

Attentional Capture by Salient Color Singleton Distractors Is Modulated by Top-Down Dimensional Set

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Three experiments examined whether salient color singleton distractors automatically interfere with the detection singleton form targets in visual search (e.g., J. Theeuwes, 1992), or whether the degree of interference is top-down modulable. In Experiments 1 and 2, observers started with a pure block of trials, which contained either never a distractor or always a distractor (0% or 100% distractors)—varying the opportunity to learn distractor suppression. In the subsequent trial blocks, the proportion of distractors was systematically varied (within-subjects factor in Experiment 1, between-subjects factor in Experiment 2)—varying the incentive to use distractor suppression. In Experiment 3, observers started with 100% distractors in the first block and were presented with “rare” color or luminance distractors, in addition to “frequent” color distractors, in the second block. The results revealed distractor interference to vary as a function of both the initial experience with distractors and the incentive to suppress them: the interference was larger without relevant practice and with a lesser incentive to apply suppression (Experiments 1–3). This set of findings suggests that distractor interference is top-down modulable.

Keywords: attentional capture, singleton distractor interference, top-down modulation, cross-trial priming

It is commonly accepted that detecting and responding to critical (target) objects in the visual environment is determined by the interplay of two attentional control mechanisms: *goal-driven* (top-down) control, which biases the allocation of attention toward information relevant to current intentions, and *stimulus-driven* (bottom-up) control, that is, the attraction of attention by salient stimuli in the environment. However, the question of whether and how these two types of control interact has recently become the subject of a debate. In particular, while some researchers have maintained that stimulus-driven control of visual attention is cognitively impenetrable, operating in a “preattentive” and automatic fashion (e.g., Cohen & Magen, 1999; Mortier, Theeuwes, & Starreveld, 2005; Theeuwes, Reimann, & Mortier, 2006), others have argued that stimulus-driven control may be modulated by top-down attentional set (e.g., Folk & Remington, 1998, 2006; Folk,

Remington, & Johnston, 1992; Found & Müller, 1996; Müller, Heller, & Ziegler, 1995; Müller & Krummenacher, 2006).

One strand of evidence for the latter position stems from the demonstration of dimension-specific intertrial and cueing effects in cross-dimensional visual search for singleton feature (pop-out) targets. In studying intertrial effects, Found and Müller (1996) had observers search for targets defined by an odd-one-out feature in one of two possible dimensions: color and orientation. That is, the target could be either color defined (a *red* or a *blue* vertical bar among green vertical bars) or orientation defined (a green *left-* or *right-tilted* bar among green vertical bars), with the critical dimension varying randomly across trials. Found and Müller found that observers could discern the presence of the target faster when the target-defining dimension remained the same across consecutive trials (e.g., color-defined target on Trials *N-1* and *N*) compared with when it changed (e.g., orientation target on Trial *N-1*, color target on Trial *N*). This effect of dimension repetition was largely unaffected by whether the target feature was repeated within the unchanged dimension (e.g., red target on Trials *N-1* and *N* vs. blue target on Trial *N-1* and red target on Trial *N*).

In studying cueing effects, Müller, Reimann, and Krummenacher (2003) introduced a trial-by-trial dimensional cueing procedure into the above search task. They observed that target detection was expedited when the target-defining dimension on a given trial was validly indicated in advance by a symbolic cue (e.g., the word “color” indicating that the target was likely defined by an odd-one-out color), relative to trials with neutral and invalid pre-cues (e.g., when a color cue was followed by an orientation-defined target). Even when the likely target-defining feature was directly pre-cued (by presentation of a copy of the target stimulus in

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advance, e.g., a red bar), detection of an alternative, but unlikely, target within the target-defining dimension (color, e.g., a blue bar) was expedited relative to targets defined within the noncued dimension (orientation, e.g., a left- or right-tilted bar). Besides producing dimension-specific cueing costs and benefits, symbolic cues were found to reduce the magnitude of the intertrial effects. For instance, the normal cost of changing the target dimension from Trial $N-1$ to Trial N (estimated on neutral-cue trials) was reduced when the cue on Trial N validly indicated the target-defining dimension. Note that although pre-cueing reduced the intertrial effects, it did not completely abolish them, even with 100% valid cues.

To explain these (and other) effects, Müller and his colleagues (2003) proposed a *dimension-weighting account* (DWA), which is essentially an extension of the guided search model developed by Wolfe and colleagues (e.g., Wolfe, 1994). The DWA assumes that attentional weight can be allocated to different basic visual dimensions (such as orientation, color, motion), with the total weight being limited. Preferential weighting of one visual dimension leads to expedited detection of singleton feature targets defined in this dimension, relative to targets defined in other dimensions. This facilitation results from enhanced coding of feature contrast (saliency) signals within the weighted dimension or amplified transmission of dimension-specific feature contrast signals onto an overall-saliency map of the visual display, which determines the allocation of focal (selective) attention. In contrast, if the target dimension changes across trials, target detection is delayed. This delay may have two causes: either sufficient attentional weight must be shifted from the old to the new target-defining dimension as a precondition for target detection (i.e., to sufficiently amplify the feature contrast signal at the overall-saliency map level), or the target is processed and eventually selected on the basis of the relatively low weight allocated to its defining dimension and the weight shift follows target detection. In any case, there is a weight shift to the new target-defining dimension, which influences the processing of any subsequent target. While this weight shift is largely bottom-up controlled by the presence of a feature contrast signal in a given dimension, it can be top-down modulated when a target is expected to be defined in another dimension. The DWA interprets weighting effects to be preattentive (perceptual) in nature, modulating dimension-based saliency signals prior to the stage of overall-saliency computation, which forms the basis for attentional selection (see Müller & Krummenacher, 2006, for a discussion).

Recently, this view has been challenged by models that assume that the weighting effects described by Müller and his colleagues are postselective, arising at a stage following focal-attentional selection (which is itself top-down impenetrable), during which detected targets are translated into responses (e.g., Mortier et al., 2005; Theeuwes et al., 2006; see also Cohen & Magen, 1999). This challenge has been based, in the main, on findings in *compound* (Duncan, 1985) search tasks in which the detection-relevant target attribute was manipulated independently of the response-relevant attribute. One example is illustrated in Figure 1a: the target is defined by an odd-one-out shape, while the response is determined by the orientation of a small line within the target object. Using variations of this task that were similar to the cross-dimensional singleton search task of Müller and colleagues described above, Theeuwes and colleagues (2006) have recently attempted to chal-

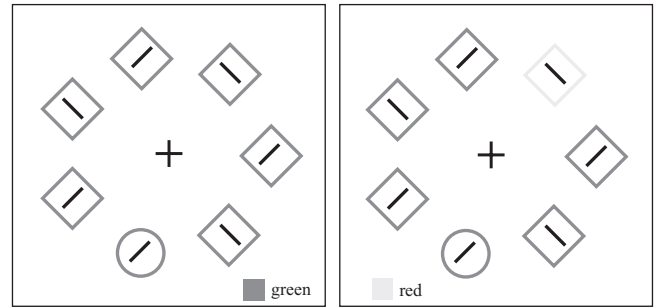


Figure 1. Illustration of the compound search task used by Theeuwes (1992) and in the present study. In the example, observers have to search for a form-defined singleton target (and respond to the slant of the line inside the target form) under conditions in which a color distractor singleton is either absent (Figure 1a, left panel) or present (Figure 1b, right panel) in the display.

lenge the assumption of the DWA that preattentive saliency computations are top-down modulable. However, fundamentally, the challenge is based on an earlier study by Theeuwes (1992) that has been influential in related debates as well (in particular, concerning the hypothesis of contingent attentional capture proposed by Folk et al., 1992). As the present study was designed to reexamine the essential findings of Theeuwes (1992), these will be described in some detail.

In his 1992 study, Theeuwes investigated the effects of the presence of a singleton distractor on the search for a singleton target, with the distractor being defined in a dimension other than the target. In the most interesting condition, the target was defined by shape (e.g., the only diamond among circles), and the distractor was defined by color (e.g., the only red shape among green shapes). In this condition, the distractor was more salient than the target; that is, reaction times (RTs) to the red circle, when it was the only odd-one-out (i.e., the to-be-detected) item in the display, were some 50–60 ms faster than the RTs to the diamond, when it was the only odd-one-out item. Observers performed two trial blocks in counterbalanced order: one without a color distractor (no-distractor block), and one with a color distractor (100% distractor block) present on each trial. The results revealed significant distractor interference: RTs were systematically slowed, by some 25 ms, for the color-distractor relative to no-distractor condition. (Note that Theeuwes did not examine the effects of order of conditions.)

