

Priming in visual search: Separating the effects of target repetition, distractor repetition and role-reversal

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Abstract

Recent studies have identified between-trial priming effects in visual search tasks, but often with constraints on the possible similarities or changes across successive trials, and usually with the main emphasis on effects of target repetition. Here we sought to obtain a more thorough characterization of between-trial priming effects in speeded visual search, where observers determined target presence or absence among a set of distractors. The results show that various separable priming effects have a major influence on visual search performance. Facilitation was evident when a target was repeated between-trials, but there was also strong priming due to repetition of distractor types, even between successive trials for which no target was presented on either trial. Search also proceeded faster if the same distractor types were repeated, even when the current target was different from the preceding target. We also investigated the possible impact of role-reversals for particular display items, from being a target on one trial to becoming a distractor on the next, and vice-versa. We find that such role-reversals substantially affect search performance, over and above the effects of repetition per se when those were held constant.

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1. Introduction

The importance of priming (or trial history) in various experimental visual tasks, and by extrapolation for visual perception more generally, is increasingly appreciated. For example, so-called “pop-out” visual search, which was once considered to operate in an efficient parallel manner, regardless of trial history, was shown by Maljkovic and Nakayama (1994, 1996) and by several others since (e.g. Goolsby & Suzuki, 2001; Hillstrom, 2000; Huang, Holcombe, & Pashler, 2004; Kristjánsson, 2006a; Maljkovic & Martini, 2005; Olivers & Meeter, 2006; Theeuwes, Reimann, & Mortier 2006) to be influenced by repetition

of particular properties (e.g. target color or location) across successive trials, in visual search tasks where a unique feature (e.g. singleton color) defined the target. Kristjánsson, Wang, and Nakayama (2002; see also Geyer, Müller, & Krummenacher, 2006; Hillstrom, 2000; Wang, Kristjánsson, & Nakayama, 2005; Wolfe, Horowitz, Kenner, Hyle, & Vasan, 2004) have more recently shown priming effects in a more difficult search task, where no single feature distinguished the target (a “conjunction” search task, see e.g. Egeth, Virzi, & Garbart, 1984; Treisman & Gelade, 1980; Wolfe, Cave, & Franzel, 1989). Such studies on priming in search have all found that a target which was searched for and found on a preceding trial will then typically be found faster than otherwise (i.e. faster than a non-repeated target) on the next trial.

A popular view of how visual search proceeds is that it reflects the combined influence of bottom-up processes reflecting local feature-contrasts or bottom-up salience

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(see e.g. Julesz, 1981; Koch & Ullman, 1985; Treisman & Gelade, 1980), together with top-down or strategic processes that selectively activate pathways appropriate for the task at hand (Duncan & Humphreys, 1989; Treisman & Sato, 1990; Wolfe, 1994). More recently, Kristjánsson et al. (2002) have argued that at least some putative top-down or strategic effects in visual search (such as prioritizing a blocked and therefore known target feature) might in fact reflect more passive repetition priming effects.

Although there have been several debates on what might be meant by terms such as ‘strategic’ or ‘top-down’, versus ‘stimulus-driven’ or ‘bottom-up’ (e.g. see Wolfe, Butcher, Lee, & Hyle, 2003, for one clear view), all we mean to highlight here is that if a particular factor or stimulus property is blocked, it can become hard to distinguish any effects of active anticipation from more passive trial-to-trial repetition effects, because a blocked factor repeats and is also expected to do so. Kristjánsson et al. (2002) instead used pseudorandom blocks with unpredictable target features, and found that just a few repetitions of the same search display, across unpredictable trials, can rapidly lead to performance approaching the same level as when a known target feature was repeated throughout a block. This result was further supported by the findings of Wang et al. (2005) and Wolfe et al. (2003, although Wolfe et al. favor a slightly different interpretation, see Section 5), indicating that the role of priming in visual search may be more extensive than previously thought. A fundamental point arising from such work is that not only do we tend to notice what we are actively seeking, but that we are also, other things being equal, more likely to notice again what we have seen in the recent past (see e.g. Kristjánsson, 2006b for review).

Of particular note in the study by Kristjánsson et al. (2002; see also Wang et al., 2005) was the considerable carry-over priming effect found even for trials on which no target was present (the ‘target-absent’ trials), thus providing some initial evidence that priming in visual search may also involve the distractor types—the rejected nontargets—not just repeated targets. In other words, if the distractor types were the same between-trials, search was faster than otherwise, irrespective of whether a target was present or not in the current display (see also Chun & Jiang, 1998, for work on an apparently similar but in fact substantially different phenomenon, contextual cueing, where the exact layout of a previous display may later be repeated, rather than just the type of target and/or type of nontargets, as we consider instead here).

1.1. The goals of the current study

The recent results of Kristjánsson et al. (2002) and Wang et al. (2005) raise several questions concerning priming in search. These studies showed that between-trial priming can arise even for ‘conjunction’ search, i.e. where a target, when present, was not unique in either its color or form, sharing these with one or other of two nontarget types; but was unique only in its specific combination of

these properties (see also Hillstrom, 2000; Theeuwes & Kooi, 1994; Wolfe et al., 2004). Their results also provided some initial evidence that priming might arise even for nontarget rejection. But those studies (Kristjánsson et al., 2002; Wang et al., 2005) had some limitations. One limitation was that possible priming from one target-absent trial to another was never isolated (as their analyses pooled over whether the preceding display, with same or different nontargets, had a target present or not). Likewise, no contrast assessed whether performance on a current target-present trial can benefit from being preceded by a target-absent trial with the same two types of distractor identities. One might expect this if repeating nontarget types can facilitate their rejection, and speed responses subsequently. But on the other hand one could perhaps expect the opposite, if instead those particular nontarget types become associated with a given response on one trial (e.g. “no”, for a target-absent trial), only to then be linked with the opposite response on the next trial (i.e. “yes”, for a subsequent target-present display that includes the same two types of nontarget).

One might also in principle assess whether repeating one but not the other type of nontarget can facilitate performance, or whether both must be repeated conjointly. Furthermore, if a target is repeated, does that always produce the maximal priming effect across two successive trials, or is there a further benefit if one or both distractor types also remain the same, as recent results by Geyer et al. (2006) suggest? As such questions illustrate, there are many possibilities to assess, and surprisingly few of these have been considered separately to date, which we attempt to rectify here with an in-detail study.

Another potentially important new issue that we sought to address here are possible effects of *role-reversals* between target and distractors across successive trials. This may represent a very special form of non-repetition. For example, across two consecutive target-present trials (when the target is defined as a unique conjunction of shape and color, among two nontarget types), if the next target differs, this could either be because the preceding target is no longer present, or instead because that particular item is now present multiple times to provide a current distractor (thus representing a potentially disruptive role-reversal, from preceding target to current nontarget). Likewise, a type of distractor on one trial could become a target on the next trial, which might then represent a special type of target-non-repeat trial. Role-reversals might represent a unique constraint, rather than merely reflecting changes in the current dimension that defines the target (cf. Krummenacher, Müller, & Heller, 2001; Müller, Heller, & Ziegler, 1995). We note also that the critical contrasts that are usually considered to isolate target-repetition versus non-repetition effects, as in the pioneering work of Maljkovic and Nakayama (1994, 1996) and others since, may in fact involve role-reversals in some cases. For instance, in Maljkovic and Nakayama, when target color was not repeated, the previous target color typically became the

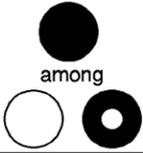
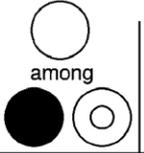
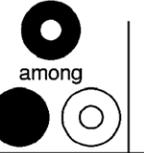
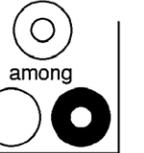
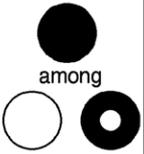
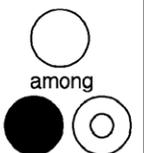
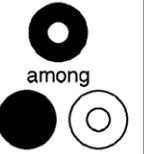
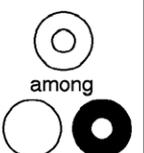
nontarget color in the next display. But the role of such role-reversals in particular have never been directly examined to our knowledge.

