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The Role of Target–Distractor Relationships in Guiding Attention and the Eyes in Visual Search

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Current models of visual search assume that visual attention can be guided by tuning attention toward specific feature values (e.g., particular size, color) or by inhibiting the features of the irrelevant nontargets. The present study demonstrates that attention and eye movements can also be guided by a relational specification of how the target differs from the irrelevant distractors (e.g., larger, redder, darker). Guidance by the relational properties of the target governed intertrial priming effects and capture by irrelevant distractors. First, intertrial switch costs occurred only upon reversals of the coarse relationship between target and nontargets, but they did not occur when the target and nontarget features changed such that the relation remained the same. Second, irrelevant distractors captured most strongly when they differed in the correct direction from all other items—despite the fact that they were less similar to the target. This suggests that priming and contingent capture, which have previously been regarded as prime evidence for feature-based selection, are really due to a relational selection mechanism. Here I propose a new relational vector account of guidance, which holds promise to synthesize a wide range of different findings that have previously been attributed to different mechanisms of visual search.

Keywords: visual search, feature priming, irrelevant distractor, contingent capture, linear separability

It has long been known that one cannot simultaneously process and consciously perceive all items present in a cluttered visual scene. In order to identify all objects present in a visual scene, one needs to allocate attention serially to different locations (e.g., Treisman & Gelade, 1980). Thus, visual attention has important implications for survival and managing everyday tasks (e.g., detecting a road sign, searching for a friend in a crowded cafeteria). Correspondingly, one of the most important goals of attention research has been to identify the factors that guide visual attention and determine which stimuli will be processed first.

In the laboratory, the dynamics of attention have traditionally been investigated with the visual search paradigm, in which observers have to search for a certain, predefined target among several irrelevant nontargets. The search items are typically objects varying in basic visual features such as color, shape, and size, and search performance is assessed by measuring response times (RTs) and accuracy in response to the target or by monitoring the eye movements of the observers during the search. Eye movements are usually preceded by covert attention shifts, allowing inferences about the location that was selected (e.g., Deubel & Schneider, 1996; Peterson, Kramer, & Irwin, 2004). The factors that guide attention can be examined by systematically varying the stimulus conditions (e.g., the kind and number of features) and/or the knowledge of the observer (e.g., a search for a known target with prespecified features vs. a search for a target whose features randomly change). Previous research has identified at least two major factors that are critical for the guidance of attention. First, the saliency, or *feature contrast*, of the target and nontargets affects search performance in a purely stimulus-driven way, that is, independent of the goals and intentions of the observers. Second, attention can also be tuned toward specific features in a top-down controlled or goal-oriented manner, allowing selection of items that match a known *feature value* of the search target.

The impact of feature contrast, or saliency, on selection has been shown by studies that systematically varied the feature contrast of the search target and the irrelevant nontargets in the display. One important result of these studies is that the feature contrast of the target is responsible for the *pop-out effect*: When the search target is very dissimilar to all nontargets (i.e., when it has a large feature contrast to the nontarget features) and the nontargets are all similar to one another (i.e., when they have a low feature contrast), the target pops out from the display and can be found immediately (e.g., Duncan & Humphreys, 1989; Wolfe, 1994). In contrast, when the target is similar to the nontargets, or when the nontargets are dissimilar to each other, it takes much longer to find the target and search proceeds inefficiently; that is, search times increase as the number of nontargets in the display (or the set size) increases (e.g., Treisman & Gelade, 1980). Theoretically, this has been taken to indicate that attention cannot be guided immediately to the target but that the nontargets have to be selected and rejected individually in a search for the target (e.g., Treisman, 1988).

Another finding that has often been cited in support of bottom-up, saliency-based guidance is that a salient distractor can capture

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attention even though it is irrelevant to the task and observers are instructed to ignore it (e.g., Theeuwes, 1991, 1992). For instance, when observers are asked to search for a pop-out target with a particular shape, then the search is slowed when one of the nontarget shapes is presented in a color different from that of the other items (an irrelevant color singleton distractor; e.g., Theeuwes, 1991, 1992).

Interestingly, however, irrelevant distractors have also been shown to capture attention and the eyes much more frequently when they were similar to the target than when they were dissimilar (e.g., Ludwig & Gilchrist, 2002, 2003)—a finding that has been typically taken to show that attentional capture by irrelevant distractors is to a large extent contingent on the top-down controlled attentional settings (e.g., Folk & Remington, 1998; Folk, Remington, & Johnston, 1992, 1993). For instance, when observers search for a red target among gray nontargets, then a red distractor presented at an irrelevant position will frequently capture attention and the eyes whereas a green distractor will capture much less or not at all (Becker, Ansorge, & Horstmann, 2009; Ludwig & Gilchrist, 2003). This *similarity effect* has commonly been interpreted as support for the top-down, feature-specific attentional system that guides attention in accordance with the intentions and goals of the observers (e.g., Ansorge & Heumann, 2003, 2004; Ansorge & Horstmann, 2007; Duncan & Humphreys, 1989; Folk et al., 1993; Itti & Koch, 2000; Wolfe, 1994).

Further evidence for a top-down, feature-specific attentional system derives from studies demonstrating that knowledge about the particular target features speeds search in more complex search displays. In some studies, observers had to search for a particular, predefined target feature (e.g., a large target among medium and small nontargets), and search performance in this condition was compared with a condition in which the target feature was uncertain (i.e., search for a target with a unique size that was randomly large, medium, or small and was presented among nontargets of the other two sizes). The results from these studies show that responses are much faster when the features of target and nontargets are known in advance than when they vary randomly (e.g., Hodsoll & Humphreys, 2001; Kristjansson, Wang, & Nakayama, 2002; Wolfe, Butcher, Lee, & Hyle, 2003).

Given this evidence, it is perhaps unsurprising that most current theories of attentional guidance propose that attention is guided by an interplay of a bottom-up, saliency-based attentional system and a top-down, feature-specific selection mechanism. According to current models of visual search, objects in the visual field are initially filtered through different feature-specific or broad categorical channels and are represented on different feature-specific maps. Top-down selectivity is achieved by tuning attention toward particular channels (e.g., red, yellow, green, or blue channels), thereby increasing the weight or gain on a particular feature map. The feature contrast of the stimuli is computed within the differentially weighted feature maps, and the information from all feature maps is then integrated into an overall saliency map, which guides attention in a serial manner toward locations with the highest activation levels (Itti & Koch, 2000; Navalpakkam & Itti, 2007; Treisman & Sato, 1990; Wolfe, 1994; Wolfe, Cave, & Franzel, 1989).

There is still some debate about whether top-down control is fine-grained or coarse-grained; that is, whether attention can be tuned toward specific feature values (e.g., Navalpakkam & Itti, 2006) or only to the broad categorical attributes of the target (e.g., Wolfe, 1994). However, most models assume that top-down control of attention is achieved by selectively enhancing and/or augmenting the feature contrast of the target and nontargets.

The main aim of the present study is to show that attention and eye movements can also be guided by a different kind of information than has been previously proposed, that is, by *relational information about the target*. Relational information specifies *how* the target differs from the features of irrelevant items and thus characterizes the target in relation to its context (e.g., the target is larger, darker, or redder than the nontargets). A relational specification of the target implies directionality (e.g., larger) and is both more specific than information about the feature contrast of the target (i.e., information *that* the target differs from the distractors) and more flexible and abstract than a specification of the feature value to search for (e.g., red). As will be described in detail below, the idea that attention can also be guided by relational information about the target was first articulated to explain some unusual results with the *feature priming effect* (Becker, 2008a).

The Feature Priming Effect

The feature priming effect was first discovered in a pop-out search task, in which observers had to search for an odd-man-out target (e.g., a red item among homogeneously green items) and to respond to an additional feature of the target (e.g., its shape). Importantly, the features of the target and nontargets randomly changed across trials such that, for instance, the target could be either a red item among green nontargets or a green item among red nontargets (Maljkovic & Nakayama, 1994). Across a sequence of trials, the features of the target and the nontargets could thus be repeated ("repetition trial") or switched ("switch trial"), compared with the stimulus conditions in previous trial(s). The key finding from these experiments was that responses were slower on switch trials than on repetition trials (e.g., Maljkovic & Nakayama, 1994, 1996). Subsequent eye movement studies, moreover, demonstrated that these switch costs were due to a *misguidance effect*; that is, switching the features between trials led to more frequent erroneous selection of the nontargets before the target was selected (Becker, 2008b, 2008c; McPeek, Maljkovic, & Nakayama, 1999).

The feature priming effect has important implications for current theories of attentional guidance. First, Maljkovic and Nakayama (1994, 1996) noted that these results imply that a salient item does not automatically capture attention solely by virtue of its feature contrast. Rather, information from the previous trial is carried over to the next trial and "primes" attention toward the feature value that the target had on the previous trial. Thus, our ability to select the target immediately and as the first item in the display depends not only on its saliency but also on the trial history.

Second, intertrial priming effects were also found to modulate distractor effects. Several studies have shown that a salient distractor from an irrelevant dimension can be successfully ignored when its features are repeated across consecutive trials: The distractor captures attention only when its feature changes such that it inherits the feature formerly associated with the target, whereas the target inherits the feature formerly associated with the distractor (e.g., when a red distractor presented among green items changes to a green distractor and is presented among all red items). This indicates that a salient distractor does not capture attention solely by virtue of its feature contrast but because of feature-specific inhibition and activation pertaining to the irrelevant features associated with the distractor and target, which carry over to the next trial (Becker, 2007; Olivers & Humphreys, 2003; Pinto, Olivers, & Theeuwes, 2005). Thus, it is possible that effects that were originally attributed to a bottom-up, saliency-based selection mechanism (i.e., distractor effects and pop-out effects) are to a large extent due to priming.

Third, theories of visual search had ascribed feature-specific effects to a top-down selection mechanism that guides attention according to the goals and intentions of the observers. However, Maljkovic and Nakayama (1994) convincingly demonstrated that feature priming effects do not originate from a top-down controlled, feature-specific selection mechanism but are based on automatic intertrial carry-over effects that do not require active support of top-down processes. Thus, in order to explain priming effects, Maljkovic and Nakayama (1994) introduced a new hypothesis, the *priming of pop-out* hypothesis. According to this account, selection of the target on a given trial activates the features associated with the target and/or inhibits the features associated with the nontargets. These activation and inhibition patterns then automatically carry over to the next trial, so that attention is initially (mis)guided toward the feature that was previously associated with the target (e.g., Lamy, Antebi, Aviani, & Carmel, 2008; Leonard & Egeth, 2008; Maljkovic & Nakayama, 1994).

The Role of Relational Information in Priming

A study by Becker (2008a) suggested that priming effects are based on a relational specification of how the target differs from the distractors (e.g., redder, darker), which is in contrast to the standard view that priming consists in intertrial transfers of feature-specific information. Evidence for this view derives from a study in which observers had to search for a size pop-out target, whereby the target and nontarget features were varied differently in three blocked conditions. In particular, the target could (a) randomly switch features with the nontargets, as in the classical condition, (b) change features such that the relationship between the target and nontargets remained constant (i.e., the target could be small or medium and was presented among consistently large nontargets), or (c) change features such that the relationship between the target and nontargets reversed from larger to smaller and vice versa (i.e., the target could be small or large and was presented among constantly medium nontargets). The results showed that reversing the relationship between the target and nontargets produced switch costs of the same magnitude as in the classical condition, in which the target and nontarget features directly switched. In contrast, switch costs were absent when the target changed such that the relationship between the target and nontargets remained the same across trials (Becker, 2008a).