This interference effect seems to be embarrassing to accounts such as DWA (or contingent attentional capture), as color was never the target-defining dimension. The question is, why were observers unable to overcome the interference of the color distractor by actively down-modulating the attentional weight assigned to the color dimension, thereby preventing the distractor from competing for focal attention? One possibility is that observers were insufficiently practiced on the task; that is, the ability to efficiently exclude the color distractor emerges only as a result of extended practice. Theeuwes (1992) investigated this possibility in another experiment in which observers performed 12 alternating (144-trial) blocks of no-distractor and 100% distractor conditions. When examining distractor interference as a function of practice (early, middle, late section of task performance), Theeuwes failed to find

a significant reduction in interference (though there was a suggestion of a decrease from the first to the second section). Theeuwes concluded from this pattern of persistent interference that preselective saliency coding is top-down impenetrable.

However, assuming a race between form- and color-based saliency signals, one would expect the faster coded color signal to win the race on a proportion of trials, even if the weight of the color dimension can be top-down modulated to some extent.¹ Thus, what Theeuwes (1992) may have shown is that the distractor interference cannot be reduced below some minimum value but not that the magnitude of the interference is not top-down modifiable. Note that during preliminary practice (prior to the experiment), Theeuwes' observers performed one block of the distractor condition, with a distractor on each trial, providing an incentive to suppress the distractor (dimension) as much as possible. During this block, observers might have learned to optimally suppress the distractor (dimension). Thus, there was no scope for down-modulating the interference further (below the minimum) over the course of experiment. Theoretically, however, there would have been scope for larger interference effects, had there been a lesser incentive for observers to use a suppression strategy.

In this alternative account of Theeuwes' (1992) findings, the extent of distractor interference is dependent on two factors: (a) acquisition of a top-down suppression strategy during (initial) practice and (b) incentive to use such a suppression strategy. Concerning the former, observers would acquire an effective suppression capability only under conditions that provide a high incentive to actively suppress the irrelevant dimension. Concerning the latter, given that distractor suppression is effortful, observers would employ such a strategy only according to the extent to which the cost of not suppressing the distractor dimension is high overall (i.e., according to the likelihood of a distractor being presented on a trial).² This alternative account was tested in three experiments. In Experiments 1 and 2, we used the same task used by Theeuwes (1992), but examined the influence of both the initial practice regime (0% distractor block vs. 100% distractor block) as well as the incentive to use a suppression strategy (i.e., varying likelihood of distractor trials in the postpractice blocks) on task performance.³ Experiment 3 was a control experiment designed to examine whether stimulus novelty (e.g., Neo & Chua, 2006), rather than top-down control, may account for the variation in interference with distractor frequency, revealed in Experiments 1 and 2.

Experiment 1

In Experiment 1, there were two conditions performed by separate groups of observers. In the first condition ("run-up group"), observers started with a no-distractor block (i.e., 0% distractor trials), providing no grounds to (learn to) suppress distractors; in the subsequent blocks, the proportion of distractor trials was systematically (monotonically) increased (20%, 50%, 80%, 100%), providing an increasing incentive to use distractor suppression. It was expected that distractor interference would be maximal in the first (postpractice) distractor block (20% distractor trials), as observers had not yet learned to use a suppression strategy. Over the subsequent blocks, interference was expected to decline, as the incentive to use a suppression strategy increased. This procedure was reversed in the second condition ("run-down group"). Observ-

ers started with a distractor block (100% distractor trials), providing the maximum incentive to suppress distractors; in the subsequent blocks, the proportion of distractors was systematically decreased (80%, 50%, 20%, 0%). Distractor interference was expected to be lower overall than under the run-up condition, as the practice block provided observers with the maximum incentive to learn to suppress the irrelevant distractor. Furthermore, interference was predicted to be relatively constant across the subsequent blocks, as observers would carry over the distractor suppression strategy acquired initially to the experimental blocks (though there would be some increase in interference as the incentive to use suppression diminishes). By and large, the results were in line with these predictions.

Method

Observers. For the experiment, we recruited 24 observers (students of psychology or computing sciences at the Ludwig Maximilian University of Munich or Technical University of Munich; 54% men, 46% women; mean age, 26 years; age range, 20–31 years; all with normal or corrected-to-normal vision, including color vision). Half the observers were (randomly) allocated to the run-up group and half to the run-down group. Observers were either paid for their participation at a rate of 8 euros per hr (~\$12 U.S.) or received a course credit.

Apparatus, stimuli, and task. Stimuli were presented on a Trinitron (Sony Electronics, San Diego, CA) color cathode ray tube (CRT; 60-Hz refresh rate), controlled by a standard PC. Observers viewed the CRT from a distance of 60 cm, maintained by the use of a chin rest. The screen background was black (0.5 cd/m² in luminance). The stimulus display (illustrated in Figures 1a and 1b) consisted of seven elements equidistantly arranged around the circumference of an imaginary circle (radius 13.5° of visual angle). All nontarget elements were green diamonds; the shape-defined target (present on each trial) was a green circle (see Figure 1a). The color-defined distractor (if present) was a red diamond, replacing a green nontarget diamond (see Figure 1b). The size of the stimulus outline frames was 2.4° (maximum extension); the luminance of the green and red outline frames was matched (9.1 cd/m²). All stimulus outline frames contained a small

¹ Müller et al. (2003; see also Müller & Krummenacher, 2006) never claimed that stimulus-driven weighting processing can be completely counteracted by top-down set. In fact, they proposed that the weight for a given dimension cannot be set to zero; this would be maladaptive, as a currently irrelevant signal in a zero-weighted dimension could then never cause an interrupt and summon focal attention.

² Similar modulatory effects have been described in the task-switching literature (e.g., Botvinick et al., 2001; Goschke, 2000, 2005a; Kerns et al., 2004; Koch & Philipp, 2005; Schuch & Koch, 2003; see General Discussion for more details).

³ Note that uncertainty effects of target (form) features have been investigated in previous studies (e.g., Caputo & Guerra, 1998; Pinto, Olivers, & Theeuwes, 2005), and since distractor (form) features were coupled with the target form, distractor effects have been investigated as well. However, in these studies, the proportion of distractor trials was constant (100% of trials in Pinto et al. and, as far as one can tell, 50% in Caputo & Guerra). Thus, to our knowledge, the present study is the first to systematically examine distractor uncertainty effects in terms of distractor presence or absence.

white line (0.5° in length, 13.7 cd/m^2 in luminance) that was randomly tilted (by 45°) to the left or to the right. Observers' task was to respond as quickly and as accurately as possible to the tilt direction of the line inside the (shape) target stimulus. They responded by pressing the right and left buttons of a serial Microsoft (Redmond, WA) mouse (right tilt—right-hand response, left tilt—left-hand response), with the track ball removed to improve timing accuracy (Segalowitz & Graves, 1990).

In a pilot experiment with a separate group of 10 observers, the relative saliency of the form (target) and the color (distractor) singleton was established. In this experiment, there was always only one target in a block of trials, either a form or a color singleton, and observers were instructed to respond as quickly and accurately to the respective singleton. Half the observers started with a form singleton block and then performed a color singleton block, and vice versa for the other half. On average, RTs to color singletons were faster than RTs to form singletons: 583 ms versus 646 ms (with balanced error rates: 5.4% vs. 5.5%); all 10 observers showed this pattern irrespective of the order of the search tasks. This result indicates that the color singleton was more salient than the form singleton (as was the case in Theeuwes' 1992 study).

Design and procedure. Each trial started with a white fixation cross ($0.5^\circ \times 0.5^\circ$) in the screen center, which remained on until the end of the trial. After 500 ms, the search array was presented and then was terminated by the observer's response or after a maximum duration of 2,000 ms. Consecutive trials were separated by a blank-screen interval of 1,000 ms in duration.

