite location is the same as the probability of an \( X \) in that location given an \( I \) in the opposite location. Therefore, the interstimulus contingencies are unbiased. This may be verified using the equation for the ISCB (see Mordkoff & Yantis, 1991, p. 523, for complete derivation):

\[
\text{ISCB}(N_j) = P(T^+ | T^0) - P(T^+ | N_j),
\]

where \( P(T^+ | T^0) \) is the probability of a target appearing in display location \( a \) given a target in location \( b \), and \( N_j \) denotes a particular nontarget (indexed by \( j \)). As can be shown, under the design in Table 1, the ISCB for the nontarget \( I \) is zero. (Note that all three designs are symmetrical across display locations, so either "\( a = \) upper and \( b = \) lower or \( a = \) lower and \( b = \) upper" may be used in Equations 3 and 4.)

3 We gloss over the issue of incomplete output (i.e., the possibility that stimuli that are not completely identified may have an effect on decision making). This issue is discussed in detail by Mulligan and Shaw (1980) and Shaw (1982); we present the results from tests using their method in the Appendix. As can be seen, consideration of this issue would not alter our conclusions.
Under the design in Table 1, the nontarget I gives no response information either, so the nontarget-response contingencies are also unbiased. This can be shown using the equation for calculating the NRCB (see Mordkoff & Yantis, 1991, pp. 523–524):

\[
\text{NRCB}(X) = P(+I) - P(+Ig)
\]

where \(P(+)\) denotes the overall probability that a go response is correct and \(P(+Ig)\) is the probability that a go response is correct given that nontarget I appeared in location \(a\).

The second design (see Table 2) includes an ISCB favoring redundant-target trials but zero NRCB. In this case, the probability that an X will appear in a given location is greater when an X appears in the opposite location than when an I appears there. The difference is .25, so the ISCB is 0.25. At the same time, the nontarget-response contingency is balanced, as it was under the first design.

The third design (see Table 3) includes only an NRCB favoring redundant-target trials. The probability that a go response is correct given an I in either location is lower than the overall probability. The difference is .25, so the NRCB is 0.25. Note that this design also includes some target-present trials with the nontarget O in the second display location (i.e., the lower left-hand and upper right-hand cells in Table 3 are not zero). As noted earlier, these trials were necessary in order to keep the interstimulus contingencies on trials including the nontarget I equal to those on redundant-target trials. (It is impossible to design an experiment that includes only a positive nontarget-response contingency bias without including some "throw-away" trials.) The data from single-target trials involving the nontarget O were not analyzed.

**Instructions and Feedback**

It is important to maintain a low false-alarm rate in redundancy-gain experiments (for discussions, see Eriksen, 1988; Miller & Lopes, 1991). Therefore, the subjects were instructed to be very conservative. 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 for the current setting of display duration and then was masked. When a response was made—whether correct or a false alarm—the masks were removed. If no response occurred within 2,500 ms, the masks were removed and the trial was considered a no go. The intertrial interval was 1,100 ms.

A 700-Hz tone sounded for 200 ms following all false alarms (and all misses during the first block of the first session). Three tones were sounded if the subject responded before the test stimulus appeared (i.e., on an anticipation). Feedback was displayed during an enforced 7-s break at the end of each block.

**Data Analysis**

All data were first corrected for guessing by adding a false-alarm rate to each of the miss rates (for the rationale, see Eriksen, 1988; Miller & Lopes, 1991). This correction procedure assumes that for each false-alarm response made by a subject, there was also a correct ("hit") guess because the probability of guessing correctly was .5. The specific corrections performed were similar to those done by Mordkoff and Yantis (1991): Each target-absent condition was paired with a target-present condition in terms of whether the two stimuli were identical or different. For example, the lower right-hand cell in Table 1 was paired with the upper left-hand cell because both types of display include two identical letters. Correcting for guessing is important because the independence- opportunities equality applies to the rates at which subjects detect targets, not how often they produce a correct response. After correction, three different analyses were conducted. The first concerned simple redundancy gains. This test compared (within subjects) the mean miss rate on redundant-target trials with the mean miss rate for single-target trials. A lower miss rate on redundant-target trials constituted a redundancy gain.