These results cannot be explained by a target-activation or distractor-inhibition view but instead indicate that what is transferred across trials and guides attention in priming is information about the relational properties of the target (Becker, 2008a). To illustrate in more detail how information about target–distractor relationships can guide attention, I will briefly describe a relational principle of attentional guidance that will subsequently be tested in a number of different search tasks.

How Can Relational Information Guide Attention?

The core idea of the relational account is that when observers are instructed to search for an odd-colored item in the display (e.g., an orange target among yellow nontargets), the visual system will not necessarily process the information about the target-defining feature in isolation. Instead, the *relationship* between the target and the distractors can be assessed and used to guide attention. In this case, attention will not be guided toward the specific feature value (e.g., orange) but toward the relational properties of the target (e.g., redder). As a consequence, all items with corresponding relational properties (e.g., items that are redder than their surroundings) will have a high attention-driving capacity.

Analogously, when the target feature varies across trials, attention can be tuned in to the direction in which the target differs from the nontargets, and this relational specification of the target then carries over to the next trial(s). Thus, on the next trial, attention will first be guided toward items that have the same relational properties as the target on the previous trial. For example, if the target was larger than the nontargets on the previous trial, attention is first guided toward the largest item in the visual field on the current trial.

Relationships between stimuli cannot be represented on separate feature maps (e.g., different feature maps coding for red and green) but instead have to be represented within a continuous *feature space*. In such a feature space, different features are encoded by their position, and features from different dimensions are represented in different feature spaces (e.g., for color, size, orientation). Figure 1 depicts an example of a greatly simplified size feature space, in which the size of the stimuli varies continuously, from "small" on the left-hand side to "large" on the right-hand side. The relationship between the target and the distractors can be represented by the *direction of a vector* pointing from the target to the distractor features (e.g., a vector pointing from left to right indicates the relation "smaller"; a vector pointing from right to left [not drawn] would indicate the relation "larger"). The feature contrast, or (dis)similarity between different items, can also be represented, by the *length of the vector*, or the distance between two features in feature space.

Within this framework, current theories of visual search would claim that attention is guided toward the stimulus whose vectors have the greatest length (i.e., are most salient) and whose absolute position in feature space has been preactivated by top-down controlled processes—whereas they would dismiss the direction of the vectors as unimportant. In contrast to this, the core idea of the relational account is that the attention-driving capacity of each feature can be predicted from the vector direction (provided that the feature contrast between the target and the nontargets is high enough to allow distinguishing these features preattentively). So, for instance, in Figure 1, all items depicted on the left-hand side differ in the same direction from the other items as the target differs from the nontarget features on the previous trial (depicted on top). Thus, all items on the left-hand side are predicted to have equally high attention-driving capacity. In contrast, stimuli on the right-hand side differ in the wrong direction from the other items and are thus predicted to have low attention-driving capacity.

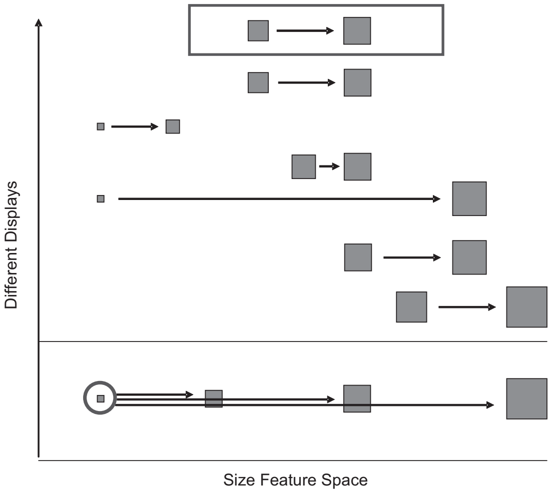


Figure 1. Illustration of the predictions of the relational vector account (see top example). Relationships are depicted as the direction of vectors in feature space that point from the target to the nontargets (here, in pop-out search displays). Vectors pointing from nontargets to other nontargets are not depicted, because the nontargets had identical features. When the target on the previous trial (see boxed example) is smaller, attention will be tuned toward smaller items. As a consequence, the attention-driving capacity of all stimuli on the left-hand side will be approximately equally high. By contrast, stimuli on the right-hand side differ in the wrong direction and should have low attention-driving capacity. When multiple features are present (see bottom example), the one that differs in the correct direction from all stimuli has the highest attention-driving capacity (see encircled item).

Thus, in a pop-out search, if one of the items on the right-hand side becomes the target item, this should result in switch costs, because one of the nontarget items is selected first. However, when displays change such that the target differs in the correct direction from the nontargets, as depicted in the example, then this should not produce any switch costs, and consequently, priming effects should be absent. Importantly, this result pattern is expected to occur regardless of the similarity of the target (or the nontargets) to the target (or the nontargets) on the previous trial and independently of the feature contrast between target and nontargets (provided that the feature contrast between the target and nontargets is not so low as to render the target and nontargets indistinguishable from one another).

When displays consist of multiple different features (see Figure 1, bottom panel), then the stimulus that differs from *all other items* in the correct direction should have the highest attention-driving capacity and capture attention and the eyes first (i.e., in the example, the leftmost stimulus). This should occur even when this item is less similar to the target than one of the other items (i.e., second stimulus from left). As can be seen in Figure 1, the stimulus that is predicted to capture attention first is, incidentally, also one of the most salient stimuli in the visual field. Thus, because capture by salient stimuli emerges as a side effect from the relational view, it is consistent with previous demonstrations that salient items can be found efficiently or that they can capture attention. However, according to the relational account, this does not hold for all salient items, because stimuli that differ in the wrong direction should not capture attention, even when they are very salient: For example, the rightmost item on the bottom panel of Figure 1 is also very salient, but it will not capture attention because it differs in the wrong direction from the other items.

Aims of the Present Study

The present study has three central aims:

1. To replicate and extend the results suggesting that feature priming is based on intertrial transfers of the relational properties of the target

2. To demonstrate that attention is guided by a relational specification even when the target feature remains constant across trials, and attending to the relational properties of the target harms performance

3. To argue that the relational idea can be merged with existing theories of visual search to account for differences in search efficiency (especially linear separability effects)

Before attentional guidance by relational information can be proclaimed as a general principle of guidance, it is important to replicate and extend previous findings on the feature priming effect. So far, guidance by relational information has been demonstrated in only one experiment, in which observers had to search for a size pop-out target. To ascertain that guidance by relational information can also explain priming effects in other dimensions, Experiment 1 investigated feature priming effects in search tasks involving size, color, and luminance targets and compared feature priming effects in the following three conditions: (a) when the target and nontargets directly switched across trials, (b) when the target feature changed such that the relationship between target and nontargets reversed, and (c) when the target feature changed such that the relationship remained constant. Experiment 2 showed that feature priming effects follow the relational principle even when the features of the possible targets and nontargets undergo large physical changes, excluding alternative explanations that assume that attention can be tuned to broad feature categories.

A second important aim of the present study was to investigate whether the relational principle of guidance can be extended to other visual search tasks. So far, relational specifications have been demonstrated to determine the deployment of attention only under the specific conditions of feature priming experiments, where the target feature is always uncertain. Experiment 3 tested whether attentional capture by an irrelevant distractor also critically hinges on the relational properties of the target when the target has a known and constant size. As in previous studies, the distractor was presented at an irrelevant position and was randomly either similar to the target (i.e., same size as the target) or dissimilar to the target. Extending on previous research, I also tested distractors that were smaller and larger than the target, allowing a critical test of the relational view.

Experiment 1: Switch Costs in Pop-Out Search for Size, Color, and Luminance Singleton Targets

The main aim of Experiment 1 was to replicate and extend earlier results suggesting that feature priming effects are based on intertrial transfers of relational information about the target. To that end, Experiment 1 tested intertrial priming effects in three different search tasks, in which observers had to search for a target in the color, luminance, and size dimensions. Each search task comprised three blocked conditions (as in Becker, 2008a). The first condition reinstated the classical *target switch condition*, in which the target and nontarget features directly switch across trials, such that the target inherits the feature formerly associated with the nontargets and vice versa. In the other two conditions, only the target feature changed across trials. In the *relationship reversal condition*, the target feature changed such that the relationship between the target and the nontarget features reversed (e.g., the target could be either larger or smaller than the consistently medium-sized nontargets). By contrast, in the *same relationship condition*, the coarse relationship between the target and the nontargets always remained constant while the target changed in the same way as in the target switch condition (e.g., the target changed between a small and medium item among large nontargets, yielding the constant relation "smaller").

These three conditions were tested separately in search for a singleton in the color, size, and luminance dimensions. Moreover, the eye movements of the observers were monitored during search to ascertain whether switch costs in the RTs were due to more frequent selection of the nontargets (see the misguidance effect; Becker, 2008a).

According the relational account, switch costs in the target switch condition and in the relationship reversal condition should be of equal magnitude, because in both conditions, the coarse relationship between target and nontarget features reverses. By contrast, switch costs should be absent in the same relationship condition, in which the relationship between the target and nontargets remains constant across repetition and switch trials.

If priming effects are based on intertrial transfers of feature-specific information, we would expect large priming effects in the target switch condition, in which the target inherits the nontarget feature on switch trials and vice versa. In the relationship reversal condition and the same relationship condition, in which only the target changes, priming effects should be of equal magnitude but could be somewhat reduced when compared with the full switch condition, in which both target and nontarget features change on switch trials.

Different degrees of target discriminability were also taken into account, by separately analyzing switch costs for the different target displays across all conditions (see Table 1). This allows comparing switch costs in identical displays between the different conditions, ensuring that differences in intertrial switch costs between the conditions are not due to differences in feature contrast.

Method

Participants. Eighteen volunteers from the University of Bielefeld, in Bielefeld, Germany, were equally divided in groups of six and assigned to each of the three search tasks of Experiment 1. They were paid 6 euros ($8) per hour for their participation. All participants had normal or corrected-to-normal vision and were naive as to the purpose of the experiment.

Materials. An Intel Pentium(R) 4CPU 3.00-GHz computer (tico) with a 19-in. SVGA color monitor (AOC) controlled the timing of events and generated the stimuli. Stimuli were presented with a resolution of 1,024 × 768 pixels and a refresh rate of 99.9 Hz. For the recording of eye movements, a video-based infrared eye tracking system (iViewX tracker, SMI, Teltow, Germany) with a spatial resolution of 0.1° and a temporal resolution of 240 Hz was used. Participants sat in a dimly lit room with their head fixated by the eyetracker's chin rest and forehead support and viewed the screen from a distance of 92 cm. For registration of manual responses, a standard USB optical mouse was used. Event scheduling and RT measurement were controlled by Presentation software (Neurobehavioral Systems).

Stimuli. The response-related stimuli consisted of five "o" or "+" signs printed in 10-point Arial black and measuring 0.15° × 0.15°. The stimuli were all colored black and located in the center of the search-relevant stimuli. In the size search task, displays consisted of five green-colored squares that could be small (0.8° × 0.8°), medium (1.5° × 1.5°), or large (2.1° × 2.1°; 2.9 cd/m^2). In the color search task, the display consisted of five disks with a diameter of 2.0° that were colored and had a yellow (2.8 cd/m^2), orange (2.2 cd/m^2), or red (1.8 cd/m^2) tinge. In the luminance search task, all disks were presented in gray and were of different luminance: The light gray target had a luminance of 3.5 cd/m^2, the medium target had a luminance of 2.3 cd/m^2, and the dark target had a luminance of 1.6 cd/m^2. In the luminance task only, the "o" or "+" symbol at the center of each object was presented on a small (0.37° × 0.37°) white background window to equate discrimination difficulty across differently colored disks. The search-relevant stimuli were all presented on the outlines of an imaginary circle with a diameter of 7° on a constantly white background (7.3 cd/m^2). The distance between adjacent stimuli was 4.3° (center to center). Figure 2 depicts an example of the stimuli in each condition.