There were two experimental conditions: run-up and run-down. Observers in the run-up group started with a no-distractor block (i.e., 0% of the trials contained a distractor); in the subsequent blocks, the proportion of distractor trials was monotonically increased: from 20% to 50% and from 80% to 100%. Observers in the run-down group started with a 100% distractor block; in the subsequent blocks, the proportion of distractor trials was systematically decreased: from 80% to 50% and from 20% to 0%. Each block consisted of 200 experimental trials (with a brief break after every 50 trials), plus 12 (4×3) warming-up trials (data not recorded). Prior to each block, observers were informed of the

ratio of distractor trials. The total number of trials performed by each observer was 1,000 (5 distractor ratio blocks \times 200 trials, not counting the 5 blocks \times 12 warming-up trials). Prior to the experiment, observers in both groups were presented with 10 samples of both no-distractor and distractor trials. Samples of the latter were presented to make observers understand that even if a distractor was present in the display, they should respond to the orientation of the line within the relevant shape target (while ignoring the irrelevant color distractor).

Results and Discussion

For each distractor ratio condition, individual observers' RTs outside the range of ± 2.5 standard deviations from the mean were discarded as outliers (overall, 1.9% of trials). Error-response trials were also excluded from the analysis (3.1% of all trials). Overall, observers' error rates were relatively balanced across the distractor ratio conditions; an analysis of variance (ANOVA) of the error rates with group (run-up, run-down) as the between-subjects factor and distractor ratio (0%, 20%, 50%, 80%, 100%) as the within-subjects factor failed to reveal any significant effects: group, $F(4, 88) < 1$, *ns*; distractor ratio, $F(1, 22) < 1$, *ns*; interaction, $F(4, 88) < 1$, *ns*.

RT effects. The RT effects are presented in Figures 2a and 2b, for the run-up and the run-down groups, respectively. For both groups, RT performance improved as a function of practice; that is, in the run-up group, RTs became faster overall as the ratio of distractor trials increased, and conversely in the run-down group, RTs became faster as the ratio of distractor trials decreased. Of greater theoretical importance, both groups showed distractor interference within blocks that contained both no-distractor and distractor trials (20%, 50%, and 80% conditions); that is, RTs were generally slower on trials in which a distractor was present rather than absent. However, overall, the amount of interference was greater for the run-up group than for the run-down group (32.7 ms vs. 12.9 ms).

Statistically, the RT data were examined in a mixed-design ANOVA with group as the between-subjects factor and trial type

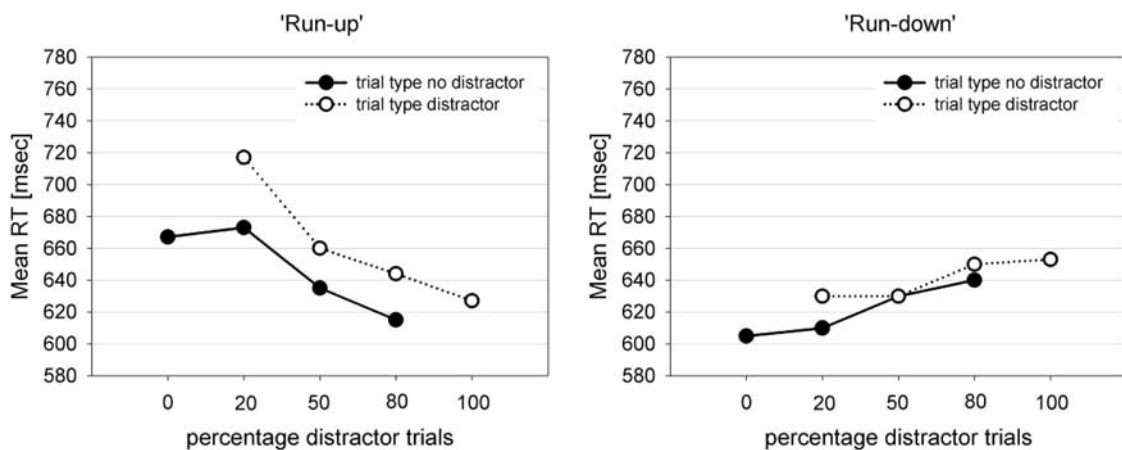


Figure 2. Experiment 1: Mean RTs in the run-up (Figure 2a, left panel) and run-down (Figure 2b, right panel) conditions as a function of the distractor ratio (0%, 20%, 50%, 80%, 100%), separately for distractor (dotted lines) and no-distractor (solid lines) trials.

(no distractor, distractor) and distractor ratio (20%, 50%, 80%) as the within-subjects factors. This ANOVA revealed no significant main effect of group, $F(1, 22) = 1.83, p = .189, MSE = 20,970.36, \eta^2_p = .08$, but a significant main effect of trial type, $F(1, 22) = 67.25, p < .01, MSE = 412.90, \eta^2_p = .85$: longer RTs for distractor compared with no-distractor trials (distractor interference). The main effect of distractor ratio was also significant, $F(1.75, 38.54) = 7.40$ (Huynh–Feldt corrected df), $MSE = 1,092.81, p < .01, \eta^2_p = .55$, due to longer RTs in 20% relative to 50% and 80% distractor blocks. Furthermore, there was a significant Group \times Distractor Ratio interaction, $F(2, 44) = 39.50, p < .01, MSE = 957.16, \eta^2_p = .70$, which essentially reflects a practice effect (recall that the two groups performed the distractor ratio conditions in ascending and descending orders). More important, the Group \times Trial Type interaction was significant, $F(1, 22) = 19.38, p < .01, MSE = 412.90, \eta^2_p = .58$, confirming that the distractor interference was reduced for the run-down group compared with the run-up group.

Separate analyses of the distractor interference effects for the two groups revealed that for the run-up group, the interference was greater for the 20% condition (44.6 ms) than for the 50% and 80% conditions (25.9 ms and 29.6 ms, respectively); there was a significant Distractor Ratio \times Trial Type interaction, $F(2, 22) = 7.30, p < .05, MSE = 488.31, \eta^2_p = .30$. In contrast, for the run-down group, the interference was largely unaffected by the distractor ratio: nonsignificant Distractor Ratio \times Trial Type interaction, $F(2, 22) = 2.32, p > .10, MSE = 221.74, \eta^2_p = .17$.

In view of the practice effects, it is hard to compare the distractor interference directly for a given distractor ratio condition between the run-up and run-down groups. However, at least one comparison is possible: that between the run-up group with 20% distractors and the run-down group with 80% distractors. By the time observers in the two groups performed these conditions, they had an equal amount of practice on the task (in terms of number of trials) However, the run-up group had never encountered a distractor in the first block (i.e., no experience in distractor suppression), while the run-down group had encountered a distractor on

every trial (i.e., experience with distractor suppression). The comparison of distractor interference between the two groups revealed the interference to be significantly larger for the run-up group in the 20% distractor condition than for the run-down group in the 80% distractor condition, 44.6 ms vs. 13.5 ms; $t(22) = 3.23, p < .01$.

In summary, distractor interference was found to be modulated by both the type of initial practice on the task (0% > 100% distractors) and the distractor ratio in a particular trial block (20% > 80%). Even though the latter contrast may be regarded as being confounded by the type of prior practice, this pattern of effects is generally consistent with the hypothesis that the amount of distractor interference depends on both relevant practice—that is, the opportunity to acquire an effective distractor suppression strategy—and the incentive to apply this strategy. This indicates that distractor interference is, at least to some extent, down-modulable. However, what remains unclear is whether the decrease of interference in the run-up group is simply the result of increasing relevant practice on the task or an increasing incentive to use suppression.

Intertrial effects. To examine these influences further, we analyzed RT performance on a given Trial N (no-distractor, distractor) dependent on the immediately preceding Trial $N-1$ (no-distractor, distractor). The relevant data are presented in Figure 3 as a function of the distractor ratio (20%, 50%, 80%), separately for the run-up and the run-down group (Figures 3a and 3b, respectively). As can be seen, RTs were (a) slowest overall on trials on which a distractor (Trial N) was preceded by a no-distractor (Trial $N-1$): 660.8 ms; (b) somewhat faster on trials on which a distractor was preceded by a distractor: 652.4 ms; and (c) fastest on trials on which there was no distractor (relatively independently of whether there was a distractor on the preceding trial): 624.6 ms.