The general aim of the present research was thus to provide a more thorough characterization of possible priming effects in visual search than available in existing studies, by separating various distinct types of repetition and non-repetition, including for distractor types as well as separately for target types, plus any role-reversals.

To address all these points, we modified the experimental paradigm introduced by Wang et al. (2005) in several ways. In that study observers performed a visual search task where a target was defined in a context-dependent manner (i.e. with respect to the multiple nontargets in the current display), and always by conjunctions of features. Each display comprised multiple instances of two distractor types, and target-present trials had in addition one item that was unique in terms of its conjunction of color and

shape, with respect to the two distractor types. These two distractor types were always picked from a set of four possible display items, so as to differ from each other in both color and shape (e.g. black disk target among black “donuts” and white disks, see examples in Table 1). The target, when present, was defined as the “odd-one-out” with respect to the two types of multiple nontarget, in terms of its conjunction of shape and color (e.g. if the target was a white disk, the nontargets would be a white donut and a black disk). The specified task was to determine the presence or absence of such a conjunctive odd-one-out. Wang et al.’s (2005) rationale was that the target in such a task cannot be distinguished by any single feature (only by its conjunction of features), while any top-down guidance or expectancy should also be minimized (since any of the four possible types of item could in principle be the target on any given trial). Traditional theories of visual search that emphasize either pop-out of unique features,

Table 1
The priming benefits from adjacent target-present trials in Wang et al. 2005^a

		Target-present trials preceded by target-absent trials			
		N-1			
					
N		-45.04	+21.54	+18.29	-17.95
		+21.86	-47.65	-22.31	+28.71
		+33.48	-20.04	-34.89	+20.96
		-13.49	+41.26	+35.61	-48.32

^a The columns under the heading $N - 1$ denote the identity of targets and distractors on the preceding trial, while the rows under the N heading denote the target and distractors on the current trial. Negative numbers mean that performance on that type of trial (denoted in the rows) was faster than the grand mean for target trials preceded by another target trial, if it was preceded by the search denoted in the columns. Positive numbers mean that the search was slower than the grand mean.

and/or guidance to anticipated target properties, might therefore predict rather inefficient search. But in fact, Wang et al. (2005) found that search under such conditions was efficient for a number of different types of stimuli. They suggested that perceptual grouping might contribute to this. They also showed that repeating targets or distractor types led to faster performance in this paradigm, performance which rapidly approached what would be expected if the target was the same on all trials and could thus be anticipated.

To summarize, firstly we sought to investigate whether, and how, priming from a given target-present trial may affect performance not only for a subsequent target trial, but also for a subsequent target-absent trial (e.g. if the non-target types were repeated); and likewise to examine any possible priming effects from a target-absent trial, upon subsequent target-absent or target-present trials. In this way, we could identify any influence from repeating non-target types, not just effects of target repetition. Secondly, we wanted to isolate possible effects of role-reversals, from target to distractor or vice-versa, across successive trials, that have not been thoroughly investigated in prior research on priming in visual search to our knowledge, nor differentiated from target and or nontarget (non)repetition.

2. Re-analysis of data from Wang et al. (2005)

Before commencing our new experimentation, we first provide a further analysis of priming patterns for the existing data of Experiment 2 in Wang et al. (2005; specifically the “topography” part of that experiment, data shown in the leftmost panel of Fig. 3 from that paper). Those data had not previously been analyzed with respect to our new questions regarding priming in visual search. Given our new questions, a re-analysis along those lines was appropriate. As will be seen, this re-analysis confirmed several new patterns that were then replicated and extended with the entirely new experiment here.

2.1. Brief overview of methods from Experiment 2 in Wang et al. (2005)

Their observers performed a visual search task where any one of four targets (a black or white disk, or a black or white ‘donut’; see Table 1) could be the target. Target presence (and identity if present) was determined randomly from one trial to the next. The two types of distractors on each trial were picked such that when a target was present, no single feature (shape or brightness, i.e. black or white) could distinguish it from the current distractors, with the target being unique only in its conjunction of shape and color with respect to the two types of nontarget presented. So, for example, if the target was a white disk, the only possible distractors were a black disk and a white donut. The instructions to the observers were to indicate by key-press whether an odd-one-out target was present or not among

the two sets of distractors. For full details of the methods in this previous experiment, see Wang et al. (2005, p. 242).

2.2. Results for the new re-analysis of Wang et al. (2005, Experiment 2)

The effects of each possible type of target-present trial upon a subsequent target-present trial are summarized in Table 1, for data drawn from all successive pairs of a target-present trial being followed by another (i.e. for 25% of all the data from the topology part of Wang et al.’s Experiment 2). The top row in Table 1 shows cartoons of the target-present display type for the preceding trial ($N - 1$). The left column shows cartoons of the target-present display type for the current trial (N). The numbers given in each cell of the table represent mean differences in RT for that particular pairing of trial $N - 1$ and trial N displays, with respect to the grand mean of all target-present trials that were preceded by a target-present trial. Thus, negative values indicate facilitation (faster RTs), while positive values indicate slower mean RTs in this particular situation than for the grand mean. There is a substantial benefit to repeating the same target-present trial (see consistent negative values in top-left to bottom-right diagonal). In addition, repeating the distractor types speeds the search, even when the target identity changes (see top-right to bottom-left diagonal of Fig. 1). Note, however, that the effect of repeating the target (top-left to bottom-right diagonal) is to some extent confounded with the effect of repeating distractors, within the Wang et al. (2005) design, since the target on each trial uniquely determines the distractors in that design. It is thus not possible to completely disentangle all possible effects of distractor repetition and target repetition in Wang et al.’s design (and likewise for most other previously published designs also, but see Geyer et al., 2006), thus providing a further rationale for the new experimentation presented subsequently here.

Table 2 summarizes changes in search time when target-present trials are preceded by target-absent trials, with different types of relation between the successive displays. Again the values shown are mean differences in RT for the second display in specific successive pairings, with respect to the grand mean for all target-present trials that had been preceded by a target-absent trial. These values suggest that repeating distractor types from a preceding target-absent trial can facilitate search on a subsequent target-present trial (see the four negative values in Table 2). Table 3 suggests that the converse benefit may also apply; repeating distractor types from a preceding target-present trial may facilitate performance on a subsequent target-absent trial (see the four negative values in Table 3).