The second analysis tested for an artifactual cause of any observed redundancy gain: a fixed-favored position strategy (cf. Biederman & Checkosky, 1970; Miller & Lopes, 1988; Mullin, Egeth, & Mordkoff, 1988). For example, if subjects monitored only one display location on each trial (e.g., the upper one), then they would "see" a target on all redundant-target trials but only on half of the single-target trials. This would cause a redundancy gain that has little to do with parallel processing. To test for this artifact, a t test similar to the first was conducted comparing each subject’s redundant-target miss rate with the smaller of their two single-target miss rates (i.e., target in the upper location only or target in the lower location).
location only). As shown by Miller and Lopes (1988), this test has the opposite bias, making it conservative.

The third test used the independent-opportunities equality. In order to conduct these tests, two values were required from each subject's data. The first, which corresponds to the left side of Equation 2, is the miss rate for the redundant-target condition. The second, corresponding to the right side of the equation, is the product of the two single-target miss rates. The difference between these two scores (right side minus left side) was calculated for each subject and a t test against zero was conducted.

In addition, the three tests presented by Mulligan and Shaw (1980) were also conducted. These tests do not "correct" the miss rates using the false-alarm rates; instead, these tests include the false-alarm rates directly within the equations. Both a review of these tests and a summary of the results of their use on the present data are provided in the Appendix.

Results

The data from 1 subject (in Experiment 3) were excluded from all reported analyses because the person appeared to have misunderstood the instructions and responded least often when two Xs were presented; the subject seemed to have read the instruction to respond when there was "at least one target" to mean "exactly one target." (No other subject showed this pattern.) The mean display durations for the data collecting phase of Session 2 was 58.3, 66.7, and 50 ms for Experiments 1–3, respectively.

For all three experiments, the miss rate on redundant-target trials was significantly lower than the miss rate on single-target trials: Experiment 1, t(5) = 11.32; Experiment 2, t(5) = 7.61; and Experiment 3, t(4) = 8.91 (all ps < .001). Thus, the expected redundancy gain was observed in all cases (see Table 4); there were fewer miss errors when there were two targets in the display. All three experiments also showed a significant redundancy gain using the more conservative, fixed-favored position test: Experiment 1, r(5) = 3.84; Experiment 2, r(5) = 5.70; and Experiment 3, r(4) = 7.22 (all ps < .01 or better).

The tests using the independent-opportunities equality (Equation 2) showed a significant violation only for the experiment that used a design with biased interstimulus contingencies (see the bottom row of Table 4). For Experiment 1, the miss rate on redundant-target trials was an insignificant 1% below that predicted by independent-decisions models, t(5) = 0.02. For Experiment 2, the miss rate on redundant-target trials was 7% below predicted, t(5) = 2.08, p < .05. For Experiment 3, the miss rate was within 0.5% of predicted, r(4) = 0.17.

Supplementary analyses using the three tests presented by Mulligan and Shaw (1980; see also Shaw, 1982) supported and extended these findings (see the Appendix).

Discussion

The results from these three experiments show that the predictions of an independent-decisions model were obeyed under most circumstances (for similar conclusions, see, e.g., Duncan, 1980 [combined-task conditions]; Gardner, 1973; Meijers & Eijkman, 1977; Mulligan & Shaw, 1980; Shaw, 1982; for an opposing view, see Kinchla & Collyer, 1974). Most important to this conclusion is that when no biased contingencies were included in the design of a redundant-target detection task (Experiment 1), performance was well predicted by models that assumed independent decisions concerning each stimulus. This was the same conclusion as that reached by Mordkoff and Yantis (1991) using RT; thus, the two methods converge on this point. The present findings also strengthen the evidence against models that posit a combined decision that is based on input from both display locations; that is, the coactivation models presented by Miller (1982b), Grice et al. (1984), Kinchla (1977), and Schwarz (1989). It is very difficult for models with any measurable amount of coactivation to explain why the independent-opportunities equality was obeyed in Experiments 1 and 3 (see also Mulligan & Shaw, 1980; Shaw, 1982).