We statistically analyzed the intertrial effects using a mixed-design ANOVA with group (run-up, run-down) as the between-subjects factor and distractor proportion (20%, 50%, 80%) and intertrial transition (no-distractor→no-distractor, distractor→no-distractor, no-distractor→distractor, no-distractor→distractor, distractor→distractor for Tri-

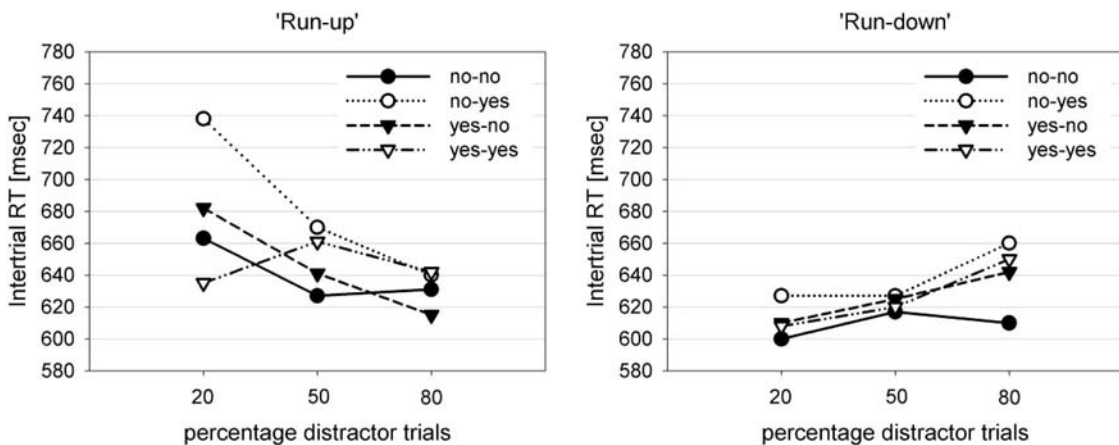


Figure 3. Experiment 1: Mean RTs for the run-up (Figure 3a, left panel) and run-down (Figure 3b, right panel) groups as a function of the proportion of distractors (20%, 50%, 80%), separately for the $N-1 \rightarrow N$ intertrial transitions: no-distractor→no-distractor (no–no), distractor→no-distractor (yes–no), no-distractor→distractor (no–yes), and distractor→distractor (yes–yes).

als $N-1 \rightarrow N$) as the within-subjects factors. This ANOVA revealed the main effect of intertrial transition to be significant, $F(3, 66) = 23.90$, $p < .01$, $MSE = 796.51$, $\eta_p^2 = .52$. The effect pattern suggests that there is general distractor interference, which is, however, attenuated when the preceding trial contains a distractor. This presumably reflects the fact that observers have to suppress the distractor on Trial $N-1$ and then carry over this suppressive task set into the next trial (or they have this strategy more readily available on the next trial), which reduces the distractor interference. While this pattern was evident for both groups of observers, it was significantly less marked for the run-down group than for the run-up group: a significant interaction between group and intertrial transition, $F(3, 66) = 5.07$, $p < .01$, $MSE = 796.51$, $\eta_p^2 = .19$. This suggests that observers in the run-down group exerted generally more top-down suppressive control than observers in the run-up group, perhaps because they had acquired an optimal distractor suppression routine during the first block of 100% distractor trials. Within the run-up group, the above pattern (of reduced distractor interference on Trial N if preceded by a distractor, rather than by no distractor, on Trial $N-1$) was more marked with 20% distractors compared with the 50% and 80% distractor conditions: a significant interaction of Group \times Distractor Ratio \times Intertrial Transition, $F(6, 132) = 3.92$, $p < .01$, $MSE = 794.44$, $\eta_p^2 = .31$. This indicates that observers in the run-up condition used less distractor suppression when the incentive to employ such a strategy was low, perhaps because the observers perceived the overall cost associated with distractors occurring on only the minority of trials as low. Separate ANOVAs for the run-up and run-down conditions revealed a significant interaction between distractor proportion and intertrial transition for the run-up group: $F(4.48, 49.26) = 3.82$ (Huynh-Feldt corrected df), $p < .01$, $MSE = 983.55$, $\eta_p^2 = .49$, but not for the run-down group, $F(6, 66) = 1.44$, $p > .210$, $MSE = 854.80$, $\eta_p^2 = .07$.

Experiment 2

Experiment 1 provided evidence that the distractor interference is modulated by both the type of initial practice on the task (0% > 100% distractors) and the distractor proportion in a particular trial block (20% > 80%). This pattern of effects is generally consistent with the hypothesis that the amount of distractor interference depends on both relevant practice (i.e., the opportunity to acquire an effective distractor suppression strategy) and the incentive to apply this strategy. However, the design of Experiment 1 did not permit us to disentangle the relative effects of these two factors, as the same observers performed all incentive (distractor ratio) conditions in either ascending or descending order, so that general practice effects were overlaying distractor interference effects.

To address this problem, we carried out Experiment 2 using a between-subject design. There were two main conditions, as in Experiment 1: observers started the experiment with either 0% or 100% distractors in the first block of trials. This block was then followed by a second block with varying proportions of distractor trials across observer groups: 0%, 20%, 50%, 80%, or 100%. In this way, it became possible to examine distractor interference effects as a function of both the type of initial practice with distractor suppression and the incentive to suppress distractors in the second block. The predictions were essentially the same as in Experiment 1.

Method

The methodological details were the same as in Experiment 1, except that there were separate groups of observers in each distractor condition. Observers (students of psychology at the Ludwig Maximilian University of Munich) in each group performed two blocks of 200 trials each, the first with either 0% or 100% distractors and the second with 0%, 20%, 50%, 80%, or 100% distractors. Thus, there were 10 groups of observers overall, which we labeled as follows (the first two or three digits denote the distractor ratio in the starting block, the second two or three digits denote the ratio in the second block): 00_00, 00_20, 00_50, 00_80, 00_100, 100_00, 100_20, 100_50, 100_80, 100_100. Ten observers were randomly assigned to each group, with the constraint that group membership was approximately balanced for age (overall mean age, 22.3 years; age range, 21–30 years), sex (72 % women, 28% men), and experience with visual-search experiments (50% experienced).

Results and Discussion

For each experimental group and trial block, individual observers' RTs outside the range of ± 2.5 standard deviations from the mean were discarded as outliers (overall, 2.9 % of trials). Error-response trials were also excluded from the analysis (3.4 % of all trials). Overall, observers' error rates were relatively balanced across the distractor ratio conditions (distractor trials: 3.4 %; no-distractor trials: 3.3 %); an ANOVA of the error rates failed to reveal any significant effects.

RT effects. The RT effects are presented in Figures 4a and 4b, plotted separately for the groups that started the experiment with either 0% or 100% distractor trials in the first block (00_XX and 100_XX groups, respectively). To ascertain whether the separate observer groups were homogeneous, we examined RT performance in the first trial block using a two-way ANOVA with starting block and second block as the factors. This ANOVA revealed a significant main effect of starting block, $F(1, 9) = 6.05$, $MSE = 4,900.55$, $p = .036$, $\eta_p^2 = .40$, with slower RTs for observers who were presented with 100% as compared with 0% distractor trials in the first trial block (100_XX groups vs. 00_XX groups: 704 ms vs. 670 ms). This effect likely reflects the presence of distractor interference in the 100_XX groups. Neither the main effect of the second block, $F(2, 18) = 2.24$, $MSE = 8,018.78$, $p = .083$, $\eta_p^2 = .20$, nor the interaction between the second and the starting blocks, $F(2, 18) = 0.88$, $MSE = 7,552.50$, $p = .48$, $\eta_p^2 = .09$, was significant. This indicates that the separate groups were reasonably homogeneous in terms of baseline RT performance, permitting the relative magnitudes of the distractor interference effects in the second block (variable distractor ratio) to be compared across groups.