Finally, Table 4 indicates that search performance can also benefit when distractor types are repeated across successive target-absent trials (see the two negative values, and compare these with the two positive values in Table 4). This suggests that priming-related facilitations in search

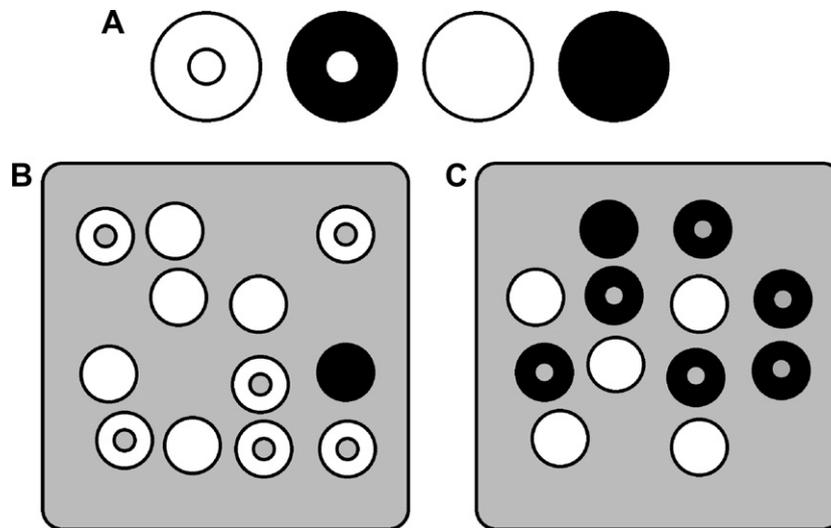


Fig. 1. The four different stimulus types used in the experiment (shown in (A)). (B) An example of a feature search, and a conjunction search is depicted in (C). An “odd-one-out” target is present in both cases, namely the black disk. The figure shows displays of set size 12. Two set sizes were used in the experiment, 12 and 24.

are not solely restricted to repetitions of target types, but can also reflect repetition of distractor types. Note that this issue was never explicitly addressed in Kristjánsson et al. (2002), nor in Wang et al. (2005; see also Geyer et al., 2006 for discussion of this issue).

Paired-*t* contrasts (with individual differences as the source of error) confirmed that all of the priming effects presented in Tables 1–4 were significantly different from zero. These analyses consider separately target-present displays preceded by target-present; target-absent preceded by target-present, and so on. Thus repeating a target across successive target-present trials leads to significant speeding ($t_{(5)} = 18.32, p < .001$; see Table 1); likewise repeating distractors from a preceding target-absent trial in a current target-present trial speeded performance ($t_{(5)} = 13.07, p < .001$; see Table 2); repeating distractors from a preceding target-present trial facilitated a subsequent target-absent trial ($t_{(5)} = 14.98, p = .001$; see Table 3); and finally repeating distractor sets between successive target-absent trials also significantly improved response time ($t_{(5)} = 16.17, p < .001$; see Table 4).

To summarize, our re-analysis of the results from Wang et al.’s (2005) Experiment 2 indicates that repetition of distractor types can enhance search performance, in addition to target repetition. Moreover, these priming benefits can extend from one trial type to another (i.e. from target-present trials to target-absence, and vice-versa, when repeating nontargets), not only between the same trial types as might have applied if just a response-association was responsible (which was a possible a priori, prior to re-analyzing the data in this way). But in the course of implementing this re-analysis, and in raising our new questions, we have also identified a few limitations in the designs of prior studies (including the Wang et al., 2005, Experiment 2 which we re-analyzed), in particular some constraints as mentioned above, that we attempt to rectify in the new experiment

that follows. In essence, we performed an expanded version of the Wang et al. (2005) design, allowing a more thorough decomposition of the potential priming effects. As will be explained, this required feature search displays and conjunction search displays to be intermingled, to minimize the constraints that the presence of one type of item in the display would place on the possible presence of others. For all displays, the observers had to judge whether or not there was an odd-one-out item in the display (i.e. a target-present trial) or not (i.e. target-absent trial).

3. New experiment on target repetition, distractor type repetition, and role-reversal effects in visual search

3.1. Methods

Fig. 1A shows the four types of stimuli used in the new experiment, along with a sample display showing two of the possible combinations of the stimuli (a feature search for the singleton black disk in Fig 1B, and a conjunction search for the same black disk in Fig 1C). All four types of stimuli (see Fig 1A) could be the target on any trial, and any two of the three other stimulus types then made up the two distractor types for that trial. Thus, the many mutual constraints that applied to Experiment 2 of Wang et al., 2005, no longer apply. The only constraint now was that only two types of nontarget appeared in any single display, and that any target had to differ from the two nontarget types, either in a single feature, or as a conjunction of shape and color. All possible combinations of target and distractors were thus now tested and the likelihood for any of the display items to take either role (target or distractor) was always comparable. The search type for each trial was selected completely randomly for each trial. This means that there were now 18 possible search displays (disregarding stimulus location, which varied randomly

Table 2
The priming benefits from target-present trials preceded by target-absent trials in Wang et al. 2005^a

		Target-present trials preceded by target-absent trial	
		N-1	
N	among	-20.87	+6.48
	among	+3.85	-17.68
	among	+6.31	-22.39
	among	-18.86	+13.24

^a The columns under the heading $N - 1$ denote the identity of the distractors on the preceding trial, while the rows under the N heading denote the target and distractors on the current trial. Negative numbers mean that performance on that type of trial (denoted in the rows) was faster than the grand mean for target trials preceded by a blank trial, if it was preceded by the search denoted in the columns. Positive numbers mean that the search was slower than the grand mean.

across 64 possibilities in an 8×8 grid, as described later), comprising 12 varieties of target-present trials and six varieties of target-absent trials. On 60% of trials a target was present, while 40% of the trials were target-absent trials. This slight probability asymmetry was used to increase slightly the number of target-present trials under each combination of nontarget types, to obtain a more complete dataset (since there were 12 possible variations on target-present trials, but only six for the blank trials, as shown in Fig. 2). Note that this 60/40 ratio of target-present to target-absent trials was held constant across all of the critical comparisons made here, so should not affect those results.

By now allowing all possible combinations of stimuli to produce target-present or target-absent trials, the unique target item could be distinguished by a singleton feature (e.g. a black disk among white disks and white donuts) or by a conjunction (e.g. a black disk among white disks and black donuts). Given the large number of possible

combinations of (unconstrained) pairs of successive trial types, a large number of trials was needed to obtain reliable results. The approach we took was therefore to extensively test the performance of two experienced psychophysical observers. One was naïve regarding the critical experimental questions, the other was an experimenter (A.K.).

3.1.1. Apparatus and stimuli

Stimuli were presented on an LCD monitor (75 Hz refresh rate) controlled by a G4 Apple Macintosh computer. Stimulus presentation was controlled with custom software prepared in C, utilizing the VisionShell programming library (see <http://www.visionshell.com>). The same software was used for response time and accuracy measurements. The stimuli were presented on an imaginary 8×8 square grid centered around a central white (54.9 cd/m^2) fixation cross. The size of each square in the invisible grid was $1.9^\circ \times 1.9^\circ$ (at a viewing distance of 60 cm). Each stimulus item (disk or donut) had a diameter of 0.9° and its position was jittered randomly by 0.4° from the centre of the square to produce some layout irregularity. On any trial the display contained 12 or 24 items. A target was present on 60% of the trials (see above). An equal number of distractors from each of the two randomly chosen distractor sets was presented on the target-absent trials (i.e. 6 or 12 of each, since set size was 12 or 24), while on the target-present trials the odd-one-out target replaced one randomly chosen distractor.

Observers were asked to fixate the central cross at trial onset, and respond by key-press as fast as possible to indicate whether a target was present or not, while also trying to be as accurate as possible. The target was defined as the “odd-one-out”; in other words, the task was to find a single unique item among the distractors, or to respond that none was present. The display was visible until the subject responded, after which the screen returned to the empty grey background with a central fixation cross. Auditory feedback was given as to whether the response was correct or not, via ‘beeps’ following the response. After 1200–1700 ms (randomly determined for each trial), the next trial started with presentation of the subsequent search display. Trials on which the response was incorrect were not included in the response time analyses that follow.