However, consistent with the distinctions between perception and decision and between RT and accuracy (Santee & Egeth, 1982), the two experimental paradigms—resource-limited RT and data-limited accuracy—do not always produce the same results. Although the RT measure has shown sensitivity to both interstimulus and nontarget-response contingencies (see Experiments 1–3 of Mordkoff & Yantis, 1991), the present accuracy tasks were sensitive only to the interstimulus contingencies that are assumed to affect perceptual processes (compare the present Experiments 1 and 2). As predicted by the notion that accuracy is affected only by perceptual manipulations (at least in simple tasks with low memory loads), a positive value of the NRCB had no effect on detection accuracy (Experiments 1 and 3). These results support the proposed architecture of the interactive race model, which places the locus of interstimulus-contingency effects within perceptual processes and the locus for the effect of nontarget-response contingencies within processes related to decision or response selection. Put more simply, these results verify the distinction between interchannel crosstalk and nontarget-driven decision bias.

Table 4
Mean Corrected Miss Rates (in Percentages):
Experiments 1, 2, and 3

<table>
<thead>
<tr>
<th>Condition</th>
<th>Experiment 1</th>
<th>Experiment 2</th>
<th>Experiment 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Single target,</td>
<td>33</td>
<td>23</td>
<td>67</td>
</tr>
<tr>
<td>upper location</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Single target,</td>
<td>59</td>
<td>68</td>
<td>48</td>
</tr>
<tr>
<td>lower location</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean, single target</td>
<td>46</td>
<td>46</td>
<td>58</td>
</tr>
<tr>
<td>Redundant targets</td>
<td>17</td>
<td>8</td>
<td>35</td>
</tr>
<tr>
<td>Redundancy gain</td>
<td>29***</td>
<td>38***</td>
<td>23***</td>
</tr>
<tr>
<td>Predicted</td>
<td>18</td>
<td>15</td>
<td>35</td>
</tr>
<tr>
<td>(redundant targets)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Violation of</td>
<td>-1*</td>
<td>-7**</td>
<td>0*</td>
</tr>
<tr>
<td>Equation 5</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* p > .10. ** p < .05. *** p < .001.
General Discussion

Previous work concerning divided attention using RT has suggested that there are at least two types of contingency-sensitive mechanisms within the visual information-processing system (Mordkoff & Yantis, 1991; see also Garner, 1962; Miller, 1987): one that affects perception via interchannel cross talk and another that affects decision (or response selection) via nontarget-driven decision bias. Previous work comparing the results from data-limited accuracy tasks and resource-limited RT tasks (Santee & Egeth, 1982) has shown that in simple, small-display and low-memory-load situations, the former measure is sensitive only to perceptual manipulations, whereas the latter is sensitive to both perceptual and decision-related factors. The conjunction of these two points makes a set of predictions: Interstimulus contingencies should have an effect on accuracy, whereas nontarget-response contingencies should not affect accuracy. These predictions were confirmed in our experiments. In particular, interstimulus contingencies were shown to affect accuracy as they affected RT, but nontarget-response contingencies have now been shown to affect only RT. These results support the distinction between these two classes of contingency and contingency-sensitive mechanisms, as well as their assumed loci within the information-processing system, and also demonstrate the utility of using RT and accuracy in converging tests.

Previous Accuracy Studies of Divided Attention

In his study of the source of dual-task interference, Duncan (1980) included a condition that was very similar to our Experiment 3. In particular, Duncan’s “combined task” condition was an accuracy study of the redundant-signals effect. On analyzing the design used for his Experiment 1, for example, we found that the value of the NRCB was 0.22 (whereas the ISCB was 0). Using a test similar to our own independent-opportunities equality, Duncan also showed that the two target detections on redundant-target trials were statistically independent events (see pp. 277–278). Our Experiment 3 replicated this finding.

Similarly, the accuracy experiment of Mulligan and Shaw (1980) included a large value of the NRCB and a small amount of ISCB. By our analysis, the value of NRCB was 0.27 and the value of ISCB was 0.10. The data from 3 of their 4 subjects showed no violation of an independent-decisions test; the 4th subject showed a marginally significant violation (see Mulligan & Shaw, 1980, p. 476), which we would interpret as being caused by the small amount of ISCB contained within their design.

Contingency-Sensitive Mechanisms

Our results have provided evidence that interstimulus and nontarget-response contingencies affect different processing stages. Therefore, it makes sense to ask whether the two types of contingency-sensitive mechanisms also differ in other ways and how each mechanism operates. For example, how do identification channels learn and exchange information?

