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Attention: Reaction Time and Accuracy Reveal Different Mechanisms

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The authors propose that there are 2 different mechanisms whereby spatial cues capture attention. The voluntary mechanism is the strategic allocation of perceptual resources to the location most likely to contain the target. The involuntary mechanism is a reflexive orienting response that occurs even when the spatial cue does not indicate the probable target location. Voluntary attention enhances the perceptual representation of the stimulus in the cued location relative to other locations. Hence, voluntary attention affects performance in experiments designed around both accuracy and reaction time. Involuntary attention affects a decision as to which location should be responded to. Because involuntary attention does not change the perceptual representation, it affects performance in reaction time experiments but not accuracy experiments. The authors obtained this pattern of results in 4 different versions of the spatial cuing paradigm.

It has long been recognized that one can fixate one's eyes on one location yet attend to another location. For example, Helmholtz (1925) noted, "It is a curious fact . . . that the observer may be gazing steadily . . . yet at the same time he can concentrate his attention on any part of the dark field he likes" (p. 455). Wilhelm Wundt extended Helmholtz's observation by noting that there are two forms of attention, voluntary and involuntary, but he thought that involuntary attention was just a simpler form of voluntary attention. He commented, "The fact that so-called involuntary attention is only a simpler form of internal volition, was entirely overlooked" (Wundt, 1897, p. 219).

Wundt's (1897) terms *voluntary* and *involuntary* attention map nicely onto terms used today, such as *endogenous* versus *exogenous* attention, *automatic* versus *controlled* attention, or *pull* versus *push* attention. Although Wundt noted that the factors driving voluntary and involuntary attention differ, he clearly believed that the same mechanism was responsible for these forms of attention. In contrast to Wundt, we propose that voluntary and involuntary attention serve different functions and may be controlled by different mechanisms. We propose that voluntary attention affects the perceptual representation and will affect both accuracy and reaction time (RT) experiments, whereas involuntary attention does not affect the quality of the perceptual representation and is manifest only in RT experiments.

To investigate this distinction, we used the spatial cuing task developed by Michael Posner and his colleagues (e.g., Posner, 1978, 1980; Posner, Nissen, & Ogden, 1978; Posner, Snyder, & Davidson, 1980). In this paradigm, observers engage in a detection or identification task with a peripherally presented stimulus. However, before the stimulus appears, observers are precued to the possible location of the stimulus. On *valid trials,* the cue indicates the target location. On *invalid trials,* the cue indicates a nontarget location. Observers are not allowed to move their eyes to the cued location, and hence, differences in performance between valid and invalid trials are said to reflect differences in attention that are independent of fixation.

Jonides (1976, 1980, 1983) investigated the distinction between voluntary and involuntary attention in this paradigm using a very simple manipulation. To investigate voluntary attention, the cue was made informative or predictive of the target location. For example, with four possible stimulus locations, the cue was valid on 80% of the trials. Thus, it is strategically advantageous for observers to voluntarily shift their attention to the cued location, because on average, the target stimulus will appear in that location. To investigate the automatic or involuntary allocation of attention, Jonides used cues that were nonpredictive or noninformative about the target location. With four possible target locations, if 25% of the trials are valid, the cue provides no information about the target location. Thus, with physically the same cue and stimulus, voluntary and involuntary attention can be operationally distinguished by whether the cue is informative or noninformative as to the stimulus location. Jonides (1976) argued that if the cue were in the

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periphery, it would activate automatic (involuntary) attention. Thus, noninformative cues activate involuntary attention. Because the informative cues are peripheral, they activate involuntary as well as voluntary attention.[1]

Generally, involuntary attention is associated with a peripheral cue, whereas voluntary attention is associated with a central cue, often symbolic in nature (see Jonides, 1981). However, recent evidence suggests that the peripheral nature of the cue is not a necessary feature of involuntary attention (Driver et al., 1999; Friesen & Kingstone, 1998; Kingstone, Smilek, Ristic, Friesen, & Eastwood, 2003; Lambert & Duddy, 2002; Langton, Watt, & Bruce, 2000; Ristic, Friesen, & Kingstone, 2002; Tipples, 2002). Other distinctions, such as push–pull cues or endogenous–exogenous cues, do not have clear operational definitions. Consider the *gaze direction effect,* in which a central cue, a face gazing to the left or right, affects involuntary attention (e.g., Friesen & Kingstone, 1998). It is not clear whether this kind of effect is push or pull or endogenous or exogenous. In the experiments reported here, involuntary and voluntary attention have clear operational definitions: Experiments with noninformative cues activate only involuntary attention, whereas experiments with informative cues activate both types of attention.

We hypothesize that voluntary and involuntary attention serve different functions and affect different processes. Voluntary attention, with cues that are informative as to the target location, affects a process we term *channel enhancement,* whereas involuntary attention, with noninformative cues, affects a process we call *channel selection* (see Figure 1).

By channel enhancement, we mean that the visual system gathers more information from attended locations than from unattended locations. Channel enhancement changes the perceptual representation so that observers have a generally more veridical representation of the stimulus that they are attending to (Prinzmetal, Amiri, Allen, & Edwards, 1998; Prinzmetal, Nwachuku, Bodanski, Blumenfeld, & Shimizu, 1997; Prinzmetal & Wilson, 1997). Channel enhancement corresponds to what Lu and Dosher (1998) term *signal enhancement*. Consider Figure 1A, in which observers have to determine whether the target letter is *F* or *T*. If the rightmost position (iv) is cued, observers will be more accurate if the target is in that position, because there is more information about the stimulus in that position. Channel enhancement should affect performance in experiments designed around accuracy with briefly presented or degraded stimuli, because there is more information in the attended location. Channel enhancement may also affect RT, because information is presumably gathered faster in the cued than in the uncued location. Our claim is that channel enhancement occurs only with voluntary attention; that is, the cues must carry information about the target location.

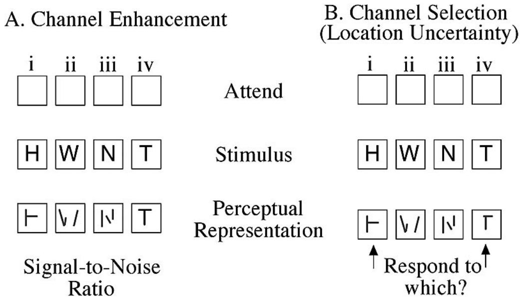

Figure 1. Two mechanisms of attention. With channel enhancement (A), if the rightmost position (iv) is cued, there is more information about the stimulus in that location than in other locations. With channel selection (B), the perceptual representation of the stimulus in the cued location is not affected. Channel selection affects accuracy only if there is uncertainty about which location contains the target.

Channel selection, on the other hand, does not affect the perceptual representation. Rather, channel selection affects a decision process: Which location contains the target? Because channel selection does not affect the perceptual representation, one would not expect channel selection to affect identification or detection accuracy under most circumstances. There is one situation, however, where channel selection can affect accuracy: when there is location uncertainty (Luck, Hillyard, Mouloua, & Hawkins, 1996; Luck & Thomas, 1999; Prinzmetal et al., 1997, Experiments 5 and 6; Shiu & Pashler, 1994; Warner, Joula, & Koshino, 1990). This situation arises when observers cannot determine, with near 100% accuracy, which location contained the target. Such a situation might arise when the stimuli are masked so completely that the observer is unsure of a target's presence and therefore unsure of the target's location. Consider an experiment in which observers must determine whether the stimulus contains the letter *F* or *T* (see Figure 1B). If the rightmost position (iv) is cued, the observer would probably respond *T* because the perceptual representation is closer to a *T* than to an *F*. However, if the leftmost position (i) is cued, the perceptual representation in that position resembles an *F* more than a *T*. Hence, reporting information from the cued position, under conditions of position uncertainty, can cause observers to be more accurate at valid trials than at invalid trials. However, if there is no location uncertainty, channel selection predicts that observers will be faster with valid trials than with invalid trials in an experiment built around RT, but there should be no difference in an accuracy experiment. We designed the present experiments so that observers could accurately determine which location contained the target but not necessarily which target was present.

Channel selection does not affect the perceptual representation and could be described formally as a decision process, whereas channel enhancement does affect the perceptual representation. One might be tempted to describe channel enhancement as "early" and channel selection as "late." This description is not appropriate. The labels "early" and "late" are as yet not well defined in terms of these attention processes. Surely channel enhancement involves some "selection," but it is selection for enhanced perceptual processing and could be mediated by a different neural mechanism than channel selection. Channel selection might involve selection for action—which object should be responded to (e.g., Allport,

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[1] For voluntary attention, we do not know whether awareness of the cue contingencies plays a role, and thus we do not know whether *voluntary* involves a conscious choice. What we mean by *voluntary* is perhaps better captured by the word *strategic*. Because of the contingencies of the experiment, it is advantageous to attend to a particular location. However, we use the terms *voluntary* and *involuntary* because these were the terms first used by Wundt, and the field already has a surplus of terms.

1989)—in which case it might be an entirely different pathway than selection for perception (e.g., Rafal, Posner, Friedman, Inhoff, & Bernstein, 1988). We do not make any claims about the time course of these processes: whether they are early or late or whether one must precede the other. We only claim that they are functionally different and that the difference has behavioral consequences.

In the experiments reported here, we test the hypothesis that voluntary attention will affect both accuracy and RT studies whereas involuntary attention will affect only RT studies. Our interpretation of such a finding would be that voluntary attention operates via channel enhancement whereas involuntary attention operates via channel selection.

Note that when we refer to accuracy or RT experiments, we do not just mean a difference in the dependent variable. These experiments are fundamentally different. In RT experiments, observers can be nearly 100% correct, and RT is the variable of interest. In an accuracy experiment, the stimulus is degraded, so that no matter how long observers take in responding, they cannot be 100% correct. To run an accuracy experiment, observers must be urged to take their time and be as accurate as possible. Furthermore, there should be a fairly strong motivation to follow these instructions. In our accuracy experiments, for example, when observers err, the computer makes a loud, obnoxious noise. Furthermore, instructions are made repeatedly to "take your time and be as accurate as possible."

It is easy to see that unless precautions are taken, an effect in RT could contaminate accuracy. Consider the Stroop effect (MacLeod, 1991; Stroop, 1935). There is fairly good agreement that the Stroop effect reflects response selection and not the perceptual representation (e.g., Baldo, Shimamura, & Prinzmetal, 1998; Virzi & Egeth, 1985). For example, if the word *RED* is printed in blue ink, observers are slower to report the color of the ink than if the word is *BLUE*. However, we do not suppose that the word *RED* causes the ink color to look reddish-blue (i.e., magenta). Rather, the word causes competition in response selection. In the Posner task, this corresponds to competition in location selection (for memory or action). Thus, like the Stroop task, one would expect involuntary attention to affect RT but not accuracy. However, if observers are under any pressure to respond quickly, accuracy will also be affected. Note that the issue is not one of a speed–accuracy trade-off (Pachella, 1974). Rather, any pressure to respond quickly can contaminate an accuracy study.

Often researchers tend to expect accuracy and RT to yield the same result, if there is no speed–accuracy trade-off or a ceiling or floor effect. However, there is precedence for accuracy and RT studies to yield different effects (e.g., Moore & Egeth, 1998; Moore, Yantis, Vaughan, & Handwerker, n.d.; Mordkoff & Egeth, 1993; Santee & Egeth, 1982). For example, Santee and Egeth (1982) were interested in the redundant targets paradigm, in which a target letter may be repeated in a display. They noticed that in RT experiments, repeating a target in a display made RT faster (B. A. Eriksen & Eriksen, 1974; C. W. Eriksen & Eriksen, 1979; C. Eriksen & Schultz, 1979), a phenomenon known as the *flanker effect*. However, in accuracy experiments, observers were less accurate when a target was repeated in the display (Bjork & Murray, 1977; Santee & Egeth, 1980). Santee and Egeth conducted almost identical accuracy and RT experiments and found that these experiments gave the opposite results. They concluded that "accuracy is sensitive to early perceptual interference between target and noise items, whereas reaction time is more sensitive to later processes involved in response interference" (Santee & Egeth, 1982, p. 489).