Design. The size, color, and luminance search tasks each comprised three blocked search conditions. First, in the target switch condition, the target varied in the following way: In the size search task, the target was randomly either small or medium and was presented among medium and small distractors; in the color search task, the target was randomly either yellow or orange and was presented among orange and yellow distractors; and in the luminance search task, the target was either light or medium gray and was presented among medium and light gray distractors, respectively. Second, in the relationship reversal condition, the stimuli were designed in the following way: In the size search task, the target was randomly either small or large and was presented among consistently medium distractors; in the color search task, the target was either yellow or red and was presented among orange distractors; and in the luminance search task, the target was light or dark gray and was presented among medium gray distractors. Third, in the same relationship condition, the following stimulus conditions were applied: In the size search task, the target was either small or medium and was presented among large distractors; in the color search condition, the target was randomly either yellow or orange among constantly red distractors; and in the luminance search condition, the target was either light or medium gray and was presented among consistently dark gray distractors.

The positions of the target and the type of distractors, as well as the combinations of each target type with each response-related item, were randomly varied, with the restriction that all displays always included equal numbers of the response-related "o" and "+" stimuli (exempting the target). In each of the three blocked conditions, participants completed 220 trials. On average, it took half an hour to complete the experiment.

Table 1
Mean Response Times (RTs) and Mean Proportion and Latencies of First Saccades to the Target in the Pop-Out Search Task of Experiments 1 and 2, Depicted Separately for the Different Target Features

Experiment, search, intertrial condition	Mean RTs in ms					
	Target switch		Relationship reversal		Same relationship	
Experiment 1						
Size	Medium	Small	Large	Small	Medium	Small
Target same	963	1,026	975	1,050	936	909
Target different	1,077	1,110	1,104	1,121	960	913
Priming	*113***	*84**	*129***	*71**	*24*	*4*
Color	Yellow	Orange	Yellow	Red	Yellow	Orange
Target same	861	844	857	812	778	774
Target different	923	894	904	835	773	778
Priming	*62**	*49***	*47**	*23*	*−5*	*4*
Luminance	Light	Dark	Light	Dark	Light	Medium
Target same	842	843	940	917	787	880
Target different	879	917	1,025	983	804	867
Priming	*38*	*74**	*85***	*67**	*17*	*−13*
Experiment 2						
Size	Medium	Small	Superlarge	Small	Large	Small
Target same	831	917	799	856	784	779
Target different	921	960	857	924	799	791
Priming	*90***	*43***	*58***	*68**	*14*	*12*
	First saccades to the target: Mean proportion (in %) / mean latencies (in ms)					
Experiment 1						
Size	Medium	Small	Large	Small	Medium	Small
Target same	55.4 / 279	35.1 / 298	58.1 / 272	34.8 / 297	57.5 / 263	63.3 / 245
Target different	40.3 / 292	27.7 / 321	48.0 / 276	26.1 / 298	56.8 / 262	73.4 / 239
Priming	*15.3*** */ 13*	*7.4 / 24*	*10.0 / 13*	*8.7** */ 1*	*0.7 / −1*	*–10.1 / –6*
Color	Yellow	Orange	Yellow	Red	Yellow	Orange
Target same	64.5 / 234	69.4 / 239	68.1 / 242	87.2 / 218	95.5 / 203	94.6 / 202
Target different	36.3 / 259	47.8 / 252	52.2 / 252	71.3 / 226	96.7 / 200	94.9 / 202
Priming	*28.2*** */ 15**	*21.7*** */ 13*	*15.8 / 10***	*15.9*** */ 7**	*–1.2 / –3*	*0.2 / 0*
Luminance	Light	Dark	Light	Dark	Light	Medium
Target same	47.3 / 234	58.2 / 229	41.3 / 239	47.9 / 235	68.1 / 219	52.4 / 238
Target different	38.7 / 236	40.2 / 231	27.3 / 248	42.1 / 244	71.5 / 218	50.4 / 230
Priming	*8.6** */ 2*	*18.0** */ 2*	*14.0*** */ 9*	*5.4 / 9*	*–3.4 / –1*	*1.9 / –8*
Experiment 2						
Size	Medium	Small	Superlarge	Small	Large	Small
Target same	84.0 / 251	50.3 / 286	80.8 / 260	62.5 / 268	77.6 / 257	79.0 / 251
Target different	50.4 / 270	34.3 / 291	63.3 / 281	53.6 / 294	82.8 / 263	77.8 / 254
Priming	*33.6*** */ 19**	*16.0** */ 5*	*17.5** */ 21***	*8.6 / 26***	*–5.2 / 6*	*1.2 / 3*

Note. Priming values in italics indicate the difference score of target different minus target same.
$^{*}p < .05$. $^{**}p < .01$.

Procedure. Each trial started with the presentation of a small black fixation cross. Participants were instructed to fixate on the center of the cross. At the beginning of each trial, a fixation control was implemented: The stimulus display was presented only if the tracking was stable (no blinks) and the gaze was within 50 pixels (1°) of the center of the fixation cross for at least 350 ms (within a time window of 3,000 ms). Otherwise, participants were calibrated anew (five-point calibration) and the next trial started again with the fixation control.

Upon presentation of the stimulus display, participants were required to search the display for the predefined target and to press the right mouse button if the response-related item inside the target was a "+" and the left mouse button if it was an "o." The stimulus display remained onscreen until a response was made and was immediately succeeded by a feedback display. The feedback consisted of the German words for "right" or "wrong" (in black, 14 points), which were presented centrally and remained onscreen for 500 ms. After an intertrial interval of 500 ms, during which a blank white screen was presented, the next trial started with the presentation of the fixation cross.

Before each block, the eye tracker was calibrated with a five-point calibration and participants were given written instructions about the next block. Moreover, participants were instructed to respond to the target as fast as possible without making mistakes.

Results

Data. Data were excluded from all analyses when the manual RT exceeded 1,500 ms or when the eyes had not been fixating on the target after 1,500 ms. The eyes were counted as fixating on the

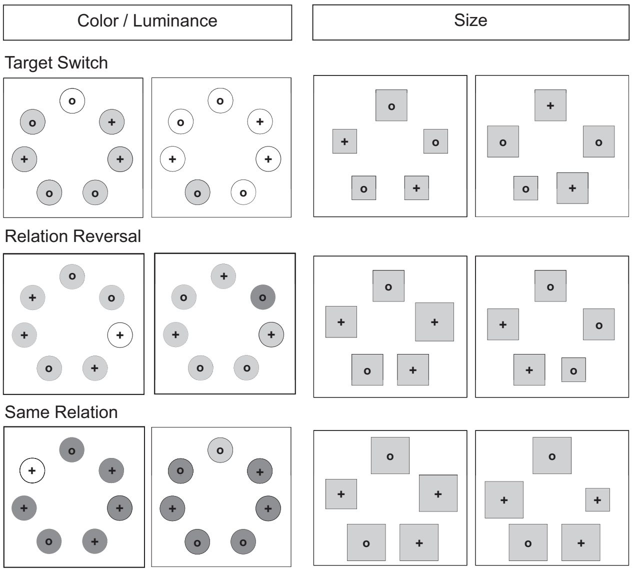


Figure 2. In Experiment 1, participants had to search for a pop-out target and to respond to the "o" and "+" characters inside the target. The left two panels show examples of the feature changes across search displays when the task was to search for a color or luminance pop-out target, and the right two panels depict the same for a size pop-out search. Sample displays in the target switch condition, the relationship reversal condition, and the same relationship condition are depicted from top to bottom. In the size search task, all squares were green, whereas in the color search task, the white, light gray, and dark gray circles in the figure were yellow, orange, and red, respectively.

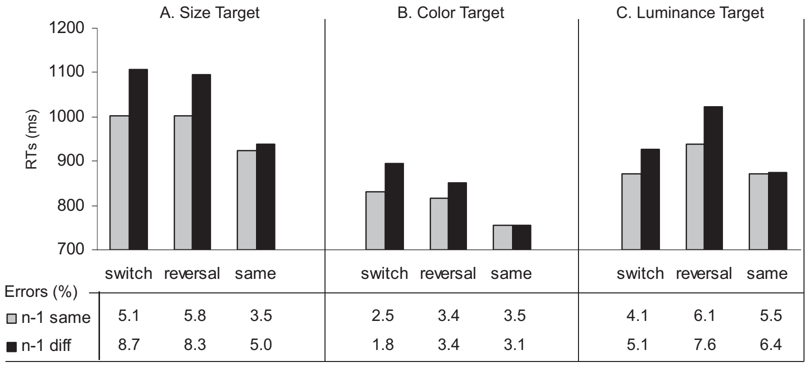
target if the gaze had been within 1° of the center of the response-related stimulus and no saccade had occurred (velocity $< 30°/s$). This led to a loss of 6.47% of all data (11.4% in the size search task, 2.0% in the color search task, and 6.0% in the luminance search task). Moreover, trials on which a manual response error occurred were excluded from the analyses of RTs and eye movements.

RTs. The mean RTs for each condition are depicted in Figures 3A–3C, separately for each search task. Considering first the size search task (see Figure 3A), there were significant priming effects in the target switch condition, $F(1, 5) = 198.6$, $p < .001$, and in the relationship reversal condition, $F(1, 5) = 32.2$, $p = .002$, but not in the same relationship condition, $F(1, 5) = 3.9$, $p = .105$. To test whether the differences in priming between the conditions were significant, I computed separate 2 × 2 analyses of variance (ANOVAs) over repetition versus switch trials across two conditions each. The results showed that priming effects in the target switch condition and the relationship reversal condition did not differ significantly from each other (i.e., nonsignificant interaction between priming and condition; $F < 1$). However, priming effects were significantly larger in the target switch condition and the relationship reversal condition than in the same relationship condition, which did not show significant switch costs, $F(1, 5) = 109.0$, $p < .001$, and $F(1, 5) = 37.6$, $p = .002$, respectively.

Of the remaining effects, the main effect of condition reached significance, reflecting that mean RTs were overall faster in the same relationship condition ($M = 931$ ms) than in both the target switch condition ($M = 1{,}055$ ms), $F(1, 5) = 35.2$, $p = .002$, and the relationship reversal condition ($M = 1{,}048$ ms), $F(1, 5) = 7.6$, $p = .04$. In contrast, mean RTs did not differ between the latter two conditions ($F < 1$).

A similar result pattern emerged in the color search task (see Figure 3B). Priming effects again occurred in the target switch condition, $F(1, 5) = 33.2$, $p = .017$, and in the relationship reversal condition, $F(1, 5) = 12.4$, $p = .01$, but not in the same relationship condition ($F < 1$). The priming effects in the target switch condition and the relationship reversal condition again did not differ significantly from each other, $F(1, 5) = 2.7$, $p = .162$, but priming effects in both conditions were significantly different from the same relationship condition, in which changing the target color did not produce switch costs, $F(1, 5) = 21.1$, $p < .006$, and $F(1, 5) = 10.5$, $p = .023$, respectively.