As shown in Fig. 1A there were four possible stimulus identities in this experiment, a black (0.7 cd m^{-2}) “donut”, a white (54.9 cd m^{-2}) donut, a black disk and a white disk, each subtending 0.9° of visual angles. These display items were presented on a grey (33 cd m^{-2}) background. As mentioned above, every possible combination of stimulus types was possible on each trial. Thus, unlike many prior visual search studies, the observers *never* knew the identity of the possible target on the subsequent trial (from among the possible stimulus set). Fig. 2 presents all the possible trial types for a given trial N , that could follow one particular target-present trial type (illustrated with one particular example for trial $N - 1$).

Table 3
The priming benefits from target-absent trials preceded by target-present trials in Wang et al. 2005^a

		Target-absent trials preceded by target-present trials			
		N-1			
		among	among	among	among
N		-23.65	+15.47	+13.29	-18.46
		+1.89	-27.36*	-23.21*	+8.74

^a The columns under the heading $N - 1$ denote the identity of the target and distractors on the preceding trial, while the rows under the N heading denote the distractors on the current trial. Negative numbers mean that performance on that type of trial (denoted in the rows) was faster than the grand mean for blank trials preceded by a target trial, if it was preceded by the search denoted in the columns. Positive numbers mean that the search was slower than the grand mean.

Table 4
The priming benefits from adjacent target-absent trials in Wang et al. 2005^a

		Target-absent trials preceded by target-absent trials	
		N-1	
N		-33.32	+30.17
		+19.97	-20.47

^a The columns under the heading $N - 1$ denote the identity of the distractors on the preceding trial, while the rows under the N heading denote the distractors on the current trial. Negative numbers mean that performance on that type of trial (denoted in the rows) was faster than the grand mean for blank trials preceded by another blank trial, if it was preceded by the search denoted in the columns. Positive numbers mean that the search was slower than this grand mean.

3.1.2. Observers

The two observers were A.M.H., a well practiced participant in psychophysical experiments, and A.K., the first author, with extensive experience of psychophysical experiments. Observer A.M.H. was not aware of the purpose of the experiment. Both observers participated in 10,000 trials each, spread over 100 blocks of 100 trials performed over a period of several days.

4. Results

4.1. Basic search performance

The overall results are presented in Fig. 3 for the search types conventionally classified as feature searches, and those conventionally classified as conjunction searches. The overall pattern shows, as expected, that the feature searches are faster than the conjunction searches, and also that responses on target-present trials are faster than on target-absent trials, again as usually found. This overall difference in difficulty between feature and conjunction searches is a typical result seen in many visual search studies before (see e.g. Egeth et al., 1984; Kristjánsson et al., 2002; Treisman & Gelade, 1980; Wolfe et al., 1989; see e.g. Wolfe, 1998 for an extensive review).

More interestingly, both feature and conjunction searches led to ‘parallel’ search performance, in the sense

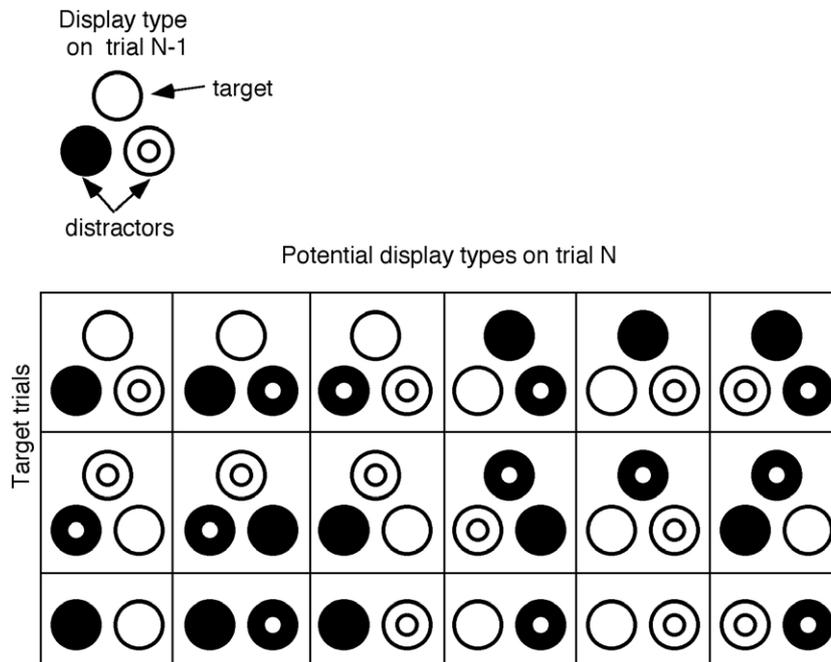


Fig. 2. The complete set of possible trial types (16) that could follow the preceding example trial type ($N - 1$) presented above the table. Note that this display only represents the possibilities for trial N following that particular display for trial $N - 1$. There were always 16 possible display types on any given trial (and thus 15 alternatives to the example shown here for $N - 1$).

that response times were not significantly slower for the larger set size (24 items in each display) than the smaller (12 items in display). For the target-present case, this result is similar to that reported by Wang et al. (2005) for their similar conjunction displays (see also Bravo & Nakayama, 1992; Nakayama, Kristjánsson, & Wang, 2000; Theeuwes & Kooi, 1994), but note that in the Wang et al. study there was a slight positive slope for the target-absent trials in the conjunction search, which was not the case here.¹ Wang et al. (2005) conjectured that efficient grouping processes (as may operate in particular for items of strong opposite contrast polarity, such as the black-on-grey and white-on-grey items used here; see Theeuwes & Kooi, 1994) may underlie such “efficient conjunction search”, in the absence of any unique feature, and when top-down guidance or strategic prioritization cannot aid performance as target identity is unpredictable (see also Duncan & Humphreys, 1989; Friedman-Hill & Wolfe, 1995; Treisman, 1982).

It should be noted that there was a large “intercept” difference between the feature and conjunction searches, i.e. a

¹ It is unclear what the reason for this apparent slight discrepancy is, but one possibility is that in the present study, relatively practiced observers participated, and they participated in a very large amount of trials. By contrast, participants in the experiments in Wang et al. (2005, see also Nakayama et al., 2000), were unpracticed naïve observers. Note, however, that in that study, for its Experiment 4, where naïve observers performed a go/no-go task on the same search displays (they were now only to respond if a target was present or only respond if the target was absent, in separate blocks) the target-absent slopes for response times against set size became close to, or even less than zero.

substantial overall increase in response times for the conjunction searches over the feature searches, that did not vary with the set size, indicating that judgments were harder in the conjunction search. But this difference in difficulty was *additive* to set size, rather than interacting with it, as statistically confirmed below.

A 3-way ANOVA with *search-type* (feature or conjunction search), *set size* (12 or 24) and *trial type* (present or absent) as factors revealed a significant main effect of search type within each observer, using trials to provide the random error term (for A.M.H.: $F_{(1,9809)} = 1590.71$, $p < .001$; A.K.: $F_{(1,9609)} = 836.00$, $p < .001$); as well as a significant main effect for both observers of whether the target was present or not (A.M.H.: $F_{(1,9817)} = 403.33$, $p < .001$; A.K.: $F_{(1,9609)} = 990.9$, $p < .001$). The main effect of set size, was, however, not significant for either observer (A.M.H.: $F_{(1,9817)} = 2.697$, $p = .101$; A.K.: $F_{(1,9609)} = 1,597$; $p = .21$).

The only significant interaction term (α -level = .05) was between search type and target presence or absence (A.M.H.: $F_{(1,9817)} = 25.20$, $p < .001$; A.K.: $F_{(1,9609)} = 21.48$, $p < .001$), as the overall difference in response times between absent and present responses was higher in the conjunction search than in the feature search.

