Running head: SEARCH EFFICIENCY AS A FUNCTION OF TARGET SALIENCY

Search efficiency as a function of target saliency: The transition from inefficient to efficient search and beyond

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Abstract

Searching for an object among distracting objects is a common daily task. These searches differ in efficiency. Some are so difficult that each object must be inspected in turn, whereas others are so easy that the target object directly catches the observer's eye. In four experiments, the difficulty of searching for an orientation-defined target was parametrically manipulated between blocks of trials via the target-distractor orientation contrast. We observed a smooth transition from inefficient to efficient search with increasing orientation contrast. When contrast was high, search slopes were flat (indicating *pop-out*); when contrast was low, slopes were steep (indicating *serial search*). At the transition from inefficient to efficient search, search slopes were flat for target-present trials and steep for target-absent trials within the same orientation-contrast block – suggesting that participants adapted their behavior on target-absent trials to the most difficult, rather than the average, target-present trials of each block. Furthermore, even when search slopes were flat, indicative of pop-out, search continued to become faster with increasing contrast. These observations provide several new constraints for models of visual search and indicate that differences between search tasks that were traditionally considered qualitative in nature might actually be due to purely quantitative differences in target discriminability.

Keywords: visual search; feature search; slope ratio; search efficiency; feature contrast

Visual search is one of the most influential paradigms for examining the workings of visual attention. Participants in visual-search experiments have to find a target object defined by a specific feature or feature combination among distracting objects. Often, participants' task is to determine whether this target object is present or absent (target-present and -absent trials, respectively). Research in this area has focused mainly on the search time per item, that is, the slope of the function relating search time to the number of objects in the display. This interest was instigated by findings that the search slope is flat when the target is defined by a single, basic visual feature (feature search; e.g., a red target among green distractors), but steep when the target is defined by a feature combination (conjunction search; e.g., a red horizontal target among green horizontal and red vertical distractors). Furthermore, when, in the latter case, slopes were calculated separately for search displays not containing versus displays containing a target, the slopes turned out to be roughly twice as steep for target-absent compared to -present trials. The flat/steep slope dichotomy and the 2:1 absent-to-present slope ratio in conditions with steep slopes led to the proposal of two search modes. For some search arrays, the target can be discerned based on (spatially) parallel processing of all objects. In this mode, the number of searched objects has no influence on either the target-present or the -absent search slopes. With other arrays, parallel processing cannot support discerning the presence and location of a target; instead, the display must be searched in serial mode, that is, focal attention must be allocated to individual objects in turn to determine whether or not a selected object is a target. On target-present trials, search can terminate as soon as the target is found, which would happen, on average, after about half the objects have been inspected. On target-absent trials, by contrast, all objects must be rejected as potential targets before an 'absent' response can be issued, yielding target-absent search slopes twice as steep as target-present slopes (Treisman & Gelade, 1980).

It soon turned out that the distinction between these two qualitatively different search modes was an oversimplification: instead of a bimodal distribution of search slopes, corresponding to serial and parallel search, a continuous gradation of slopes, from flat to steep, was observed across experiments (e.g., Duncan & Humphreys, 1989; Wolfe, 1998b). This was taken to indicate that there are not two categorically different search modes, but rather a continuum of search difficulties. Consequently, search performance since then has often been described in terms of the proximity of observed slopes to the poles of this continuum as “efficient” or “inefficient”, instead of “parallel” or “serial”, respectively (Duncan & Humphreys, 1989; Wolfe, 1994). A typical indicator for efficient search is slopes less than 5 ms per item (e.g., Treisman & Souther, 1985; Wolfe, 1998a).

Guided Search, the currently most popular theory of visual search, explains search as a two-stage process (e.g., Wolfe, 1994, 2007). In a first, pre-attentive stage, all objects in the display are processed in parallel. This pre-attentive stage serves the purpose of constructing a saliency map, that is, a topographically organized map that represents the conspicuity or *saliency* of each object in the display, thus providing pointers to likely target candidates. In a second stage, objects are inspected individually in decreasing order of saliency, that is, serial, focal-attentional scanning starts at the most salient object, which is the most promising target candidate (Wolfe, 1994, 2007). Within this framework, Wolfe (1998b) interpreted the observed variance in slopes among search tasks as indicating that the various tasks differ in the degree of *guidance* the respective search displays afford, that is, the degree to which the target stands out at the saliency map level. The more guidance a display provides, the fewer the number of objects that must be inspected serially before the target is found. In the most extreme case, search displays provide so much guidance that the target is always selected first; phenomenally, the target then *pops out* of the display (see also Chun & Wolfe, 1996; Wolfe & Horowitz, 2004). In a search display that provides only little or no guidance, by contrast, usually many objects are inspected before the target is found. Other theories make similar predictions concerning the influence of target saliency on search performance (Duncan & Humphreys, 1989; Eckstein, Thomas, Palmer, & Shimozaki, 2000; Swensson & Judy, 1981; Moran, Zehetleitner, Liesefeld, Müller, & Usher, 2015; Verghese, 2001).[1]

The range of search slopes documented in Wolfe (1998b) can be taken to indicate that there is a continuous dimension (guidance) along which all visual search tasks vary, which yields the differences in search slopes. However, the roughly 2,500 individual data sets that Wolfe re-examined stem from many different experiments that differed in many respects, including major differences in stimuli and search type (e.g., feature search, conjunction search, spatial-configuration search). Therefore, the differences in guidance were probably driven by multiple differences between the search displays and it remains unclear which specific characteristics of the displays had an influence. In the present study, we directly examined one plausible candidate characteristic, namely, target-distractor discriminability (see Wolfe & Horowitz, 2004, for a similar conjecture), with discriminability being manipulated by the difference in orientation (i.e., one basic feature dimension) between targets and distractors.

Many studies have shown that absolute search times decrease with a parametric increase in target-distractor feature contrast, indicating that target saliency depends on the target discriminability (e.g., orientation: Arun, 2012; Töllner, Zehetleitner, Gramann, & Müller, 2011; color: Nagy & Cone, 1996; Nagy & Sanchez, 1990, Exp. 2, Nagy, Sanchez, Hughes, 1990; Weidner, Krummenacher, Reimann, Müller, & Fink, 2009). An effect on saliency should translate into an effect on guidance, which is why variations along the feature-contrast continuum should directly influence the patterns of search slopes. Unfortunately, however, the studies cited above did not include set-size manipulations (i.e., set size was fixed), so they provide no information as to effects of their parametric contrast manipulations on search slopes.

Another set of studies included set-size manipulations and showed that feature search (where the target is defined by a single basic visual feature) either produces flat (efficient) or steep (inefficient) search slopes, depending on the target-distractor feature contrast. This was shown for several feature dimensions as, for example, color (e.g., Nagy & Sanchez, 1990, Exp. 1; Nothdurft, 1993), orientation (Nothdurft, 1993; Wolfe, Klempen, & Shulman, 1999, Exp. 5), motion (Nothdurft, 1993), line length, luminance, curvature, gap size (Treisman & Gormican, 1988, Exp. 1, 2, 4, and 11, respectively), and shape (Roper, Cosman, & Vecera, 2013). Unfortunately, though, all these studies used only two or three levels of feature contrast, limiting examination of the (hypothesized) parametric effect of feature contrast on search slopes. An exception is Wolfe et al. (1999, Experiments 1-2), who used several feature contrasts and two set sizes. While the search slopes did indeed differ as a function of contrast, none of the search slopes was actually flat in Wolfe et al.'s experimental conditions – that is, the whole range of sampled feature contrasts produced inefficient searches. At the other end of the efficiency spectrum, several control studies from our lab (Goschky, Koch, Müller, & Zehetleitner, 2014, Footnote 3; Töllner et al., 2011, p. 3; Zehetleitner, Hegenloh, & Müller, 2011, Footnote 2; Zehetleitner, Koch, Goschky, & Müller, 2013, Supplementary Material; Zehetleitner, Krummenacher, Geyer, Hegenloh, & Müller, 2011, Appendix; Zehetleitner, Krummenacher, & Müller, 2009, Appendix B; Zehetleitner & Müller, 2010, p. 6; Zehetleitner, Proulx, & Müller, 2009, p. 1776) used at least two levels of feature contrast and two set sizes, but all were in the efficient range, thus again limiting the examination of parametric effects of feature contrast on search slopes.

Thus, somewhat surprisingly, there is a shortage of studies that feature a parametric manipulation of feature contrast crossed with a set-size manipulation, and, to the best of our knowledge, no direct evidence exists for a continuous transition from inefficient to efficient search as a function of an experimentally controlled independent variable. Such a transition should become evident in a gradual decrease in the search slopes (search time per item) with increasing feature contrast: search should be slow when targets and distractors are similar (low contrasts) and fast (flat slopes) for high target-distractor contrasts. Such a pattern would constitute a direct proof that feature contrast is a characteristic of search displays that directly influences guidance, with guidance being a truly continuous factor.

Although effects on guidance would manifest themselves in an effect on search slopes, other cognitive mechanisms involved in visual search might also be influenced by a manipulation of target saliency via feature contrast. Sole reliance on analyses of search slopes would then miss important aspects of the data (see, e.g., Müller, Humphreys & Donnelly, 1994). In addition to its slope, the function relating search time to the number of objects in the display is characterized by its intercept. A speed-up in the time required to examine the target object, for example, would influence the intercept, but not the slope (because there is always only one target, independent of the number of distractors). In line with this hypothesis, the above-mentioned control studies (Goschky et al., 2014; Töllner et al., 2011; Zehetleitner et al., 2011; Zehetleitner et al., 2013; Zehetleitner et al., 2011; Zehetleitner et al., 2009; Zehetleitner et al., 2009; Zehetleitner & Müller, 2010) suggest a range of feature contrasts for which the slopes are flat, but the intercepts continue to decrease with increasing contrast; restated, search time continues to decrease well beyond the point where search becomes efficient. How effects on the intercept parameters are related to the transition from efficient to inefficient search (the leveling-off of search slopes at zero), however, has never been systematically explored, and the above-mentioned studies examined only target-present intercepts, while a similar effect might also occur on target-absent trials. If it did, it could then, of course, not be attributed to the processing of a target object.

Here, we report four visual-search experiments with parametric manipulations of target-distractor orientation contrast as well as set-size manipulations, designed to fill these gaps in the literature. In particular, we manipulated the orientation of a target object presented among vertically oriented distractor objects and examined the effect of increasing target-distractor differences in orientation (feature contrast manipulation) on search slopes and intercepts on target-present and -absent trials. To anticipate our results, as expected, we found a smooth transition from inefficient to efficient search as a function of orientation contrast: Whereas search slopes were steep for low contrasts, they were flat for high contrasts with a continuous transition in between the extremes. Even for the range with flat slopes, the search intercept for target-present displays further decreased monotonically as a function of feature contrast. Strikingly, this intercept speed-up pattern generalized to target-absent displays as well. Finally, our experiments yielded a non-anticipated finding: an intermediate range of contrasts where search appears to be efficient for target-presents trials but inefficient for target-absent trials. As discussed below, these findings bear important implications with respect to search mechanisms, in particular, with respect to influences of feature contrast on guidance and item-identification times and with respect to the decision to quit search when no target is found. Accordingly, these findings provide important constraints for theories of visual search.