Correspondingly, mean RTs were faster in the same relationship condition ($M = 756$) than in the target switch condition ($M = 863$ ms), $F(1, 5) = 20.9$, $p = .006$, and in the relationship reversal



Errors (%)	switch	reversal	same	switch	reversal	same	switch	reversal	same
▨ n-1 same	5.1	5.8	3.5	2.5	3.4	3.5	4.1	6.1	5.5
■ n-1 diff	8.7	8.3	5.0	1.8	3.4	3.1	5.1	7.6	6.4

Figure 3. Mean manual response times (RTs) and errors in Experiment 1 in the pop-out search for a size singleton target (Panel A), a color singleton target (Panel B), and a luminance singleton target (Panel C), depicted separately for each search condition. Gray histograms show mean RTs on repetition (rep) trials, in which the target–distractor features from the previous trial were repeated; black histograms show mean RTs on trials in which displays underwent the change specified by the single conditions (diff).

condition (M = 834 ms), $F(1, 5) = 14.8$, $p = .012$, whereas the mean RTs between the latter conditions did not differ significantly from one another, $F(1, 5) = 2.7$, $p = .162$.

In the luminance search task (see Figure 3C), significant priming effects occurred in the target switch condition, $F(1, 5) = 22.5$, $p = .005$, and in the relationship reversal condition, $F(1, 5) = 50.2$, $p = .001$, but not in the same relationship condition ($F < 1$). The priming effects in the target switch condition and the relationship reversal condition also did not differ significantly from each other, $F(1, 5) = 2.1$, $p = .207$, but in both conditions the priming effect was significantly different from the effect in the same relationship condition, in which changing the target luminance did not produce any switch costs, $F(1, 5) = 25.9$, $p < .004$, and $F(1, 5) = 15.7$, $p = .011$, respectively.

Additionally, mean RTs were slowest in the relationship reversal condition (M = 980 ms), $F(1, 5) = 9.3$, $p = .028$, and significantly different from mean RTs in the same relationship condition (M = 873 ms) and in the target switch condition (M = 900 ms), $F(1, 5) = 13.8$, $p = .014$, whereas performance between the latter two conditions did not differ significantly, $F(1, 5) = 2.7$, $p = .168$.

Table 1 displays the mean RTs separately for all different displays and each search task. As can be seen in the table, intertrial priming effects were significant across all displays in the relationship reversal condition and the target switch condition, the only exceptions being red–orange displays in the relationship reversal condition (23 ms, *ns*), and the light–dark displays in the target switch condition (38 ms, *ns*). In contrast, there were no significant priming effects in any of the displays of the same relationship condition. This indicates that the absence of switch costs in the same relationship condition was not due to a larger mean feature contrast between targets and nontargets across the different displays.

Errors. The mean error scores are depicted separately for each search condition at the bottom of Figures 3A–3C. The same ANOVA calculated over the mean errors of the size search task did not show any significant differences between the three blocked conditions (all ps > .110). In the color search task, significantly or marginally significantly fewer errors were made in the target switch condition than in the same relationship condition, $F(1, 5) = 6.8$, $p = .048$, and the relationship reversal condition, $F(1, 5) = 5.0$, $p = .075$; all other ps > .65. In the luminance search task, none of the differences between the three search conditions approached significance (all ps > .15). Thus, interpretation of the data is not complicated by a speed–accuracy trade-off.

Mean proportion and latencies of first saccades to the target. The mean proportion and latencies of first saccades to the target are depicted separately for each display in Table 1. As can be seen in the table, changing the target across trials reliably reduced the mean proportion of first fixations on the target when the relationship between the target and nontargets reversed across trials, whereas changing the target did not affect selectivity when the relationship remained the same. Moreover, the mean saccade latencies showed similar (nonsignificant) trends, with longer latencies on switch trials in the relationship reversal condition and the target–nontarget switch condition but slight facilitation when the target changed and the relationship remained constant. This indicates that impairments in selectivity upon reversals of the relationship were not due to a speed–accuracy trade-off.

Discussion

The results of Experiment 1 indicate, first, that previous findings with respect to feature priming effects (Becker, 2008a) can be replicated with targets from the color and luminance dimension. Consistent with the relational account, switch costs were absent when the target feature changed such that the coarse relationship between target and nontargets (e.g., “redder” or “darker”) remained the same across trials. However, when changes in the target features led to a reversal of the relationship between target and nontargets, switch costs emerged. Moreover, switch costs in the relationship reversal condition, in which only the target feature changed, were of the same magnitude as in the target–nontarget switch condition, in which both the target and nontarget features changed. This indicates that switch costs were observed in the target switch condition because the coarse relationship between the target and nontarget features reversed across trials, not because the target inherited a previously deactivated nontarget feature or because the nontargets inherited a previously activated target feature (cf. Becker, 2008a).

The analysis of the mean proportion of first saccades to the target showed that reversing the relationship between target and nontargets led to more frequent selection of one of the irrelevant nontargets. This replicates previous findings showing that switch costs are due to erroneous attention shifts and eye movements toward a nontarget (Becker, 2008a, 2008b, 2008c; McPeek et al., 1999). Selection of the inconspicuous nontargets is unexpected according to strong versions of the saliency-based view, which propose that the guidance of attention is completely determined by feature contrast information (e.g., in the *singleton detection mode*, Bacon & Egeth, 1994; or in the *singleton capture view*, Theeuwes, 1992).

Similarly, the finding that feature priming effects occurred only at reversals of the target–distractor relationship is inconsistent with the common feature-specific explanation of the priming effect, which attributes priming effects to carryover effects of target activation (e.g., Maljkovic & Nakayama, 1994, 1996) or nontarget inhibition (e.g., Geyer, Mueller, & Krummenacher, 2006; Kristjansson et al., 2002). According to the target activation view, switch costs should also have occurred in the same relationship condition, in which only the target feature changed, contrary to the present findings. On a nontarget inhibition view, feature priming effects should have been absent in both the same relationship condition and the relationship reversal condition, in which the nontargets always remained constant, which is also contrary to the observed result pattern.

In sum, the results of Experiment 1 are inconsistent with the standard explanation of feature priming effects, which is based on feature-specific activation and inhibition, whereas they are well in line with the predictions of the relational account. However, as will be discussed in more detail in the next section, it is still possible to explain the results from Experiment 1 without reference to the relational properties of the target.

Experiment 2: Priming in a Pop-Out Search for Dissimilar Size Targets

In the same relationship condition of Experiment 1, the possible target features were quite similar to one another and occupied adjacent regions in feature space. Thus, it is possible that switch costs were absent in this condition because attention could be tuned toward broadly defined feature categories or regions in feature space, which encompassed both target features (e.g., small and medium) but excluded the feature of the nontargets (e.g., large; e.g., Wolfe, 1994). Tuning attention toward broad categorical features was not an option in the relationship reversal condition, because the possible target features (e.g., small and large) were separated by the nontarget features (e.g., medium). Thus, it is possible that selectivity in Experiment 1 was achieved not by tuning attention toward the relational properties of the target but by preactivating broad categorical channels or a broad region in feature space (e.g., *feature divider account*; Huang & Pashler, 2005; see also Wolfe, 1994, 1998a, 1998b).

Experiment 2 was designed to provide a critical test of the categorical versus the relational explanation of intertrial switch costs. To that end, priming effects in the classical switch condition were compared with the relationship reversal and same relationship conditions when the two possible size targets were very dissimilar to one another. In the classical, target–nontarget switch condition, the target was randomly either small and presented among medium nontargets or vice versa. In the remaining two conditions, the target and nontarget features were chosen such that they were associated with completely different features. In the relationship reversal condition, the target was either a small item among medium nontargets or an extra-large item presented among large nontargets. In the same relationship condition, the target was either a small item presented among medium nontargets or a large item presented among extra-large nontargets (see Figure 4 for an example of the stimulus displays).

If switch costs arise because of reversals of the relationship between the target and nontarget features, then priming effects should occur only in the first two conditions and not in the last condition, in which the target is consistently smaller than the nontargets. On the other hand, if switch costs critically depend on activating or inhibiting broadly defined feature categories or regions in feature space, then switch costs should occur in all three conditions of Experiment 2. This holds because, across all conditions, the target categories are interspersed with nontarget categories, so that it is impossible to selectively activate a single category that subsumes all possible target features and excludes the nontarget features. Critically, in the same relationship condition, it is impossible to find the target by tuning attention to a single channel, because the target feature values (small and large) are separated by a nontarget value (medium). Thus, if intertrial priming effects critically depended on preactivation and inhibition of specific broadly categorical channels or regions in feature space, then priming effects should occur in all three conditions. Switch costs may also be stronger in the switch condition than in the other two conditions, because in the latter, the target and nontarget features change such that they occupy completely new regions of feature space, whereas in the switch condition, the target inherits the previously deactivated features of the nontargets and vice versa (e.g., Lamy et al., 2008).

Method

Participants. Six volunteers from the University of Bielefeld took part in the experiment and were paid 6 euros ($8) per hour. All participants had normal or corrected-to-normal vision and were naive as to the purpose of the experiment.

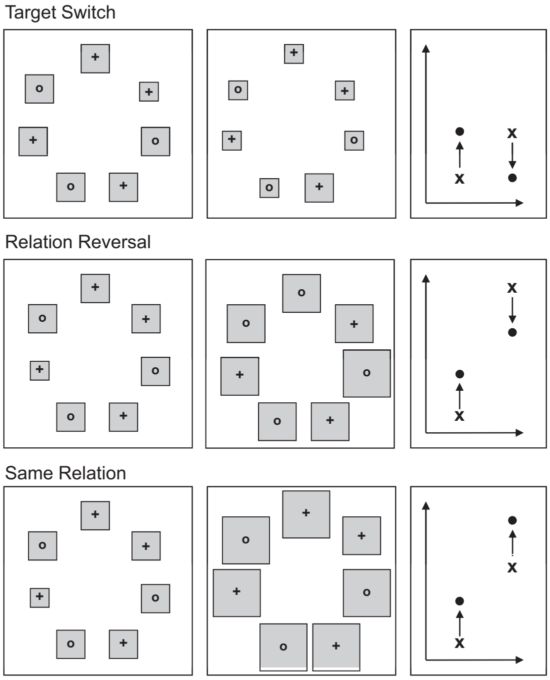


Figure 4. Sample displays of Experiment 2, in which participants had to search for a size pop-out target and to respond to the "o" and "+" characters inside the target. The two left columns of panels depict examples of the stimuli in each of the search conditions. The rightmost column of panels provide a schematic description of the stimuli in each condition, whereby the size of a stimulus is depicted by its locations in feature space and its relationships are depicted by vectors (arrows). The target features are represented by crosses, and the distractor features by filled circles.

Materials. These were the same as in Experiment 1.

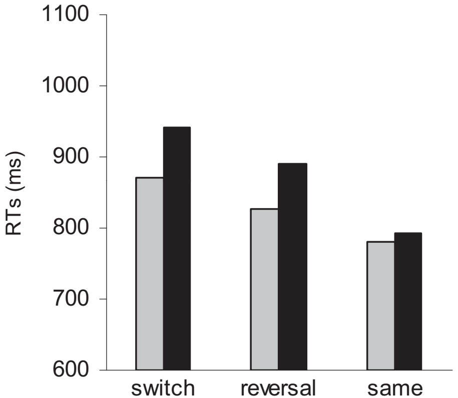
Stimuli, design, and procedure. These were the same as in the previous experiment, with the following exceptions: The small square measured 0.62° × 0.62°, the medium item measured 1.25° × 1.25°, the large one was 1.9° × 1.9°, and the extra-large square measured 3.0° × 3.0°.

Results

Data. Applying the same data exclusion criterion as in Experiment 1 to the data of Experiment 2 led to a loss of 1.68% of all data.