4.1.1. Error rates

The overall error rate for A.K. was 3.83%, while for A.M.H. the overall error rate was 1.83%. Table 5 presents error rates broken down by search type, response type (present versus absent) and set size. These error rates do not indicate that there was any speed/accuracy trade-off, since the error rates for the feature searches were lower

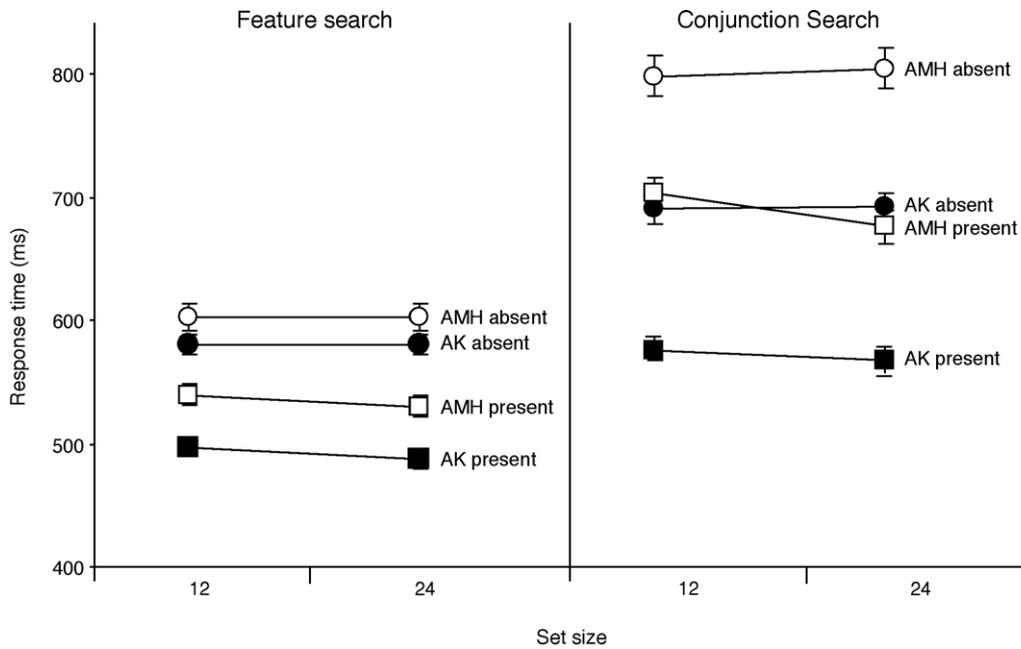


Fig. 3. The overall results for the two subjects (A.K. in black, A.M.H. in white), separated by whether the search in each case would traditionally be termed a feature search, or a conjunction search, and by whether the target was present (squares) or absent (circles). The error bars are the 95% confidence intervals; in some cases shorter than the symbols in the graph and thus not visible (recall that there were 10,000 trials in total per subject).

Table 5
Error rates (percent) from the experiment for the two observers as a function of search type, trial type (target present or absent) and the set size

	Set size	Feature search		Conjunction search	
		A.K.	A.M.H.	A.K.	A.M.H.
Target Trials	12	2.3	1.3	4.5	2.5
	24	3.8	1.9	4.3	2.1
Blank Trials	12	3.2	1.1	3.8	2.7
	24	3.4	1.3	4.9	2.0

than for the conjunction searches. The error rates tended to co-vary positively with the response times. The pattern of error rates is not much different from other visual search studies, where a higher number of errors is usually observed for the target-absent trials, even when target-absent displays comprise 50% of the trials (unlike the 40% of trials here). Finally, there was no significant evidence for more errors with the higher set size, again suggesting relatively efficient and parallel search, even for the ‘conjunction’ searches here.

4.2. Priming effects

As we emphasized before, the main purpose of this experiment was to obtain a more thorough characterization of priming effects in visual search than previously available. Since performance for the two different set sizes was very similar (see above), we pooled the two set sizes when investigating the priming patterns, to increase the power of these statistical analyses. We also pooled over feature and conjunction searches, as results were comparable for these.

We assessed differences in response times in relation to (non)repetition or role-reversals, separately for the four possible combinations of successive trials (in terms of target presence or absence): (1) target trials preceded by target trials; (2) target trials preceded by absent trials; (3) absent trials preceded by target trials, and (4) absent trials preceded by absent trials. We performed ANOVA analyses to test the significance of effects on response times from the different possible repetitions between-trials.

4.2.1. Target trials preceded by target trials

When a target trial is preceded by another target trial, there are a number of different possibilities for repetition. First, the target can be the same or different. Second, both distractor types may be the same as on the prior trial, or one of them may be different, or both can be different. Third, it is potentially important to look at any role-reversals (not considered by prior visual search studies, to our knowledge), since the target on the previous trial may become a distractor on the current trial, and similarly a distractor on a preceding trial could change its role to become a target on the current trial.

The basic effect of target repetition is shown in the left panel of Fig. 4 for the two observers. It is clear that repeating the target across successive trials has a large facilitatory effect on response times. This is as expected in light of various other studies showing target-repetition priming in visual search (e.g. Found & Müller, 1996; Goolsby & Suzuki, 2001; Hillstrom, 2000; Kristjánsson, 2006a; Maljkovic & Martini, 2005; Maljkovic & Nakayama 1994, 1996; Meeter & Olivers, 2006; Theeuwes et al., 2006; Wang et al., 2005; Wolfe et al., 2004, to name a few). F-contrasts

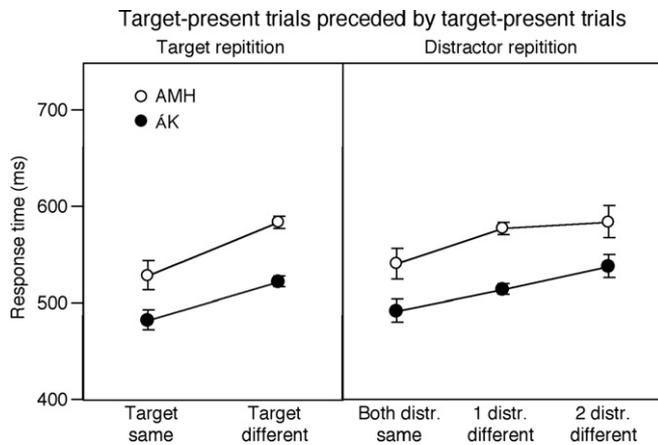


Fig. 4. The effects of target repetition and distractor repetition, for target-present trials preceded by another target-present trial, upon response times for the two observers. The graph shows performance as a function of whether particular types (target or distractors) of display items on the preceding trial were the same or different. The left panel shows the effect of target repetition, while the panel on the right shows response times as a function of how many of the two distractor types changed between-trials (zero, one or two). The error bars show the 95% confidence intervals. The different confidence intervals can reflect differences in the number of trials behind each mean score, not just increased variance per se.

for each observer showed that the benefit from repeating the target (versus non-repeat) was highly significant (with individual trials again providing the source of error; A.K.: $F_{(1,3535)} = 46.41$, $p < .001$; A.M.H.: $F_{(1,3543)} = 47.66$, $p < .001$).