Experiments 1a (3°, 4°, and 5°) and 1b (6°, 8°, 10°, 12°, 22°, and 45°)

In Experiments 1a and 1b, we covered a large range of feature contrasts, so as to gain a broad overview of the effect of feature contrast on search slopes and intercepts. In order to keep testing sessions at a reasonable duration, we examined a set of low contrasts with one group of participants (Experiment 1a) and a set of high contrasts with another group (Experiment 1b). As we expected lower feature contrasts to give rise to longer reaction times (RTs) and, thus, require more time overall to solve a fixed number of trials, we sampled fewer contrasts in the low-contrast (Experiment 1a) than in the high-contrast group (Experiment 1b).

Methods

Participants. Sixteen university students participated in Experiment 1a (median age: 25 years, range: 19-30 years; 12 female) and 16 additional students participated in Experiment 1b (median age: 23 years, range: 19-34 years; 11 female). All participants reported normal or corrected-to-normal vision and gave informed consent. Analyses on overall RTs did not indicate any 'outlying' participants. Here, and in the subsequent experiments, outliers are defined as values 1.5 interquartile differences above the third or below the first quartile of the respective empirical distribution (Tukey, 1977).

Stimuli and design. Stimulus presentation and response collection was controlled by a Matlab (The Mathworks) program, using functions from the Psychophysics Toolbox (Brainard, 1997). Search displays (Figure 1) were presented on a CRT monitor (1024 × 768 pixels, 120 Hz), at a viewing distance of 70 cm. Search arrays consisted of either 19 or 37 gray bars (1.35° × 0.25° in size) presented on a black background (Figure 1). Bars were arranged on two or three (imaginary) concentric circles around a central bar (radii of 2.1°, 4.2°, and 6.3°). On half of the trials, all bars were vertically oriented (target-absent trials). On the remaining half, one bar—the target—was tilted to either the left or the right (target-present trials). Possible target tilts were 3°, 4°, or 5° for Experiment 1a and 6°, 8°, 10°, 12°, 22°, and 45° for Experiment 1b. These target tilts constitute our main manipulation of interest (target-distractor orientation contrast). Orientation-contrast conditions were blocked, with the order of blocks randomized for each participant. Each block started with 30 training trials, with the target (if present) always appearing in the center of the search array, permitting participants to become acquainted with a given orientation contrast before actual data collection. On the following 144 experimental trials per block, the target (if present) could appear anywhere in the array except for the central position and the outer ring.

The concentric stimulus arrangement served to maintain a high and constant display density. This was important, because the distance between target and surrounding distractors is known to influence the target's discriminability (Nothdurft, 2000). The only purpose of the outer ring was thus to render this local target-distractor contrast comparable for all possible target positions (including those on the second-to-outer ring). One potential problem with concentric-ring arrangements is that, for the larger set size, the array takes up a larger display area – and the target can appear farther in the periphery compared with the smaller set size. As target discriminability becomes increasingly poorer in the periphery (e.g., Carrasco, Evert, Chang, & Katz, 1995; Carrasco & Frieder, 1997; Carrasco & Yeshurun, 1998), concentric rings imply the risk of confounding set size and periphery effects. To mitigate this confound, in the present study, the smaller array was not always positioned centrally; rather, the whole array was randomly repositioned on each trial so that, on average, it occupied the same area as the larger array.

Procedure. The search display remained on-screen until participants made their response. Target-present and -absent responses were given on a keyboard (affording high-temporal resolution) using the left or right index finger, respectively (counterbalanced across participants). If a response was wrong, participants received immediate feedback: the German word “Fehler” (i.e., “error”) was shown for 500 ms. Trials were separated by an inter-stimulus interval jittered between 700 ms and 1100 ms, during which a fixation cross was shown. Participants were instructed to respond as quickly and accurately as possible and received feedback on their performance (error rate and reaction times) during a self-terminated break after the training trials as well as in the middle and at the end of each block.

Data analysis. For all experiments, practice trials and trials with incorrect responses were excluded from the RT analyses. Additionally, outlying log-transformed RTs were identified separately for each Participant × Orientation-Contrast × Set-Size × Target-Presence cell and removed from the respective average (resulting in the removal of around 3% of all correct trials per experiment). Averaged RTs were then examined by repeated-measures analyses of variance (ANOVAs). Greenhouse-Geisser corrected *p* values (p_c) and the respective εs are reported for effects with two or more degrees of freedom in the numerator. ANOVA main effects and interactions were followed up by planned paired *t* tests. For effects of orientation contrast, the *t* tests were directed (one-tailed), as higher contrasts were predicted to yield shallower slopes and lower intercepts (faster RTs). Also, tests of slopes against zero were one-tailed, because slopes should not be negative. All other *t* tests were two-tailed. To provide the full picture, all *t* tests of interest are reported, even if the respective ANOVA effect was not significant. In the rare cases in which a significant *t*-test was not supported by the respective ANOVA effect, this *t*-test result is not interpreted without independent replication in a similar orientation-contrast condition from another experiment. Error bars display 95% within-subject confidence intervals (Jarmasz & Hollands, 2009; Loftus & Masson, 1994), based on the error term from the respective main effect of set size (separate one-way ANOVAs for each Orientation-Contrast × Target-Presence condition). Our hypotheses relate to RTs and not to error rates. Therefore, only RTs for correct responses (and the derived measures search slopes and intercepts) are reported in the main text and discussions focus on the RT results. For analyses on error rates, the interested reader is referred to the respective tables in Supplement A.

In order to extract slopes and intercepts for each Participant × Orientation-Contrast × Target-Presence cell, we regressed mean reaction time on set size as $RT = b_1 + b_2 * set\ size$ (i.e., we calculated the line that connects the mean RTs for the two set sizes). Values for *set* *size* involved in the calculation of slopes and intercepts were 7 and 19 items, assuming that participants never searched the outer ring where the target could never appear[2]. The second weight (b_2) was our estimate of the search slope. We defined the intercept as an extrapolation of the regression line to a display with only *one* object ($b_1 + b_2$).

It turned out that slopes for target-absent trials were often more than twice the slopes for target-present trials. For statistical confirmation of this observation, we conducted two-tailed *t*-tests of two times target-present slopes against one time target-absent slopes (we did not test ratios against 2, because population estimates of ratios are notoriously biased and unreliable).

Results

The resulting overall data pattern is displayed in Figure 2. For *Experiment 1a*, an Orientation-Contrast × Set-Size ANOVA on the mean target-present RTs revealed all effects to be significant: orientation contrast, $F(2,30) = 28.24$, $p_c < .001$, $\eta_p^2 = .65$, $\varepsilon = .65$; set size, $F(1,15) = 25.68$, $p < .001$, $\eta_p^2 = .63$; interaction, $F(2,30) = 10.70$, $p_c = .002$, $\eta_p^2 = .42$, $\varepsilon = .64$. The ANOVA of the target-absent RTs revealed the same pattern of effects: orientation contrast, $F(2,30) = 12.91$, $p_c < .001$, $\eta_p^2 = .46$, $\varepsilon = .73$; set size, $F(1,15) = 42.86$, $p < .001$, $\eta_p^2 = .74$; interaction, $F(2,30) = 17.26$, $p_c < .001$, $\eta_p^2 = .54$, $\varepsilon = .84$. The effects of set size and the Orientation-Contrast × Set-Size interactions reflect the fact that the search slopes (i) were overall non-flat (i.e., they were > zero) and (ii) differed among the orientation-contrast conditions.

To further explore these effects, we analyzed the *search slopes* directly. The slopes differed from zero for each orientation contrast, for target-present trials, all *t*s > 3.38, all *p*s < .003, all *d*s > 0.84, as well as for target-absent trials, all *t*s > 4.35, all *p*s < .001, all *d*s > 1.08 (see Table 1). Furthermore, slopes were steeper for 3°- compared to 4°- and 5°-target conditions for target-present and -absent trials, all *t*s > 3.11, all *p*s < .004 all d_zs > 0.77. The difference in slope between 4°- and 5°-target conditions just missed the significance criterion for target-absent trials, $t(15) = 1.73$, $p = .052$, $d_z = 0.43$, and was clearly not significant for target-present trials, $t(15) = 0.83$, $p = .210$, $d_z = 0.21$. Interestingly, inspection of Table 1 reveals that mean slopes were more than twice as steep for target-absent compared to target-present trials for all three orientation contrasts. This observation was substantiated by significant *t*-tests for 4°- (ratio of mean slopes across participants: 5.84), $t(15) = 3.87$, $p = .001$, $d_z = 0.97$, and for 5°-target conditions (ratio: 6.90), $t(15) = 3.30$, $p = .005$, $d_z = 0.82$. For conditions with 3° targets, the slope ratio was close to 2:1 (ratio: 2.46), $t(15) = 1.40$, $p = .18$, $d_z = 0.35$.

For *Experiment 1b*, the ANOVAs on the absolute RTs yielded a qualitatively different pattern of results (compared to Experiment 1a). For *target-present trials*, the only significant (main) effect was that of orientation contrast, $F(5,75) = 24.31$, $p_c < .001$, $\eta_p^2 = .62$, $\varepsilon = .54$; there were no significant effects involving set size (main effect, $F(1,15) < 0.01$, $p = .943$, $\eta_p^2 < .01$; Orientation-Contrast × Set-Size interaction, $F(5,75) = 0.61$, $p_c = .691$, $\eta_p^2 = .04$, $\varepsilon = .31$). The same was true for *target-absent trials*: significant main effect of orientation contrast, $F(5,75) = 14.60$, $p_c < .001$, $\eta_p^2 = .49$, $\varepsilon = .52$ (main effect of set size, $F(1,15) = 1.46$, $p = .25$, $\eta_p^2 = .09$; Orientation-Contrast × Set-Size interaction, $F(5,75) = 2.81$, $p_c = .103$, $\eta_p^2 = .16$, $\varepsilon = .26$). Concerning the absence of set-size effects, none of the search slopes was even close to significance, all *t*s < 1.14, all *p*s > .137, all *d*s < 0.29, except for the 6° contrast, target-absent condition, $t(15) = 1.64$, $p = .061$, $d = 0.41$ (see Table 1; we return to this exception in the discussion of Experiment 2).

As foreshadowed by the main effects of orientation contrasts on mean RTs, one-way ANOVAs on the *intercepts* revealed the (main) effects of orientation contrast to be significant for both target-present trials, $F(5,75) = 23.80$, $p_c < .001$, $\eta_p^2 = .37$, $\varepsilon = .53$, and target-absent trials, $F(5,75) = 7.01$, $p_c < .001$, $\eta_p^2 = .16$, $\varepsilon = .63$. Some comparisons between adjacent pairs of orientation contrast did not yield a significant intercept difference. However, when comparing conditions two levels apart (6° vs. 10°, 8° vs. 12°, 10° vs. 22°, 12° vs. 45°), all tests were significant – with lower intercepts for the respective higher orientation contrast; all ts > 2.32, all ps < .018, all d_zs > 0.58, for target-present trials, and all ts > 2.22, all ps < .022, all d_zs > 0.55, for target-absent trials, except for 6° vs. 10° contrast, target-absent trials, $t(15) = 1.35$, $p = .098$, $d_z = 0.34$.