RTs. The mean RTs of Experiment 2 are depicted in Figure 5. Significant priming effects occurred only in the target switch condition, $F(1, 5) = 76.1$, $p < .001$, and in the relationship reversal condition, $F(1, 5) = 11.8$, $p = .019$. However, there were no significant priming effects in the same relationship condition, in which the target was consistently smaller than the nontargets ($F < 1$).

The priming effects in the target switch and relationship reversal conditions did not differ significantly from each other ($F < 1$), but priming effects in both conditions were significantly larger than in the same relationship condition, which did not show any switch



Errors (%)	switch	reversal	same
☐ n-1 same	5.5	6.5	5.1
■ n-1 diff	8.6	7.4	7.4

Figure 5. Mean response times (RTs) and errors in the size pop-out search task of Experiment 2, depicted separately for the three search conditions. Gray histograms show mean RTs on repetition (rep) trials, in which the target–distractor features from the previous trial were repeated; black histograms show mean RTs on trials in which displays underwent the change specified by the single condition (diff). Mean error scores are depicted in the lower graph.

costs, $F(1, 5) = 11.3$, $p = .020$, and $F(1, 5) = 8.6$, $p = .033$, respectively.

Correspondingly, mean baseline RTs were significantly shorter in the same relationship condition ($M = 787$ ms) than in both the target switch ($M = 905$ ms), $F(1, 5) = 35.8$, $p = .002$, and relationship reversal ($M = 858$ ms), $F(1, 5) = 29.7$, $p = .003$, condition, whereas the latter two conditions did not differ significantly from each other, $F(1, 5) = 3.6$, $p = .118$.

Errors. The same analyses computed over the mean errors did not reveal any significant main effects or interactions (all ps $> .13$; see the bottom of Figure 5).

Mean proportion and latencies of first saccades to the target. The mean proportion and latencies of first saccades to the target are depicted in Table 1. As can be seen in the table, the pattern exactly matched the results from the mean RTs, with significant switch costs either in the latency or in the proportion of first saccades to the target occurring only in the switch condition and the relationship reversal condition, not in the same relationship condition.

Discussion

The results of Experiment 2 support the relational account. Contrary to the predictions of the alternative categorical and region-based accounts, there was no reliable switch cost in the same relationship condition, but there was a reliable switch cost in the relationship reversal condition, although the conditions were equal in terms of their affordances to change activation between different categories or regions in feature space. As in the previous experiment, switch costs were restricted to the conditions in which the relationship between target and nontargets reversed, and switch costs were again of the same magnitude in the target switch and relationship reversal conditions. Closer inspection of displays that were identical across both conditions (i.e., small target) shows that switch costs amounted to 43 ms in the switch condition and to 68 ms in the relationship reversal condition (see Table 1). This result pattern is consistent with the relational account but not with any of the feature-based views, which would have predicted switch costs to be stronger in the classical target–nontarget switch condition than in the relationship reversal condition (e.g., Maljkovic & Nakayama, 1994; Lamy et al., 2008).

Experiment 3: The Role of Relational Information in Capture by Similar Distractors

Taken together, Experiments 1 and 2 provide compelling evidence for a relational account of intertrial priming effects. However, all these results were obtained in search tasks in which the target was an odd man out whose features varied randomly, so observers had no explicit incentive to tune attention toward a particular feature value. The important question now is, of course, whether attention is also guided by relational information when the target feature's value remains the same across all trials and the optimal strategy would be to tune attention toward it. This question is usually tested with the *additional singleton paradigm* (e.g., Theeuwes, 1992), in which observers have to search for a particular target feature while ignoring an irrelevant distractor. What is typically found in these experiments is that a distractor which is similar to the target feature captures attention and the eyes to a much higher degree than a distractor which is dissimilar to the target (e.g., Ludwig & Gilchrist, 2003). Currently, this *similarity effect* is thought to depend on the degree of featural similarity between the distractor and the target features and is assumed to result from a top-down, feature-specific activation of the target feature (e.g., Duncan & Humphreys, 1989; Folk et al., 1992, 1993; Itti & Koch, 2000; Wolfe, 1994).

However, according to the relational account, the similarity effect could be due to the fact that the similar distractor usually differs in the correct direction from the nontargets (i.e., differs in the same direction from the nontargets as the target). In other words, the finding that a red distractor captures attention more than a green one does when searching for a red target could be because observers tuned attention toward "redder."

If this alternative interpretation is correct, then distractors that are slightly dissimilar to the target could capture attention and the eyes even more if (and only if) they differ in the correct direction from the nontargets and the target itself. For instance, when the target is smaller than the nontargets, distractors that are even smaller than the target should capture attention more than should distractors that are featurally identical to the target—despite the fact that the distractor is less similar to the target. This holds because, according to the relational account, attention is first guided toward the item that best matches the relational specification of the target (e.g., smaller), and items that are smaller than all other items (including the target) match the specification best (see Figure 1).

Experiment 3 tested this unique prediction of the relational account. In two blocked conditions, observers had to search for either a medium-sized target among large nontargets or a large target among medium-sized nontargets. The irrelevant distractor could be small, medium, large, or extra-large (see Figure 6). As in

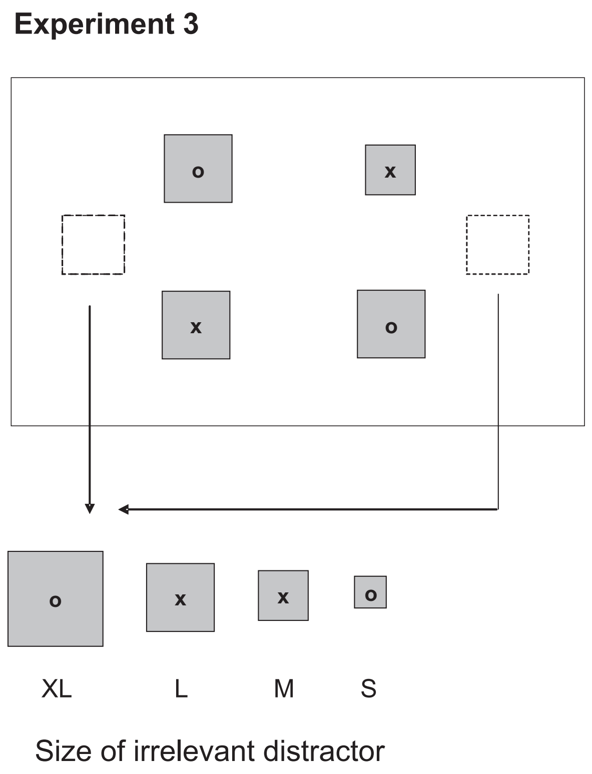


Figure 6. Schematic description of the stimulus display used in Experiment 3. Depicted is the condition in which observers had to search for a medium target among large nontargets. On all trials, one of the four distractors (S, M, L, XL; depicted below) was presented at one of the irrelevant positions either to the left or right of the stimuli. Observers were asked to make a fast eye movement to the target and to respond to the item located inside (x or o), whereas they should ignore the irrelevant distractor. S = small; M = medium; L = large; XL = extra-large.

previous experiments, the eye movements of the observers were monitored during search.

According to the feature-based selection accounts, the distractor that is most similar to the target should be selected most frequently; that is, in the block where the target is the medium item, the medium distractor should be selected most frequently, whereas in the block with the large target, the large distractor should be selected more often than the other distractors.

In contrast, according to the relational account, observers can tune attention to the relational properties of the target, resulting in strong capture by the small distractor when searching for a medium target among large nontargets. Accordingly, search for a large target among medium nontargets should prompt observers to search for larger items, resulting in the most frequent selection of the extra-large distractor.

Method

Participants. Six volunteers from the University of Queensland, in St. Lucia, Queensland, Australia, took part in the experiment. Participants were paid 10 euros ($13.50) per hour, and they all had normal or corrected-to-normal vision and were naive as to the purpose of the experiment.

Materials. An Intel Duo 2 CPU 2.4-GHz computer with a 17-in. FP92E color monitor was used to generate and display the stimuli and to control the experiment. Stimuli were presented with a resolution of 1,280 × 1,024 pixels and a refresh rate of 75 Hz. A video-based infrared eye-tracking system was used (Eyelink 1000, SR Research, Mississauga, Ontario, Canada) with a spatial resolution of 0.1 and a temporal resolution of 500 Hz. Participants were seated in a normally lit room, with their head fixated by the eyetracker's chin rest and forehead support, and viewed the screen from a distance of 62 cm.

Stimuli, design, and procedure. All stimuli were displayed against a white background. Displays contained five green squares of different sizes, each of which all contained an "o" or an "x" (0.28° × 0.28°) as a response-defining feature. Observers had to search for a target square on four possible target locations located at the corners of an imaginary rectangle (centered on the screen; 10.8° × 7.8°), whereas they were instructed to ignore squares presented at one of the irrelevant positions to the right or left of the four target positions. Performance was tested in two blocked search conditions: In the first condition, the target square was large (2.9° × 2.9°) and was presented among medium distractors (2.0° × 2.0°), whereas in the second condition, the features of the target and nontargets were reversed. On all trials, an irrelevant distractor was presented at one of the two irrelevant positions; distractors were one of four different sizes: small (1.4° × 1.4°), medium (2.0° × 2.0°), large (2.9° × 2.9°), or extra-large (3.8° × 3.8°). Figure 6 depicts an example of the stimulus display and the four possible distractor types.

The conditions were combined such that the target with each response-related item ("x" and "o" in Figure 6) appeared at each of the four possible target positions with each of the four distractor types (squares at the bottom of the figure) at each of the two possible distractor positions (dotted outlines) once, yielding $2 \times 4 \times 4 \times 2 = 64$ combinations. These were repeated four times, yielding 256 trials per search condition (with the large or medium target), or 512 trials per participant. The procedure was identical to that used in previous experiments (including the fixation control, feedback, etc.), with the exceptions that instead of a 5-point calibration a 9-point calibration procedure was used and feedback was provided in English.

Results

Data. Excluding all trials with RTs lower than 150 ms and higher than 2,000 ms and trials in which the eyes had not been fixating on the target within 2,000 ms led to a loss of 2.77% of all data.

Proportion of first saccades to the irrelevant distractor and saccade latencies. Figure 7A depicts the mean proportion of first saccades to the distractor prior to target selection. A 2 × 4 ANOVA comprising the factors search condition (search for a large target vs. a medium target) and distractor type (small, medium, large, and extra-large) was computed over the mean proportion of first eye movements to the distractor. The analysis showed a significant main effect of the distractor type, $F(3, 15) = 14.5$, $p = .001$, and a significant interaction between search condition and distractor type, $F(3, 15) = 47.2$, $p < .001$, but no significant main effect of search condition, $F(1, 5) = 3.5$, $p = .12$.