In summary, we hypothesize that the attentional mechanism responsible for voluntary attention is different from the mechanism responsible for involuntary attention and that the former will affect accuracy whereas the latter will not. There is evidence that voluntary and involuntary attention do differ. The stimulus onset asynchrony (SOA; time between onset of cue and target) needed is less for involuntary than for voluntary attention (e.g., Posner, Cohen, & Rafal, 1982; Warner et al., 1990). In our experiments, noninformative cues can have an effect even if they are presented simultaneously with the target. Furthermore, with noninformative cues, if the SOA increases beyond about 500 to 1,000 ms, the facilitation in RT for valid trials turns into inhibition, whereas this is not the case with informative cues (Posner & Cohen, 1984). Thus, we expect voluntary attention with informative cues and fairly long SOAs. Hence, differences in accuracy with informative cues should occur only with long SOAs (Luck et al., 1996).

This article is rather long, with many experiments, and so we have divided it into four parts. Each part explores a different version of the spatial cuing paradigm. The experiments reported in Part 1 use a visual cue and letter discrimination task. Part 2 explores cross-modal cuing with auditory cues and visual targets. Part 3 uses visual cues but a face discrimination task. In the final section we investigate a recent article by Handy, Jha, and Mangun (1999) that was very similar to our experiment but reported that noninformative cues did affect accuracy (see also Luck & Thomas, 1999). In this section we also account for other claims that involuntary attention affects accuracy (e.g., Dufour, 1999; Henderson & Macquistan, 1993; Klein & Dick, 2002; McDonald, Teder-Saelejaervi, & Hillyard, 2000). In each of these four paradigms, we found that voluntary attention (informative cues) affected performance in both RT and accuracy studies (as long as the SOA was long enough) but that involuntary attention influenced performance only in RT studies.

Part 1: The Basic Findings

Experiments 1–4: Core Experiments

The first four experiments form a set: Two experiments were RT experiments (Experiments 1 and 3) and two were accuracy experiments (Experiments 2 and 4). There were four possible stimulus locations (see Figure 2). Experiments 1 and 2 investigated voluntary attention: The cue was informative, indicating the target position on 80% of the trials. Experiments 3 and 4 investigated involuntary attention: The cue was noninformative; only 25% of trials were valid. In Experiments 1 to 4A, there were two SOAs: 0 ms and 300 ms. We predicted that in the RT experiments, valid trials would have faster responses than invalid trials, regardless of SOA or whether the cue was informative. In the accuracy experiments, we believed accuracy would be higher with valid than with invalid trials only with informative cues and long SOAs. Because with involuntary attention we predicted no effect on accuracy (i.e., the null hypothesis), we added Experiment 4B, which was identical to Experiment 4A but included additional SOAs of 50 ms and 150 ms.

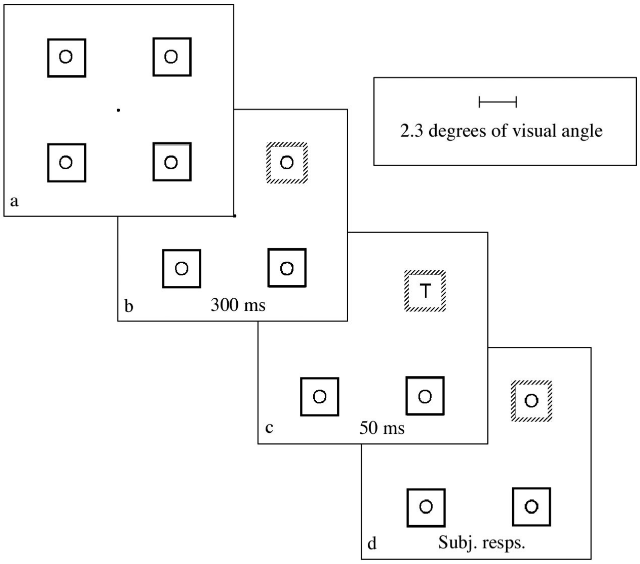

Figure 2. Sequence of events in a long stimulus onset asynchrony trial. The dashed square was a red square in the experiment. The figure is drawn to scale. Subj. resps. = subject responds.

Method

Procedure. The observer's task was to indicate whether a briefly presented display contained the letter *F* or *T* by pressing one of two buttons. The sequence of events in a long SOA trial is shown in Figure 2. The trial began with four gray squares and the letter *O* marking the possible stimulus positions. On each trial, one of the gray squares was replaced with a red square (illustrated with dots in the figure). After 300 ms, one of the letter *O*s was replaced with either *F* or *T,* which remained in view for 50 ms. The cue remained in view until the observer responded in order to minimize interference by the cue on the target (Tassinari, Aglioti, Chelazzi, Peru, & Berlucchi, 1994). The lines that made the gray squares were 1 pixel wide, and the lines of the red square were 4 pixels wide. On trials with 0 ms SOA, the stimulus letter appeared simultaneously with the red square (Panel B in Figure 2 was eliminated). Eye movements were monitored with an Applied Science Laboratory 210 monitor with infrared-sensitive diodes. When an eye movement was detected, the computer emitted a two-tone sequence that sounded like a foghorn. Trials on which eye movements were detected were eliminated from the analysis. In none of the experiments did this exceed 1% of the trials. The maximum percentage of trials eliminated for any one observer's data was 3%.[2]

Each observer had at least one block of practice trials followed by five blocks of test trials. In the experiments with informative cues (Experiments 1 and 2), there were 40 trials per block (32 valid and 8 invalid). In the experiments with noninformative cues, there were 48 trials per block (12 valid and 36 invalid). Half of each cue type used the short SOA, and half used the long SOA. Each experiment took about 30 min.

In the RT experiments, observers were encouraged to be fast but near 100% accurate. Helvetica 36-point type was used (subtending approximately 1.29° in height). In the accuracy experiments, observers were encouraged to take their time and be as accurate as possible. The type size was varied during practice and between blocks, individually for each observer, to obtain approximately 75% accuracy. In all experiments, the computer emitted a tone when the observer responded incorrectly. The tone was 150 Hz square-wave, 0.2 s in length, and approximately 80 db. The average type size in the two accuracy experiments was 14 points and ranged from 11 to 20 points (approximately subtending 0.64° to 0.40° in height).

Stimuli. The stimuli were presented on an Apple 17-in. monitor. The viewing distance was 40 cm, and a chin rest was used to minimize head movements. The stimuli were drawn as shown in Figure 2, which is drawn to scale. The cue boxes subtended 2.3° of visual angle (40 × 40 pixels). The letter *O* was black and was the same size as the target letter (36 point in the RT experiments; smaller in the accuracy experiments). The boxes were drawn in gray with lines 1 pixel wide (approximately 0.06° of visual angle), except the red cue box, which was 4 pixels wide (approximately 0.24° of visual angle). The monitor was set to 832 × 624 pixels and 75 Hz. The room was illuminated with overhead fluorescent lighting.

Observers. Twelve observers, ages 18 to 25, were tested in each experiment. In all of the experiments reported in this article, observers were recruited from the subject pool of the Psychology Department at the University of California, Berkeley and were given course credit for their participation. Over the experiments, approximately half of the observers were female and half were male. No observer served in more than one experiment.

Results

RT experiments. Results from RT experiments are shown in Figure 3. For the RT experiments (Experiments 1 and 3), we examined only correct responses when no eye movement was detected. Responses were faster following a valid rather than an invalid cue. This effect was reliable when the cue was informative and when it was noninformative, $F(1, 11) = 42.91$ and 19.56, $p < .01$, for informative and noninformative, respectively. The effect sizes (Cohen's *d*) for informative and noninformative validity effects were 1.14 and 0.35, respectively.[3]

We compared the magnitude of the cuing effect across experiments, with observers nested in experiment. As is evident in Figure 3, the cuing effect was greater with informative cues than with noninformative cues, $F(1, 22) = 19.35$, $p < .01$. This result is not surprising, because in the noninformative cue experiment, only involuntary attention is present, whereas the informative cue experiment has both involuntary and voluntary components. It is noteworthy that considering only the noninformative cue and short SOA condition (0 ms), observers were significantly faster with valid than with invalid cues, $t(11) = 1.80$, $p < .05$ (two-tailed), $d = 0.24$.

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[2] We tested the accuracy of our ability to detect eye movements with both the apparatus used in Part 1 and the direct observation of eye movements in the other sections in the following manner. We created a test program with eye movement targets, laid out as in Figure 2, but with the distance from the fixation point to the center of the square subtending 2° of visual angle (less than half the distance used in these experiments). In calibrating the apparatus, we found that our hit rate to detect movements was above .99 and our false alarm rate was about .03. The high false alarm rate was due to an inability to discriminate eye movements from blinks with the infrared spectacles. When we went to direct video observation of eye movements (see Part 2), the false alarm rate dropped to about .01, with no decrement in hit rate.

[3] Cohen's *d* was calculated from the individual-condition standard deviations as if the experiment was an independent groups experiment, as suggested by Dunlap, Cortina, Vaslow, and Burke (1996). The correlated-samples version can be calculated from the *F* or *t* value (see Dunlap et al., 1996).

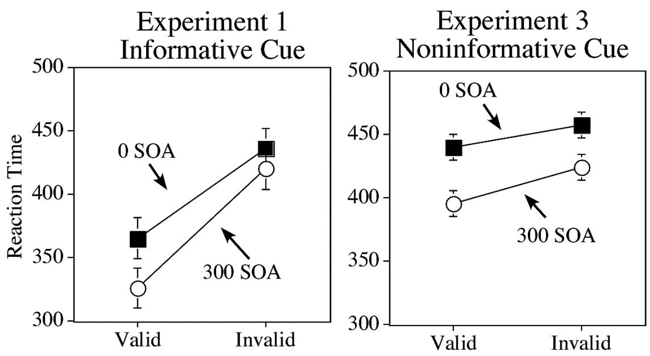

Figure 3. Reaction time results from Experiments 1 and 3. Error bars represent 95% confidence intervals for a within-subject design (Loftus & Masson, 1994). SOA = stimulus onset asynchrony. Reaction times and SOAs are given in milliseconds.

In both experiments, observers were faster with long than with short SOA, $F(1, 11) = 9.60$ ($d = 0.58$) and $F(1, 11) = 24.17$ ($d = 0.60$), $p < .05$, for informative and noninformative experiments, respectively. This result was probably the consequence of the general alerting function of the cue and did not interact with the cue effect in either experiment.

These results are not a consequence of a speed–accuracy tradeoff (Pachella, 1974), because in both experiments, observers were more accurate following a valid cue (98.1% vs. 95.0% and 99.4% vs. 98.7% for Experiments 1 and 3, respectively). The difference was reliable in Experiment 1, $F(1, 11) = 5.39$, $p < .05$ ($d = 0.98$), but not quite reliable in Experiment 3, $F(1, 11) = 3.64$, $p = .08$ ($d = 0.88$).

Accuracy experiments. The results for the accuracy experiments appear in Figure 4 and Table 1. The results in the accuracy experiments were different than in the RT experiments in almost every condition.