Discussion

Stimulus displays that differed only in the orientation of the target object but were otherwise identical elicited significant search slopes (set-size effects) in Experiment 1a (in which orientation contrast was low), but flat slopes in Experiment 1b (in which orientation contrast was high), thus replicating the finding that feature search can be efficient or inefficient, depending on the target-distractor feature contrast (e.g., Nagy & Sanchez, 1990; Treisman & Gormican, 1988). Complementing these earlier studies, Experiment 1a showed that search slopes change gradually as a function of target-distractor orientation contrast. The strength of this effect differed between target-present and target-absent slopes. Accordingly, markedly different absent-to-present slope ratios were observed for the different conditions – notably, for conditions with 4° and 5° targets, the ratios were significantly higher than the 2:1 ratios typically observed in feature search tasks (Wolfe, 1998b).

In addition, we found not only the slope, but also the intercept of the function relating search times to the number of items to vary systematically with orientation contrast on target-present trials: the higher the contrast, the lower the intercept. This decrease in intercept continued even in the contrast range of Experiment 1b in which search slopes were flat. This means that at the point where search had become efficient, the intercepts had not yet reached asymptote.

Experiment 2 (4°, 5°, 6°, and 7°)

In Experiment 1a, we observed a decrease in search slopes with increasing target-distractor feature contrast. This decrease and the flat search slopes in Experiment 1b indicate that there might be a smooth transition from inefficient to efficient search. Unfortunately, however, none of the Experiments sampled feature contrasts in a range that spans this transition. The strong qualitative differences between the data patterns observed in Experiments 1a and 1b could therefore also be influenced by qualitative differences in strategy use or experimental context. For example, participants in Experiment 1a might have adopted a ‘serial-search mode’, because all conditions where rather difficult, whereas participants in Experiment 1b might have adopted a ‘parallel-search mode’, because all conditions were relatively easy. To mitigate alternative interpretations along these lines, the aim of Experiment 2 was to sample contrasts at the transition from inefficient to efficient search within a single experiment.

Methods

Participants. Sixteen university students participated in Experiment 2 (median age: 24.5 years, range: 19-37 years, 9 female). All participants reported normal or corrected-to-normal vision and gave informed consent. No participant had outlying overall reaction times.

Design and Procedure. Stimuli were presented on a TFT screen (1920 × 1080 pixels, 60 Hz) at a viewing distance of 60 cm. We examined orientation contrasts of 4°, 5°, 6° and 7°. The experimental design and procedure was essentially the same as in Experiment 1, with the following four methodological changes: (i) In each block, after the initial training phase with the target always appearing in the screen center (8 trials), a second training phase (12 trials) followed in which the displays were identical to the experiment proper; these trials were also discarded from the data analysis. Following these (8 + 12) training trials, participants were presented with 72 experimental trials. Participants received performance feedback (RTs, accuracy) after the first training phase and at the end of each block.

Orientation contrast blocks (of 72 experimental trials each) were repeated four times, each time in a different (random) order, yielding 288 experimental trials per orientation-contrast condition in total. (ii) Immediate feedback was given as a 1-s color change of the fixation cross to green (correct response) or red (wrong response). (iii) A third methodological difference relates to the fact that in Experiment 1, the smaller search array (but not the larger one) could occur in different random positions on the screen so that the target had, on average, the same spatial eccentricity in both set-size conditions. A potentially problematic consequence of this procedure was that small arrays 'jumped' across the screen, whereas large arrays were always presented at the same position (centered on the middle of the screen). Potentially, with small arrays, this might have made a first, coarse allocation of attention to the global (array) region necessary before the search proper could commence. Because such a first allocation was unnecessary for the large array, this might have weakened or eliminated set-size effects. To avoid this potential confound, in Experiment 2, we extended the range of possible array locations such that large set-size arrays too (as well as the small arrays) occupied different random positions on each trial. The average eccentricity of the target still remained equal between both set-size conditions. (iv) As a last change from Experiment 1, the target in Experiment 2 could also appear in the center of the concentric rings (but never in the center of the screen, where the fixation cross was shown before display onset).

Results

Figure 3 gives an overview of the pattern of RTs. For *target-present* trials, an Orientation-Contrast × Set-Size ANOVA on mean RTs revealed only the main effect of orientation contrast to be significant, $F(3,45) = 60.46$, $p_c < .001$, $\eta_p^2 = .80$, $\varepsilon = .71$, there were no significant effects involving set size (main effect, $F(1,15) = 2.37$, $p = .145$, $\eta_p^2 = .14$; interaction, $F(3,45) = 1.45$, $p_c = .242$, $\eta_p^2 = .09$, $\varepsilon = .95$). For *target-absent* trials, by contrast, there was a clear effect of set size and an interaction, in addition to the main effect of orientation contrast: $F(3,45) = 48.16$, $p_c < .001$, $\eta_p^2 = .76$, $\varepsilon = .84$; main effect of set size, $F(1,15) = 22.86$, $p < .001$, $\eta_p^2 = .60$; interaction, $F(3,45) = 16.62$, $p_c < .001$, $\eta_p^2 = .53$, $\varepsilon = .73$.

The effect of set size and the Orientation-Contrast × Set-Size interaction for *target-absent* trials reflect the fact that the search slopes (i) were overall non-flat and (ii) differed among the contrast conditions. Indeed, target-absent RT slopes differed from zero for each orientation-contrast condition, all *t*s > 3.46, all *p*s < .002, all *d*s > 0.86 (see Table 1). Furthermore, all but one of the pairwise comparisons of slopes between orientation-contrast conditions were significant, all *t*s > 2.37, all *p*s < .016, all d_zs > 0.59, except for 6°- vs. 7°-target conditions, $t(15) = 1.50$, $p = .077$, $d_z = 0.38$; all *t*s > 3.83, all *p*s < .001, all d_zs > 0.95, when comparing orientation-contrast conditions two levels apart. For *target-present* trials, by contrast, a significant search slope emerged only for 4° targets, $t(15) = 1.93$, $p = .036$, $d = 0.48$ (for all other contrasts, *t*s < 0.81, *p*s > .215, *d*s < 0.21, see Table 1), but even this slope was rather shallow (2.7 ms/item) and clearly below the typical criterion of 5 ms/item. For all orientation contrasts, slopes were clearly more than twice as steep for target-absent compared with target-present trials, all *t*s > 2.65, all *p*s < .019, all d_zs > 0.65 (see Table 1; range of ratios across orientation contrasts: 6.73 – 23.29).

The finding that slopes are steep for target-absent trials but flat for target-present trials is surprising, because this pattern would indicate that, even when targets popped out on target-present trials (i.e., no serial search was necessary) in a given trial block, participants still engaged in serial checking on target-absent trials within the same block. Arguably, if the target popped out on every target-present trial of a given contrast condition, an efficient strategy for search termination on target-absent trials would have been to rely on ‘pop-out failure’. Had a target-absent response been issued whenever no pop-out occurred, target-absent slopes would have been flat. Given this, the steep target-absent slopes that we obtained indicate that observers engaged in a more extensive process prior to quitting the search. One possible explanation is that, because of uncontrolled or stochastic influences, the same target did not pop-out on every target-present trial within a given block and that observers thus had to resort to serial search to find the target on a minority of target-present trials. They would then perform a serial search on the majority of target-absent trials, because on such trials pop-out never occurs. As a consequence of serial search in a majority of target absent-trials and in a minority of target-present trials, mean target-absent slopes would be steep even in conditions for which mean target-present slopes are flat.

To examine for this possibility, we calculated the 95% RT quantiles (using the method of Heathcote, Brown, & Mewhort, 2002). For these slowest trials, clear search slopes emerged even on target-present trials with 4° targets (14.6 ms/item), $t(15) = 3.31$, $p = .003$, $d = 0.83$, 5° targets (8.2 ms/item), $t(15) = 2.58$, $p = .010$, $d = 0.65$, and 6° targets (7.8 ms/item), $t(15) = 2.57$, $p = .011$, $d = 0.64$. Only for 7° targets were the target-present slopes for the slowest trials (3.5 ms/item) not significantly different from zero, $t(15) = 1.33$, $p = .102$, $d = 0.33$.

Analyses of target-present *intercepts* revealed a strong main effect of orientation contrast, $F(3,45) = 30.83$, $p_c < .001$, $\eta_p^2 = .22$, $\varepsilon = .86$. Intercepts differed between all conditions, all *t*s > 2.51, all *p*s < .012, all d_zs > 0.62. As slopes were steep on target-absent trials, the respective intercepts were not analyzed (see Footnote 2).

Discussion

The goal of Experiment 2 was to examine the transition from efficient to inefficient search. Sampling within this contrast range led to the unexpected discovery of an intermediate range of search difficulties where slopes are flat for target-present trials but steep for target-absent trials. Recall that a tendency towards such an effect was already evident in the 6°-target condition of Experiment 1b (see also Exp. 4 of Treisman & Souther, 1985). A closer look at only the slowest target-present trials indicated that although search slopes were flat on average, search was inefficient on some target-present trials, thus explaining why search was inefficient on a considerable number of target-absent trials. In line with Experiment 1a, we again observed target-absent-to-target-present slope ratios clearly higher than the typical 2:1 ratio (Wolfe, 1998b). In contrast to the respective findings of Experiment 1a (and the 1.5° contrast condition of Experiment 3 below), the latter finding must, however, be interpreted with caution as target-present slopes were not considerably different from zero and, consequently, small measurement errors in target-present slopes would exert an excessive influence on the ratio (see also Wolfe, 1998b). Furthermore, for target-present trials, we replicated the finding from Experiment 1b that search intercepts decrease with increasing feature contrast even in a range where search slopes are flat.

For target-present trials, we observed a significant search slope for low target-distractor orientation contrast (4° targets), but not for higher contrasts. However, although statistically significant, the slope in the 4°-target condition was rather shallow (2.7 ms/item). As search slopes less than 5 ms/item are usually considered flat and as the Orientation-Contrast × Set-Size interaction did not reach the traditional significance criterion, the orientation-contrast conditions sampled in Experiment 2 were obviously not ideal for providing strong evidence for a transition from inefficient to efficient search. The choice of orientation-contrasts was guided by the results of Experiment 1, but we observed somewhat shallower slopes in Experiment 2 than expected. In fact, a direct between-subjects comparison of slopes for similar orientation contrasts in Experiment 1a and 2 revealed systematic differences between experiments for 4°- and 5°-target conditions for target-present and target-absent displays (all ts > 2.13, all ps < .041). This might be owing to the differences in the experimental procedures between Experiments 1 and 2, which render a comparison across experiments problematic[3]. To obtain clear evidence for a transition from inefficient to efficient search within the same set of participants, we introduced more extreme contrast conditions in Experiment 3.