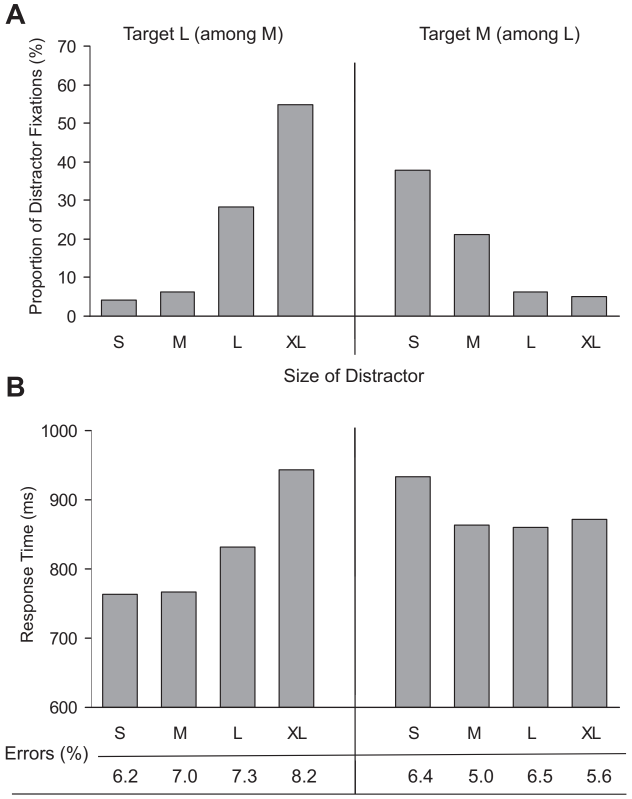


Figure 7. The mean proportion of first fixations on each distractor (Panel A) and the mean response times and errors (Panel B) in Experiment 3, depicted separately for each distractor condition when the target was large (left half of figure) or medium (right half of figure). S = small; M = medium; L = large; XL = extra-large.

In search for the large target, the extra-large distractor was selected most frequently—and significantly more frequently than all other distractors—$t(5) = 4.8$, $p = .005$, for the large distractor; $t(5) = 10.3$, $p < .001$, for the medium distractor; and $t(5) = 9.1$, $p < .001$, for the small distractor. The large distractor was also selected more frequently than the medium distractor, $t(5) = 3.5$, $p = .017$, and the small distractor, $t(5) = 3.3$, $p = .021$, whereas the medium and small distractor were selected equally infrequently, $t(5) = 1.5$, $p > .19$.

When the target was medium and was presented among large nontargets, the small distractor was selected most frequently—and significantly more often than any of the other distractors—$t(5) = 4.9$, $p = .004$, for the medium distractor; $t(5) = 6.1$, $p = .002$, for the large distractor; and $t(5) = 6.7$, $p = .001$, for the extra-large distractor. Moreover, the medium distractor, which had the same size as the target, was also selected more frequently than the large distractor, $t(5) = 5.2$, $p = .003$, and the extra-large distractor, $t(5) = 6.1$, $p = .002$. In contrast, the large and extra-large distractors were selected equally infrequently ($t < 1$).

The distractors were not all selected frequently enough to permit a complete statistical analysis of the saccade latencies; however, critical comparisons could still be computed. When the target was medium, latencies of saccades directed toward the medium distractor were nonsignificantly shorter ($M = 256$ ms) than saccade latencies to the small distractor ($M = 263$ ms; $t < 1$). Similarly, when the target was large, mean saccade latencies to the large distractor were nonsignificantly longer ($M = 272$ ms) than to the extra-large distractor ($M = 266$ ms; $t < 1$). This indicates that more frequent selection of the extra-large and small distractors was not due to a speed–accuracy trade-off (i.e., to the fact that saccades to these distractors were executed earlier than to the most similar distractors).[1]

RTs. The mean RTs and the mean error scores of Experiment 3 are depicted in Figure 7B. The same 2 × 4 ANOVA computed over the mean RTs showed a significant main effect of distractor type, $F(1, 5) = 35.0$, $p < .001$, and a significant interaction between search condition and distractor type, $F(3, 15) = 29.2$, $p < .001$, whereas the main effect of search condition was not significant, $F(1, 5) = 1.6$, $p = .26$.

When observers were searching for a large target among medium distractors, RTs were shortest when the distractor was small or medium, and RTs did not differ between the small and medium distractor conditions ($t < 1$). In comparison, mean RTs were slowed in the presence of a large distractor, which was the same size as the target: $t(5) = 3.3$, $p = .020$, for the small distractor and $t(5) = 4.9$, $p = .004$, for the medium distractor. However, when the distractor was extra-large, mean RTs were slowest—and significantly slower than in the presence of either a large distractor, $t(5) = 8.9$, $p < .001$, or a small or medium distractor: $t(5) = 8.1$, $p < .001$, for the small distractor and $t(5) = 15.8$, $p < .001$, for the medium distractor.

When observers searched for a medium target among large nontargets, mean RTs did not differ reliably between the small, medium, and large distractor conditions (all *t*s < 1; all *p*s > .41). Mean RTs were significantly elongated only in the presence of a small distractor, compared with all other distractor conditions: $t(5) = 4.4$, $p = .007$, for the medium distractor; $t(5) = 3.1$, $p = .027$, for the large distractor; and $t(5) = 5.4$, $p = .003$, for the extra-large distractor.

Errors. The 2 × 4 ANOVA computed over the mean errors did not reveal any significant main effects or interactions (all *p*s > .15; see Figure 7B).

Discussion

Experiment 3 shows that distractors that differ in the correct direction from all other items capture attention to a greater extent than distractors that are more similar to the target. These results support the relational account and are at odds with the standard views, which would have predicted that the similar distractor

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[1] Mean saccade latencies of saccades to the distractors were generally lower than mean saccade latencies of eye movements to the target; however, these differences were not always significant. Specifically, when the target was large, target saccade latencies in the large and extra-large distractor conditions were 25 ms and 57 ms longer, respectively, than the distractor saccade latencies, $t(5) = 2.1$, $p = .095$, and $t(5) = 3.2$, $p = .025$, respectively. When the target was medium, target saccade latencies were 23 ms longer than distractor saccade latencies in the small distractor condition, $t(5) = 1.8$, $p = .125$, and they were 33 ms longer in the presence of a medium distractor, $t(5) = 4.8$, $p = .005$.

should capture most (e.g., Duncan & Humphreys, 1989; Folk et al., 1992, 1993; Ludwig & Gilchrist, 2003).

The findings shed new light on the similarity effect by indicating that this effect may not result from feature-specific activation but from tuning attention toward the relational properties of the target. Thus, it is possible that in previous studies, the similar distractor captured attention not because it was featurally similar to the target but because it differed in the correct direction from the nontargets.

However, it should be noted that the results of Experiment 3 may still be compatible with dual process theories of visual search, which predict the attention-driving capacity of an item from its saliency and its similarity to the target (e.g., Navalpakkam & Itti, 2007; Wolfe, 1994). On these accounts, it might be argued that the small and extra-large distractors were more salient because they possessed unique feature values and this extra salience more than made up for the fact that they were less similar to the targets than the medium and large distractors.

Although it may be possible to account for the results in this way,[2] the explanation is slightly implausible because it implicates the bottom-up mechanism to explain stronger capture by relationally better distractors. Experiment 3 showed that, contrary to this, using virtually identical displays and changing only the task (or target definition) across blocks dramatically changed the result pattern (i.e., from no capture for small distractors and strongest capture for extra-large distractors to the reverse result pattern). These observations are more readily accounted for by changes in the top-down attentional control settings, which have to be adjusted to meet different task demands, than by changes in bottom-up saliency computations.

A better way to explain the present results on the basis of current feature-specification models of search would be to combine them with the relational idea, by claiming that observers in Experiment 3 voluntarily tuned attention away from the nontargets, that is, to channels that are located farther away from the nontarget feature. If one assumes that the feature value distributions of the target and nontarget features overlapped, then it might have been difficult to find the target by tuning attention toward the exact feature value of the target, because channels that respond maximally to the target feature would also have responded to the nontarget feature, leading to a poor signal-to-noise ratio. To enhance the signal-to-noise ratio, observers may have tuned attention to channels located farther away from the nontargets, which do not respond to the features of the nontargets anymore, thereby allowing a better target–nontarget discrimination. These channels would respond more strongly to the features of distractors that differ in the correct direction from the target, which would explain why these distractors captured attention more strongly than did distractors that were identical to the target (see Figure 8A for an illustration).

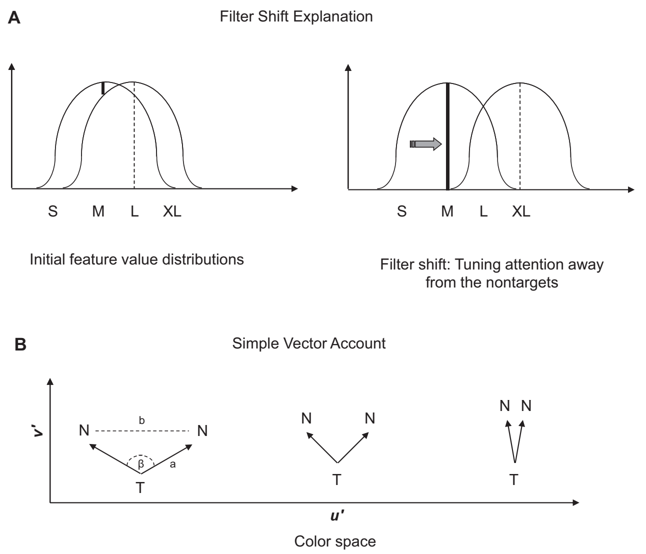

Figure 8. A: Illustration that a filter shift away from the medium nontargets might favor the XL distractor over the L distractor (and the L target). Vertical dotted lines display the peak activation for a given feature value, and vertical bold lines show that discrimination improves with the filter shift. B: Illustration that a simple vector account can account for differences in search efficiency, which are commonly predicted by the (dis)similarity between the target and the nontarget items (length of line *a*) and the dissimilarity between the nontargets (length of line *b*). The panels show that nontarget–nontarget dissimilarity can be replaced by the concept of angular differences between vectors (angle β). S = small; M = medium; L = large; XL = extra-large; N = nontarget; T = target.

Note that theories adopting such a "channel shift," or "filter shift," explanation already subscribe to the relational idea, because the position of the optimal channel is given by the direction in which the target differs from the nontargets. Thus, the filter shift explanation can serve as an example of how the relational idea could be implemented into existing, feature-based accounts. Although current models of visual search could profit from adopting the relational idea, the filter shift explanation cannot account for the earlier results of the present study and thus is not superior to the relational account (see especially Experiment 2, which ruled out a categorical or feature-based mechanism).

A second possible explanation of the results of Experiment 3 can be derived from the *linear separability* account (e.g., Bauer, Jolicoeur, & Cowan, 1996b; D'Zmura, 1991). It should be noted that the presence of a relationally better distractor in Experiment 3 (e.g., extra-large or small distractor) rendered the target nonlinearly separable from the irrelevant items, because it is impossible to draw a single boundary through size feature space such that the target is located on one side whereas all other irrelevant features are located on the other side. Targets that are not linearly separable often produce inefficient search (e.g., Bauer et al., 1996b, Bauer, Jolicoeur, & Cowan, 1998; D'Zmura, 1991; Hodsoll & Humphreys, 2001; Navalpakkam &

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[2] It can also be doubted that the visual search models are consistent with the findings of Experiment 3: Typically, in order to find the target efficiently, the models have to assign a large top-down weight to the target feature, emphasizing similarity over feature contrast in their equations. Moreover, the design of Experiment 3 places some unique restrictions on the computations of bottom-up feature contrast and top-down similarity, because a distractor will be less similar to the target the more salient it becomes, and vice versa. Thus, it is an open question whether the parameters of the models can be adjusted such that (a) the dissimilar distractor captures attention more than does the similar distractor, whereas (b) other dissimilar distractors that differ in the wrong direction from that of the nontargets do not capture attention at all and (c) the target can still be found nonrandomly.