The effect of repeating distractors between two successive target-present trials is shown in the right panel of Fig. 4. There is clearly a beneficial effect on response times from repeating nontargets (see also Geyer et al. 2006; Kristjánsson et al. 2002). This reinforces the conclusion that it is not simply target identity that can be primed from one trial to the next, but that repeating distractor types also has a substantial effect on performance. Note that what seems to be primed between successive trials here are relatively abstract properties of the whole display, since the *location* of any distractor was always determined randomly for any given trial here; thus, it was distractor *types* that could repeat, rather than a particular display layout (cf. Chun & Jiang, 1998). Univariate ANOVAs for each observer (again with trials providing the error term) revealed that the main effect of distractor repetition was significant (A.M.H.: $F_{(2,3604)} = 8.67$, $p = .005$; A.K.: $F_{(2,3599)} = 13.2$, $p = .008$). Post-hoc tests (at the α -level of .05) showed that the difference between repetition of 1 and 0 distractor types was only significant for A.K., but the differences between repeating two types versus none, or two types versus 1 were significant for both A.K. and A.M.H.

To address any interaction between the two factors (target or distractor repetition), a further 2 (target repeated or not) \times 2 (two distractor types repeated, or one) ANOVA was performed for each observer. The reason why there

were now only two levels for the distractor repetition effect in this ANOVA analysis was that including the case where both distractor types are changed would have resulted in a blank cell in such a 2 \times 3 ANOVA, since it is not possible to have trials where the target identity is the same as on the previous trial but both distractors are changed, with the stimuli we used, which comprised only four different possible display items. The interaction between the two factors was not significant for either observer (A.M.H.: $F_{(1,2945)} = 1.75$, $p = .186$; A.K.: $F_{(1,2939)} = 0.015$, $p = .902$) indicating that the effect of target repetition is independent of whether two or one distractor types were repeated, and vice-versa.

4.2.2. Role-reversals for target trials preceded by target trials

Not only can target- and/or distractor-identity change or repeat between-trials; it is also possible that a particular display item changes its role from one trial to the next. For example, a search display where the odd-one-out is a black donut among white donuts and black disks, can be followed by a trial on which one of these distractor types becomes the target, and the previous target becomes a distractor type (e.g. with the target now being a white donut among black disks and black donuts).

The foregoing analyses did not take into account any specific effects of role-reversals (which have likewise not been considered in prior visual search studies to our knowledge). Accordingly we next looked at role-reversals in a separate analysis that now held any target and/or nontarget (non)repetition constant when seeking any 'pure' effects of role-reversal that could not be attributed to standard repetition effects. The purest measures for this are provided by those successive trials where the target changed identity but only one distractor type changed (which provide, for instance, ~56% of trials with a target-present followed another target-present trial, on which the present section focuses). This situation can arise with or without a target to distractor reversal; and, orthogonally, with or without a distractor to target reversal. Such contrasts thus ensure that any target and distractor (non)repetition is held constant when assessing possible role-reversal effects.

Fig. 5 shows the effect of a display item undergoing a role-reversal from being a target on the preceding trial to a distractor on the current one; or separately the effect of a role-reversal from distractor to target. Both types of role-reversal can have effects upon response times, as shown by F-contrast results for A.K. (cost of role-reversal from target to distractor: $F_{(1,1998)} = 12.52$, $p = .001$; for role-reversal from distractor to target: $F_{(1,1998)} = 22.3$, $p < .001$; no interaction: $F_{(1,1998)} = 1.11$, n.s.). For A.M.H., the target to distractor role-reversal effect was significant ($F_{(1,2065)} = 3.39$, $p < .05$), as was the distractor to target role-reversal effect ($F_{(1,2065)} = 17.69$, $p < .001$), again with no significant interaction between the two effects ($F_{(1,2065)} = 1.98$, n.s.)

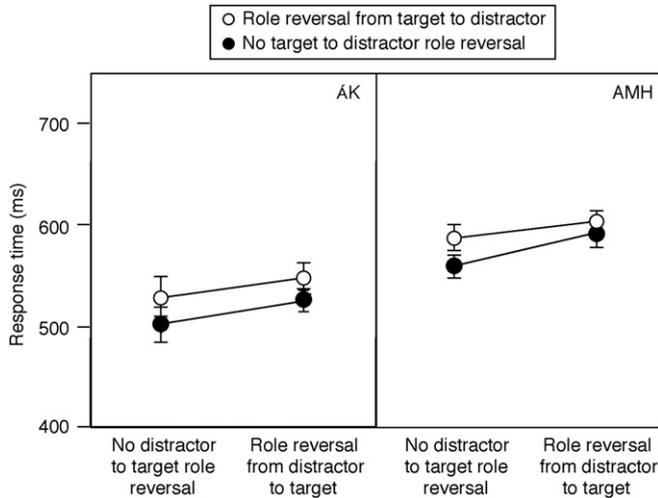


Fig. 5. The effects of role-reversals from a former target to a current distractor, or vice-versa, for target-present trials preceded by another target-present trial, upon response times for the two observers. In all of the analyzed trials, one distractor type changed and so did the target, hence target and distractor (non)repetition is held constant. The error bars show the 95% confidence intervals.

4.2.3. Target-present trials preceded by target-absent trials

The effects of repeating distractors, for target-present trials preceded by target-absent trials, are shown in Fig. 6. Performance was much faster if the distractor types remained constant, than if they changed. *F*-tests performed separately for each subject confirmed that the main effect of distractor repetition was significant (A.M.H.: $F_{(2,2376)} = 9.713, p < .001$; A.K.: $F_{(2,2320)} = 6.491, p = .002$). Post-hoc tests also revealed that for both observers the difference between repetition of two versus one distractor types, and one versus zero distractor types were each significant (α -level of .05) for both observers. This provides particularly clear evidence for Kristjánsson et al.’s (2002) conjecture

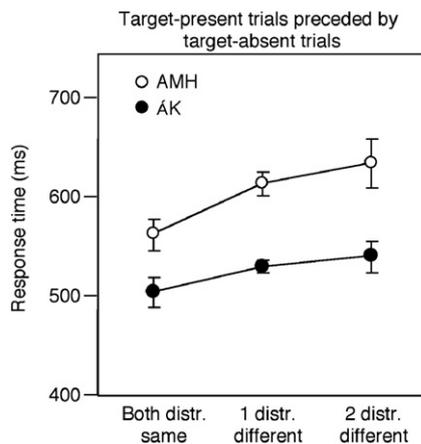


Fig. 6. The effects of distractor repetition, for target-present trials preceded by target-absent trials, upon response times for the two observers. The graph shows performance as a function of whether the distractors were the same as on the preceding trial, or whether one or both of the distractor types changed. The error bars show the 95% confidence intervals.

that priming between visual search trials can be based on abstract properties of the whole display (as here, for the nontarget types), not just on target repetition (see also Geyer et al., 2006), and that this can even occur for repetition of only one distractor type out of the two.

4.2.4. Role-reversals for target trials preceded by target-absent trials

When a target trial is preceded by a target-absent trial in the paradigm used here, a distractor type from the preceding trial can change its role to become the target on the next. Fig. 7 shows the effect of such a role-reversal for both observers, by necessity on trials where only one distractor changes identity between-trials (to hold nontarget repetition constant when assessing role-reversals, see also Section 4.2.2). There is a large difference in the search times depending on whether there is a distractor-to-target role-reversal or not, and this effect was significant for both observers (A.M.H.: $F_{(1,1601)} = 24.7, p < .001$; A.K.: $F_{(1,1540)} = 21.8, p < .001$). This means that having a current target which was previously a nontarget leads to slower performance than otherwise, potentially an example of active inhibition or ‘negative priming’ when a previous nontarget becomes a target (cf. Tipper, 1985, 1992).