Experiment 3 (1.5°, 6°, 10°, and 20°)

In the previous experiments, we observed the following effects: (i) Search slopes were steep for low but flat for high orientation contrasts; (ii) with increasing contrast, RTs continued to decrease even beyond the point at which slopes had become flat (intercept effects); and (iii) search slopes were steep for target-absent trials and flat for target-present trials at intermediate contrast levels. The aim of Experiment 3 was to replicate all these effects within a single sample of participants. To this end, we introduced a condition for which we expected steep slopes for both target-absent and target-present trials (1.5° orientation contrast), a condition for which we expected steep slopes for target-absent trials but not for target-present trials (6° orientation contrast), and two conditions for which we expected flat slopes for target-absent as well as target-present trials, but a difference in the intercepts (10° and 20° orientation contrasts).

Methods

Sixteen university students participated in Experiment 3 (median age: 23.5 years, range: 18-32 years, 14 female). Two participants did not indicate their age. All participants reported normal or corrected-to-normal vision and gave informed consent. No participant had outlying overall RTs. – The orientation contrast levels tested in Experiment 3 were 1.5°, 6°, 10°, and 20°. In all other respects, the design and procedure were the same as in Experiment 2.

Results

Orientation-Contrast × Set-Size ANOVAs on mean RTs (Figure 4), conducted separately for target-present and target-absent trials, revealed all effects to be significant (main effect of orientation contrast, target-present, $F(3,45) = 79.84$, $p_c < .001$, $\eta_p^2 = .84$, $\varepsilon = .34$; target-absent, $F(3,45) = 107.89$, $p_c < .001$, $\eta_p^2 = .88$, $\varepsilon = .34$; main effects of set size, target-present, $F(1,15) = 27.02$, $p < .001$, $\eta_p^2 = .64$; target-absent, $F(1,15) = 51.82$, $p < .001$, $\eta_p^2 = .78$; and interaction, target-present, $F(3,45) = 24.78$, $p_c < .001$, $\eta_p^2 = .62$, $\varepsilon = .34$; target-absent, $F(3,45) = 41.64$, $p_c < .001$, $\eta_p^2 = .74$, $\varepsilon = .34$).

For *target-absent* trials, slopes differed from zero for 1.5°-, $t(15) = 6.70$, $p < .001$, $d = 1.68$, as well as for 6°-target conditions, $t(15) = 4.84$, $p < .001$, $d = 1.21$. For the 10°-target condition, although target-absent slopes differed from zero, $t(15) = 2.98$, $p = .005$, $d = 0.75$, they were significantly below 5 ms/item, the standard criterion for differentiating steep from flat slopes, $t(15) = 2.14$, $p = .049$ (two-tailed *t* test against 5 ms/item), $d = 0.54$. For the 20°-target condition, target-absent slopes were also below 5 ms/item, $t(15) = 5.12$, $p < .001$, $d = 1.28$, and did not differ from zero, $t(15) = 0.60$, $p = .278$, $d = 0.15$.

For *target-present* trials, search slopes (Table 1) were larger than zero only for 1.5° targets, $t(15) = 5.05$, $p < .001$, $d = 1.26$, but not for any other target orientation, $t(15) = 1.48$, $p = .080$, $d = 0.37$ (6°), $t(15) = 0.41$, $p = .345$, $d = 0.10$ (10°), and $t(15) = -0.53$, $p = .607$ (two-tailed), $d = -0.13$ (20°). In fact, for 6°, 10°, and 20° targets, slopes were clearly lower than 5 ms/item, all *t*s > 3.67, all *p*s < .003 (two-tailed), all $d > 0.91$. Nevertheless, for 6° targets, target-present slopes calculated for only the slowest trials (95% quantiles; average across participants: 12.6 ms/item) again differed from zero, $t(15) = 2.13$, $p = .025$, $d = 0.53$. As expected, only for the 6°-target condition were mean target-absent slopes more than twice as high as the mean target-present slopes (ratio: 10.38), $t(15) = 3.88$, $p = .002$, $d_z = 0.97$; all other *t*s < 1.65, *p*s > .12, $d_z < 0.42$. Above, we explained the finding of flat mean target-present slopes for contrasts that produced steep target-absent slopes and steep 95%-quantile target-present slopes by assuming that observers perform serial search on a large proportion of target-absent trials when the target fails to pop out on only some target-present trials of the respective block. If this is the case, contrasts that produce flat target-absent slopes should produce flat target-present slopes even if only the slowest trials (95% quantiles) are considered. In line with this prediction, 95%-quantile target-present slopes were flat for 10° contrast (-5.5 ms/item), $t(15) = -1.61$, $p = .128$, $d = -0.40$, and 20° contrast (-7.7 ms/item), $t(15) = -1.62$, $p = .126$, $d = -0.41$ (both two-tailed, because slopes were negative).

For *intercepts*, we again analyzed only conditions with flat slopes. Target-present intercepts for 6° targets differed from those for 10° and 20° targets, $t(15) = 6.92$, $p < .001$, $d_z = 1.73$, and $t(15) = 6.54$, $p < .001$, $d_z = 1.64$, and intercepts differed between 10° and 20° targets , $t(15) = 2.98$, $p = .005$, $d_z = 0.74$. Target-absent intercepts for 10°- and 20°-target conditions also differed, $t(15) = 3.04$, $p = .004$, $d_z = 0.76$.

Discussion

Experiment 3 replicated all effects observed in Experiments 1 and 2, but within the same participant sample. In particular, (i) search slopes were steep for the 1.5°-target condition and flat for 10°- and 20°-target conditions; (ii) search intercepts further decreased from 10°- to 20°-target conditions; and (iii) search slopes were steep for target-absent trials and flat for target-present trials at an orientation contrast of 6°. In the latter condition, target-present slopes still clearly differed from zero for the slowest trials.

General discussion

The present set of experiments examined how a parametric manipulation of target discriminability via target-distractor feature contrast, within the same general-task and stimulus design, influences search performance, including major RT indices standardly used to draw inferences about the underlying cognitive processes. In particular, we observed (i) a dependence of search slopes on orientation contrast in Experiments 1a and 2; (ii) a further reduction of search intercepts with high orientation contrasts beyond the point where search slopes had become flat (i.e., where search had become ‘efficient’) in Experiments 1b and 2; and (iii) and a persistence of (above-zero) search slopes on target-absent trials after target-present slopes had already become flat at intermediate orientation-contrast levels in Experiment 2. All these effects were replicated within the same participant sample in Experiment 3.

Search efficiency depends on target-distractor discriminability

In a meta-analysis of some 2,500 individual data sets involving many different kinds of stimuli and task designs, Wolfe (1998b) demonstrated that visual-search tasks cannot be simply partitioned into two distinct categories based on the search slopes they generate – rather, there is a continuum of slopes varying with task demands. Here, we replicated this continuum within one task design (feature search) using one set of stimuli (oriented bars). To induce the range of different search slopes, we only had to manipulate the target-distractor orientation contrast. This simple manipulation was sufficient to fundamentally change the empirical data pattern and produce the full range of search slopes usually observed with very heterogeneous stimuli. In line with Wolfe (1998b) and Duncan and Humphreys (1989), the change from inefficient to efficient search did not occur in a discrete 'step' from steep to flat slopes; rather, the observed change of search slopes with target-distractor feature contrast was continuous (see Table 1). This provides strong evidence that feature contrast directly influences guidance and is further indication that guidance is a truly continuous factor.

Target-distractor feature contrast as examined here is, of course, only one possible manipulation of target-distractor discriminability. Arguably, though, discriminability is the underlying dimension determining guidance across the full range of typical visual-search designs (as examined in Wolfe, 1998b). Besides target-distractor feature contrast, other display factors, such as distractor-distractor similarity, too, would influence discriminability (Duncan & Humphreys, 1989) and would, thus, yield similar effects.

Interestingly, a recent study by Roper et al. (2013) has shown that search efficiency can be employed as a measure of perceptual load (e.g., Lavie, Hirst, De Fockert, & Vilding, 2004). Similar to our study, Roper et al. manipulated target-distractor similarity and found effects on search slopes in a visual-search task and on the magnitude of (flanker) interference in a flanker task. Strikingly, the more efficient search was for a given target-distractor combination, the larger were the flanker-interference effects, with a strong correlation between search slopes and flanker-interference effects. In another line of research, Alvarez and Cavanagh (2004) used the same objects in a visual-search task and, respectively, a visual-working-memory task (change detection); they found a strong relation between search slope and working-memory capacity across object categories. This indicates that the continuum described here is not special to the processes underlying visual search; rather, it is likely to be much broader relevance for visual cognition.

Search can speed up even beyond the point where search slopes become flat

Within the 'dimension-weighting' framework of visual singleton search (e.g., Found & Müller, 1996; Müller, Reimann, & Krummenacher, 2003; Müller, Geyer, Zehetleitner, & Krummenacher, 2009), the importance of analyzing intercepts in addition to slopes has been stressed earlier on (see, e.g., Müller et al., 1994). In particular, Töllner, Rangelov, and Müller (2012) and Müller, Krummenacher, and Heller (2004) predicted decreases in intercept (as observed here) in a range where search slopes are flat. They reasoned that, in tasks requiring target detection and localization, full focal-attentional identification of the selected item might be unnecessary when target saliency is very high. Instead, a target-present (or localization) response could be issued based on detecting a 'significant' saliency signal alone. If there is a saliency signal that can trigger an attentional orienting response, it would seem reasonable to assume that observers can also use this signal to directly elicit a target-present response. Indeed, for such a response, observers might not even need to be consciously aware of the target (see, e.g., Klotz & Neumann, 1999). According to this account, focal-attentional analysis of the selected item to establish that it actually is the target would constitute an additional process that is necessary only when the salience signal is not strong enough to make a clear decision (or when, in a compound-search task, some non-search-critical feature of the selected item needs to be extracted to determine the response). The more salient a target is, the more often this double check is skipped and the lower average RTs become, thus yielding an intercept decrease – as observed in the present study.

Within the Guided Search (GS) model family (Moran et al., 2013; Wolfe, 1994, 2007), several processes could in principle explain the observed decrease in search intercepts beyond the point where search becomes efficient. One possibility is that when a target gains a higher weight on the saliency map due to its increased saliency, attention would be drawn faster towards this object. The main support for this proposal stems from Töllner et al.'s (2011) event-related potential (ERP) study, which revealed the posterior contralateral negativity (PCN) component (an ERP marker of the allocation of focal attention to the target) to emerge faster with higher target-distractor feature contrasts. The PCN (also known as N2pc) is a lateralized event-related potential component extracted from the human electroencephalogram at posterior electrodes (PO7, PO8) and is usually interpreted to reflect the allocation of visuo-spatial ('focal') attention. Shorter PCN latencies have also been demonstrated for singleton targets redundantly defined in two dimensions (e.g., a target that is not only differently tilted but also differently colored compared to non-targets; e.g., Krummenacher, Grubert, Töllner, & Müller, 2014) and symbolic pre-cueing of the likely target-defining dimension (Töllner, Zehetleitner, Gramann, & Müller, 2010). The finding that PCN latencies are shorter for more salient targets provides strong evidence in favor of attentional selection operating faster under such stimulus conditions. A mechanism implementing this can be easily incorporated into any GS-type model: even when two search targets are each so salient that they are selected first with a probability close to 1 (i.e., even if both ‘pop out’), any difference in saliency between them would still lead to different amounts of time necessary for selection.