Itti, 2006), with RTs increasing (approximately) linearly with the number of nontargets. By contrast, linearly separable targets often produce efficient search that is independent of the number of nontargets, indicating that attention can be guided more effectively toward linearly separable targets than toward nonlinearly separable targets.[3]

To explain this linear separability effect, it has been proposed that search for a nonlinearly separable target may be more inefficient because it is more difficult to tune attention toward a nonlinearly separable target (e.g., Hodsoll & Humphreys, 2001). For instance, the *similarity* theory has been modified accordingly, so that it now includes different rules of top-down weighting for linearly separable versus nonlinearly separable targets. This modified account predicts a higher attention-driving capacity for target-similar, linearly separable features than for target-similar, nonlinearly separable features (Hodsoll & Humphreys, 2001). In such a combined similarity plus linear separability view, stronger capture by the relationally better distractors could be due to the fact that (a) they rendered the target nonlinearly separable—which diminished the attention-driving capacity of the target—and (b) they were themselves linearly separable from the other items whereas they were still quite similar to the target feature (e.g., Hodsoll & Humphreys, 2001; Hodsoll, Humphreys, & Braithwaite, 2006).

Again, such a combined view is probably not the best explanation for the results, first, because it cannot account for the results of earlier experiments, in which the target was presented among only one type of nontarget feature and thus was always linearly separable.[4] Secondly, such a combined linear separability plus similarity view is less parsimonious than the relational account, because it has to rely on two variables and mechanisms to explain the results, whereas the relational account posits only a single principle of guidance.

The relational account could potentially provide a more parsimonious explanation of the linear separability effect as well, so that the similarity account could profit from adopting the relational idea. Note that the similarity view has not provided a reason why it should be more difficult to tune attention toward nonlinearly separable targets than toward linearly separable targets (see Hodsoll & Humphreys, 2001). However, from the perspective of the relational account, a nonlinearly separable target is fundamentally different from a linearly separable target, because a linearly separable target differs only in a single direction from that of the nontargets (e.g., smaller), and hence it is possible to select the target by tuning attention toward the relational properties of the target. In contrast, a nonlinearly separable target, such as a medium target among small and large nontargets, differs in two directly opposing directions from the nontargets (i.e., it is larger than half of the nontargets and smaller than the other half of the nontargets). In these conditions, the target cannot be found by tuning attention in to a single direction, because this would result in selection of one of the nontargets.

In Experiment 3, observers maintained a relational search strategy despite the fact that the relationally better distractor rendered the target nonlinearly separable on a portion of trials; this was presumably due to the fact that only a single distractor had to be rejected on each trial, allowing immediate selection of the target after inhibiting the irrelevant distractor. However, in the typical search efficiency paradigm, in which a nonlinearly separable target is always sandwiched between a larger number of different types of nontargets, attention presumably cannot be guided toward the target by applying a relational search setting, which alone can explain the drop in search efficiency for nonlinearly separable targets.

The idea that the relational account could help explain search efficiency is bolstered by the observation that the relational idea can be easily implemented in the similarity account. Note that differences in similarity (or feature contrast) between the target and nontargets can be represented by the length of vectors pointing from the target to the nontargets. Moreover, the feature contrast between the nontargets can be represented by the angular difference between these vectors. Thus, a relational account that assumes that search efficiency is determined by (a) the length of the vectors between the target and the nontargets and (b) the angular difference between the direction(s) of vectors pointing from the target to the distractors is virtually indistinguishable from the original similarity theory, which proposes that search efficiency is determined by (a) the similarity between the target and the nontargets and (b) the dissimilarity between the nontargets (Duncan & Humphreys, 1989). As shown in Figure 8B, the angular difference between the vectors β corresponds to the feature contrast between the nontargets, given by the length of line *b*, so that the angular difference β can be taken as an alternate measure for the feature contrast between the nontargets.

The only notable difference between such a vectorized account of search efficiency and the original similarity theory is that the vector account predicts double benefits at increases of the feature contrast between the target and nontargets, because this will also decrease the angular vector difference. However, results to this effect are usually interpreted as showing that target activation is more important for attentional guidance than nontarget rejection. With this provision, the combined similarity plus linear separability view will make exactly the same predictions as the vector account. Thus, the vector account appears to have the same explanatory power as the similarity theory, while it is simultaneously more parsimonious (because it can explain the linear separability

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[3] Previous research has shown that these differences also cannot be attributed to differences in feature contrasts, that is, to a diminished feature contrast between a nonlinearly separable target and the nontargets or to a higher feature contrast between the nontargets. As Bauer and colleagues (1996b) have shown, a nonlinearly separable target produces inefficient search even when it is quite dissimilar from the nontargets and the feature contrast should be sufficient to allow an efficient search (see Figure 17 in Bauer et al., 1996b; see also Bauer et al., 1996a, 1998; D'Zmura, 1991; Hodsoll & Humphreys, 2001; Wolfe & Bose, 1991).

[4] In Experiment 1, switch costs occurred regardless of whether the target and/or the nontargets were more or less similar to the target and nontargets in the previous displays, as shown by the analysis of individual displays (see Table 1). Thus, the combined similarity plus linear separability view cannot explain intertrial switch costs. This problem remains even when linear separability is assessed across different displays: In Experiment 2, the possible target features were never linearly separable from all possible nontarget features across different displays (see Figure 4, right panels). Despite this, switch costs were absent when the coarse relation between the target and nontargets remained intact, indicating that linear separability across displays also does not help explain intertrial switch costs (see Figure 4, bottom, and Figure 5; see also Becker, Folk, & Remington, 2009, Experiment 4).

effect without introducing special rules for linearly separable targets versus nonlinearly separable targets). Thus, it appears that similarity theory has much to gain and nothing to lose from adopting the relational idea.

General Discussion

Research on attention has commonly treated the search target as separate from its context: The visual system has been assumed to treat the features of the target and nontargets as separate entities, thereby achieving selectivity by activating the features of the target and inhibiting the feature(s) of the nontarget (e.g., Geyer et al., 2006; Kristjansson et al., 2002; Lamy et al., 2008; Maljkovic & Nakayama, 1994). The present study questioned this view and introduced a new relational account of attentional guidance, which proposes that features can be assessed relative to their context and that attention can then be guided by the relational properties of the target (e.g., larger, redder, darker).

The present study provided strong evidence for this novel, relational account by showing that a relational target template can guide attention in diverse search tasks. In particular, Experiments 1 and 2 showed that guidance by relational information determined attention shifts and eye movements when the target features varied randomly in the size, luminance, or color dimension, resulting in a specific pattern of intertrial switch costs: Switch costs occurred only when the coarse relationship between the target and nontargets reversed (e.g., from larger to smaller), whereas switch costs were absent when the relationship remained the same—despite the fact that both target and nontargets underwent large physical changes between trials (see Experiment 2). These results are at odds with a feature-specific explanation of intertrial priming effects, which attributes intertrial switch costs to intertrial carryover effects of target activation and/or nontarget inhibition. More importantly, the results indicate that a relational specification of the target guides attention even when the relational properties of the target are unknown, because they vary unpredictably. This shows that knowledge about the exact features of the target and nontargets is not necessary to prompt guidance by relational information.

On the other hand, knowledge about the exact target feature also does not prevent guidance by a relational target template: As shown by Experiment 3, guidance by relational information determined capture by an irrelevant distractor even when the features of the target and nontargets were known to the observers and remained constant. Again, the central finding—that capture depended on the relational properties of the irrelevant distractor and not on its similarity to the target—was predicted from the relational account but is difficult to explain on alternative, saliency-based or feature-based views.

In contrast, the relational idea seems to be well equipped to explain previous findings that were usually explained by saliency or feature-specific activation of the target. First, a relational search setting differs notably from a feature-specific search strategy insofar as it is more open-ended and will attribute the highest attention-driving capacity to stimuli located at the end of the continuous feature space. Incidentally, a stimulus located at the end of the continuum will also be one of the most salient items in the visual field (i.e., one of the items that have the largest mean feature contrast from the other items). Thus, the relational account is consistent with previous studies reporting capture by salient stimuli but offers an alternative interpretation for this observation (see also Becker, Folk, & Remington, 2009). Second, the relational account is also consistent with the finding that similar distractors capture attention much stronger than do dissimilar distractors. In previous studies, the similar distractor differed in the same direction from the nontargets as the target (e.g., both were red and presented among white nontargets; e.g., Folk et al., 1992; Ludwig & Gilchrist, 2002), whereas this was not the case with the dissimilar distractor. Stronger capture by the similar distractor could thus have been due to the fact that observers tuned attention to the relational properties of the target. In sum, the relational account holds promise for synthesizing a wide range of results that were previously attributed to a feature-based or a saliency-based selection mechanism.

The present experiments also appear to provide a strong test of the relational account, because tuning attention toward the relational properties of the target was not the ideal strategy in any of the experiments. In the pop-out search tasks of Experiments 1 and 2, the logically optimal strategy would have been to tune attention toward singletons (i.e., toward the unique feature, or toward the item with the highest feature contrast) because, across all trials, the target was reliably the item with the highest feature contrast in the display (in fact, the target was the only item in the display with any feature contrast). Thus, this strategy (singleton search mode; Bacon & Egeth, 1994) would have allowed immediate selection of the salient target on all trials without incurring any switch costs. Nevertheless, attention was apparently tuned toward the relational specification of the target, as revealed by the pattern of intertrial switch costs (see Experiments 1 and 2).

Moreover, in the feature search task of Experiment 3, observers apparently tuned attention toward the relational properties of the target, although a feature-based search strategy would have been the more optimal choice. Tuning attention toward the relational properties of the target was suboptimal, because it rendered observers vulnerable to distraction by two types of distractors (i.e., the similar distractor and the relationally best distractor), which were present in 50% of all trials. Tuning attention toward the specific target feature would have been more optimal, because this would have rendered all distractors ineffective, the only exception being the most similar distractor.

At a first glance, these observations suggest that a relational search strategy may constitute a default search setting, which is applied across a multitude of different search tasks and stimulus conditions regardless of its optimality. On the other hand, the design of the present experiments did not actively discourage a relational search setting. To encourage a feature-based search strategy more strongly, for instance, in priming, one could vary only the nontarget features across trials, for example by presenting a constantly medium target among distractors that are randomly either all large or all small. Such conditions would clearly favor a feature-based search strategy over a relational one, because the relationship always reverses on switch trials whereas the target feature always remains constant. Moreover, in the context of attentional capture, presenting a relationally better distractor on all trials (instead of only a portion of trials) should frustrate a relational search strategy and encourage a feature-based search mode. So far, none of these critical conditions have been tested. Hence, further research is needed to determine the contingencies that favor a relational search strategy over a feature-based search strategy, or a singleton detection mode. Moreover, further studies are necessary to determine whether these different search strategies constitute mutually exclusive search modes or whether information from some or all of these sources can combine in the guidance of attention.

The fact that attention can also be tuned to a relational specification of the target indicates that attention can be guided by information that is more flexible and abstract than was previously thought. However, it is clear that relational tuning also has its limitations. One foreseeable and rather interesting limitation is that attention is presumably preferentially tuned to a single direction and thus cannot be effectively tuned to the relational properties of a nonlinearly separable target. It has been argued that this limitation may be responsible for the linear separability effect, providing the reason for why it should be more difficult to tune attention toward nonlinearly separable targets (e.g., Hodsoll & Humphreys, 2001).

Moreover, it has been suggested that an account of search efficiency proposing feature-specific top-down tuning (e.g., similarity theory; Duncan & Humphreys, 1989) may be empirically indistinguishable from accounts proposing relational search settings (i.e., vector account of search efficiency). If this is correct, then other criteria (e.g., parsimony) and collateral evidence about the generality of relational settings are required to decide between relational and feature-specific explanations of search efficiency. However, even at the present stage, the relational account holds promise for synthesizing a range of results that were previously explained by different mechanisms of visual search and thus indicates a fruitful avenue for future research.