For informative cues (Experiment 2; see Figure 4, left panel), there was an interaction between cue type and SOA, $F(1, 11) = 11.70$, $p < .01$. For short SOAs, observers were reliably *less* accurate with valid cues than with invalid cues, $t(11) = 3.09$, $p < .01$, $d = 0.76$. For the long SOAs, observers were *more* accurate following a valid cue, $t(11) = 2.18$, $p < .05$, $d = 0.72$. Thus, only with an informative cue and a long SOA do the accuracy results match the RT results.

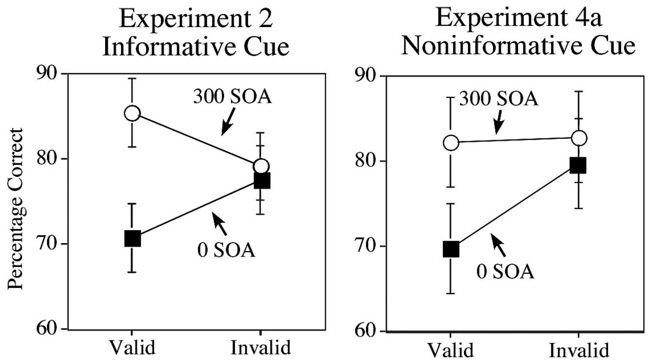

Figure 4. Accuracy results from Experiments 2 and 4A. Error bars represent 95% confidence intervals for a within-subject design. SOA = stimulus onset asynchrony. SOAs are given in milliseconds.

Table 1
Results From Accuracy Experiments

			Informative		Noninformative	
Experiment	*n*	SOA	Valid (%)	Invalid (%)	Valid (%)	Invalid (%)
			Part 1			
Experiment 2	12	0	76.6	77.5		
Experiment 2	12	300	85.4	79.1		
Experiment 4A	12	0			69.7	79.7
Experiment 4A	12	300			82.2	82.8
Experiment 4B	12	50			80.5	79.3
Experiment 4B	12	150			80.3	80.1
			Part 2			
Experiment 7	16	50/200	80.0	75.1		
Experiment 8A	16	50/200			81.6	81.9
Experiment 8B	12	50/200			79.7	80.3
Experiment 8C	16	50			79.2	79.4
			Part 3			
Experiment 10	18	100	81.3	75.8		
Experiment 11	18	100			79.1	79.4
			Part 4			
Experiment 14	16	166.7			80.9	80.3

Note. SOA = stimulus onset asynchrony. SOAs are given in milliseconds.

For noninformative cues (Experiment 4), observers were more accurate on invalid trials than on valid trials, $F(1, 11) = 7.87$, $p < .05$, $d = 1.00$ (see Figure 4, left panel). Performance was more accurate with long (300-ms) than short (0-ms) SOA, $F(1, 11) = 12.90$, $p < .01$, $d = 1.38$. The interaction between cue and SOA was not reliable, $F(1, 11) = 4.04$, $p = .07$, but the disadvantage for valid trials was evident only for the short SOA. For the 0-ms SOA, observers were significantly less accurate on valid trials than on invalid trials, $t(11) = 2.69$, $d = 1.14$. The difference in accuracy for the 300-ms SOA was not reliable (82.2% vs. 82.8%), $t(11) = 0.29$, $p = .77$, $d = 0.09$.

It was suggested to us that the 0-ms SOA condition was atypically short (see Figure 4), and the long SOA may have been too long to pick up an accuracy effect on involuntary attention, so we ran 12 more observers with 50-ms and 150-ms SOA (Experiment 4B). These results are shown in Table 1. These resemble the 300-ms SOA condition. There is no evidence of an effect of the cue, $F(1, 11) = 0.21$, $d = 0.18$. It is probably true that the 0-ms SOA condition is atypical; it is the only condition in which valid trials were less accurate than invalid trials in our experiments (however, Luck et al., 1996, found this effect with 100-ms SOA).

One might wonder whether the failure to obtain a significant advantage for valid trials with nonpredictive cues was simply a matter of not enough power. In order to have a more powerful analysis for an advantage of cues with involuntary attention on accuracy, we conducted a post hoc meta-analysis that combined Experiments 4A (300-ms SOA only) and Experiment 4B (both SOAs) with experiment as a between-subjects factor and cue validity as a within-subject factor. The average percentages correct were 81.3% and 81.2% for valid and invalid trials, respectively, $F(1, 22) = 0.003$, $p = .94$, $d = 0.01$. Of these 24 observers, 12 were more accurate with valid and 12 were more accurate with invalid trials. Thus, it does not seem that merely running more observers would cause the effect to be reliable.[4]

We ran a post hoc analysis of all the accuracy experiments that compared voluntary attention (Experiment 2) with involuntary attention (Experiments 4A and 4B) but excluded the 0-ms SOA condition because that condition is atypical (see Figure 4). The interaction of cue (valid vs. invalid) and type of attention (voluntary vs. involuntary) was reliable, $F(1, 34) = 5.10$, $p < .05$.

The results with accuracy are probably not a consequence of a speed–accuracy trade-off. Unlike the main analysis of RT, in checking for a speed–accuracy trade-off in accuracy experiments, we used all RTs, not just correct RTs. The reason for this approach is as follows. Consider an experiment with two conditions, A and B, with more errors in A. An explanation of the difference that appeals to a speed–accuracy trade-off is that observers are responding faster on A than on B. Some of these fast responses lead to errors. Removing error responses would hide the speed–accuracy trade-off. Thus, in the experiments designed around RT, we used correct RTs as the dependent variable, but in the analysis of experiments designed around accuracy, we used all responses in order to check for a speed–accuracy trade-off.

For the informative cues, there was a significant effect of SOA with RT that was similar to the accuracy data. Observers were significantly faster with long than with short SOA trials (753 ms vs. 887 ms), $F(1, 11) = 7.03$, $p < .05$, $d = 0.51$. There was also a significant effect of cue type: Observers were faster with valid than with invalid cues (752 ms vs. 889 ms), $F(1, 11) = 34.32$, $p < .01$, $d = 0.55$. However, the interaction between SOA and cue type was not significant, $F(1, 11) = 2.43$, $p = .14$. Note that these RTs are considerably longer than in the experiments designed around RT, because observers were urged to take their time and be as accurate as possible.

An analysis of variance on the RTs with noninformative cues yielded no significant effects in Experiment 4. Observers were slightly faster with valid than with invalid cues (659 ms vs. 673 ms), but this difference was not significant, $F(1, 11) = 0.08$, $p = .78$, $d = 0.08$.

Discussion

The most striking thing about these experiments is that in every condition but one, the RT experiments and the accuracy experiments yielded different results. The only case in which the RT experiment and the accuracy experiment had the same result was with an informative cue and a long SOA. Here, observers were both faster and more accurate with a valid cue. This situation is the paradigmatic case of voluntary attention: The cue is informative; therefore, it should strategically engage attention, and there is sufficient time for the focus of attention to be shifted.

We wanted to be sure that the effect with voluntary attention was a genuine attention effect and not the result of location uncertainty. We ran 4 additional observers in a similar accuracy experiment, except that the observers' task was to indicate the location of the target letter. We used 10-point type, which was smaller than we had used in Experiments 2 and 4. In all other respects, the experiment was identical to Experiment 2. Location judgments were 99% correct for both valid and invalid cues with the short SOA and 100% correct for valid and invalid cues with the long SOA. The effect of voluntary attention on accuracy is a genuine effect of channel enhancement and not the result of location uncertainty.

The failure to obtain an effect in the accuracy experiment with noninformative cues would not be particularly interesting if we had not obtained significant benefits of the cues in the RT experiments for both informative and noninformative cues. Hence, our results cannot be dismissed by arguing that we did not use effective cues. One might be tempted to argue that the discrimination in our task was one that was data limited and hence would not show effects of attention (Norman & Bobrow, 1975). However, we did obtain significant effects of the cues in accuracy with voluntary attention. Hence our cues could have been effective in drawing attention in such a manner as to engage channel enhancement and affect the perceptual representation with informative cues.

Finally, it was suggested to us that the failure to find an effect on accuracy with a noninformative cue was due to a speed–accuracy trade-off. Consider the long SOA condition of the accuracy experiment with a noninformative cue (Experiment 4). Observers were faster on valid trials than on invalid trials, 573 ms versus 673 ms—an attention effect of 100 ms. Perhaps observers were responding so fast on valid trials that they then made more errors and this negated the advantage in accuracy that would have occurred had there not been this trade-off. This explanation does not take into account the informative-cue accuracy experiment (Experiment 2), where in the long SOA condition, observers were 182 ms faster on valid than on invalid trials (i.e., 661 ms vs. 843 ms) yet were still significantly more accurate on valid trials. If there was a speed–accuracy trade-off, it should have had a greater influence reducing accuracy on valid trials with informative cues. Yet despite the difference in RT, with informative cues observers were significantly more accurate on valid than on invalid trials. A speed–accuracy trade-off cannot account for the null results with a noninformative cue and, at the same time, the positive results with an informative cue.

Experiment 5: Hybrid Accuracy and RT

An attribute of our accuracy experiments is that accuracy was repeatedly emphasized and observers were explicitly told to take their time and be as accurate as possible. Further, the consequences of an error were that the computer emitted a harsh, unpleasant sound. Thus, we believe that they were pure accuracy experiments.

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[4] The 95% confidence interval in the combined analysis was ±1.9; thus, we can be 95% confident that if cues have an effect, this effect is no greater than 1.9%. However, perhaps the best way to approach the issue of proving the null hypothesis is with several independent replications, and that is part of the motivation of Parts 2, 3, and 4.

There are "hybrid" experiments that emphasize both speed and accuracy and that find noninformative cues affect accuracy (e.g., Cheal & Chastain, 1999). Instructions to be both fast and accurate may yield errors that reflect nonperceptual processes. Consider the Stroop effect, for example. The Stroop effect is usually not considered the result of perceptual processes but rather the result of a decision or response stage of processing (e.g., MacLeod, 1991). Yet in the Stroop effect, instructions to be both fast and accurate will yield more errors in inconsistent color–word trials than in consistent color–word trials. These errors are probably not related to perceptual processes.

We wanted to determine whether instructions to be fast and accurate would yield different results than instructions to be as accurate as possible. Hence, Experiment 5 was nearly identical to the Experiment 1, 0-ms SOA condition, in which accuracy was nearly perfect. However, instead of instructing observers to be as accurate as possible, we instructed them to be fast and accurate. Furthermore, we did not provide trial-by-trial auditory feedback.

Method

Experiment 5 was similar to Experiment 1 (RT). We used large, 36-point type and informative cues. We used only the short SOA (0 ms), for which observers had previously performed less accurately on valid trials.[5] The only differences between this experiment and Experiment 1 were that (a) observers were told to be fast *and* accurate and (b) there was no trial-by-trial auditory feedback. We suspected that any pressure to respond quickly would contaminate the accuracy results. That is, informative cues and 0-ms SOA would appear to affect accuracy and the perceptual representation much as any speed pressure would cause an accuracy effect in the Stroop task. Eight observers were tested.

Results and Discussion

Not surprisingly, observers were faster with valid than with invalid cues (see Figure 5). This difference was reliable for the incorrect trials (321 ms vs. 447 ms), $t(7) = 2.39$, $p < .05$, $d = 0.61$, and marginally reliable for the correct trials (325 ms vs. 389 ms), $t(7) = 2.13$, $p = .07$ (two-tailed), $d = 0.52$.

The accuracy results were very different than in the previous equivalent accuracy experiment (Experiment 2), where with short SOA, observers were significantly less accurate with valid trials. In this experiment, observers were significantly more accurate with valid than with invalid trials (89.4% vs. 82.5%), $F(1, 7) = 10.89$, $p < .05$, $d = 1.24$. However, we know that the errors are not because the stimuli were difficult to perceive, because with identical stimuli in Experiment 1, observers averaged 97.2% correct. Thus, some speed pressure can cause observers to err not because they cannot correctly perceive the stimuli but because they respond too quickly.