4.2.5. Target-absent trials preceded by target-present trials

The impact of distractor repetition for target-absent trials preceded by a target trial are shown in Fig. 8. Even though there is some trend for a benefit of repeating distractors, this effect did not reach full significance for either observer (repeated measures ANOVA; A.M.H.: $F_{(2,2309)} = 2.091, p = .124$, or $p = 0.06$ one-tailed; A.K.: $F_{(2,2233)} = 1.435, p = .157$, or $p = 0.07$ one-tailed), possibly because of the changed response (or because of more uncertainty on target-absent trials, since the distractor repetition effects

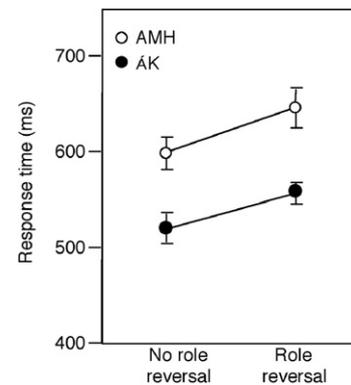


Fig. 7. The effects of role-reversals from distractor to target, for target-present trials preceded by target-absent trials, in the two observers. In all of the analyzed trials, one distractor type changed and the target did not repeat (obviously, since absent in the preceding trial), hence target and distractor (non)repetition is held constant. The graph shows performance as a function of whether one of the distractor types on the preceding trial became a target on the next. The error bars show the 95% confidence intervals.

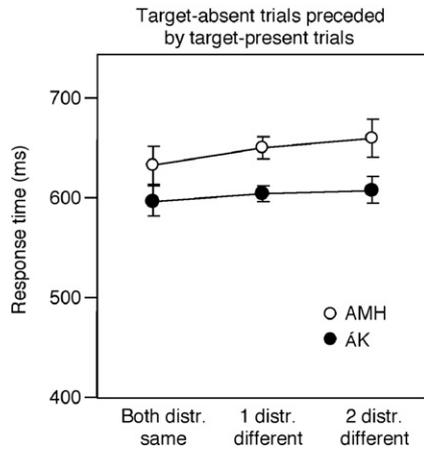


Fig. 8. The effects of distractor repetition, for target-absent trials preceded by a target-present trial, upon response times for the two observers. The graph shows performance as a function of whether the distractors were the same as on the preceding trial, or whether one or both of the distractor types changed.

were highly significant for target-present trials preceded by target-absent, see above, which had also required a change in response across the two successive trials).

4.2.6. Role-reversals for target-absent trials preceded by target-present trials

Again in order to isolate pure role-reversal effects uncontaminated by other aspects of repetition we focused on trials where one distractor always changed identity between-trials (see also Sections 4.2.2 and 4.2.4 above). Role-reversals from target to distractor induced an RT cost (see Fig. 9) which was individually significant for observer A.M.H. ($F_{(1,1540)} = 4.2, p < .05$), as well as for A.K. ($F_{(1,1474)} = 3.93, p \leq .05$).

4.2.7. Target-absent trials preceded by target-absent trials

The effects of distractor repetition for target-absent trials preceded by another target-absent trial are shown in

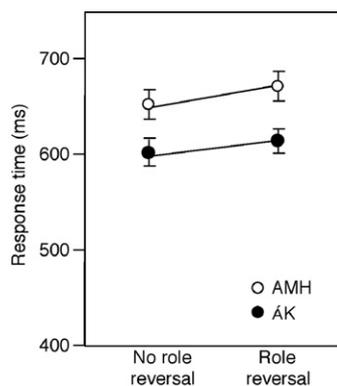


Fig. 9. The effects of role-reversals from target to distractor, for target-absent trials preceded by target-present trials, in the two observers. The error bars show the 95% confidence intervals. In all of the analyzed trials, one distractor type changed and the target did not repeat (obviously, since absent in the second trial in each successive pair here), hence target and distractor (non)repetition is held constant across the role-reversal comparison.

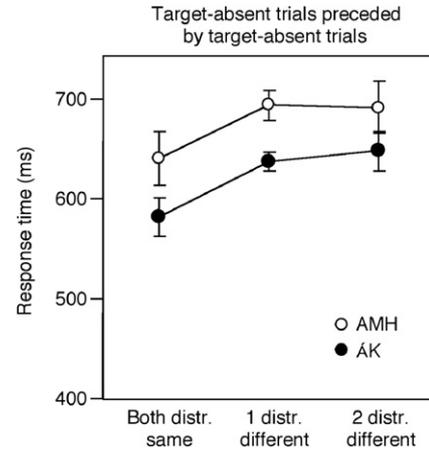


Fig. 10. The effects of distractor repetition, for target-absent trials preceded by a target-absent trial, upon response times in the two observers. The graph shows performance as a function of whether the distractors were the same as on the preceding trial, or whether one or both of the distractor types changed. The error bars show the 95% confidence intervals.

Fig. 10. In line with our previous findings there was a substantial advantage when repeating distractor types from one trial to the next, now even between two successive target-absent trials. This main effect was significant for each observer (A.M.H.: $F_{(2,1573)} = 6.253, p = .002$; A.K.: $F_{(2,1517)} = 16.156, p < .001$). Post-hoc tests revealed that only the differences between repetition of both distractor types, versus 1 or 2 distractor types changing, was significant (as clearly seen in Fig. 10), while the difference between changing 1 or 2 distractor types was not significant for either observer. This result suggests that repeating both distractor types is particularly important for improving performance when successive target-absent trials are considered. Note that role-reversals were not possible for two successive target-absent displays, as any repeated item would always be a distractor.

4.3. Error rates as a function of the repetition of target or distractor types

Table 6 shows error rates as a function of repetition (broken down as a function of what sort of trial pair is involved), revealing no evidence for speed/accuracy trade-offs that might undermine interpretation of the many clear RT effects reported above.

5. General discussion

There has recently been increasing interest in possible priming influences upon visual search (e.g. Found & Müller, 1996; Goolsby & Suzuki, 2001; Hillstrom, 2000; Kristjánsson, 2006a; Kristjánsson, Vuilleumier, Malhotra, Husain, & Driver 2005; Kristjánsson, Vuilleumier, Schwartz, Macaluso, & Driver 2007; Lamy, Carmel, Egeth, & Leber, 2006; Maljkovic & Martini, 2005; Meeter &

Table 6
Error rates (percent) as a function of priming patterns in the experiment

	TT ^a					TA			AA			AT		
	TSD		DSD			DSD			DSD			DSD		
	S	D	S	1D	BD									
A.K.	2.9	3.9	4.1	3.5	3.9	4.4	2.7	3.8	4.2	4.0	3.8	4.3	3.9	3.7
A.M.H.	2.6	1.9	1.1	1.5	2.3	0.9	1.1	3.5	2.5	2.9	1.3	1.3	1.6	1.5

^a Abbreviations: TT, target-present trial preceded by target-present trial; TA, target-present trial preceded by target-absent trial; AA, target-absent trial preceded by target-absent trial; BT, target-absent trial preceded by target-present trial; TSD, target same or different; DSD, distractors same or different; S, target or distractors the same; D, target or distractor different; 1D, 1 distractor different; BD, both distractors different.

Olivers, 2006; Olivers & Meeter, 2006; Theeuwes et al., 2006; Wolfe et al., 2004), ever since the pioneering studies of Maljkovic and Nakayama (1994, 1996) and Treisman (1992). More recently, Kristjánsson et al. (2002) and Wang et al. (2005) had emphasized (see also Kristjánsson, 2006b) that priming influences might be more pervasive than previously thought, and might even contribute to effects that might previously have been considered to reflect strategic considerations (such as prioritizing specific target features that can be expected when target type is blocked, but that also then repeat across successive trials, due to such blocking). Moreover, Kristjánsson et al. (2002) also speculated that nontarget repetition may facilitate search, not just target repetition as also indicated indirectly by Wang et al. (2005) and subsequently by Geyer et al. (2006).