This, however, cannot explain the intercept decreases on target-absent trials, on which no target is present and, consequently, no pop-out occurs. Notably, none of the previous, comparable studies discussed in the Introduction (Goschky et al., 2014; Töllner et al., 2011; Zehetleitner et al., 2011; Zehetleitner et al., 2013; Zehetleitner et al., 2011; Zehetleitner et al., 2009; Zehetleitner et al., 2009; Zehetleitner & Müller, 2010) did collect target-absent RTs (rather, either a target was present on all trials and the response was determined by an additional feature of the target or the location of the target; or, while there were target-absent trials, the task was to withhold the response on such, ‘no-go’, trials). Given this, the present study is, to our knowledge, the first one in which a decrease of the intercept with increasing target-distractor feature contrast was observed for target-absent trials. This speed-up on target-absent trials indicates that another mechanism comes into play, in addition to any bottom-up influences of stimulus properties: When participants, in trial blocks with high target-distractor feature contrasts, learn that discerning the presence (vs. the absence) of a target is easy and fast, they might adapt the time taken for item identification accordingly. Shortened identification times would yield decreased search intercepts on target-absent as well as on target-present trials.

To illustrate, in the GS family of models, two aspects of search influence RTs: the number of items that are inspected and the time it takes to identify each inspected item as either a target or a distractor. Flat search slopes on target-present trials are taken to indicate that the target is most often the very first and only item inspected (because it affords enough guidance). As even in this extreme case one item is inspected, the GS family of models could account for the observed decrease in the search intercepts (beyond the point where search slopes become flat) by assuming a speed-up of the identification process. Correspondingly, flat search slopes on target-absent trials indicate that, regardless of set size, a fixed number of distractors are inspected before a target-absent response is issued. Potentially, one or more items are inspected on target-absent trials, to verify that the most salient objects are not the target. Decreases in target-absent intercepts could then also be explained by speed-ups of identification times.

Reproducing the qualitative data pattern with the Competitive Guided Search model

Competitive Guided Search (CGS) is one specific implementation of GS that has proven successful in predicting RT distributions in search tasks of varying levels of difficulty (Moran, Zehetleitner, Müller, & Usher, 2013). In CGS, identification time is modeled by a single-boundary diffusion process. The rate of evidence accumulation depends on the incoming visual information: the higher the feature contrast between a target and a distractor, the better the discriminability between these two objects, thus supporting higher identification-drift rates (for both targets and distractors) and, as a consequence, faster identification times. Presumably, participants' ability to benefit from higher target-distractor discriminability was facilitated by the current experimental design: since orientation-contrast conditions were blocked, participants could set an identification-drift criterion that was most efficient for the respective contrast block.

To test whether identification speed-up can explain the decrease in search intercept well beyond the point where search slopes become flat, we attempted to account for the qualitative pattern of slopes and intercepts from all four experiments by means of the CGS model. In Figure 5, the mean slopes in all conditions of all four experiments are plotted as a function of the respective mean intercept. As can be seen, the figure neatly summarizes the phenomenon under investigation here (i.e., a decrease in intercept from left to right, continuing beyond the point at which slopes had become flat). In the attempt to account for this pattern with CGS, we allowed four parameters to vary between conditions according to a power law of angular contrast: (i) the weight of the target (guidance), (ii) the increase in the weight of the 'quit unit' after each inspection (the probability to abort search and issue a target-absent response increases with the weight of the quit unit!), and (iii) the mean identification time for target-items and (iv) the mean identification time for distractor-items (see the Appendix for details).

This model captures reasonably well the overall trends in the empirical patterns: Search slopes are steep for high search intercepts and fall off with decreasing intercept; even after slopes have reached an asymptote at zero (flat slopes), the intercept continues to decrease further. Consider first the flat slope range. In CGS, flat target-present slopes (indicating efficient search) are accounted for by assuming that target salience (and thus guidance) is so high that the target is almost always selected first. If the target is invariably selected first, set size has no influence on RTs and, consequently, search slopes are flat. Target weights calculated from the power-function parameters given in Table A1 of the Appendix were indeed high for orientation contrasts above or equal to 6° (range: 342.8 – 434,787.7; i.e., in 45°-target blocks, the probability that the target was the first item attended was 434,787.7 times higher than the probability for any distractor). Similarly, flat target-absent slopes are accounted for by huge boosts to the weight of the quit unit (range: 31.12 – 3,778.4 for orientation contrasts ≥ 8°), so that the quit unit is almost always selected after the first distractor identification, once again cancelling any effect of set size (e.g., in 45°-target blocks, after only one inspection, the probability of selecting the quit unit was 3,778.4 times higher than that of selecting any display item). Thus, in the efficient range, only one item is inspected on each trial and the decrease in intercept (with increasing orientation contrast) can, thus, be attributed solely to a speed-up of the identification process. Indeed, mean identification times for both the target and the distractors decreased considerably as a function of orientation contrast (target range for orientation contrasts ≥ 6°: 42 ms – 245 ms; distractor range for orientation contrasts ≥ 8°: 64 ms – 176 ms).

If, however, the increase in the quit unit's weight is only moderate (range: 0.3 – 21.5 for contrasts < 8°), search on target-absent trials is often not terminated after the first distractor identification and the number of inspected distractors will increase as a function of set size. Thus, when target salience is very high but the increase in the quit unit's weight is moderate (e.g., for 6° orientation contrasts: 342.8 and 14.0, respectively), the model predicts flat target-present slopes together with steep target-absent slopes – the pattern observed in our empirical data for intermediate orientation contrasts (e.g., Experiment 2).

Finally, in the range of low contrasts, where both guidance and the increase in the quit unit's weight are low (ranges for orientation contrasts < 6°: 3.5 – 180.0 and 0.3 – 8.4), non-zero search slopes emerge for both target-present and target-absent trials. Slow identification times also contribute to the steepness of slopes and, additionally, yield effects on intercepts (ranges for orientation contrasts < 6°: 286 ms – 826 ms for targets and 231 ms – 466 ms for distractors).

The upshot from our model-fitting study is that the differences in data patterns among conditions cannot be selectively explained by differences in the amount of guidance afforded by the displays (as a function of target contrast). Indeed, to explain the change in search slope for target-absent trials, changes in the decision to quit search and issue a 'target-absent' response had to be assumed; to explain the decrease in intercepts well beyond the point where search slopes become flat, changes in the identification time per item (different times for targets and distractors) were necessary. Thus, the present modeling attempt illustrates how CGS can account for the qualitative data patterns and how the patterns of search slopes and intercepts on target-present and target-absent trials (observed in the current study) can provide a new test bed and additional constraints for models of visual search.

Admittedly, while the model captures the qualitative pattern rather well, the quantitative fit is far from perfect – especially in the high slope range, where the model either overestimates the intercept (left-most data point in the target-absent panel of Figure 5) or underestimates the search slopes (data points 2–4 in the target-absent panel and the second data point in the target-present panel). The question of whether a more elaborated model variant (e.g., one that does not constrain parameters to vary according to a power law) could adequately account for the quantitative empirical pattern remains to be addressed in future studies.

Parallel and serial search within the same condition?

Sampling at the transition from inefficient to efficient search in Experiment 2 (see also 6° orientation contrast of Experiment 3), we observed significant search slopes for target-absent trials when the slopes for target-present trials were flat. According to the traditional interpretation, this would indicate that although the target popped out on target-present trials within a given trial block, participants still decided to engage in (some) serial scanning of the display when the target was absent. This was the case even though the target-present slopes were flat in all orientation-contrast conditions of Experiment 2, thus ruling out carry-over effects from blocks with more inefficient search[4]. How soon search is terminated on target-absent trials depends on the participant's criterion to quit search when no target is found. When the target is always selected first (pop-out), this criterion is effective only on target-absent trials; it does then not influence search on target-present trials. Therefore, a conservative quit criterion can theoretically produce steep target-absent slopes even when target-present slopes are flat, which is the pattern that we observed in the intermediate range of orientation contrasts.

However, why would participants operate a serial strategy (i.e., a conservative quit-criterion) on target-absent trials when they know that a target, if present, would pop out and that they could simply 'have trust' in pop-out failures? One possible answer comes from the analysis of the trials that generated the slowest responses, for which significant search slopes were revealed also for target-present trials in the intermediate orientation-contrast range. This analysis suggested that targets did, in fact, not always pop out on target-present trials even when slopes calculated from mean RTs were flat. Instead, participants apparently reverted to serial scanning after the parallel process failed to yield evidence of target presence. Participants would, thus, engage in serial scanning on those few target-present trials on which the parallel process failed. When the target is absent, the parallel process can never yield evidence of target presence and scanning would therefore occur on most target-absent trials.[5]

Termination of search

Target-absent-to-target-present slope ratios have traditionally been taken to be diagnostic as to whether search is exhaustive on target-absent trials and (together with the linear increase in RT with set size) a 2:1 (2.0) ratio was originally considered compelling evidence for serial self-terminating search (e.g., Treisman & Gormican, 1988). The logic was that, on average, about half the display items must be searched to hit upon the target on target-present trials, whereas the whole display must be searched to verify target absence on target-absent trials. Smaller ratios (e.g., 1.5) would indicate that search was aborted prematurely for target-absent displays. This assumption would predict an upper bound of 2.0 on slope ratios, a prediction that was strongly violated in the present set of studies, where we found slope ratios of up to 6.90 when considering only those conditions in which both the target-present and target-absent slopes were significantly greater than zero (otherwise up to 23.29!).

One explanation for these disproportionately high target-absent slopes would be that participants searched through the display several times (up to 6.90/2 = 3.45 times) when they knew that discerning target presence was difficult. Such repeated searching of target-absent displays would be an indication of the absence of a memory for inspected locations (see, e.g., Horowitz & Wolfe, 1998, 2001, 2003; but see, e.g., von Mühlenen, Müller, & Müller, 2003). Such a quantitative interpretation, however, would depend on the (unrealistic) assumption that search order is not informed by stimulus features.

If search order is determined by the saliency of the searched objects as determined by a preceding parallel process (Guided Search; Wolfe, 1994, 2007), fewer than half the objects present in a display are usually searched on average on target-present trials. The number of objects searched on target-absent trials can then easily become more than twice the number on target-present trials, depending on the criterion to terminate search on target-absent trials. Furthermore, search might often be aborted prematurely on target-present trials, thus further invalidating the assumption that half the items are searched through on target-present trials. A look at the error pattern in Supplement A confirms this hypothesis: there are strong effects of orientation contrast on target-present error rates, which are owing to very high miss rates (up to 36.8%) for low-contrast conditions. Effects of set size on error rates are most pronounced for conditions with steep search slopes, indicating that the additional effort invested for larger set sizes is not sufficient to compensate for the additional number of items that must be inspected in larger displays when search is serial, providing additional indication that search is terminated too early in these cases.