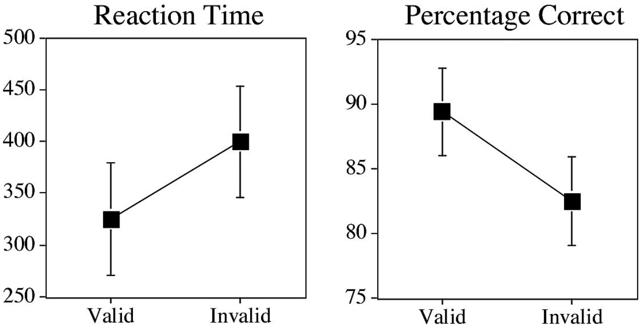

Figure 5. Results of Experiment 5, in which observers were told to be fast *and* accurate. Error bars represent 95% confidence intervals for a within-subject design. The reaction time results are in milliseconds.

Note that the pressure to respond too quickly can contaminate an accuracy experiment. However, this contamination is not the same problem as a speed–accuracy trade-off. There was a speed–accuracy trade-off in this experiment in the sense that overall, observers made more errors than in Experiment 3 because we encouraged them to respond quickly and did not penalize them for errors. However, the *pattern* of results cannot be accounted for by a speed–accuracy trade-off. Observers were both faster and more accurate with valid trials. However, we cannot conclude that the results reflect the perceptual representation, because we know that observers could have been nearly 100% correct if there had been no speed pressure.

We believe that the pressure to respond too quickly can be very subtle. Consider an experiment by McDonald et al. (2000, Experiment 2), which may have been a hybrid experiment.[6] Although observers were instructed to be accurate, there were several aspects of the experiment that suggest that accuracy could have been contaminated by RT. For example, there was no trial-by-trial accuracy feedback. Hence, there was little immediate incentive for observers to take their time and be as accurate as possible. Furthermore, observers had to respond within 2.0 to 2.5 s or the next trial commenced, whereas our observers were instructed to take as much time as they wanted. An indication that theirs may have been a hybrid experiment is that the RTs in their Experiment 1 (with instructions to be fast and accurate) were only about 100 ms faster than in their Experiment 2 (with instructions to be accurate). The RTs in our RT experiments are often half as long as in the equivalent accuracy experiment.

In summary, instructions to be fast and accurate are not equivalent to instructions to be as accurate as possible. As in the Stroop task, speed pressure can cause errors of a nonperceptual nature.

Summary of Part 1

In designing these experiments we had four strict criteria. First, we monitored eye movements so that the results we obtained reflected covert attention, not eye position. Second, we designed the stimuli so that there was no location uncertainty. We checked this by asking observers to indicate the target location, and they were nearly 100% correct. Third, except for in Experiment 5, we took precautions so that the accuracy experiments were not contaminated with processes that affect RT only. Finally, in all respects, valid and invalid trials were equivalent (this point is relevant in Part 4). Our main finding was that although voluntary attention affected RT and accuracy (provided SOA was long

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[5] We ran the informative cue version with the 0-ms SOA (rather than the noninformative version) because we wanted to ensure that we obtained a robust effect on RT.

[6] This suggestion was made to us by John McDonald.

enough), involuntary attention affected only RT. The failure to obtain a benefit of the cue in accuracy with involuntary attention was not because the cue lacked salience. We know this because the cue obtained the usual effect in RT experiments. Further, we know that a cue would benefit the discrimination task, as long as the cue was informative of the target location and the SOA was sufficiently long.

Two conclusions can be drawn from these experiments. First, one should not assume that results in experiments designed around RT generalize to experiments designed around accuracy (see also Moore & Egeth, 1998; Moore et al., n.d.; Mordkoff & Egeth, 1993; Santee & Egeth, 1982). They may give the same results. However, if they do yield different results, this fact may be important in characterizing the underlying mechanisms.

Second, voluntary and involuntary attention must involve different processes, at least in the present experiments. Voluntary attention clearly affects the perceptual representation, whereas involuntary attention does not. Voluntary attention is easy to account for by channel enhancement, as the signal-to-noise ratio is better in the attended location (see Prinzmetal, in press). Involuntary attention does lead to robust benefits in RT, and these effects need to be accounted for. We hypothesize that the effect of involuntary cues involves channel selection. In the Posner cuing task, it is competition over which location to respond to. Regardless of whether our explanation is correct, it is misleading to call the effects of both voluntary and involuntary cuing simply "attention" when they clearly involve different processes.

Part 2: Cross-Modal Cuing

Experiments 6–8

Experiments 6–8 examined cross-modal cuing: auditory cues and visual targets. Our initial motivation for these cross-modal studies was a potential confound in the initial experiments, with visual cues and visual targets. With a visual cue and visual target, there is always a possibility that the cue causes some kind of masking of the target. Indeed, with involuntary attention and 0-ms SOA, observers were significantly less accurate on valid than on invalid trials. This result is not unique; Luck et al. (1996) found in four experiments with short SOAs that observers were less accurate on valid than on invalid trials. In our experiments, the disadvantage for valid trials was reliable only with 0-ms SOA. However, there may have been some visual masking by the cues at all SOAs that lowered the accuracy for valid trials. Such masking would have hidden what could otherwise have been an advantage for valid trials.

Cross-modal cuing has a unique advantage over within-modality cuing. With a visual cue, there is always the possibility of some type of visual masking of the target by the cue. An auditory cue cannot visually mask a visual target and therefore provides an opportunity to study cuing without the possibility of within-modality masking of a visual target by a visual cue.

There are dozens of studies of cross-modal cuing (see Spence, 2001, for an excellent review). Most of these studies are RT studies and so do not shed much light on the dissociation reported here. Several studies have examined effects that are possibly related to the present paradigm but differ in some way. There are two studies that follow the spatial cuing paradigm used in this report and that used accuracy as the dependent variable with noninformative auditory cues. McDonald et al. (2000) used a presence–absence detection task, and Dufour (1999) used a four-alternative forced-choice task. With SOAs in the range of the present experiments, both reported higher accuracy on valid than on invalid trials. Thus, auditory cues to a visual stimulus may be an exception to our finding of only informative cues affecting accuracy.

The goal of Experiments 6–8 was to determine whether cross-modal cuing operated differently with respect to voluntary and involuntary attention. These experiments are not an attempt at exact replication of the McDonald et al. (2000) or Dufour (1999) experiments. Rather, the question is whether cross-modal cuing will be an exception to our observation that voluntary attention affects accuracy and RT whereas involuntary attention affects only RT.

Our paradigm, illustrated in Figure 6, used an auditory cue followed by visual targets. The cues were bursts of white noise, and the targets were vertical or horizontal line segments. Experiment 6 was an RT study of involuntary attention (nonpredictive cues). Experiments 7 and 8A were designed around accuracy: Experiment 7 studied voluntary attention (informative cues) and Experiments 8A and 8B studied involuntary attention (noninformative cues), much as the original experiments did. We did not run a voluntary attention RT experiment, because the cue (a sound in the periphery) includes an involuntary component. Experiment 8B was a replication of 8A except that the speakers were hidden from view and the apparent location of the cue was the stimulus location. Experiment 8C allowed eye movements.

In the RT study, the vertical and horizontal lines were long enough so that observers could be nearly 100% correct. In the accuracy experiments, the line length was adjusted so that observers were between 75% and 80% correct.

Method

Procedure. The general procedure is shown in Figure 6. Each trial began with two fixation boxes. This was followed by a white noise tone on the left or right (described below) for 250 ms. Either 50 ms or 200 ms after the onset of the tone, the target line was presented for 100 ms, followed by the mask shown in the figure.

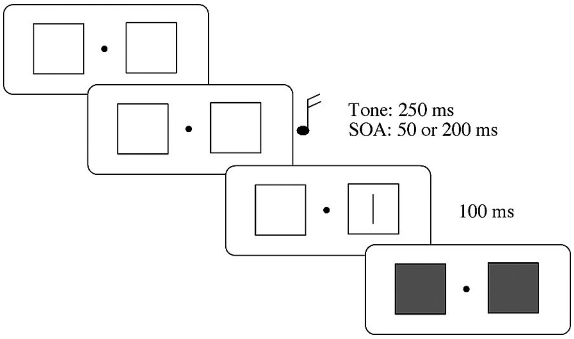

Figure 6. Basic procedure in the cross-modal experiments. The tone was white noise 250 ms in duration. The onset of the tone preceded the onset of the target by either 50 ms or 250 ms. SOA = stimulus onset asynchrony.

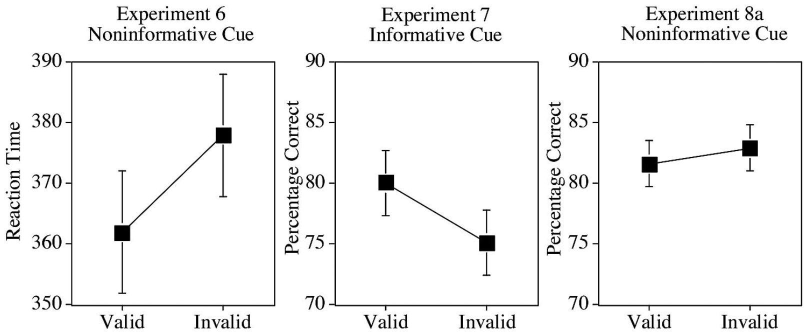

Figure 7. Results in the cross-modal experiments (Experiments 6, 7, and 8A) averaged over stimulus onset asynchrony. Error bars represent 95% confidence intervals for a within-subject design. The reaction time results are in milliseconds.

Each observer participated in two practice blocks, and then data were collected over five blocks of trials. In the accuracy experiments, the length of the line was adjusted so that observers were 75% to 80% correct. The average target line length was 3.1 pixels in Experiment 7 and 3.7 pixels in Experiment 8. In the RT experiment, the target line was always 24 pixels long. In the noninformative cue experiments (Experiments 6 and 7), half of the trials were valid and half were invalid. There were 48 trials in a block. In the informative cue experiment, we used a procedure that we felt certain would engage voluntary attention.[7] There were 56 trials in a block: 32 valid, 16 invalid, and 8 on which there was no auditory cue. In these trials, a target line was presented, but observers were to refrain from responding. Feedback was as in Experiments 1–4.

Stimuli. The boxes were aligned with the horizontal meridian. The distance from the center of each box to the center of the screen subtended a visual angle of 9.0°. The boxes subtended 3.4° on a side. The mask was created with Macintosh Toolbox Pattern 31. The viewing distance was 40 cm, as before.

The cue was a burst of white noise, 250 ms in duration and approximately 88 db. The sound was delivered via four Cambridge Soundworks speakers, two on the observers' left and two on the right. One speaker was located on each side of the monitor. The other two speakers were located on a table, about 16 cm in front of the observer and approximately 34 cm to either side of the observer's line of sight.[8] The cue was presented by either the left pair or the right pair of speakers.

In this and all subsequent experiments, we monitored eye movement using a different apparatus than in Experiments 1–5. We used a video camera, located approximately 15 cm from the observer's left eye. The image of the observer's eye was presented on a monitor and magnified about 4 times. The experimenter monitored eye movements and signaled the computer whenever an eye movement was suspected. We found that this method was more accurate than the infrared device. In particular, we could more easily discriminate blinks from saccadic eye movements. Trials with eye movements were eliminated, and the observer was signaled as in the previous experiments. There were 16 observers in each experiment.