One possibility flows directly from the equation used to calculate the value of the ISCB (Equation 3). Assume that after each trial, the upper channel is provided with the identity of the letter that appeared in the lower display location. When combined with already-present information concerning what appeared in the upper location, this could be used to update the known conditional probabilities. The updated probabilities would be used to generate interchannel crosstalk on subsequent trials. Similar changes would also occur within the lower channel.

Another, more mechanistic model of both identification and interchannel crosstalk might assume that there is a complete set of letter representations (e.g., type nodes or logos) within each identification channel. As information accrues (in a given channel) favoring one letter over all others, the corresponding node becomes the most active. Next, also assume that there is a link between every node in the upper channel and every node in the lower channel. As interstimulus contingencies are learned over a series of trials, some of these links become stronger (or positive) and others become weaker (or negative). In this way, the contents of one channel may influence the processing within the opposite channel. Clearly, this particular viewpoint is most consistent with a connectionist approach (e.g., Rumelhart & McClelland, 1988).

A more simple model of interchannel crosstalk would discard the idea that each channel contains a set of nodes for every complete letter. Note that the stimuli (X, I, and O) used in the present accuracy study, as well as the previous RT study (Mordkoff & Yantis, 1991), were highly discriminable, being letters that share few features. Thus, we need only assume that information concerning the presence of certain features (e.g., line crossings, closed circles) may be exchanged.

A similar set of models can also be developed concerning nontarget-driven decision bias. However, Mordkoff (1991) presented evidence suggesting that interchannel crosstalk and nontarget-driven decision bias have at least one crucial difference: The former is specific to spatial locations, but the latter generalizes across locations (see, also, Miller, 1988). Thus, it is possible to learn both that an I in the upper location implies the presence of an X in the lower location and that an I in the lower location implies the absence of an X in the upper location because these are location-specific interstimulus contingencies. By contrast, it is not possible to learn both that an I in the upper location implies that a response should be made and that an I in the lower location implies that no response should be made because nontarget-response contingencies generalize (or “average out”) across locations. This difference is under further investigation.

To this point, we have argued that interchannel crosstalk occurs between identification channels (i.e., that the two sets of processes assigned to identify the two stimuli exchange information). An alternative explanation of the effects of interstimulus contingencies might posit that each identification channel instead has direct access to both stimuli. From this perspective, no information exchange (i.e., crosstalk) need be assumed because each channel would already contain all of the necessary data.
Although neither is a fatal flaw, there are at least two problems with this alternative. First, it seems implausible that stimuli are identified by mechanisms that are simultaneously sensitive to different features of the same object. Second, this view appears to violate the idea that stimuli are processed separately in the absence of contingencies, which our data directly supports.

Conclusion

This study had two purposes: to verify the two central claims of the interactive race model and to illustrate the usefulness of the converging application of RT and accuracy tasks to address a single question or issue. We believe that our data support both of these points.

References


Appendix

The Mulligan and Shaw (1980) Tests

Test of the Sharing Model

This test is identical to the independent-opportunities equality (Equation 2) when no false-alarm responses are made; the important difference is that one does not correct the miss rates prior to use. Only an independent-decisions model should satisfy this test (see Mulligan & Shaw, 1980, p. 472). In their notation,

\[ \ln P_U + \ln P_L = \ln P_{U+L} + \ln P_N, \]  

(A1)

where \( P_U \) and \( P_L \) are the probabilities of a miss error given a target in the upper location (only) and lower location (only), respectively; \( P_{U+L} \) is the probability of a miss given redundant targets; \( P_N \) is the probability of a correct rejection (i.e., no response given no targets); and \( \ln \) denotes the natural logarithm.

Test of the Type 1 Mixture Model

The Type 1 mixture model is, in essence, a large class of models including both the fixed- and random-favored-position models (for discussions of the fixed- and favored-position models in terms of RT, see Biederman & Checkosky, 1970; Miller & Lopes, 1988; Mullin, Egeth, & Mordkoff, 1988; van der Heijden, La Heij, & Boer, 1983). The model assumes that one display location receives all of the available attentional capacity on each trial (see Mulligan & Shaw, 1980, p. 473). Thus, this test is important when combined with any test for independent decisions: If the present test is failed but the tests for independent decisions are passed, then one may conclude more firmly in favor of models that posit independent, parallel decisions. The test is the same as the previous one, without taking logarithms:

\[ P_U + P_L = P_{U+L} + P_N. \]  