Here we provided clear new evidence that nontarget repetition can indeed facilitate search, and we also highlight new influences from ‘role-reversals’ (i.e. when a preceding nontarget unpredictably becomes a nontarget on the next trial, or vice-versa). We first provided preliminary evidence for such possibilities from a careful re-analysis of existing data from Wang et al. (2005, Experiment 2), which had not previously been considered from this perspective. We then ran a new experiment, in which the possible roles of four different stimulus types (black or white donuts and disks) were entirely unconstrained, such that any of these could serve as target or nontargets on each trial in an entirely unpredictable order.

We found that conjunction search was efficient with flat slopes for reaction time against set size in this new paradigm (at least for practiced psychophysical observers, as here), albeit leading to slower responses than for feature search overall. In accord with much prior research, we found that unpredictable target repetition across successive trials led to faster search. A more novel finding here was that repeating *nontarget* types (rather than their exact locations, which were always randomized here) facilitated judgments, not only for target-absent displays followed by another target-absent display (where the repeated nontargets would be associated with the same response), but also for target-absent followed by target-present displays (where there was still a benefit when repeating nontarget types, despite the change in required response); and for target-present followed by target-present trials. This indepen-

dence of priming effects from repetition or non-repetition of response is consistent with the findings of Sigurdardottir, Kristjánsson, and Driver (in press) who recently showed, using signal-detection methods with brief (~200 ms) displays that priming of visual search can be independent of response repetition. The impact of nontarget repetition was less clear for target-present followed by target-absent trials, possibly due to the increased uncertainty associated with responses to the latter. Despite this one ‘wrinkle’, our study provides unequivocal new evidence that repeating nontarget types can benefit search in many cases, even when nontarget identity is not predictive for the next trial, and does not overly restrict which particular type of target might be the ‘odd-one-out’ if such a target is present.

We have also demonstrated, for the first time to our knowledge, the importance of possible ‘role-reversals’ from one trial to a next, whereby a particular type of item might be a target on one trial but a distractor on the next, or vice-versa. Such role-reversals would often have arisen in Maljkovic and Nakayama (1994, 1996) and in similar paradigms since, when (say) the color of their target changed, but the possible impact of role-reversals was not explicitly considered for those studies, nor for others since to our knowledge. Here we isolated role-reversal effects that could not be attributed to other aspects of target or distractor (non)repetition, because we held those aspects constant for the critical comparisons in Figs. 5, 7, and 9. Observers were additionally slowed in responding to target items that had served as one type of nontarget in the previous trial, possibly reflecting some form of ‘inhibition’ of nontargets that were rejected when searching the previous trial. This might conceivably represent a new version of the ‘negative priming’ effect (see Tipper, 1985, 1992), applying to visual search in this case. But note that we also found some costs when a previous target became a nontarget on the next trial. Our analyses showed that these effects of role-reversals were separable from the effects of target or nontarget repetition per se.

These new results thus accord with but go well beyond Kristjánsson et al.’s (2002) proposal that priming effects may be more pervasive than previously thought in visual search. Our new experiment also resolved or circumvented several of the potential constraints of the design in Wang et al. (2005), whose data we had re-analyzed. Previous

studies, such as Egeth (1977), Wolfe et al. (2003) or Müller et al. (1995), had already indicated that intermixing different visual search tasks unpredictably can typically lead to slower performance than for a constant search task. While such effects had sometimes been attributed to anticipatory strategic effects or ‘prioritization’ of particular features or dimensions, the present study shows that various types of repetition, or of constant roles rather than role-reversals, can have substantial impacts on search performance even when nontarget and target types are unpredictable from one trial to the next (within the stimulus set used).

5.1. *The nature of priming in search*

We think of the present priming effects as reflecting altered representational states for a given feature or feature combination, that can result in facilitated (or impaired) processing for that property (see e.g. Kristjánsson, 2008). Priming in search can be impervious to prior knowledge or expectations about what should come next (as shown convincingly by Maljkovic & Nakayama, 1994, 2000; see also Kristjánsson & Nakayama, 2003), and arose reliably here even when there were no systematic contingencies between successive trials whatsoever. It is also important to point out that such priming is, most likely distinct from traditional conceptions of perceptual learning (Ahissar & Hochstein, 2004; Doshier & Lu, 1999; Goldstone, 1998; Karni & Sagi, 1993; Sireteanu & Rettenbach, 2000). The effects here arose on a short-term trial-to-trial basis, and were not tied to a particular location in the visual field, unlike many perceptual-learning phenomena. Wolfe et al. (2003, p. 483) considered priming in visual search as implicit top-down guidance, since some form of “knowledge” of recent searches is arguably required, even though this may be implicit. They stated (their p. 483): “Because it relies on what the observer has learned about the trials and does not rely solely on the state of the stimulus, we consider this to be a form of implicit guidance.” Deciding whether to use terminology such as ‘guidance’ or ‘top-down’ is to some extent a semantic issue, but we think that in future it may be possible to resolve in neural terms whether a top-down influence (in the sense of a back-projection influence) is required to explain the present priming effects, rather than bottom-up activation due to the preceding display leaving some trace within the visual system that may then influence bottom-up processing of the next display. Recently we have demonstrated that some forms of priming in visual search (as for the phenomena identified by Maljkovic & Nakayama, 1994, 1996) can remain intact in patients with damage to higher-level brain regions in parietal and frontal cortex (Kristjánsson et al., 2005; Sævarsson, Jóelsdóttir, Hjaltason, & Kristjánsson, 2008). More recently, we have also shown how fMRI can now be used to study the neural correlates of visual search priming non-invasively in the human brain (Geng et al., 2006; Kristjánsson et al., 2007; see also Bichot & Schall, 2002,

for related invasive studies in behaving monkeys). Findings from neurophysiology and neuropsychology have indicated that priming may arise at several sites in the brain (Campana, Cowey, & Walsh, 2006; Kristjánsson et al., 2005, 2007; see also discussion in Campana, Pavan, & Casco 2008; Kristjánsson, 2008), including the attentional systems (see e.g. Corbetta & Shulman, 2002; Ruff, Kristjánsson, & Driver, 2007) as well as in regions in early visual areas. It should be of considerable interest to adapt such methods, to study the new forms of priming identified for the first time here. It should also be of interest in future research to examine whether such priming effects may differ qualitatively for different types of research. Here, preliminary analyses (not reported in full for brevity) indicated similar priming effects for our feature and conjunctive searches, but this was in very practiced observers who produced flat search-slopes for both. It could be interesting to test if more serial searches exhibit disproportionately larger benefits and costs from target and nontarget (non)repetition, and from role-reversals, though clearly floor and ceiling effects would need to be avoided when assessing this. Our present priming and role-reversal results were also very similar for the first and second halves of our new experiment, but this again might reflect our use of very practiced observers, so it is not inconceivable that some learning effects might become apparent in individuals undergoing fewer trials. But, once again, we emphasize that no trial-to-trial contingencies applied here, so the present effects should not reflect learned expectancies concerning repetition or role-reversal, since no expectancies could be learned except that any succession of trial types was possible.

6. Conclusions

Target repetition has recently been acknowledged as an important factor in visual search. Here we show that repetition of nontarget types also matters, as do role-reversals from targets to nontargets and vice-versa, across successive trials during search. Recent history clearly has important influences on visual search, affecting how rapidly we will notice both featural and conjunctive targets, even when that history is nonpredictive, as for the random sequence of display types here.

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