Results

RT experiment, involuntary attention. The results from the RT experiment with involuntary attention (noninformative cues; Experiment 6) were similar to all the RT experiments reported here. As shown in Figure 7, observers were significantly faster on valid than on invalid trials (362 ms vs. 378 ms), $F(1, 15) = 24.02$, $p < .01$, $d = 0.18$ (correct trials only). There was also a significant effect of SOA. RT was slower with the short SOA than with the long SOA (401 ms vs. 340 ms), $F(1, 15) = 24.00$, $p < .01$, $d = 0.70$. However, SOA did not interact with cue type, $F(1, 15) = 0.31$, $p = .59$. The effect of SOA probably reflects a general alerting effect (i.e., Posner & Boies, 1971). Observers made few errors. The average percentages correct for valid and invalid trials were 97.1% and 96.8%, respectively, but this difference was not reliable, $t(15) = 0.58$, $p = .57$ (two-tailed), $d = 0.13$. Note that in Figure 7, SOA is not plotted, because SOA and cue type did not significantly interact in any of the experiments in Part 2.

Accuracy experiment, voluntary attention. With accuracy as the dependent variable, voluntary attention also affected performance (Experiment 7; see Figure 7). Observers were significantly more accurate on valid trials than on invalid trials (80.0% vs. 75.1% correct), $F(1, 15) = 14.88$, $p < .01$, $d = 0.71$. Neither SOA, $F(1, 15) = 0.01$, $p = .98$, $d = 0.04$, nor the SOA × Cue interaction, $F(1, 15) = 0.13$, $p = .72$, approached significance. Although we previously obtained effects of SOA within modality, we do not know what between-modality SOAs are equivalent to these within-modality SOAs. Observers were faster on valid trials than on invalid trials (785 ms vs. 874 ms), $t(15) = 2.15$, $p < .05$, $d = 0.35$.

We wanted to make sure that this positive effect of voluntary attention was not due to location uncertainty. Hence, we ran 4 additional observers in an experiment identical to Experiment 7, except that the observer's task was to indicate whether the target line was on the left or right side of the screen. The line length was 3 pixels. The 4 observers averaged 99.2% correct on the location judgments (range, 97.9% to 100% correct). Thus, our effect with informative cues cannot be explained in terms of location uncertainty.

Accuracy experiment, involuntary attention. The results from Experiment 8A (accuracy with noninformative cues) are easy to summarize. There were no significant effects of cue, SOA, or

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[7] This method of making the cue more effective was suggested to us by Lawrence Ward.

[8] In extensive pilot testing, we found that the effect of a noninformative auditory cue in an RT experiment was much larger when we used this arrangement of speakers than when we used either just two speakers or headphones.

interaction of SOA and cue. Observers were as accurate on valid as on invalid trials (81.6% vs. 81.9% correct, respectively), $F(1, 15) = 0.17$, $p = .69$, $d = 0.11$. Observers were slightly faster on valid than on invalid trials (663 ms vs. 679 ms, respectively, including correct and incorrect trials), $t(15) = 2.00$, $p < .05$, $d = 0.11$.

There is evidence that different results can be obtained if the sound appears to be coming from the same source as the visual target and the speakers are hidden (e.g., Eimer & Schröger, 1998; see Spence, 2001, for a discussion).[9] We wanted to see whether we would obtain different results if the speakers were hidden and the apparent source of the sound was at the position of the visual stimuli. In Experiment 8B, we ran 12 additional observers in an experiment identical to Experiment 8A, except the speakers were hidden and the apparent location of the sound was the two boxes on the screen. To accomplish this, we mounted two foam strips, approximately 12 × 35 × 1 cm, on the monitor.[10] One strip was mounted horizontally just above the stimulus boxes and one just below the boxes. Two 1.5-in. speakers were mounted behind the foam strips just above and below the stimulus box on the left, and two speakers were similarly mounted on the right. Thus, when the two speakers on the left emitted the cue, the apparent location of the sound was between the speakers. Furthermore, the speakers were hidden from view. The results were identical to those of Experiment 8A (involuntary attention): There was no significant difference in accuracy between valid and invalid trials (79.7% vs. 80.3%, respectively), $t(11) = 0.56$, $p = .59$ (two-tailed), $d = 0.14$. Thus, hiding our speakers did not change our results.

In everyday experience, a loud noise seems to capture our attention in that we will turn to the source of the noise. Perhaps our instructions to maintain fixation might cause the voluntary system to focus on the fixation point, overriding the involuntary system. In Experiment 8C we tested the possibility that instructions to not move one's eyes precluded an effect of a noninformative cue on accuracy. Experiment 8C was almost identical to Experiment 8A, which measured accuracy with noninformative auditory cues. The only differences were that we only used the 50-ms SOA, and observers were not instructed to maintain fixation but were free to move their eyes. In the cross-modal experiments above, 50-ms SOA had the same effect on performance as 200 ms. We felt that with a 50-ms SOA, 100-ms stimulus exposure, and masked targets, there would not be sufficient time to complete a saccade to the target location. There were 16 observers. Even though we allowed eye movements, the results were unchanged. The percentages correct for valid and invalid trials were 79.2% and 79.4%, respectively, $t(15) = 0.17$, $p = .87$, $d = 0.05$.

Discussion

In summary, cross-modal cuing does not appear to be different from within-modality cuing in that voluntary attention affects both RT and accuracy, but involuntary attention affects only RT. We obtained a robust effect of a noninformative cue in an experiment designed around RT but no effect with an experiment designed around accuracy. Hiding the speakers did not change the effect (Experiment 8B), nor did allowing eye movements (Experiment 8C). Overall, the results of the accuracy experiments were remarkably consistent (see Table 1). Across the three experiments with accuracy and involuntary attention, only 16 of 44 observers were more accurate on valid than on invalid trials.

In contrast, with voluntary attention (informative cues), observers were significantly more accurate on valid trials than on invalid trials (Experiment 7). These results appear to be a genuine effect on the perceptual representation and not a result of location uncertainty, contamination by speeded responses, or eye movements.

Part 3: Face Discrimination

The purpose of the experiments in Part 3 was to examine a potential confound between the RT experiments and the accuracy experiments in Parts 1 and 2 (as well as Part 4). Furthermore, we wanted to extend the findings to a more naturalistic stimulus set.

The potential confound between the RT experiments and the accuracy experiments is that in the RT experiments, we always used relatively large objects (i.e., large letters and long lines) so that observers could be nearly 100% correct in identifying them. However, in the accuracy experiments, we used very small objects (i.e., tiny letters and short lines) so that no matter how long observers took to respond, they would never be 100% correct.[11] There is some evidence that attention has different effects depending on the spatial frequency content of the stimuli (Yeshurun & Carrasco, 1998; but see Prinzmetal et al., 1998). We needed a method of making discrimination difficult without changing the size or spatial frequency content of the stimuli. We also did not want to change the temporal properties of the stimuli or the masks.

To accomplish these goals, we used the face discrimination task illustrated in Figure 8. Observers had to decide on each trial which of two faces was present. The stimuli were created using digital photographs of two people who were similar in appearance. We made a series of 10 morphs of these photographs, so that we could select a pair of photographs that were arbitrarily similar. For the RT experiment (Experiment 9), we used morphed pairs of photographs that were easy to discriminate. For the accuracy experiments (Experiments 10 and 11), we selected a pair of photographs, separately for each observer, so that observers were between 75% and 80% correct (see Figure 9). In the RT experiment and one of

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[9] Eimer and Schröger's (1998) study is widely cited as demonstrating that it is critically important whether the auditory cue and visual stimulus come from different locations or from the same apparent location. In their Experiment 1, the effect of the cue on event-related potential visual cues did not affect auditory processing, but they did affect auditory processing in Experiment 2. One hypothesis the authors proposed to account for this difference was that the auditory and visual stimuli were in different locations in Experiment 1 but in the same location in Experiment 2. However, they point out that there were many other differences between the two experiments that could have accounted for the results. For example, the experiments differed in tasks, stimulus discriminations, and eccentricity. Thus, it is still an open question as to whether it makes a difference if the stimulus and cue come from the same or from different locations. In a behavior experiment, we found no difference, as indicated below.

[10] We thank Charles Spence for suggesting this simple method of hiding the speakers and having the apparent location of the sound near the visual stimulus location.

[11] We thank Roger Remington for pointing out this confound to us. The studies reported here were initially done as preliminary studies for an experiment using functional magnetic resonance imaging.

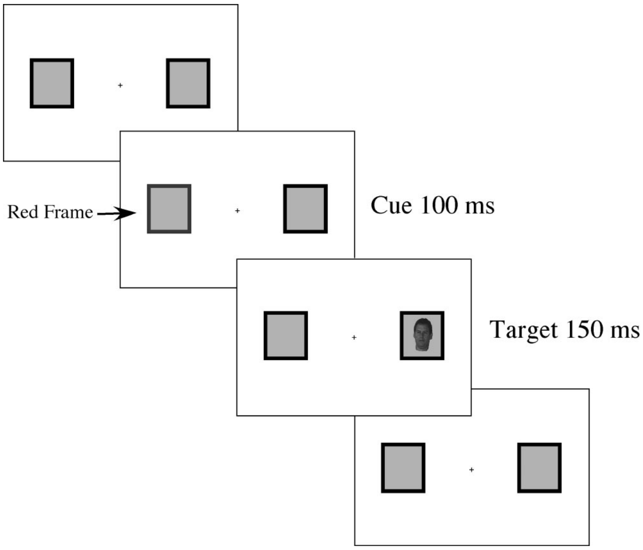

Figure 8. Procedure used in the face identification experiments (Part 3).

the accuracy experiments, we studied involuntary attention (the cues were noninformative). In the other accuracy experiment, we studied voluntary attention (informative cues).

Method

Procedure

The general procedure is shown in Figure 8. Each trial began with a fixation cross and two boxes, as shown. The frame of one of the boxes turned red and remained red for 100 ms (the cue). The cue was followed by one of the faces, which remained in view for 150 ms, followed by the two boxes and the fixation point. Observers responded by pressing one of two keys on the computer keyboard. The same auditory feedback was used as in the previous experiments.

Each observer participated in two practice blocks of 16 trials, and then data were collected over four blocks of trials of 64 trials. For voluntary attention, 75% of the trials were valid; for involuntary attention, 50% were valid. For the RT experiment, the discrimination difficulty level (described below) was set at 3 for the entire experiment. For the accuracy experiments, the level was set at 3 for the first practice block but then adjusted between blocks so that observers averaged 75% to 80% correct. The averaged morphed difficulty levels for voluntary and involuntary accuracy experiments were 5.4 and 6.0, respectively (see Figure 9). For the accuracy experiment, observers were told repeatedly to take their time and try to be as accurate as possible. Twelve observers participated in the RT experiment, and 18 observers participated in each of the accuracy experiments. The observers were selected from the same subject pool as in the previous experiments, and eye movements were monitored as in Part 2.

Stimuli

The faces were created with two digital color photographs. The background was made a homogeneous magenta (red = 200, green = 168, and blue = 216 out of 255). The photographs were morphed with the program Morph 2.5 (Gryphon software) to create a series of 10 pairs of images on a linear morph trajectory. The first pair was barely morphed (Level 1) and resembled the original pictures. The last pair was morphed to be nearly identical (Level 10).

The stimuli were presented on a Gateway 15-in. monitor controlled by a Windows 95 computer running E Prime. The viewing distance was 50 cm, and a chin rest was used, as before. The distance from the fixation cross to the edges of the boxes subtended 6.00° of visual angle. The boxes subtended 5.14° by 4.57° of visual angle. The thickness of the edge of the box subtended approximately 0.23° of visual angle (0.2 cm). In the fixation field, the frame of the boxes was black, and they were filled with magenta. For the cue, the frame of one of the boxes turned red (red = 255, green = 0, and blue = 0 out of 255).