(A2)

Test of the Weighted Integration Model

The weighted integration model (Kincla, 1977; Kinchla & Collyer, 1974) is one specific coactivation model that applies to response accuracy under data-limited situations. According to this model, a combined decision that is based on weighted inputs from both display channels is made on each trial. Following the theory of signal detection (Green & Swets, 1966), the internal values of signal strength are assumed to be Gaussian distributed. Thus, the model predicts the following relation (see Mulligan & Shaw, 1980, p. 473):

\[ z_U + z_L = z_{U+L} + z_N, \]  

(A3)

where \( z_U \), for example, is the inverse unit normal (i.e., the "z score") of the miss rate for the condition with a target in the upper location (only).

Application of the Tests to Our Data

Our use of these three tests differs slightly from previous methods. Where Mulligan and Shaw (1980) applied each test to the data from each subject individually, we applied each test to all subjects at once using paired *t* tests.

The results from these tests are summarized in Table A1. In general, these tests verified the conclusions afforded by our use of the independent-opportunities equality (Equation 5). First, the data from Experiments 1 and 3 satisfied the test of the sharing model (Equation A1), but both sets of data violated both other tests (see Table A1). These findings support the argument that under these two situations (i.e., under experimental designs with no biased interstimulus contingencies), decisions are made independently. That

Table A1

<table>
<thead>
<tr>
<th>Model tested</th>
<th>Experiment 1</th>
<th>Experiment 2</th>
<th>Experiment 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sharing*</td>
<td><em>ns</em></td>
<td><em>p &lt; .05</em></td>
<td><em>ns</em></td>
</tr>
<tr>
<td>Type 1 mixture**</td>
<td><em>p &lt; .025</em></td>
<td><em>ns</em></td>
<td><em>p &lt; .025</em></td>
</tr>
<tr>
<td>Weighted Gaussian integration*</td>
<td><em>p &lt; .01</em></td>
<td><em>ns</em></td>
<td><em>p &lt; .01</em></td>
</tr>
</tbody>
</table>

* Independent decisions. ** Favored positions. * A coactivation model.
both violated the test of the Type 1 mixture model is important in
that this rules out an alternative explanation of why the data from
these two experiments did not violate the independent-opportunities
equality: In particular, a model that posits that only one location is
monitored on each trial is ruled out by this test.

The results from application of these tests to the data from Ex-
periment 2 also support the conclusions reached in the text. In this
case, the sharing model was rejected, but neither of the non-
independent-decisions models could be ruled out (see Table A1).

Search Opens for Editor of New APA Journal

The Publications and Communications Board has opened nominations for the editorship of
a new journal, Psychological Methods, for the years 1996–2001. Candidates must be
members of APA and should be prepared to start receiving manuscripts early in January of
1995 to prepare for issues published in 1996 and beyond. Please note that the P&C Board
encourages participation by members of underrepresented groups in the publication process
and would particularly welcome such nominees. To nominate candidates, prepare a state-
ment of one page or less in support of each candidate. Submit nominations to

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Austin, TX 78712
or
Foss@psyvax.psy.utexas.edu

Psychological Methods will be devoted to the development and dissemination of methods
for collecting, understanding, and interpreting psychological data. Its purpose is the
dissemination of innovations in research design, measurement, methodology, and statistical
analysis to the psychological community; its further purpose is to promote effective
communication about related substantive and methodological issues. The audience is
diverse and includes those who develop new procedures, those who are responsible for
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who employ those procedures in research. The journal solicits original theoretical, quanti-
tative, empirical, and methodological articles; reviews of important methodological issues;
tutorials; articles illustrating innovative applications of new procedures to psychological
problems; articles on the teaching of quantitative methods; and reviews of statistical
software. Submissions will be judged on their relevance to understanding psychological
data, methodological correctness, and accessibility to a wide audience. Where appropriate,
submissions should illustrate through concrete example how the procedures described or
developed can enhance the quality of psychological research. The journal welcomes
submissions that show the relevance to psychology of procedures developed in other fields.
Empirical and theoretical articles on specific tests or test construction should have a broad
thrust; otherwise, they may be more appropriate for Psychological Assessment.

First review of nominations will begin December 15, 1993.