As can be seen from Figure 4a, which presents the RT data for observers who were never presented with a distractor in the first block (00_XX groups), distractor interference was largest when the distractor ratio in the second block was 20%, and it decreased when the ratio was increased to 50% and 80%: the magnitude of interference was 77 ms, 36 ms, and 24 ms with 20%, 50%, and 80% distractor trials, respectively. A similar pattern is evident in Figure 4b, which presents the RT data for observers who were presented with a distractor on each trial in the first block (100_XX

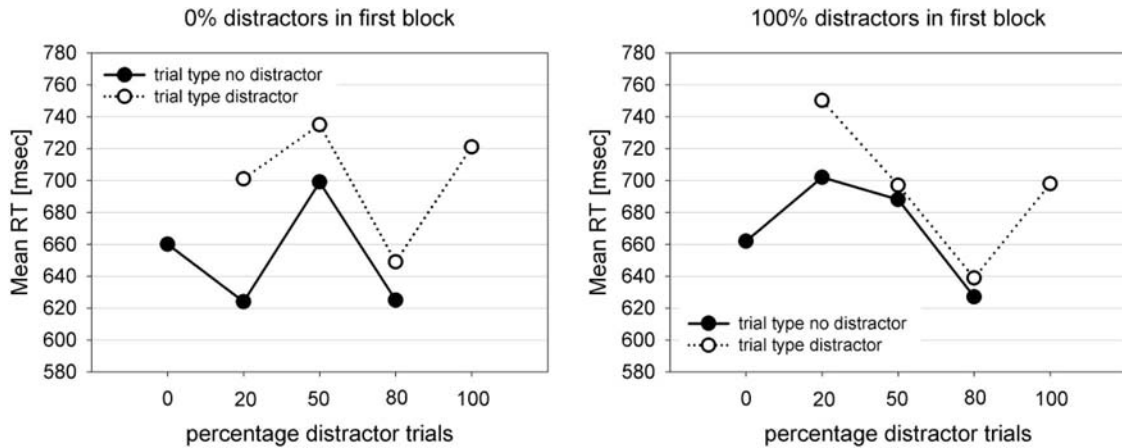


Figure 4. Experiment 2: Mean RTs for the groups that started with 0% distractors (Figure 4a, left panel) and 100% distractors (Figure 4b, right panel) in the first block as a function of the proportion of distractors (0%, 20%, 50%, 80%, 100%) in the second block plotted separately for distractor (dotted lines) and no-distractor (dashed lines) trials.

groups): the magnitude of interference was 48 ms, 9 ms, and 12 ms with 20%, 50%, and 80% distractor trials, respectively. Overall, the interference was less marked for the groups that started with 100%, as compared with those that started with 0%, distractor trials (23 ms vs. 46 ms). In fact, when there were 50% or more distractor trials in the second block, the 100_XX groups showed hardly any interference at all: for 50% distractor trials, 9 ms vs. 0 ms, $t(18) = 1.08, p = .36$; and for 80% distractor trials, 12 ms vs. 0 ms, $t(18) = 1.64, p = .14$. Consistent with this, the observers in the 100_100 group showed a (practice-dependent) RT gain of 24 ms in performance in the second compared with the first block: 698 ms vs. 722 ms, $t(9) = 1.85, p < .05$. In contrast, the 00_XX groups showed significant interference even with 50% and more distractor trials in the second block: for 50% distractor trials, 36 ms vs. 0 ms, $t(18) = 3.29, p < .01$; and for 80% distractor trials, 24 ms vs. 0 ms, $t(18) = 3.91, p < .01$. For observers in the 00_100 group, there was only a (nonreliable) RT gain of 12 ms in the second (100% distractors) compared with the first trial block (0% distractors): 721 ms vs. 709 ms, $t(9) = 1.42, p = .09$.

A mixed-design ANOVA of the RTs in the second block, with starting block (0%, 100% distractors) and second block (20%, 50%, 80% distractors) as the between-subjects factors and distractor (present, absent) as the within-subjects factor revealed the main effects of the second block and distractor to be significant: for the second block, $F(2, 18) = 5.52, p = .014, MSE = 10,342.03, \eta^2_p = .38$; and for distractor, $F(1, 9) = 43.58, p < .001, MSE = 807.69, \eta^2_p = .83$. The following two-way interactions involving distractor were also significant: for Starting Block \times Distractor, $F(1, 9) = 6.21, p = .034, MSE = 644.95, \eta^2_p = .41$, reflecting larger interference for 00_XX than for 100_XX groups (effect of type of initial practice on distractor suppression), and for Second Block \times Distractor, $F(2, 18) = 16.73, MSE = 362.55, p < .01, \eta^2_p = .65$, reflecting the decrease in interference with increasing distractor ratio (effect of incentive to use distractor suppression). No other effects were significant.⁴

In summary, the results of Experiment 2 are in line with those of Experiment 1 but are not confounded by practice effects:

distractor interference was modulated by both the type of initial practice on the task (0% > 100% distractors: effect of type of practice on distractor suppression) and the distractor ratio in a particular trial block (20% > 50% = 80%: effect of incentive to use distractor suppression). It is interesting that groups that had consistent practice with distractors in the first block and a high incentive to suppress distractors (i.e., with 50% and 80% distractors) in the second block showed no significant interference at all.

Intertrial effects. As with Experiment 1, we examined RT performance for the second block on a given Trial N (no-distractor, distractor) dependent on the preceding Trial $N-1$ (no-distractor, distractor). The relevant data are presented in Figure 5 as a function of the distractor ratio (20%, 50%, 80%), separately for the 00_XX and the 100_XX groups (Figures 5a and 5b, respectively). We analyzed RTs on Trial N using a mixed-design ANOVA with starting condition (00_XX, 100_XX) and distractor ratio (20%, 50%, 80%) as the between-subjects factors and transition (no-distractor \rightarrow no-distractor, distractor \rightarrow no-distractor, no-distractor \rightarrow distractor, distractor \rightarrow distractor for Trials $N-1 \rightarrow N$) as the within-subjects factor. Most important, this ANOVA revealed the interaction between starting condition and intertrial transition to be significant: $F(3, 27) = 4.09, p < .05, MSE = 638.61, \eta^2_p = .43$. For the groups that started with the 100% distractor condition

⁴ In the present experiments, RT performance tended to be slow overall (Experiment 1: 655 ms; Experiment 2: 680 ms), for example, slower than in Theeuwes' (1992) experiments (RTs < 600 ms). Regarding the relationship between overall RT speed and distractor interference, van Zoest, Donk, and Theeuwes (2004) reported that the magnitude of interference (indicative of attentional capture by salient distractors) was greater with faster response speeds. Consequently, the reduction in interference with larger proportions of distractors observed in the present experiments may be attributable to the relatively slow overall RTs. However, a re-analysis of the size of the interference effect displayed by observers dependent on their overall response speed failed to establish a significant relationship for the present data (linear regression analysis in Experiment 1: $\beta = .03, t(8) = 0.11, p = .91$; Experiment 2: $\beta = .48, t(8) = 1.55, p = .15$).

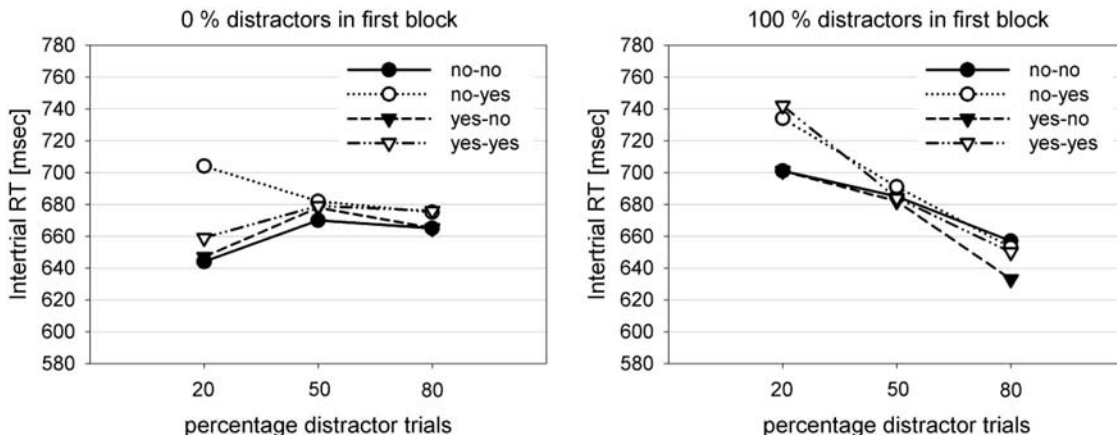


Figure 5. Experiment 2: Mean second-block RTs for the groups that started with 0% distractors (Figure 5a, left panel) and 100% distractors (Figure 5b, right panel) in the first block as a function of the proportion of distractors (20%, 50%, 80%) in the second block plotted separately for the $N-1 \rightarrow N$ intertrial transitions: no-distractor \rightarrow no-distractor (no-no), distractor \rightarrow no-distractor (yes-no), no-distractor \rightarrow distractor (no-yes), and distractor \rightarrow distractor (yes-yes).