Results

RT Experiment, Involuntary Attention (Experiment 9)

The results were similar to all previous RT experiments with noninformative cues. Observers were significantly faster on valid trials than on invalid trials (549 ms vs. 574 ms), $F(1, 11) = 5.28$, $p < .05$, $d = 0.19$ (correct trials without eye movements). Ob-

Figure 9. Sample of the morphed faces used in Part 3.

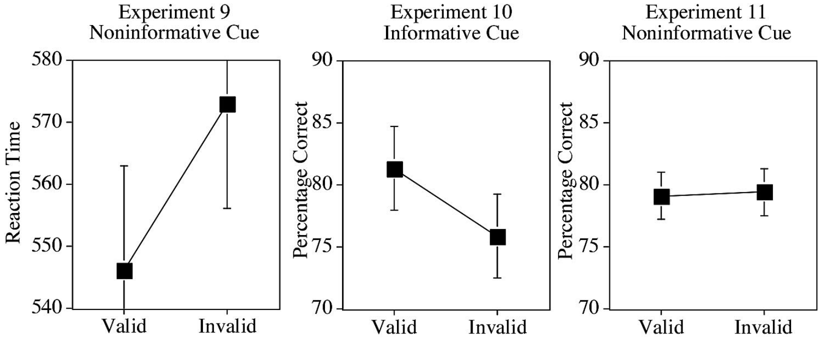

Figure 10. Results in the face discrimination experiments (Experiments 9, 10, and 11). Error bars represent 95% confidence intervals for a within-subject design. The reaction time results are in milliseconds.

servers averaged 95.6% correct for both valid and invalid trials. The results for all three experiments are shown in Figure 10.

Accuracy Experiment, Voluntary Attention (Experiment 10)

As in the previous experiments, observers were significantly more accurate on valid trials than on invalid trials (81.3% vs. 75.7%), $F(1, 17) = 5.95$, $p < .05$, $d = 0.66$. Observers were faster on valid trials than on invalid trials (1,005 ms vs. 1,112 ms), but this difference was not significant, $t(17) = 1.72$, $p = .10$, $d = 0.15$. Because the target stimuli were so conspicuous and there was no mask, it seems unlikely that these results were due to location uncertainty.

Accuracy Experiment, Involuntary Attention (Experiment 11)

With noninformative cues, there was no difference between valid and invalid trials. The percentages correct for valid and invalid trials were 79.1% and 79.4%, respectively, $F(1, 17) = 0.03$, $p = .87$, $d = 0.04$. Observers were slightly faster on valid trials than on invalid trials (929 ms vs. 933 ms), but this difference was not significant, $t(17) = 0.10$, $p = .91$, $d = 0.009$.

Discussion

The results were nearly identical to those in Parts 1 and 2: Involuntary attention affected performance only in the experiment designed around RT, whereas voluntary attention affected both the experiment designed around RT and those designed around accuracy. Because only voluntary attention affects accuracy, we conclude that voluntary attention enhances the perceptual representation but involuntary attention does not.

One interesting aspect of these results is that we were able to obtain an effect of voluntary attention in an accuracy experiment without using a poststimulus mask. Recently, Smith (2000) argued that one will not obtain an effect of voluntary attention in an accuracy experiment with spatial cues unless there is a poststimulus mask. His evidence, and his review of past research, is convincing. However, it may be that face discrimination involves a different kind of processing than used in most other experiments that used letters, Gabor patches, or lines. Arguably, face discrimination may involve holistic processing. We found that we could discriminate very similar morphed faces, with 150-ms exposure duration, but it seemed difficult to describe a single feature that reliably discriminated the faces. Under the conditions investigated here, voluntary attention enhanced performance, even without a poststimulus mask. However, as in the previous experiments, involuntary attention had no effect on an experiment designed around accuracy.

We found the same pattern of results with letters, lines, and faces as targets and with visual and auditory cues. Our failure to find an effect with involuntary attention in accuracy experiments is not because the cues lacked salience: Identical cues affect performance in RT experiments. Furthermore, it is not because our tasks were not attention demanding, because voluntary attention consistently affected performance in accuracy experiments. Our tentative conclusion is that voluntary attention affects the perceptual representation, whereas involuntary attention does not. However, there are a few reports in the literature on noninformative cues affecting accuracy. We account for these effects in Part 4.

Part 4: Other Reports (Handy et al., 1999)

We designed our experiments to meet the following four methodological criteria: (a) Experiments must clearly be accuracy experiments, not hybrid experiments; there must be no speed pressure whatsoever. (b) Eye movements should be monitored to ensure that one is measuring changes in covert attention. (c) There must be no location uncertainty; observers should be able to accurately identify the target location, if not the target identity. (d) There cannot be confounds, so that valid and invalid trials must be as identical as possible. Under these conditions, involuntary attention (noninformative cues) did not affect performance. There have been a handful of studies that found higher accuracy on valid than on invalid trials with noninformative cues. However, in almost every case, these studies violated one of the methodological considerations. We discuss each of these studies later.

However, there are two studies that did not seem to violate any of these considerations but found an effect in an accuracy experiment of involuntary attention: Handy et al. (1999) and Luck and Thomas (1999). These were similar to each other in that they used bright stimuli on a darker background. To address the problem of location uncertainty, they masked only the target position. Handy et al. was a two-alternative forced-choice task, and Luck and Thomas used a four-alternative forced-choice task. Because the former is more amenable to running as an RT experiment as well as an accuracy experiment, we decided to study it. However, as will become apparent latter, we can account for what had been puzzling results of Luck and Thomas.

We began by replicating Handy et al. (1999) in our Experiment 12. We then proposed an alternative nonattention explanation of their results that is a consequence of the fact that they masked only the target location, causing valid and invalid trials to be different. We tested this explanation in two accuracy experiments. First, we reversed the order of events so that the cue followed the target and mask. Under these circumstances, it is impossible to argue that the cue is drawing attention to the target location before the mask (or target) appears (Experiment 13). We still obtained a difference between valid and invalid trials, suggesting that the results obtained by Handy et al. were not due to attention (see, e.g., Schmidt, Vogel, Woodman, & Luck, 2002; Woodman, Vecera, & Luck, 2003, for a similar argument). Second, we found that when we masked both locations, the effect of automatic attention on accuracy vanished (Experiment 14). Finally, we ran an RT version and found the usual effect of automatic attention on RT (Experiment 15).

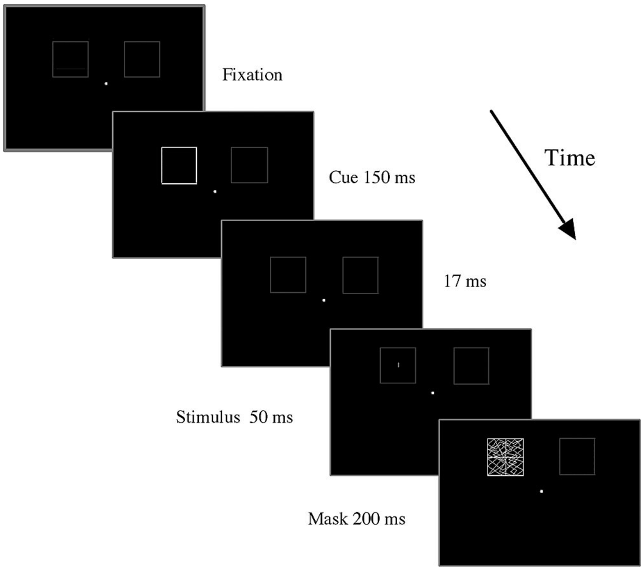

Figure 11. Sequence of events in Experiments 12, 13, and 14. The cue was the brightening of a gray box. The target line and mask were yellow, as in Handy et al. (1999).

Experiment 12: Handy et al. (1999) Replication

We began with a replication of the experiment by Handy et al. (1999; Experiment 2), because that experiment was similar to our experiments in that the cue was the brightening of a box, and it was a two-alternative forced-choice task that was identical to the experiments in Part 2. There were two possible stimulus locations, and half the trials were valid so that the cue provided no information as to the target's location. Our replication was as similar to their experiment as we could make it, except where noted below. We first describe our version and then note the differences between their experiment and ours. The differences were minor, and as it turns out, we successfully replicated their results.

Method

Procedure. The procedure is shown in Figure 11. Each trial began with a fixation point and two faint gray boxes. One of the boxes brightened for 150 ms. This was followed by the reappearance of the gray boxes for 16.67 ms. The target, a short yellow vertical or horizontal line segment, was presented for 50 ms, followed by a mask consisting of yellow squiggles and two lines. After 200 ms, the mask was replaced by the gray boxes. Note that the mask was only in the location of the target line. The observer's task was to indicate whether the line segment was vertical or horizontal. The experiment was run as a pure accuracy experiment, so observers were instructed to be as accurate as possible and to take as much time as they needed. To make the task difficult, the length of the line segments was shortened between blocks so that the observers were between 75% and 80% correct. Feedback was provided as in the previous experiments.

Each observer participated in two blocks of practice trials and five blocks of trials on which data were collected. There were 48 trials in a block. Half of the trials were valid and half were invalid. Each target (vertical and horizontal) and each position (left and right) was used equally often. The order of trials within each block was random. On the first block of practice trials, the line segment length was 50 pixels, but the length was reduced as described above.

Stimuli. The stimuli were designed to be as similar to those used by Handy et al. (1999) as we could make them, except as described below. Observers viewed the stimuli from a distance of 40 cm. The square boxes subtended 1.5° of visual angle. The centers of the boxes were located 2.5° above the horizontal meridian and 3.7° from the fixation point (the same dimensions used by Handy et al.). The average length of the target line was 6.5 pixels (subtending approximately 0.35° of visual angle). The experiment was run on an Apple 17-in. monitor, as in Parts 1 and 2.

Sixteen observers, selected as before, participated in the experiment.

Differences from Handy et al. (1999). There were some differences between our procedure and Handy et al.'s. In theirs, half of the trials were long SOA trials, with a total of 960 ms between the onset of the cue and the onset of the target. We did not run these long SOA trials. On 20% of their trials, there was no vertical or horizontal target line, whereas we had a target line on each trial. Half of their observers were instructed to respond *target present* to a vertical bar and *target absent* to a horizontal line or no line. Our observers responded that the target was vertical or horizontal in each trial. Their observers did not receive trial-by-trial feedback on their performance, and they had to respond within 2.5 s. The mask that they used did not include the large cross. The cross was included in our experiment to ensure that the long-persistence phosphoresce trace of the target line was masked.

Results

Despite these differences in procedure, we replicated their results. Observers were significantly more accurate on valid trials than on invalid trials (79.8% vs. 76.3%), $t(15) = 1.85$, $p < .05$ (one-tailed), $d = 0.33$.

Handy et al. (1999) analyzed their data using the signal detection theory measure, d'. It is not clear that d' is an appropriate measure in Handy et al.'s experiment because there were two noise distributions (opposite target and no line). Nevertheless, we analyzed our data using signal detection theory. We arbitrarily designated vertical line trials target present trials and horizontal line trials target absent trials. Analyzed by this dependent variable, performance was significantly greater on valid trials than on invalid trials ($d' = 1.87$ and 1.57, respectively), $t(15) = 1.75$, $p < .05$ (one-tailed), $d = 0.40$. The magnitude of the effect was approximately that observed by Handy et al., where performance for valid and invalid trials was $d' = 1.52$ and 1.20, respectively. Our results, like Handy et al.'s, were not due to a speed–accuracy trade-off. Observers were faster on valid trials than on invalid trials (673 ms vs. 738 ms), $t(15) = 1.92$, $p < .05$ (one-tailed), $d = 0.20$.