How the Factors Determining Search Efficiency Differ From Factors Determining Capture and Priming

It has been argued that relational search settings may also play a role in explaining differences in search efficiency and, specifically, linear separability effects (see also Becker, 2008a). However, it is important to note that linear separability effects are thought to result from the applicability versus inapplicability of a relational search strategy, which is notably different from the processes that presumably underlie similarity effects and priming effects. Whereas similarity effects and priming effects are thought to result from the observer's tendency to tune attention to a particular direction, linear separability effects do not result from the actual tuning behavior. In particular, tuning attention to the wrong direction should not lead to an inefficient search.

In inefficient search is characterized by a positive set size function (i.e., increases in RT with increases in the number of nontargets) and presumably reflects that observers are scanning serially through the items (e.g., Treisman & Gelade, 1980; Wolfe, 1994). In contrast, tuning attention to the wrong direction produces a misguidance effect, which results in erroneous selection of one of the nontargets prior to target selection, but this effect is independent of the set size and does not affect search efficiency or the set size function. Experiment 3 shows that the misguidance effect also occurs when only one distractor is present, demonstrating that the effect does not depend on the set size.[5] Moreover, misguidance effects did not result in inefficient search in any of the experiments: Upon erroneously selecting the irrelevant distractor or one of the nontargets (e.g., on switch trials), observers did not serially scan through the remaining items—a strategy that would result in a set size effect—but quickly shifted their eyes to the target (see also Becker, 2008b). This reflects that observers can quickly reconfigure the attentional control settings upon noticing an erroneous selection, which allows selection of the target. In Experiment 3, target selection was presumably achieved by inhibiting the location of the distractor, whereas in Experiments 1 and 2, observers may have quickly changed the direction in which attention was tuned (e.g., from larger to smaller), allowing immediate selection of the target.

This view is also consistent with previous studies showing that knowledge about the target color reliably shortens the baseline RTs by 100–300 ms, whereas it does not affect the set size effect much (e.g., Bauer, Jolicoeur, & Cowan, 1996a; D'Zmura, 1991; Hodsoll & Humphreys, 2001; Kristjansson et al., 2002). In a similar vein, studies on the feature priming effect have showed that, in efficient and inefficient searches alike, repeating the target or nontarget features across trials did not affect search efficiency much, but it led to large decreases in baseline RT (e.g., Becker & Horstmann, 2009; Geyer et al., 2006; Hodsoll & Humphreys, 2001; Kristjansson et al., 2002). These findings indicate that tuning attention to a particular direction will produce fixed costs across different set size conditions without affecting search efficiency, or the set size effect, to a large extent.

Importantly, the studies also indicate that a lack of target foreknowledge does not discourage observers from tuning attention to the properties of the target. Experiments 1 and 2 show that observers obviously tuned attention in a particular direction despite the fact that the target feature varied randomly, as shown by the pattern of intertrial switch costs. Moreover, Kristjansson and colleagues (2002) found that repeating the target and nontargets across three or more consecutive displays shortened baseline RTs to such an extent that they matched search performance in a condition in which the target and nontargets were known and remained constant.

In sum, linear separability presumably determines search efficiency not by influencing the actual tuning behavior of the observers but by defining the boundary conditions or preconditions for search. Linear separability determines whether a relational search setting is in principle applicable or not: If the target is linearly separable, it differs in a single direction from the nontargets, and hence, a relational setting can be applied successfully—whereas this is not the case with a nonlinearly separable target. The finding that search for a nonlinearly separable target tends to be inefficient suggests that a relational search setting renders the target more discriminable from the nontargets than does an alternate, feature-specific search strategy. On the basis of the filter shift explanation, it could be speculated that attention can be tuned away from the nontargets when the target is linearly separable, which would render the target more discriminable from the nontargets: For

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[5] Naturally, in Experiment 3, observers did not tune attention to a different direction upon erroneously selecting the relationally better distractor, because they knew that the target would differ in the same direction from the nontargets as the distractor. Presumably, upon erroneously selecting the distractor, observers simply inhibited the location of the distractor and immediately selected the target by applying the same search setting again.

instance, in search for an orange target among yellow and white nontargets, attention could be tuned toward the red channel, which would reduce activation from the yellow nontargets to a larger extent than could be achieved by tuning attention toward the orange channel. Because the discriminability of a nonlinearly separable target (e.g., an orange target among yellow and red nontargets) cannot be enhanced by relational tuning in a similar way, this could lead to decrements in search efficiency, especially when the feature distributions between target and nontargets overlap.

Admittedly, it is not yet clear whether this explanation is correct; however, it can serve as a good example of how linear separability and feature contrast can jointly define the boundary conditions for top-down tuning of attention. In this, the proposed explanation is akin to the combined similarity plus linear separability view, which also attributes an inefficient search to the fact that attention cannot be tuned as effectively to nonlinearly separable targets as to linearly separable targets.

Priming and Contingent Capture: Top-Down or Bottom-Up?

In the present study, both intertrial priming effects and capture by similar distractors are attributed to the way attention is tuned toward the (relational) properties of the target. However, this view may be criticized, because contingent capture is usually attributed to top-down controlled processes, whereas priming effects are usually regarded as bottom-up. Thus, the aspiration of the relational account to explain these effects within a single theoretical framework may seem inappropriate.

The view that feature priming effects are bottom-up is supported by studies showing that switch costs persist even when observers know the upcoming target's feature; for instance, when target and nontarget features are switched in a regular foreseeable sequence (e.g., Becker, 2008b; Hillstrom, 2000; Maljkovic & Nakayama, 1994). This was commonly taken to indicate that priming is based on automatic, stimulus-driven processes that are impenetrable to top-down knowledge (e.g., Maljkovic & Nakayama, 1994).

There are two possible replies: First, even if this strong bottom-up view on feature priming were correct, it remains possible that priming and capture by similar distractors are based on the same underlying processes. Although capture by target-similar distractors has usually been interpreted as resulting from top-down controlled processes, it is possible that capture was mediated by priming. In studies investigating capture, the target feature was usually presented repeatedly over a block of trials (e.g., the target was always red). Thus, it is possible that similar distractors captured attention because their features corresponded to the features that the target had on the previous trial (*priming-induced capture*; Becker, Ansorge, & Horstmann, 2009) and not because they were similar to a top-down target template (see also Kristjansson et al., 2002). If this view is correct, then it would be mandatory to explain priming and capture by similar distractors within a single framework—though, naturally, without invoking the notion of top-down tuning of attention.

Second, and more important, there is ample evidence that a pure bottom-up view on priming is untenable. An alternative view that is much better in line with the available evidence is that priming and capture both rely to some extent on automatic processes but are still contingent on top-down controlled processes (e.g., Becker, 2007). According to the *contingent capture hypothesis*, visual selection can be automated during repetitive search tasks so that, for instance, attention shifts can be triggered automatically by the onset of the search display. However, automatic selection is still governed by top-down controlled processes, which determine in advance, or "offline," the feature(s) that can serve as a trigger and as a target for attention shifts, so that the "automatic" selection process, which proceeds in a fast reactive mode, is still contingent on the top-down attentional control settings (e.g., Folk et al., 1992, 1993; Folk & Remington, 1998). The contingent capture view was originally developed to explain involuntary attentional capture and is widely accepted as an explanation of the similarity effect (e.g., Ludwig & Gilchrist, 2002; Yantis, 2000).

In a similar way, priming presumably reflects that attention can also be tuned to particular properties of the target in an automatic or reactive fashion. According to the *contingent priming hypothesis* (Becker, 2007), top-down controlled processes determine in advance the feature(s) that will be subject to priming, by selecting the kind of information that is then automatically transferred across trials (Becker, 2007; see also Müller, Heller, & Ziegler, 1995; Olivers & Humphreys, 2003). Selection and intertrial transfers, however, are supposed to occur automatically, in a reactive way,[6] resulting in an automatic or "reactive" tuning of attention toward the properties that the target had on the last trial (Becker, 2007, 2008b; see also Olivers & Humphreys, 2003).

Note that admitting that tuning can be automatic or reactive already implies that the underlying processes are to a large extent stimulus-driven. The primary goal of automating a process is to make a response contingent on a particular stimulus input, presumably to free up capacity for other tasks. Hence, it is somewhat trivial to emphasize the stimulus-driven nature and/or top-down impenetrability of corresponding effects: By definition, automatic behavior will exhibit no signs of active support from top-down controlled processes.

However, priming effects are not stimulus-driven in the more interesting sense that the stimuli determine which features in the display will be primed for attention. According to this view, we would expect feature-priming effects to depend on the stimulus characteristics, so that, for instance, priming of color should always be stronger than priming of shape or size. However, previous studies show that priming effects critically depend on the task: Regardless of the stimulus properties, priming effects are regularly stronger for the search-relevant feature than for other features of the target (e.g., response-defining features or task-irrelevant features; Becker, 2007; Becker, Ansorge, & Horstmann, 2009; Folk & Remington, 2008; Kristjansson et al., 2002; Olivers & Humphreys,

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[6] Instead of characterizing priming effects as relying on active processes such as intertrial transfers (e.g., priming of pop-out; Maljkovic & Nakayama, 1994) or an active retrieval of memory traces (e.g., Hillstrom, 2000), one can describe priming effects as the visual system's passive tendency to operate on the attentional control settings that led to successful target detection on the previous trial. In this case, switch costs would reflect that the attentional control settings had to be changed—either because of advance information that indicates a change in the task or target definition (e.g., Fecteau, 2007) or because the current input fails to meet the so far existing search settings (e.g., Becker, 2007; Becker, Ansorge, & Horstmann, 2009; see also Müller et al., 1995).

2003; Wolfe et al., 2003). This indicates that priming is contingent on top-down controlled processes that strictly distinguish between task-relevant and task-irrelevant features of the target.

Moreover, a recent study by Fecteau (2007) showed that feature-priming effects are also top-down penetrable, in the sense that advance information about the target can be sufficient to completely eliminate priming effects. Previous failures to obtain such an effect are probably rooted in the failure to make the advance information fully task-relevant. Studies showing that sequence knowledge cannot override or eliminate priming effects typically used displays in which the target was unequivocally defined and thus could be found without sequence knowledge (e.g., the target was always a color singleton; Maljkovic & Nakayama, 1994). By contrast, Fecteau (2007) used ambiguous displays that all contained a color and a shape singleton, and the target was defined by a word cue prior to each trial. When advance information was critical for search, priming effects occurred only when the target definition was repeated, and not when the target definition changed, compared with the previous trial. Thus, priming seems to be fully top-down penetrable in the sense that it can be neutralized by advance information about the target, provided that this information is critical for the task at hand.

Further research is necessary to explore why priming effects persist when advance information is not critical for the task. One possibility is that automating processes of selection may have such large advantages that it is not easily abandoned when the benefits consist only in preventing intertrial switch costs.

However, the evidence does not leave much room for doubt that priming is contingent on the goals and intentions of the observers and thus reflects *contingent automaticity* (Bargh, 1989, 1992). In sum, closer inspection of capture and priming effects reveals that none of these effects can be unanimously attributed to either top-down or bottom-up controlled processes. As Yantis (2000) has emphasized,

> Although the goal-directed and stimulus-driven aspects of attentional control are typically treated as separate and distinct, with most empirical studies focusing on only one of these factors, it has become increasingly clear that this distinction is untenable. Every episode of selection necessarily manifests both types of influence. (p. 75)

Here, it has been argued that this view is also correct with respect to individual acts of tuning of attention, which further calls into question the merits of classifying processes as top-down versus bottom-up (see also Becker, Ansorge, & Horstmann, 2009).

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