(100_XX), RTs were little affected by cross-trial transition: 681 ms for the no-distractor \rightarrow no-distractor, 676 ms for the distractor \rightarrow no-distractor, 693 ms for the no-distractor \rightarrow distractor, and 692 ms for the distractor \rightarrow distractor transitions. Essentially, there was a 14-ms cost when there was a distractor on Trial N , independent of whether there was a distractor on Trial $N-1$. In contrast, for the groups that started with the 0% distractor condition (00_XX), RTs were slower for trials on which a distractor (Trial N) was preceded by a no-distractor on Trial $N-1$ (687 ms) compared with trials on which a distractor in Trial N was preceded by a distractor on Trial $N-1$: 671 ms versus 687 ms; $t(9) = 1.87$, $p < .05$. In addition, there was a tendency for RTs to be fastest on Trial N no-distractor trials (relatively independent of whether there was a Trail $N-1$ distractor: 662 ms versus 671 ms, $t(9) = 1.72$, $p = .14$). This pattern suggests that the 100_XX groups exerted overall more top-down suppressive control than the 00_XX groups. However, while the latter groups showed a marked distractor interference effect when a distractor trial followed a no-distractor trial (25 ms), the interference was reduced, if not abolished, when a distractor trial followed a distractor trial (9 ms). This presumably reflects the fact that when observers have to suppress a distractor on Trial $N-1$, they carry over this suppressive task set onto the next trial (or they have this strategy more readily available on the next trial), which prevents distractor interference.

Furthermore, the Starting Condition \times Distractor Ratio \times Intertrial Transition interaction was significant, $F(6, 54) = 2.29$, $p < .05$, $MSE = 531.41$, $\eta^2_p = .36$. This interaction (illustrated in Figure 5) was due to the 00_XX group of observers who were presented with 20% distractors (00_20) exhibiting greater distractor interference compared with the 00_XX groups presented with a larger proportion of distractors (00_50 and 00_80). More precisely, observers in the 00_20 group showed very pronounced interference when a distractor trial followed a no-distractor trial (58 ms), with a very substantial reduction in interference (to a level similar to those of the 00_50 and 00_80 groups) when a distractor trial followed a distractor trial (some 13 ms). That is, observers in the 00_XX groups used little distractor suppression especially

when the incentive to use such a strategy was low (00_20 group)—in which case suppression was applied only when it was necessitated (or when the strategy was made readily available) by the presence of a distractor on the preceding trial. This is in contrast with observers in the 100_20 group who showed less of an interference effect when a distractor trial followed a no-distractor trial (33 ms compared with 58 ms), but no reduced effect when a distractor trial followed a distractor trial (41 ms compared with 13 ms). An ANOVA of the distractor interference effects for the XX_20 groups revealed this differential behavior to be significant: There was no difference in overall interference between the 00_20 and the 100_20 groups, $F < 1$, and no overall effect of whether or not there was a distractor on the previous trial, $F < 1$. However, the interaction was significant: only for the 00_20 group (but not for the 100_20 group) did the magnitude of interference depend on whether a distractor was present on the preceding trial, $F(1, 12) = 8.56$, $p = .017$, $MSE = 810.68$, $\eta^2_p = .49$. That is, the 100_20 group of observers used a distractor suppression strategy more consistently than the 00_20 group (albeit less efficiently even when the need for suppression was signaled by the preceding trial). Overall, this suggests that the degree to which distractor suppression is applied is under voluntary control.

Experiment 3

Recent findings may lead one to be cautious in interpreting the pattern of results in Experiments 1 and 2 in terms of top-down distractor suppression. In particular, in a letter discrimination task comprising a target presented among three distractor letters, Neo and Chua (2006) found that singleton onset distractors (i.e., one of the three distractors singled out by the onset of four spots in its surround) interfered with target discrimination when they occurred on a minority (17%), but not on the majority (75%), of trials. We should note that infrequent-onset distractors led to a slowing of RTs even when the location of the target letter remained constant across trials, that is, when observers could top-down allocate focal attention to the target location (similar results were reported by

Forster & Lavie, in press; their observers could top-down set themselves to the target color or shape). Neo and Chua (2006) attributed the interference caused by rare distractors to their novelty, with reference to a form of bottom-up, implicit perceptual learning first described by Sokolov (1975) in the context of the orienting response. According to this account, repeated exposure to a set of stimuli results in a cortical representation of the prevailing stimulus environment. Stimuli that are inconsistent with this representation (i.e., novel stimuli) have the potential to capture attention. By contrast, consistent stimuli have lost the power to attract attention, as a result of the longer term adaptation (habituation). Applied to Experiments 1 and 2, the effects of distractor proportion may possibly reflect the bottom-up novelty of a distractor in Sokolovian terms, rather than top-down distractor suppression, as assumed by the DWA.

One way to decide between these alternatives is to introduce a frequent distractor to which the system can adapt (as suggested by Neo & Chua, 2006) and then examine the effects of a rare novel distractor. For example, in a block of trials, there may be a high probability of a color singleton distractor, but its feature value may be relatively novel on some rare trials. Under such conditions, the two accounts would make different predictions: If novelty per se is important, the rare color feature value should lead to attention being captured even if color singletons are relatively frequent. In contrast, if the interference is due to top-down (dimension-based) distractor suppression, rare distractors should not necessarily capture attention. Alternatively, distractor interference may be influenced by both bottom-up stimulus novelty and top-down distractor suppression. In this case, rare distractors should capture attention, but their interference effect should be reduced relative to conditions in which observers had never experienced a (color) distractor, as in the 00_20 condition of Experiment 2. This is because in the former condition (with a frequent and a rare distractor), a color distractor would be present on the majority of trials—which would make a (generalized) distractor suppression strategy more readily available.

We designed Experiment 3 to decide between these hypotheses. Singleton distractors were present on 80% and absent on 20% of the trials. The distractor on 70% of the trial displays was defined by a “frequent” feature value, and the distractor on 10% was defined by a “rare” feature value. Furthermore, the rare distractor feature could be of the same dimension as the frequent feature (i.e., color: frequent feature “red,” infrequent feature “blue”), or it could be a feature in a different dimension (luminance: frequent feature “red,” infrequent feature “bright”).⁵ These two conditions were introduced in order to examine how distractor suppression (if at all demonstrable) may be achieved. According to the DWA, when the target-defining dimension needs to be shielded from interference, the weight is increased for this dimension at the expense of the weight of the standard (frequent) distractor dimension, as well as any other (rare) dimensions (see Zehetleitner, Müller, & Krummenacher, 2007, for a simple competitive scheme in which such a cross-dimensional weight exchange mechanism would be implemented). In this case, the RT interference should be comparable between rare luminance and rare color distractors. Alternatively, shielding may also work by selectively inhibiting the frequent distractor dimension (or at least inhibiting this more than the rare dimension), in which case rare luminance distractors should yield larger RT interference than rare color distractors. In terms of the

novelty account, one could argue that a rare distractor defined in the standard dimension is more similar (dimensionally) to the frequent distractor and, thus, less novel than a rare distractor defined in a nonstandard dimension. As a result, again, luminance distractors should yield larger RT interference (qua being more novel) than rare color distractors

Method

In terms of method, Experiment 3 was similar to Experiments 1 and 2, with the following exceptions:

Observers. In Experiment 3, participants were 24 new observers (from the subject panel of the Department of Psychology, Ludwig Maximilian University of Munich). These observers (42% men, 58% women; mean age, 25.5 years) all had normal or corrected-to-normal color vision.

Stimuli. Four types of singletons were used. The target singleton (present on each trial) was a green circle. The distractor singletons were red, blue, or bright-green diamonds. The luminance values of the green target and of the red and blue distractors were matched (9.1 cd/m²). The luminance of the bright-green distractor singleton was roughly twice that of the other singletons (22.8 cd/m²).

Design and procedure. Singleton distractors were present on 80% and absent on 20% of the trials. In the color condition, rare singleton distractors, presented on 10% of the trials, were blue diamonds. In the luminance condition, rare singleton distractors, also present on 10% of the trials, were bright-green diamonds. In both conditions, frequent distractors (present on 70% of the trials) were red diamonds.