Slope ratios as criteria for assigning search tasks to categories

Wolfe (1998b) observed that the distributions of search slopes from different types of search tasks strongly overlap and thus argued that search slopes are not diagnostic as to which type of search task they come from. Instead, he provided an alternative criterion to assign the different types of search tasks to categories. In particular, in Wolfe's set of studies, the ratio of target-absent to target-present search slopes was significantly lower for feature search (1.7 on average) as compared to other types of search that are traditionally considered more difficult (conjunction search, 2.9, and spatial-configuration search, 2.8); from this, Wolfe argued that these types of searches can be differentiated via the slope ratio. In the feature-search task examined here, by contrast, we observed ratios of at least up to 6.90, which were significantly larger than 2.0 and thus clearly more similar to Wolfe’s conjunction-search ratios than to his feature-search ratios. Arguably, such high slope ratios for a feature-search task disqualify slope ratios as criteria for categorizing search tasks. Consequently, there would appear to remain no known criteria for differentiating search tasks based on the resulting data patterns – thus challenging the general assumption of qualitative differences between these tasks. Instead, the lack of distinguishing criteria and the parametric effect of target-distractor feature contrast on search slopes might indicate that all differences between studies involving different stimuli and task designs (with perhaps a few exceptions, like Donnelly, Humphreys, and Riddoch’s, 1991, heterogeneous-form conjunction search) are simply driven by differences in discriminability. In other words, a single hypothetical dimension, target-distractor discriminability, may prove sufficient to explain the vast variability in empirical patterns across a wide range of search tasks.

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Footnotes

[1]For conciseness, all considerations here focus on models that assume a serial scanning stage. This is not meant to imply that a purely parallel model (Eckstein et al., 2000; Swensson & Judy, 1981; Verghese, 2001) could not also account for the observed phenomena. An exploration of how exactly such models can explain these phenomena remains a task for future research.

[2]This assumes that participants included the central item in their search and excluded the outer ring. Results are essentially the same when all tests are based on set sizes 6 and 18 items (assuming that participants did never inspect the central item) or on set sizes 19 and 37 (assuming that participants searched through all items, including the outer ring). The specific set-size values chosen for calculating slopes and intercepts influence estimates of slopes, but do not influence significance tests on slopes against zero or comparisons between slopes in different conditions (because changing set-size values amounts to scaling, e.g., slope{7,19}= (37-19)/(19-7)*slope{19,37}). The choice of set-size values has a considerable influence only on analyses of intercepts in conditions with slopes larger than zero. Because we cannot test the assumption that participants excluded the outer ring from their searches, we refrain from statistical analyses of intercepts in those conditions that produced non-flat slopes.

[3]Note that the aim of the present study was not to ‘discover’ slopes or intercepts for specific orientation contrasts. Such an endeavor would probably be futile, because these parameters are likely influenced by a large number of variables (e.g., stimulus size, luminance, presentation duration, or specific instructions), rather than solely by orientation contrast. These variables are not the topic of the present article. Instead, we were interested in the general patterns of effects (systematic shift of slopes and intercepts due to systematic changes in feature contrast) within the same experimental session.

[4]Following the advice of one anonymous reviewer, we explicitly examined for carry-over effects in Experiment 3, which included conditions with steep (1.5°) and flat (remaining conditions) target-present slopes (Supplement B). None of the analyses disclosed such carry-over effects.

[5]The intertrial variability in search efficiency is of interest in its own right. Apparently, uncontrolled or stochastic influences impact search efficiency even when the orientation contrast is kept constant within a block of trials. In fact, such variability was not only present for target-present but also for target-absent trials, thus indicating that, in addition to guidance, the criterion to quit search might also vary across trials. For orientation-contrast conditions that showed mean search slopes not significantly different from zero (see Table 1), 95% quantile slopes still significantly differed from zero in the contrast range below 20° (all *t*s > 1.84, all *p*s < .043, all *d*s > 0.46, except for Exp. 1b, 6°: $t(15) = 1.71$, $p = .054$, $d = 0.43$). A detailed exploration of the nature and the causes of this intertrial variability is beyond the scope of the present study.

Tables

Table 1.

Mean Search Slopes (in ms/Item) and Intercepts (in ms), with Standard Errors (in Brackets), for Target-Present and Target-Absent Trials.

	Slope		Intercept	
Contrast	Present	Absent	Present	Absent
		Experiment 1a		
3°	**30.6***** (7.12)	**75.3***** (9.71)	*853.2 (41.31)*	*969.2 (126.27)*
4°	**8.0***** (1.99)	**46.8***** (7.90)	*806.4 (44.71)*	*804.6 (62.39)*
5°	**5.5**** (1.58)	**38.1***** (8.47)	*705.1 (34.57)*	*697.1 (43.34)*
		Experiment 1b		
6°	0.5 (1.93)	9.0 (5.32)	613.5 (24.63)	582.2 (31.58)
8°	0.6 (0.77)	2.3 (1.95)	519.5 (18.89)	553.5 (29.80)
10°	-0.8 (0.55)	0.4 (1.15)	499.0 (18.82)	542.6 (30.30)
12°	0.1 (0.54)	0.2 (0.84)	479.2 (16.12)	503.1 (18.62)
22°	0.4 (0.31)	-0.8 (0.60)	452.1 (19.10)	491.3 (22.90)
45°	-1.0 (0.26)	-0.6 (0.64)	438.8 (12.89)	447.3 (16.01)
		Experiment 2		
4°	2.7* (1.35)	**18.1***** (3.25)	*743.1 (26.88)*	*661.5 (24.39)*
5°	1.0 (1.17)	**11.7***** (2.51)	691.0 (31.62)	*643.7 (25.81)*
6°	0.6 (0.94)	**8.7**** (2.42)	645.0 (21.54)	*629.4 (29.22)*
7°	0.3 (0.45)	**6.6***** (1.54)	596.4 (20.39)	*602.8 (17.52)*
		Experiment 3		
1.5°	**69.9***** (13.40)	**149.3***** (21.57)	*1298.1 (94.09)*	*1120.7 (138.17)*
6°	1.4 (0.94)	**14.9***** (2.99)	685.0 (22.70)	*633.0 (22.12)*
10°	0.2 (0.43)	2.9** (0.95)	581.3 (18.43)	614.4 (17.64)
20°	-0.3 (0.50)	0.5 (0.85)	531.5 (21.68)	576.0 (17.08)

Note. Asterisks indicate that the respective slope differs significantly from zero (one-sample directed *t*-tests). Slopes that are significant and larger than 5 ms/item are printed in bold. Intercept values printed in italics strongly depend on not verifiable assumptions on how many items were searched, and should therefore be interpreted with caution (see Footnote 2).

$^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table A1

Best-Fitting Parameter Estimates.

Parameter	Value (unit)
α_{wt}	0.59
β_{wt}	3.55
α_{wq}	0.10
β_{wq}	2.78
α_{idt}	0.85 (s)
β_{idt}	0.88
α_{idn}	1.70 (s)
β_{idn}	0.58
t_{non}	0.39 (s)

Figures

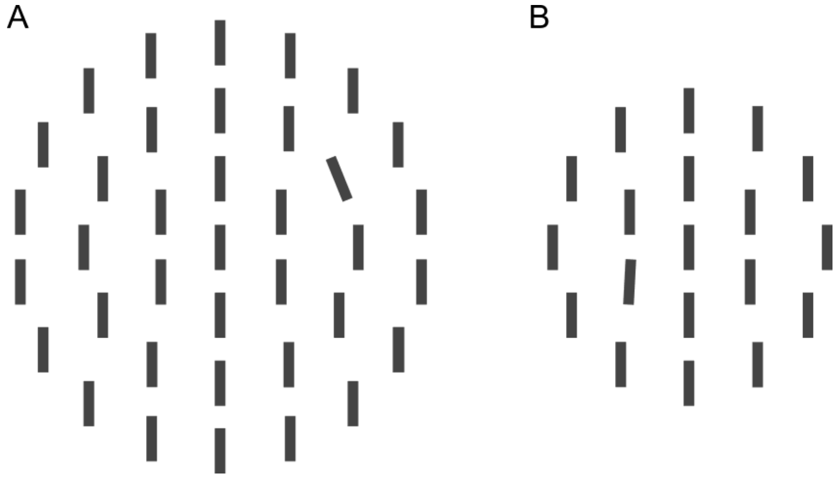

Figure 1. Schematic illustrations of two sample search displays. Panel A shows a set-size-19 target-present display (recall that the target could never appear on the outer ring) from a 22°-target block; panel B shows a set-size-7 target-present display from a 3°-target block. Participants' task was to indicate whether a tilted bar was present or not. In the actual displays, the bars were gray and shown on a black background.

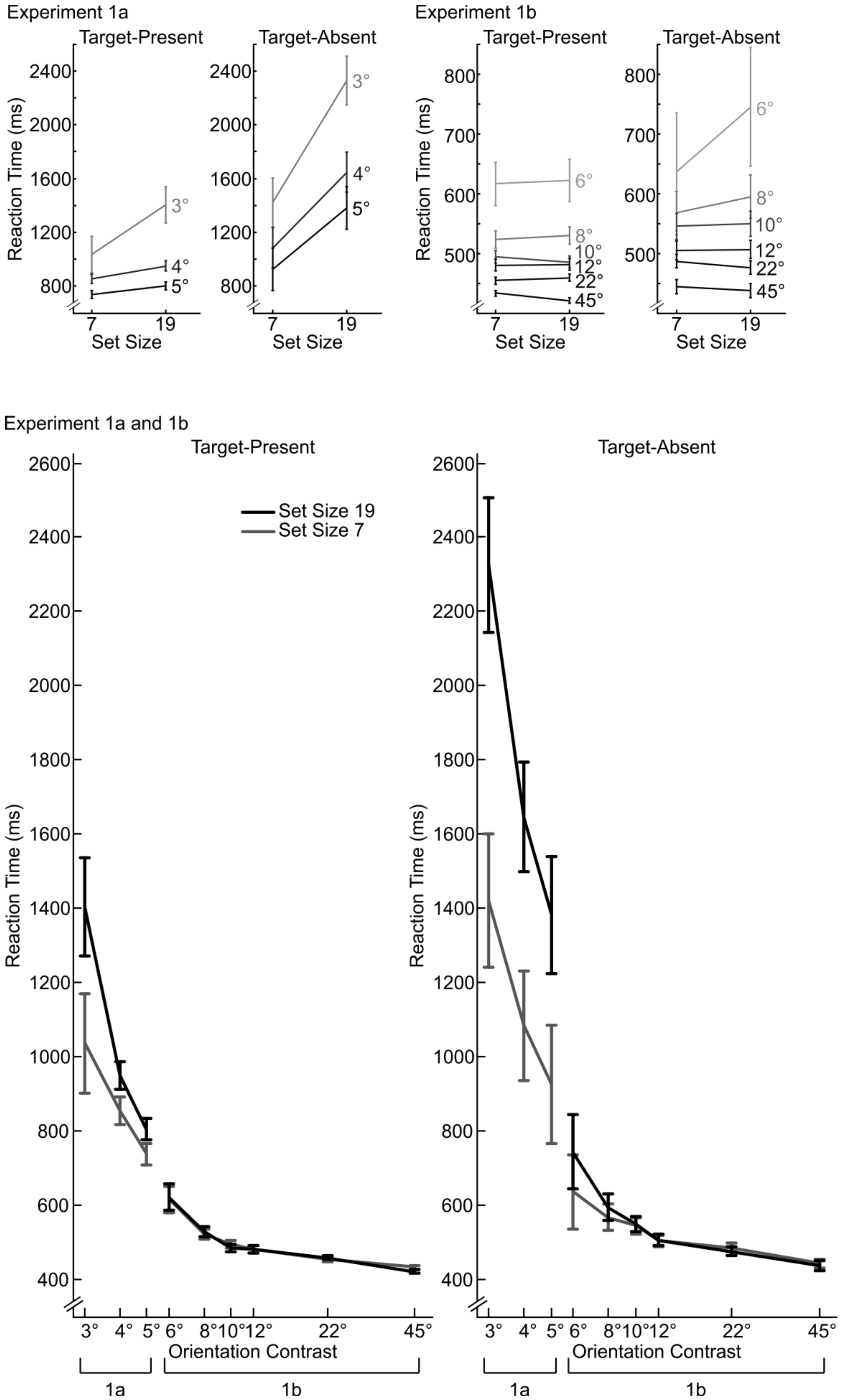
Experiment 1a
Target-Present
Target-Absent
Reaction Time (ms)
Set Size
3°
4°
5°
Experiment 1b
Target-Present
Target-Absent
6°
8°
10°
12°
22°
45°
Experiment 1a and 1b
Target-Present
Target-Absent
Set Size 19
Set Size 7
Reaction Time (ms)
Orientation Contrast
1a
1b