Discussion

We successfully replicated the results of Handy et al. (1999). In fact, we ran a second replication, with 15 observers. This additional experiment differed from the one reported here in that the boxes were horizontally aligned with the fixation point (instead of above it) and the mask did not contain a cross. We obtained the same results.

Our replications rule out several alternative explanations of their results. First, our replication was clearly an accuracy experiment, not a hybrid experiment. There was no pressure to respond quickly. Second, because only the target location was masked, there was no location uncertainty (Luck et al., 1996; Shiu & Pashler, 1994). However, before accepting that a noninformative cue can affect accuracy, it would be good to know precisely what procedural detail accounts for the difference between our previous experiments and Handy et al. (1999) and Luck and Thomas (1999).

Experiment 13: Handy et al. (1999) in Reverse Order

There are, of course, a number of differences between Handy et al.'s (1999) experiment and our previous experiments (Parts 1–3). In a series of pilot experiments, we eliminated most factors except those that involved the mask. There were two salient differences between their mask and the ones we used. First, their mask consisted of yellow "spaghetti." In our previous experiments, we used the letter *O* (Part 1 experiments), a dense cross-hatched pattern (Part 2; see Figure 6), and no mask (Part 3 experiments). Perhaps their mask was simply more effective and one needs a very effective mask to obtain accuracy effects. Recently, Smith (2000), using a slightly different paradigm (100% valid cues vs. no cue), found an effect on accuracy only with a mask, commenting, "The results show that the cueing effect in simple detection depends on the use of backward masks" (p. 1401). The idea is that the cue summons attention, which quickly processes information before a mask can have an effect. We know that attention can affect masking (e.g., Di Lollo, Enns, & Rensink, 2000). However, it would be surprising to us that a mask would be necessary to demonstrate the effects of channel enhancement, because a mask was not necessary in our experiments in Part 3 and in other attention paradigms (e.g., Prinzmetal et al., 1997, 1998; Prinzmetal & Wilson, 1997). The second difference, and the one that proved critical, was that we masked all of the stimulus locations, whereas the Handy et al. experiment masked only the location with the target.

Because a mask can have many different effects, Charles Eriksen (1980) warned, "The use of a visual mask may seriously confound your experiment" (p. 89). Eriksen argued that one effect of a mask is to temporally integrate with the target (e.g., C. W. Eriksen & Collins, 1967). We thought it would be useful to consider what the temporal integration of the cue, stimulus, and mask would look like. An approximation of this temporal integration is shown in Figure 12 (which is drawn to scale). The figure on the left represents the integration of cue, target, and mask in a valid trial, and the right panel represents the stimulus elements in an invalid trial.

These panels are not completely equivalent: In valid trials, it is as if there is one object on the screen, and in invalid trials it is as if there are two objects on the screen. A difference in performance might be related more to this "one thing" versus "two things" factor than to attention shifting to the cued location. The difference between a stimulus with one thing and two things is akin to the display-size effect and therefore could be considered an attention effect. However, if the results are due to this factor, it is certainly different from enhancing the perceptual representation in a particular spatial location.

If the results of Handy et al. (1999) were due to the cue drawing attention to the target location and enhancing the perceptual representation, then the cue must precede the target to affect performance. If the results were due to one thing (on valid trials) and two things (on invalid trials), then the timing of the sequence should not matter as long as the cue, target, and mask are sufficiently close together in time. In Experiment 13 we tested these two explanations by having the target precede the cue and the mask. The logic of reversing the cue and target in time to separate perceptual and postperceptual processes has been used by other investigators (e.g., Schmidt et al., 2002; Woodman et al., 2003).

Method

Each trial began with the presentation of the target line. After 50 ms, the target line was immediately replaced by the mask and cue. The mask and cue were presented simultaneously and remained in view for 200 ms. If the cue in the Handy et al. (1999) experiment affected performance by enhancing the speed of processing of the stimulus, so that processing of the stimulus was complete before the appearance of the mask, then with this order of events, cue validity should have had no influence on performance. If, however, the cause was differences in the integrated representation of the cue and mask in valid and invalid trials, then accuracy with valid trials should still have been higher than with invalid trials. Other than the order

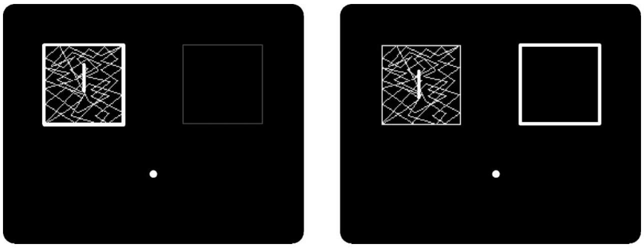

Figure 12. Simulation of the temporal integration in the display elements in Handy et al. (1999). Left and right panels are valid and invalid trials, respectively.

of events in a trial, this experiment was identical to Experiment 12. The average length of the target line was 6.5 pixels (approximately 0.29° of visual angle).

Results and Discussion

Observers were significantly more accurate on valid trials than on invalid trials (78.4% vs. 75.6%), $t(15) = 2.88$, $p < .01$ (one-tailed), $d = 0.41$. The d' scores, calculated as before, were 1.62 and 1.50 for valid and invalid trials, respectively, $t(15) = 1.85$, $p < .05$, $d = 0.26$. The results were probably not due to a speed–accuracy trade-off, as the average RT for valid and invalid trials was identical (692 ms).

There is little doubt that attention affects masking such that when observers are attending to the stimulus there is less backward masking than when observers are not attending to the stimulus. However, it is difficult to argue that the difference between valid and invalid trials in this experiment was caused by the cue summoning attention, so that on valid trials, stimulus processing was completed before the appearance of the mask. The cue appeared at the same time as the mask, after the stimulus. We suggest that the effect was due to the asymmetrical nature of the mask, which appeared only on the side of the target. Valid and invalid trials were not equivalent.

Experiment 14: Accuracy With Two Masks

Method

If our speculation is correct, presenting the mask on both sides of the display should abolish the difference between valid and invalid trials. Experiment 14 was identical to Experiment 12, except that identical masks were simultaneously presented on both sides. The average length of the target line was 4.7 pixels (approximately 0.21° of visual angle).

Results and Discussion

The effect of the cue was eliminated. The average percentages correct were 80.9% and 80.3% for valid and invalid trials, respectively, $t(15) = 0.52$, $p = .16$ (one-tailed), $d = 0.10$. Only 7 of the 16 observers were more accurate on valid than on invalid trials. The d' scores, calculated as before, were 1.89 and 1.81 for valid and invalid trials, respectively, $t(15) = 1.04$, $p = .31$, $d = 0.19$.

The results were not due to a speed–accuracy trade-off. Observers were faster on valid trials than on invalid trials (563 ms vs. 616 ms), $t(15) = 1.73$, $p < .05$ (one-tailed), $d = 0.29$. We also wanted to be sure that the results were not due to location uncertainty. Location uncertainty is usually evoked to explain the finding that observers are more accurate at valid trials than invalid trials, but we did not obtain that result (Luck et al., 1996; Prinzmetal et al., 1997; Shiu & Pashler, 1994). However, if most of the errors arose because observers did not know where the stimulus was, this fact might obscure an effect of the cue. To determine whether observers knew the target location, we ran an additional experiment that was the same as Experiment 14, except that rather than responding whether the line was horizontal or vertical, they indicated the line location (left or right box). First we ran 2 observers with the target line 4 pixels long, and both observers located the target with 100% accuracy. We ran 3 more observers with the target line 3 pixels long, and they were also 100% accurate. (In the main experiment, the average line length was 4.7 pixels.) Apparently the limit on performance was determining whether the line was vertical or horizontal, not where the line was located.

Experiment 15: RT With Two Masks

Method

Masking both sides obliterated the effect on accuracy, but it is possible that masking both sides simply reduces the salience of the cue so that it cannot have any effect, even an involuntary effect as measured in an RT study. To examine this possibility, we repeated Experiment 14 (masking both locations) but as an RT experiment. The target lines were left long (50 pixels in length) so that observers could be nearly 100% correct. We told them that we were primarily interested in the speed of their responding but that they should try to be nearly 100% accurate. There were 8 observers.

Results

Each observer was faster on valid trials than on invalid trials (297 ms vs. 336 ms for correct responses), $t(7) = 4.45$, $p < .01$, $d = 0.33$. The results were not due to a speed–accuracy trade-off. Although the difference in accuracy was small, observers were significantly more accurate on valid than on invalid trials (97.5% vs. 96%), $t(7) = 2.81$, $p < .05$, $d = 0.75$.

In summary, we were able to faithfully replicate Handy et al.'s (1999) results of an accuracy effect with noninformative cues at short SOAs. However, we demonstrated that this effect was probably due to the fact that they masked only the target location, creating displays that were not equivalent for valid and invalid trials. When we masked both locations, the effect vanished. This result is entirely consistent with Luck and Thomas (1999). Note that even though we did not obtain a true effect of involuntary attention in an accuracy experiment, we did obtain an effect in an RT experiment. Hence, rather than being an exception to our original findings, Experiments 12–15 extend the generality of our finding to the paradigm used by Handy et al.

Discussion

It seemed to us that the experiments by Handy et al. (1999) and by Luck and Thomas (1999) were the best candidates to obtain an effect on involuntary attention in an accuracy experiment that could not be explained by other causes. In both of these experiments, eye movements were monitored, the authors addressed the problem of location uncertainty, and the results did not appear to be contaminated by RT. However, the results appear to be a consequence of masking only the target location, rendering valid and invalid trials not equivalent.

Other claims of an effect of involuntary attention on accuracy in this paradigm do not meet our methodological criteria. For example, Henderson and Macquistan (1993; see also Horstmann, 2002) were the first to report that a noninformative cue affected accuracy. In the accuracy experiments, either four or eight display locations were heavily masked. There is no indication that observers could accurately determine the location of the target; thus, the results could have been a consequence of location uncertainty (see also

Luck & Thomas, 1999; Shiu & Pashler, 1994, for the same account).

We have examined other studies that reported that nonpredictive cues affect response accuracy. For example, Dufour (1999) found that noninformative auditory cues affected accuracy. Recently, we demonstrated that these results were due to eye movements (Prinzmetal, Park, & Garette, 2004). A study by Klein and Dick (2002) also reported that a noninformative visual cue affected accuracy in a rapid visual serial presentation task. We found that the results were probably due to a confounding of the condition and order of stimulus presentation, with possibly a contribution of location (and temporal) uncertainty (Prinzmetal, Park, & Garette, 2004). Thus, in the spatial cuing studies that we are aware of, there is no conclusive evidence that involuntary attention affects performance in experiments designed around accuracy. Involuntary attention does affect RT, and voluntary attention affects both RT and accuracy.

General Discussion

The empirical results of our experiments are very clear. In four different versions of the spatial cuing paradigm, we obtained identical results. In the experiments designed around RT, observers were faster on valid trials than on invalid trials regardless of whether we were studying voluntary attention (informative cues) or involuntary attention (noninformative cues). The results were quite different for the accuracy experiments. Table 1 summarizes all of the accuracy experiments (except those with only the target position masked). In every experiment with voluntary attention, observers were more accurate on valid trials than on invalid trials. However, in seven experiments with involuntary attention, using over 102 observers, we found no advantage for valid trials. Although there may be conditions under which involuntary attention affects accuracy, we have demonstrated a wide range of experiments with involuntary attention in which RT and accuracy experiments do not yield the same results.