Twelve observers performed in the color condition, and 12 performed in the luminance condition. In both conditions, observers performed a total of 450 trials (9 blocks × 50 trials): 315 trials (70%) with the frequent distractor present, 45 trials (10%) with the rare distractor present, and 90 trials (20%) without singleton distractor. Before the experiment, observers performed a practice block of 200 trials on which a (frequent) distractor was present on each trial. This was intended to familiarize observers with shape target and color distractor singletons, so that they could acquire a color suppression strategy (DWA) or learn to incorporate color distractors in their internal model of the search environment (novelty account).

Pre-experimental testing of the relative saliencies of the target and distractor singletons showed that whenever there was only one target in a block of trials (i.e., either a form, color, or luminance singleton) and observers had to indicate the orientation of a line presented inside the respective singleton, on average, RTs to color and luminance singletons did not significantly differ from each other but were faster than RTs for shape singletons (626 ms for red, 624 ms for blue, and 632 ms for luminance vs. 685 ms for shape singletons; all *ps* < .01).

⁵ It may be debatable whether color- and luminance-defined items are really different dimensionally. However, at least when considered in terms of early visual feature coding, color signals are as different from luminance signals as they are from orientation, or shape, signals: in both cases, there are feature analyzers sensitive to either one or the other, or to both dimensions (color and luminance: Johnson, Hawken, & Shapley, 2001; color and orientation: Friedman, Zhou, & von der Heydt, 2003).

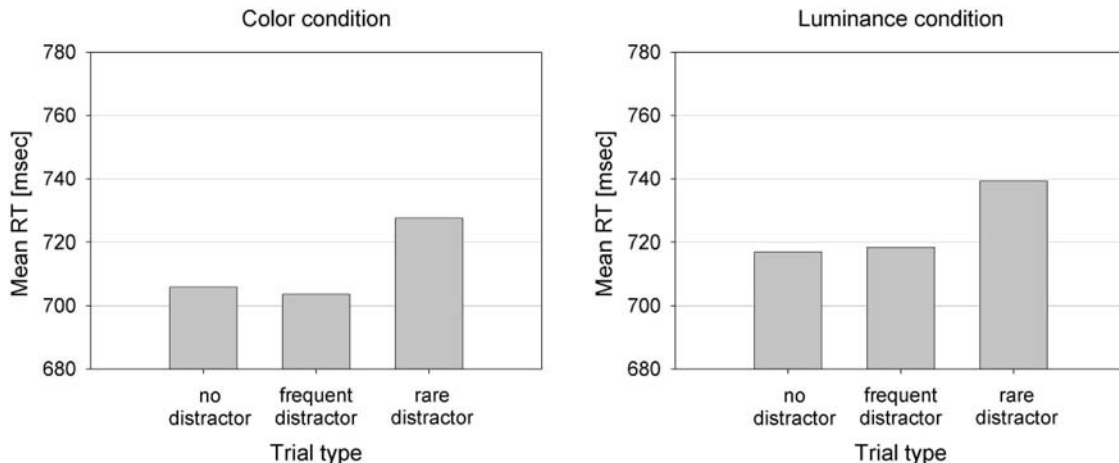


Figure 6. Experiment 3: Mean correct RTs to the shape singleton target on no-distractor, frequent-distractor, and rare-distractor trials plotted separately for the groups with a rare color (Figure 6A, left panel) and a rare luminance (Figure 6B, right panel) distractor.

Results and Discussion

Overall, 2.8% of the trials were discarded as outliers from further analyses. Also, trials on which an error response occurred were excluded from the analysis (3.2% of all trials). Response errors were well balanced among frequent (3.3%), rare (3.4%), and no-distractor (3.0%) trials. A mixed-design ANOVA of the error rates with trial type (no-distractor, frequent distractor, rare distractor) as the within-subjects factor and search condition (color- or luminance-defined rare distractor) as the between-subjects factor revealed no significant effects.

RT effects. Figure 6 presents the mean correct RTs to the singleton target as a function of trial type (frequent, rare, no-distractor trial), separately for the color (left panel) and luminance (right panel) conditions. As can be seen, while RTs were almost comparable between frequent-distractor and no-distractor trials (color condition: 704 ms and 706 ms; luminance condition: 719 ms and 717 ms), they were somewhat slower on rare-distractor than on no-distractor trials (color condition: 728 ms and 706 ms; luminance condition: 739 ms and 717 ms). This was confirmed by a mixed-design ANOVA with trial type (no-distractor, frequent distractor, rare distractor) as the within-subjects factor and search condition (color- or luminance-defined rare distractor) as the between-subjects factor, which revealed the main effect of trial type to be significant, $F(2, 22) = 15.67, p < .01, \text{MSE} = 249.93, \eta^2_p = .59$. No further effects were significant. The main effect of trial type indicates that rare distractors have the potential to capture attention, even when frequent distractors occur on the majority of trials.

However, although rare distractors resulted in an RT disadvantage of 22 ms (data combined across the color and luminance conditions), the disadvantage was much larger (77 ms) when observers had never had experienced singleton distractors, that is, in the 00_20 condition of Experiment 2, in which observers had not been presented with any distractors in the first block of trials and received a (color) distractor only rarely in the second block (i.e., independent group), one-tailed $t(18) = 3.81, p < .01$.⁶

Thus, singleton distractor interference cannot entirely be explained by stimulus novelty (22-ms effect); rather a substantial, and numerically larger, part of the interference effect can be attributed to distractor suppression (55-ms effect). In other words, bottom-up novelty effects are reduced when observers have learned to operate a top-down distractor suppression strategy. Furthermore, given that rare color and luminance distractors led to almost identical interference effects (23 ms and 22 ms, respectively) in Experiment 3, it is also possible to conclude that top-down distractor suppression modulates attentional processing not only in the color but also in other dimensions (at least the luminance dimension).

Intertrial effects. Intertrial effects were also analyzed for Experiment 3⁷ because they are potentially informative about the mechanisms underlying distractor interference. Experiments 1 and 2 had shown that that intertrial effects were largely dependent on the likelihood of a distractor occurring on a trial: when distractors were relatively frequent (i.e., with 50% or 80% distractors), the magnitude of the interference effect on Trial N was nearly independent of whether a distractor was present on the preceding Trial $N-1$. In contrast, with relatively infrequent distractors (20%), a large interference effect resulted on Trial N when there was no distractor on Trial $N-1$ but a substantially reduced effect when

⁶ We further analyzed RTs in Experiment 3 dependent on whether rare distractors were presented on Trials 1 through 225 (“unpracticed” performance) or Trials 226 through 450 (“practiced” performance). A repeated-measures ANOVA, with practice and rare distractor type as factors, revealed neither the main effect of practice, $F(1, 11) = 2.02, p = .18, \text{MSE} = 1,040.21, \eta^2_p = .16$ (unpracticed vs. practiced performance: 31-ms vs. 17-ms distractor effect) nor the Practice \times Distractor Type interaction, $F(1, 11) = 0.31, p = .58, \text{MSE} = 467.82, \eta^2_p = .03$, to be significant. Thus, it is unlikely that the higher number of trials in Experiment 3 (450) relative to the number in Experiment 2 (200 in the 00_20 condition) led to a smaller interference effect in the former experiment.

⁷ This account of intertrial effects was suggested to us by an anonymous reviewer.

there was a distractor. Thus, given that the overall distractor probability was high in Experiment 3, one would expect little effect of the intertrial transition on distractor interference, at least with frequent singleton distractors on Trial N . This was indeed the case: with Trial $N-1$ containing no distractor, a frequent distractor, or a rare distractor, the RTs on a frequent-distractor Trial N were nearly equivalent (706 ms, 710 ms, and 701 ms, respectively).