Figure 2. Mean reaction times as a function of target-distractor orientation contrast (different lines in the upper part, abscissas in the lower part), set size (abscissas in the upper part, different lines in the lower part), and target presence (left/right panels) for Experiment 1. The upper and lower parts display the same data. The upper part serves to illustrate search slopes, whereas in the lower part the effect of orientation contrast on intercepts and the pattern across experiments are seen more readily. In the lower part, slopes of the function relating RTs to set size are proportional to the distances between the two lines. The lines are discontinued between the contrast levels of 5° and 6°, because the data for the sets of low and, respectively, high contrasts stem from different experiments (Experiments 1a and 1b, respectively). Note that (i) slopes level out with increasing orientation contrast in Experiment 1a, and (ii) the overall decrease of RTs continues even after set-size effects have vanished in Experiment 1b (in the range with flat slopes/high contrasts); also note (iii) the much stronger set-size effects for target-absent compared to target-present trials in Experiment 1a. Error bars denote 95% confidence intervals based on the respective main effect of set size, and abscissas in the lower part are log-scaled.

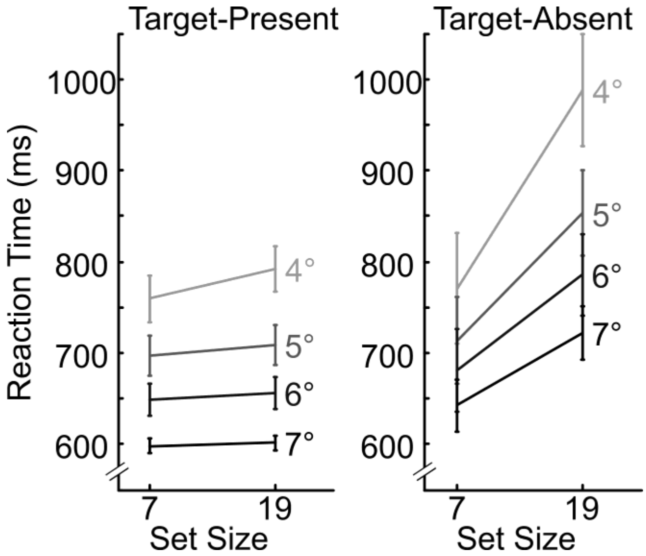

Figure 3. Mean absolute reaction times as a function of set size (abscissa), target-distractor feature contrast (different lines), and target presence (left/right panels) for Experiment 2. Note (i) the difference in set-size effects between target-present trials (left) as compared with target-absent trials (right); also note that (ii) the set-size effect on target-absent RTs diminishes with increasing orientation contrast and that (iii) overall target-present RTs decrease even in the absence of set-size effects. Error bars denote 95% confidence intervals based on the respective main effect of set size.

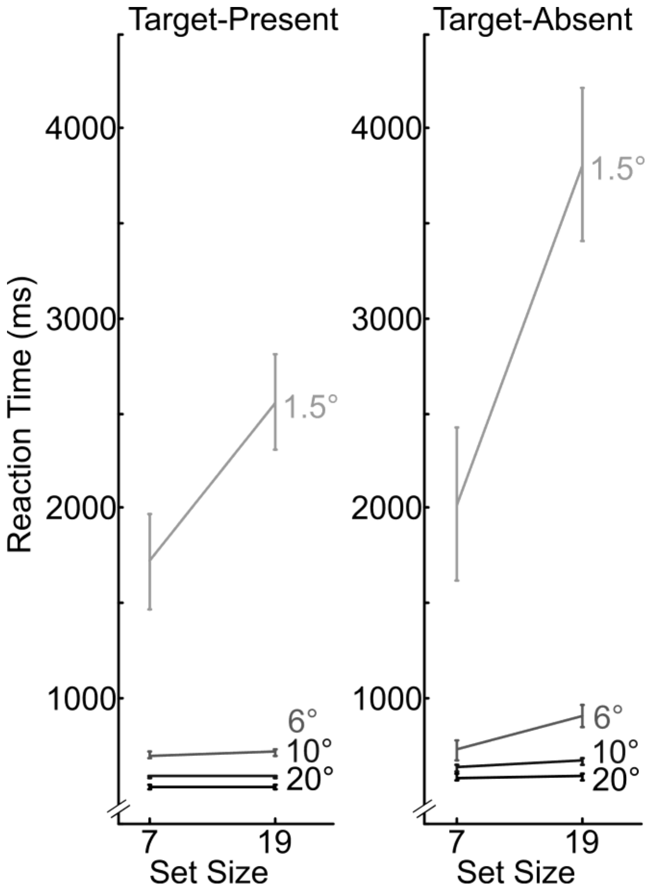

Figure 4. Mean absolute reaction times as a function of set size (abscissa), target-distractor orientation contrast (different lines within panels), and target presence (left/right panels) for Experiment 3. Error bars denote 95% confidence intervals based on the respective main effect of set size.

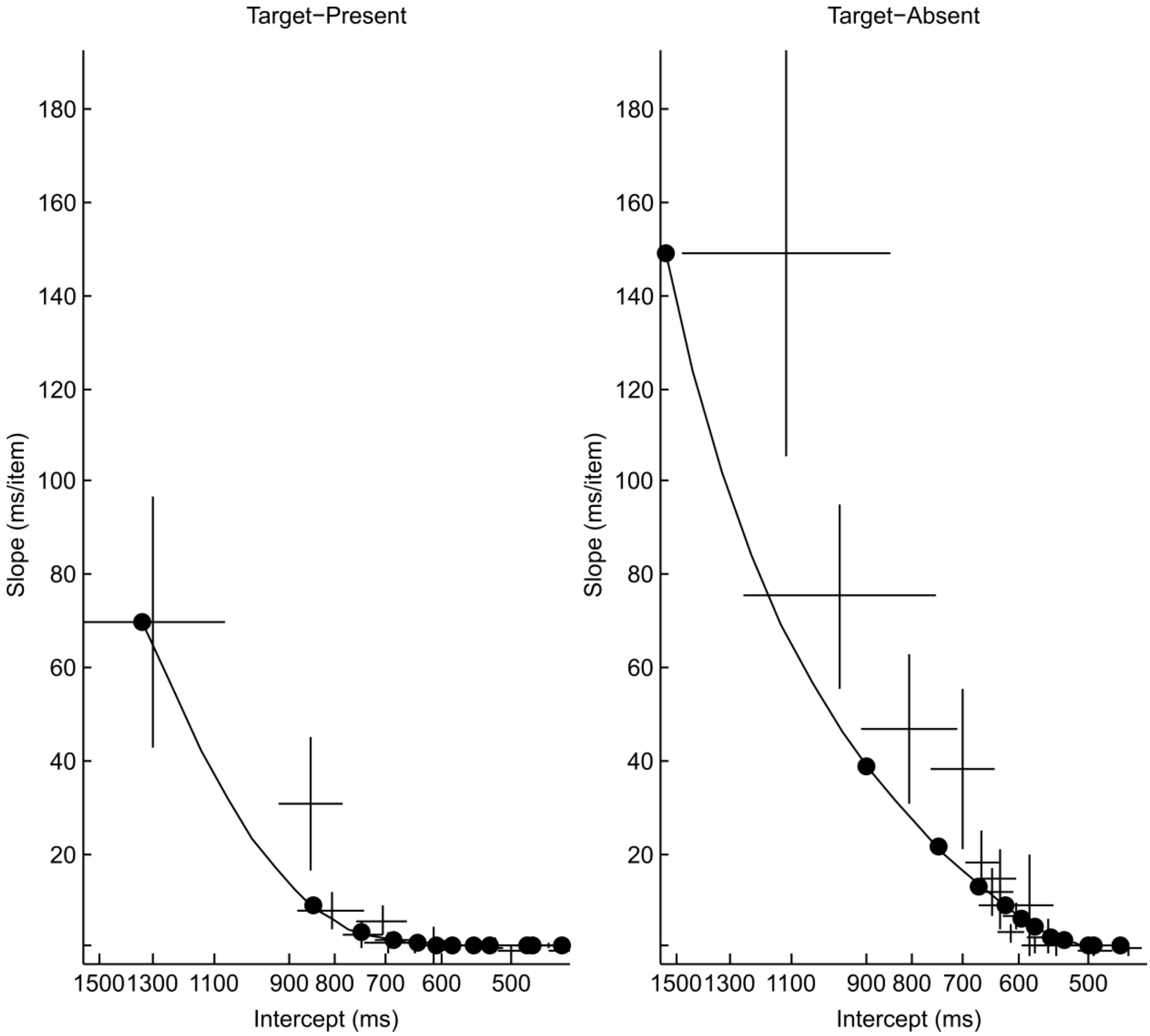

Figure 5. Search slopes as a function of search intercept across all four experiments, separately for target-present (left panel) and target-absent trials (right panel). The abscissa is reversed and log-scaled so as to maintain the ordering of data plotted in Figure 2 (from difficult to easy search) and to amplify the low-intercept range, respectively. Empirical data points are indicated by crosses formed by the associated error bars, giving the 95%-confidence intervals for the slopes (vertical bars) and intercepts (horizontal bars), respectively. To gain an overview, we simulated a range of orientation contrasts in between 1.5° and 45° based on the parameters that produced the best fit of the data to CGS. The black curves display these simulated results. The black dots indicate the predictions for the orientation contrasts corresponding to the empirical data.