To account for these effects, we hypothesized two different mechanisms, channel enhancement and channel selection. Channel enhancement changes the perceptual representation so that there is more information in the attended location, and it corresponds to what most researchers mean by the spotlight of attention. Channel selection, on the other hand, does not affect the perceptual representation but involves a decision as to which location should be responded to. If there is a conflict as to which location should be responded to, responses are delayed.

In this discussion, we highlight three issues. First, we need to explain the results at hand. Our dichotomy between channel enhancement and channel selection turns out to have explanatory power beyond the current experiments. Second, we address the issue of why a noninformative cue did not affect accuracy. Will a noninformative cue never affect accuracy, or are there circumstances in which it will? In the final section, we speculate on possible neural mechanisms that are responsible for voluntary and involuntary attention.

Even if conditions exist in which a noninformative cue affects performance in an accuracy experiment, we need an explanation of the current dissociation. Other researchers have noted that voluntary and involuntary attention can have different effects. For example, Warner et al. (1990; see also Posner et al., 1982) conducted an experiment in which there was a high probability that the target was on the opposite side of the display as a peripherally presented cue. At short SOAs, RT was faster at the cued (low-probability) location than at the uncued (high-probability) location. However, at longer SOAs, these results reversed. In a similar manner, Posner and Cohen (1984) demonstrated that with noninformative cues and very long SOAs (over about 1 s), the facilitation in RT at the cued location turned into inhibition. They termed this reversal *inhibition of return* (IOR), which we discuss later. Informative cues did not lead to IOR. Both of these findings are eloquent double dissociations between voluntary and involuntary attention in RT experiments. However, they do not shed much light on the nature of the mechanisms that underlie these effects.

With voluntary attention, there is no question that attention can enhance the perceptual representation, in that observers will be more accurate when the stimulus appears in the expected location (e.g., Luck et al., 1996; Smith, 2000). We replicated this result in our experiments and were able to rule out location uncertainty as a cause of the effect. Attention in this manner can be thought of as a zoom lens, or spotlight, so that there is more information in the attended location than in the unattended location (e.g., Eriksen & St. James, 1986). There are several models that can account for this effect (see Prinzmetal, in press, for a discussion). For example, attention may be thought of as taking more samples in the attended location than in the unattended locations (e.g., Bonnel & Miller, 1994; Luce, 1977).

Involuntary attention is a greater puzzle. What kind of mechanisms could affect RT without affecting the perceptual representation? There are at least three candidate processes. The first is location uncertainty. Note that in our accuracy experiments, observers could indicate the target location with near 100% accuracy. However, it may take longer to correctly reach this decision in an invalid trial than in a valid trial. This mechanism can be postperceptual: The observer first forms a perceptual representation of the cued and uncued locations but subsequently decides which location contains the target. By this explanation, the cue need not precede the target but must only be sufficiently close to it in time (e.g., Schmidt et al., 2002; Woodman et al., 2003).

The second possibility is that channel selection is selection for action (Allport, 1989) and has little to do with perceptual processes. There are limits in action: The same hand cannot reach for two different objects at the same time. Peripheral cues may affect the efficiency of selection for action. For example, in reaching movements, a peripheral cue can eliminate bimanual cross-talk in reaching, but a symbolic cue does not (Diedrichsen, Hazeltine, Kennerley, & Ivry, 2001). In tasks where participants make eye movements to targets, 100% noninformative cues slow saccade latency relative to control conditions (e.g., Fischer & Weber, 1998). That is, a cue in one visual field slows saccadic latency for a target in the other visual field. Thus, peripheral cues automatically affect various motor movements. The spatial cuing paradigm is not a reaching or saccadic task, yet the mechanisms of selection for action (i.e., Which object should I respond to?) could influence RTs, particularly in a speeded task. Thus, the "selection" might be selection for action, without affecting the perceptual representation.

A third possibility is that channel selection is selection for visual working memory (e.g., Schmidt et al., 2002; Sperling, 1960). The idea is that very little information from our complex visual world enters visual working memory. A dramatic example of this is change blindness with natural scenes (e.g., Rensink, O'Regan, & Clark, 1997). Only a small amount of visual information is stored in a durable form. Information that is not attended (i.e., selected) is lost. Even though the stimuli and task that we used do not exceed visual working memory, the selection mechanisms that have evolved are not disabled just because the task and stimuli are simple (Luck & Thomas, 1999). Visual working memory might be akin to a computer's cache memory in that it has low capacity but a fast access time. Information selected for the cache memory might not be more veridical than other information, but it is accessed (responded to) faster than other information. Selection for action and selection for visual working memory are two examples where location selection might affect performance in an RT experiment. These processes do not affect the perceptual representation and hence do not affect accuracy.

These explanations are admittedly tentative, but the general perspective may illuminate some findings in the field. It is clear from the IOR studies that informative (predictive) and noninformative (nonpredictive) cues lead to different results. An explanation of IOR proposed by Klein and Taylor (1994) is that it is reluctance to respond to a previously inhibited location. This theory is consistent with our account of the facilitation with a noninformative cue. We propose that observers must decide which location contains the target. If it is a noninformative cue experiment, observers need to inhibit responding to the information in the cued location. This inhibition process takes time, but having inhibited responding to the cued location, if the stimulus subsequently appears in that location, responses will be slowed. Although this explanation is speculative, it accounts for many of the findings in the IOR literature (see Klein & Taylor, 1994). Furthermore, the flanker effect also shows "inhibition of return" like behavior: If the flankers appear about 600 ms before the target, the flanker effect is reversed (Flowers & Wilcox, 1982).

The problem with this account of IOR is that it describes the account in terms of channel selection, and one would not expect an IOR effect in a pure accuracy experiment. Yet there are at least four reports of an IOR effect in accuracy (Cheal & Chastain, 1999; Handy et al., 1999; Klein & Dick, 2002; McDonald et al., 2000). Three of these we have already discussed in detail (i.e., Handy et al., 1999; Klein & Dick, 2002; McDonald et al., 2000). We do not know what would happen in the Handy et al. experiment at long SOAs if both target locations were masked. Two of the studies might be what we called hybrid studies (Cheal & Chastain, 1999; McDonald et al., 2000). Thus, it is still an open question as to whether IOR can be explained by some sort of channel-selection-type mechanism. Furthermore, it may be that the mechanism responsible for IOR is different from the mechanism responsible for effects with shorter SOAs. Sapir, Soroker, Berger, and Henik (1999) found that a patient with a focal lesion in the superior colliculus showed no IOR in the eye related to the damage, but the early effects of the cue seemed to be unaffected.

We realize that we are questioning some basic assumptions and findings. The assumption that we are questioning is that pure accuracy experiments will always lead to the same results as RT experiments and that hybrid experiments will be equivalent to accuracy experiments. Previous empirical results may reflect a confirmation bias. The finding of higher accuracy in valid than in invalid trials seems to make sense, and therefore one might not have reason to question whether the experiment was a hybrid experiment or whether there was some other confound. Heretofore, there was simply no alternative theoretical account that would make one scrutinize these findings.

We cannot claim that a noninformative cue could *never* affect performance in a pure accuracy experiment. We have all experienced a loud noise, or bright flash of light, attracting our "attention." On the one hand, it might be that a noninformative cue never captures our attention in terms of channel enhancement. After all, the sound of gunfire in a quiet neighborhood on a Sunday morning is informative of an important change in the environment, but the same sound in a rifle range is not informative. Alternatively, there might be attributes of our experiments that mitigate such effects.

For example, with a noninformative cue, perhaps observers learn to not enhance the representation at the cued location at the expense of other locations. After all, observers are usually tested on hundreds of trials. This factor makes the typical attention experiment unlike the experience of a sudden unique noise capturing attention. Gibson and Jiang (1998) investigated the failure of a uniquely colored item to capture attention (see Rauschenberger, 2003, for a review). They speculated that the failure of a uniquely colored item to capture attention might be due to the fact that observers learn that the unique color is uninformative. They tested this by introducing a uniquely colored target on Trial 192 in an accuracy experiment. They found no evidence for color singleton "popout" on this trial. More recently, Horstmann (2002) found that an unexpected singleton could lead to popout if it preceded the stimulus. However, Horstmann's results can be accounted for by location uncertainty, so the issue is still unresolved. Still, this possibility needs to be investigated in our paradigm. It would be interesting to track performance changes for an informative or a noninformative cue over time.

Rather than enhancing the signal, involuntary attention may play a different role in perception. We suggest that the involuntary allocation of attention may select an object for response (i.e., action) or memory. Voluntary attention, on the other hand, allocates resources for identification. Our distinction between these two types of attention may be related to a distinction made by Posner et al. (1980). They distinguished *orienting* from *detecting.* Orienting involves "aligning the sensory (i.e., eyes) or central systems with the input channel over which the signal is to occur" (p. 162). This idea is related to channel selection: One must select a channel in order to align input. On the other hand, detecting, for Posner et al., is related to the central processing necessary to make an arbitrary response.

There is now ample evidence that voluntary and involuntary attention have different effects. It seems likely that different physiological mechanisms might underlie these effects, at least in part. For example, Rafal et al. (1988) proposed that midbrain areas, especially the superior colliculus, are responsible for the effects of automatic "exogenous" cues, whereas parts of the cortex may be responsible for voluntary "endogenous" cuing (see also Rafal, Henik, & Smith, 1991). However, it is probably not that simple. For example, Dorris, Klein, Everling, and Munoz (2002) recently demonstrated that although the superior colliculus is important for IOR, cortical areas probably also contribute to that effect.

The present methodology might contribute to isolating the underlying neural mechanisms of voluntary and involuntary attention. In our experiments, exactly the same physical stimulus is used for voluntary attention (informative cues) and involuntary attention (noninformative cues). Noninformative cues invoke only involuntary attention, whereas informative cues contain both components. Subtractive logic could be used with, for example, functional magnetic resonance imaging or event-related potentials to isolate the voluntary component. Using this strategy, Friedrich, Egly, Rafal, and Beck (1998) compared informative and noninformative visual cues in patients with temporal–parietal junction lesions and in patients with more superior parietal lesions. Only the patients with temporal junction lesions showed very slow RTs for invalid trials in the contralateral visual field ("extinction-like RT"; see Posner, Walker, Friedrich, & Rafal, 1984). This result contrasted with early classic positron emission tomography studies that demonstrate that voluntary cuing activated the superior temporal lobe (i.e., Corbetta, Miezin, Shulman, & Petersen, 1993). It is interesting to note that the effect of predictability was smaller and later with superior parietal lesion patients.

Note that involuntary attention tested in a spatial cuing paradigm is only one version of stimulus-driven capture (Rauschenberger, 2003; Ruz & Lupianez, 2002). Nearly all of this research involves RT experiments. We do not know whether other forms of stimulus-driven capture will follow the pattern as we found with the spatial cuing paradigm (i.e., effects in RT experiments but not accuracy experiments). In a recent review of this literature, Rauschenberger (2003) pointed out that the results in this literature are often ambiguous. For example, it may be that responses to a cued or probed stimulus are faster than to other stimuli because they are placed at the top of a processing queue without affecting the perceptual representation. Thus, future experiments, following our four methodological criteria, could shed light on the form of attention reflected in other stimulus-driven capture paradigms.

In summary, we found that informative cues did enhance the perceptual representation so that observers were more accurate in target identification in valid than in invalid trials, even when there was no location uncertainty. However, in otherwise identical experiments with noninformative cues, there was no effect in accuracy but substantial effects in parallel RT experiments. We suggested that the effect with informative cues was related to the strategic and voluntary allocation of attention and that it affects channel enhancement, whereas noninformative cues affect channel selection. Whether this latter description is correct or not, these experiments demonstrate that one cannot assume in attention research that RT and accuracy experiments will yield the same results.

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