Appendix: Competitive Guided Search (CGS)

CGS can explain the distributions of RTs in a wide range of visual-search tasks using eight free parameters (Moran et al., 2013). According to CGS, display items are selected in turn and identified as either a target or a distractor. Identification time is determined by two free parameters: the drift rate (δ) and the threshold (θ) of the identification process. The probability of selecting an object on a given turn is determined by its relative weight (according to Luce's, 1959, choice axiom). The third free parameter is the weight of the target object (w_{target}), which corresponds to its saliency. All distractors have the same weight, which serves as a scaling parameter fixed to 1. When the target object is selected, a 'target-present' decision is made. When a distractor is selected, it is fully inhibited, so that it cannot be reselected later on (this is achieved by reducing its weight to 0). The model features a 'quit unit', whose selection terminates the trial with a 'target-absent' decision. The quit unit competes with display items for selection. At the start of a trial, the weight of the quit unit is set to 0, but it increases following each (successive) distractor identification. The magnitude of this increase is the fourth free parameter of the model (Δw_{quit}). In addition to identification time, all other time-consuming processes are subsumed under 'residual, non-decision processes'. The temporal distribution for residual processes follows a shifted exponential function with a rate (γ; 5th free parameter) and a shift parameter. This shift parameter is different for target-present and target-absent responses (T_{yes}, T_{no}; 6th and 7th free parameter). Additionally, there is the probability that a response is selected (target-present or target-absent), but, due to a motor error (*m*; 8th free parameter), the respective other response is executed (see Moran et al., 2013, for full details of the CGS model and its parameters).

Here, we fitted the model to RT measures (intercepts and slopes) that are sensitive only to mean RTs (for the different set sizes), but not to the shape of the distributions. Thus, several of the model parameters that control the shape of the RT distributions would not be identifiable. This motivated a model simplification: the drift rate (δ) and the threshold (θ) of the identification process were combined into a single mean-identification-time parameter ($t_{id} = \theta / \delta$) and all non-decision, residual-time parameters (γ, T_{yes}, T_{no}) were combined into a single mean-residual-time parameter ($t_{non} = T_{yes/no} + 1/\gamma$; it turned out that different shift parameters for target-present and target-absent trials were unnecessary for predicting the present data pattern; hence, we assumed an equal mean residual time for 'present' and 'absent' responses). Furthermore, preliminary explorations revealed that the fit improved when we allowed for different identification times for targets (t_{idt}) and non-targets (t_{idn}). Differences in the identification process for targets and distractors were not provided by the original model (Moran et al., 2013), but such differences make intuitive sense, as targets and distractors are physically different and targets match the search template, whereas distractors do not.

The residual time parameter (t_{non}) was maintained at a fixed level for all angular contrast (c) conditions. The remaining parameters, however, were influenced by contrast. We did not allow these parameters to vary freely across contrast conditions, because with 12 different contrast conditions, this would have resulted in 44 additional parameters. Such high numbers of parameters can give rise to various problems in model fitting, including the risk of over-fitting and being caught in local minima, along with an enormous increase in computation time. Thus, rather than allowing parameters to vary freely across contrast conditions, we constrained them to vary according to power law functions, which can capture many monotonic trends of different shapes and require only one additional parameter for each model parameter:

$$w_{target} = 1+\alpha_{wt} * c\text{\^{}}\beta_{wt},$$

$$\Delta w_{quit} = \alpha_{wq} * c\text{\^{}}\beta_{wq}, \text{ and}$$

$$t_{idt} = \alpha_{idt} * c\text{\^{}}\beta_{idt}$$

$$t_{idn} = \alpha_{idn} * c \wedge \beta_{idn}$$

Additionally, we fixed the motor error parameter to zero, because we were focusing on RTs. In sum, our highly-constrained CGS model variant consisted of 9 free parameters (see Table A1). We fitted this model to the data from all four experiments (using one common set of 9 free parameters for all experiments) and thus to 68 free empirical observations in total: 2 (slope, intercept) × 2 (target presence) × 17 (orientation contrasts across the experiments).

The values of these parameters were determined by a simplex routine (Nelder & Mead, 1965) as implemented in Matlab (The Mathworks). We calculated absolute differences between predicted and empirical mean slopes and intercepts per Orientation-Contrast × Target-Presence-cell, divided by the empirical standard errors of intercepts or slopes (across participants) from the respective cell. The simplex routine minimized the total sum of these standardized differences.

Supplement A: Error Rates

Table S1: *Mean Error Rates and Standard Errors (in %) From All Four Experiments (Without Practice Trials).*

	Set size 7		Set size 19	
Contrast	Present	Absent	Present	Absent
		Experiment 1a		
3°	20.4 (4.47)	2.8 (0.74)	28.5 (3.27)	6.7 (1.80)
4°	9.6 (2.15)	1.5 (0.56)	11.5 (2.08)	2.6 (0.65)
5°	4.2 (1.25)	1.8 (0.60)	8.2 (1.75)	3.0 (1.18)
		Experiment 1b		
6°	11.1 (1.54)	3.2 (0.71)	14.4 (1.41)	3.8 (0.94)
8°	7.4 (1.42)	2.9 (0.52)	10.1 (1.75)	2.5 (0.65)
10°	5.4 (1.46)	2.3 (0.59)	6.1 (1.04)	3.2 (0.76)
12°	4.7 (0.93)	2.9 (0.70)	4.8 (1.34)	2.9 (0.77)
22°	3.8 (0.99)	3.6 (0.91)	6.4 (1.39)	2.1 (0.68)
45°	5.7 (1.21)	5.3 (1.01)	4.3 (1.00)	3.1 (0.95)
		Experiment 2		
4°	7.4 (1.70)	1.9 (0.68)	9.3 (1.59)	2.9 (0.88)
5°	3.9 (0.87)	0.8 (0.27)	4.4 (0.92)	1.9 (0.70)
6°	4.2 (1.26)	0.8 (0.37)	3.0 (0.69)	1.9 (0.40)
7°	2.1 (0.46)	0.9 (0.37)	2.9 (0.56)	1.0 (0.42)
		Experiment 3		
1.5°	34.0 (3.24)	10.7 (3.31)	36.8 (3.13)	28.0 (3.05)
6°	4.4 (0.70)	0.8 (0.30)	7.4 (1.97)	0.9 (0.32)
10°	1.5 (0.47)	0.9 (0.30)	2.0 (0.59)	0.5 (0.21)
20°	1.0 (0.29)	1.1 (0.31)	2.0 (0.56)	1.0 (0.24)

Table S2

Analysis of Variance Results for Mean Error Rates (Without Practice Trials).

	Target-present					Target-absent				
Effect	*df*	*F*	η_p^2	ε	p_c	*df*	*F*	η_p^2	ε	p_c
					Experiment 1a					
Contrast	**2,30**	**36.88**	**.71**	**.58**	**< .001**	**2,30**	**4.80**	**.24**	**.85**	**.021**
Set size	**1,15**	**5.68**	**.27**	**1.00**	**.031**	**1,15**	**5.65**	**.27**	**1.00**	**.031**
Interaction	2,30	1.59	.10	.65	.228	2,30	1.05	.07	.93	.360
					Experiment 1b					
Contrast	**5,75**	**22.67**	**.60**	**.63**	**< .001**	5,75	0.97	.06	.73	.427
Set size	1,15	4.15	.22	1.00	.060	1,15	0.68	.04	1.00	.422
Interaction	5,75	1.38	.08	.67	.259	5,75	1.62	.10	.76	.185
					Experiment 2					
Contrast	**3,45**	**15.23**	**.50**	**.57**	**< .001**	3,45	3.01	.17	.55	.077
Set size	1,15	1.88	.11	1.00	.191	**1,15**	**5.36**	**.26**	**1.00**	**.035**
Interaction	3,45	2.00	.12	.73	.148	3,45	0.83	.05	.71	.452
					Experiment 3					
Contrast	**3,45**	**130.75**	**.90**	**.41**	**< .001**	**3,45**	**38.33**	**.72**	**.34**	**< .001**
Set size	1,15	2.67	.15	1.00	.123	**1,15**	**26.24**	**.64**	**1.00**	**< .001**
Interaction	3,45	0.42	.03	.48	.601	**3,45**	**32.07**	**.68**	**.36**	**< .001**

Note. Significant *effects* are printed in bold.

Supplement B: Analyses of Carry-Over Effects

There is a potential alternative explanation for our finding of flat target-present slopes together with steep target-absent slopes within the same contrast condition: When a block with high orientation contrast (thus providing high salience and resulting in flat target-present search slopes) is preceded by a block with low orientation contrast (steep slopes), participants might continue to perform serial search (for some trials) whenever the target does not pop out of the display (thus resulting in steep target-absent slopes). Although this alternative explanation cannot explain the pattern in Experiment 2 (in which all conditions produced flat target-present slopes), it might (partly) explain the respective pattern in Experiment 3, where we observed steep target-present slopes for contrasts of 1.5° and flat target-present slopes for all other conditions (see Table 1). To test for such potential carry-over effects, we separated blocks with 6°, 10°, and 20° targets into those that were preceded by 1.5° targets and those that were preceded by one of the other orientation-contrast conditions (resulting in a 3 × 2 design). As the experiment was not designed for such an analysis, not all cells were filled for each participant. If a data point for a participant was missing for this reason (e.g., none of the 6° blocks of the participant was preceded by a 1.5° block), data from this participant were not included into the respective paired *t*-test (therefore the degrees of freedom in the tables below are lower than in the main analyses and vary across tests). The results, presented in Table C1, show that it did not matter whether a block with flat target-present slopes was or was not preceded by a block with steep target-present slopes.

Table S3.
Statistical Comparison of Target-Absent Search Slopes for Blocks That Were (Yes) vs. Were Not (No) Preceded by a 1.5°-Target Block in Experiment 3.

Contrast	*M* (yes)	*M* (no)	difference	*df*	*t*	*p*	d_z
6°	13.94	17.88	3.94	12	-0.99	.343	-0.27
10°	3.33	2.58	-0.75	11	0.35	.734	0.10
20°	-0.31	1.55	1.87	11	-1.20	.257	-0.35

A potential carry-over effect might occur only on early trials of a block, because later on participants might have adapted to the new orientation contrast. To test this possibility, we separately extracted RTs from the first third (early trials) and the last third (late trials) of each block. Early trials, too, were not influenced by whether or not the block was preceded by a 1.5°-target block (Table C2). Additionally, we tested whether any influence of a preceding 1.5°-target block is stronger on early trials as compared to late trials. This was also not the case (Table C3). In sum, the analyses presented in Table C1-C3 do not provide any evidence for the hypothesis that our observation of flat target-present slopes together with steep target-absent slopes was driven by carry-over effects.

Table S4.

Statistical Comparison of Target-Absent Search Slopes Calculated From Only the First Third of Each Block for Blocks that Were (Yes) vs. Were Not (No) Preceded by a 1.5°-Target Block in Experiment 3.

Contrast	*M* (yes)	*M* (no)	difference	*df*	*t*	*p*	d_z
6°	9.43	16.66	7.23	12	-1.43	.179	-0.40
10°	3.81	4.30	0.49	11	-0.12	.906	-0.04
20°	1.51	2.12	-0.62	11	-0.18	.858	-0.05

Table S5.

Statistical Comparison of the Effect of a Preceding 1.5° Target-Block on the Target-Absent Search Slopes on Early (First Third) vs. Late (Last Third) Trials Within a Block in Experiment 3.

Contrast	*M* (early)	*M* late)	difference	*df*	*t*	*p*	d_z
6°	-7.23	1.01	8.24	12	-1.44	.177	-0.40
10°	-0.49	1.48	1.96	11	-0.44	.667	-0.13
20°	-0.62	-3.68	-3.06	11	0.74	.475	0.21