While this would be consistent with either of the alternative accounts (either observers have a top-down suppression strategy readily available, or a frequent distractor fails to activate capture mechanisms responding to stimulus novelty), the two accounts would differ with respect to the effect of a rare distractor on Trial N . Assuming that distractor interference is modulable by top-down control (DWA), the interference generated by rare distractors on Trial N should also be relatively uninfluenced by whether a (rare) distractor was present on Trial $N-1$ —because top-down distractor suppression should have a similar influence on the processing of rare as well as frequent distractors. In contrast, assuming distractor interference to be under bottom-up control (stimulus novelty), Trial N interference by rare distractors should show a dependency on the presence versus the absence of a (rare) distractor on Trial $N-1$. In particular, with no distractor (or a frequent distractor) on Trial $N-1$, a rare distractor on Trial N should cause a relatively large interference effect (in the same way as a rare distractor caused a large interference in Experiment 2 when it followed a no-distractor trial); in contrast, with a rare distractor on Trial $N-1$, the interference caused by a repeated rare distractor on Trial N should be significantly reduced. This is because, according to Sokolov's (1975) novelty-based account, the presentation of a rare distractor may be assumed to lead to a "local" updating of the cortical model (i.e., on Trial $N-1$, the rare distractor's stimulation profile may be partly incorporated into the neuronal model). This in turn would reduce the Trial N interference effect, relative to the absence of a distractor or the presence of a frequent distractor on Trial $N-1$.⁸ However, the data of Experiment 3 revealed no evidence of such a pattern: with Trial $N-1$ containing no distractor, a frequent distractor, or a rare distractor, the RTs on a rare-distractor Trial N were near-equivalent (730 ms, 729 ms, and 740 ms, respectively). If anything, RTs were somewhat faster when a rare-distractor trial was preceded by a no-distractor trial, compared with a rare-distractor trial (no→rare RT minus rare→rare RT: -9 ms) or a frequent-distractor trial (no→rare RT minus frequent→rare RT: -7 ms).

Statistically, the intertrial effects were examined in a mixed-design ANOVA with Trial $N-1$ ($N-1$ no-distractor, $N-1$ frequent distractor, $N-1$ rare distractor) and distractor type (N frequent distractor, N rare distractor) as the within-subject variables and search condition (color rare distractor, luminance rare distractor) as the between-subject variable. There was a significant main effect of distractor type, $F(1, 11) = 7.79$, $p < .01$, $MSE = 1,217.32$, $\eta^2_p = .41$, with somewhat slower RTs when there was a rare, as compared with a frequent, distractor in the current search display (733 ms vs. 706 ms). Most interesting was the finding that neither the main effect of Trial $N-1$, $F(2, 22) < 1$, ns , nor the theoretically important interaction of Trial $N-1$ with distractor type, $F(4, 44) < 1$, ns , was significant. This indicates that Trial N interference effects were independent of the presence and type of distractor on Trial $N-1$.

Thus, the results of the intertrial analysis cast further doubts on a pure bottom-up explanation of the distractor interference effects; in particular, there was no evidence of any local updating of the prevailing cortical representation of the task situation. Instead the results suggest that distractor interference and its on-line (cross-trial) modulation with rare distractors in Experiments 1 and 2 were largely under top-down control.

General Discussion

Summary of Results

Three experiments examined whether salient color singleton distractors automatically interfered with the detection of singleton form targets in visual search (e.g., Theeuwes, 1992), or whether the degree of interference is top-down modulable. In Experiments 1 and 2, observers started with a pure block of trials, which contained either "never a distractor" or "always a distractor" (0% and 100% distractors)—varying the opportunity to learn distractor suppression. In the subsequent trial blocks, the proportion of distractors was systematically varied (20%, 50%, 80%; within-subjects in Experiment 1; between-subjects factor in Experiment 2)—providing differential incentives to use distractor suppression. In Experiment 3, observers started with a pure block of trials always containing a (frequent) distractor; in the second block, the frequent distractor as well as a rare distractor, the latter defined in either the same or a different dimension, could be presented. The results revealed that the distractor interference varied as a function of both the initial experience with distractors and the incentive to suppress them: the interference was larger without relevant practice and with less incentive to apply suppression. The practice effects are consistent with Leber and Egeth (2006), who found carryover of attentional set from one condition, in which one particular strategy was required to perform the task, to another condition, in which observers had a choice between two possible strategies. Furthermore, Experiment 3 provided evidence that the large distractor interference seen when observers have little relevant practice and little incentive to use distractor suppression only partly reflects a novelty effect of the rare distractor (though novelty significantly influenced the distractor interference).

This set of findings shows that distractor interference is top-down modulable—as predicted by the DWA (e.g., Müller et al., 1995, 2003). According to this account, observers can top-down modulate the weight assigned to a given dimension: that is, up-modulate the weight for the target dimension or down-modulate the weight for the distractor dimension. This enhances the saliency signal produced by the target at the master map level, while attenuating that generated by the distractor, so that the target is more likely to win the competition for focal attention (which is a prerequisite for deciding on the compound-task response). Because operating a top-down dimensional weight set is effortful, the extent to which it is (consistently) used depends on the incentive to do so: the greater the likelihood of a distractor potentially causing interference, the higher the incentive to operate a top-down set. Also,

⁸ In order to investigate this issue in Experiment 3, we "pseudo-randomized" the order of trial types to ensure sufficient numbers of trials for cases in which a rare distractor followed a rare-distractor trial and cases in which a rare distractor followed a no-distractor trial.

the efficient use of this set depends on relevant practice, perhaps to optimally tune the weight distribution for the target and distractor dimensions. The DWA can also explain the intertrial effects, especially the differential effects between the 00_20 and 100_20 groups of observers in Experiment 2: the 00_20 group applied a lot of reactive top-down control after encountering a distractor; the 100_20 used more consistent control whether a distractor was encountered or not. Thus, dimension weighting provides a straightforward account of the present set of findings. Alternative accounts of singleton search will be considered in the following sections.

Relation to Other Accounts of Singleton Search

Automatic-attentional-capture account. The automatic-attentional-capture account, as proposed by Theeuwes (1992, 1996, 2004), is at odds with the present findings. It cannot explain, without giving up its core assumption, that attentional capture by a salient singleton distractor—that is, a singleton more salient than the target—is (top-down) modulated by relevant practice and the incentive to apply distractor suppression. Particularly at variance with the automatic-capture hypothesis is the finding that, given relevant practice and sufficient incentive, the interference can be reduced almost completely to a nonsignificant level.

Perhaps one way out for the automatic-capture account would be to propose that attention is always captured by the salient distractor but that, through practice, observers acquire an automatic, rather than a top-down, routine (e.g., in terms of the *instance theory* of automaticity described by Logan, 1990, 2002) that permits them to circumvent the distractor interference. For example, along the lines suggested by Theeuwes, Atchley, and Kramer (2000), observers may learn to quickly disengage focal attention from the salient distractor singleton (after it has summoned attention and been found not to match the target description) and reallocate it to the target singleton. This rapid disengagement routine, once invoked by a distractor on a trial, is then passively carried over to, or is more readily available on, the next trial. When distractors are rarely encountered, there would be less frequent carryover of this routine, giving rise to increased overall interference.⁹

Whether disengagement and reallocation of focal attention are at all possible within a few milliseconds—specifically, 12 ms in the 100_50 and 9 ms in the 100_80 conditions of Experiment 2—is questionable (according to Theeuwes et al., 2000, who systematically varied the stimulus onset asynchrony, or SOA, between distractor and target presentation, recovery from capture would take in excess of 100 ms; see also Folk & Remington, 2006, for a critical assessment of the rapid-recovery hypothesis). However, even conceding this for the sake of argument, the extended automatic-capture account is inconsistent with the intertrial effects obtained in Experiment 2. Recall that observers in the 00_20 group applied distractor suppression only when there was a distractor on the preceding trial, which is potentially consistent with the revised automatic-capture account. In contrast, observers in the 100_20 group showed less of an interference effect when a distractor trial followed a no-distractor trial (100_20: 33 ms; 00_20: 58 ms), but no reduced effect when a distractor trial followed a distractor trial (100_20: 41 ms vs. 33 ms; 00_20: 13 ms vs. 58 ms; data for the no→yes and yes→yes transitions, respectively). That is, the 100_20 group of observers applied distractor suppression more

consistently than the 00_20 group, independently of whether suppression was required on the preceding trial. Overall, this implies that the degree to which distractor suppression is used is under top-down (rather than automatic) control.

Contingent-attentional-capture account. According to the contingent-capture account of Folk, Remington, and colleagues (e.g., Folk et al., 1992), attentional capture by an irrelevant singleton depends on the perceptual task set: a singleton captures attention only, or more readily, if it fits with the set developed to detect the target on a trial. Given this, it is difficult to use a strong version of the contingent-capture account to explain why a task-irrelevant color distractor would capture attention when observers are consistently searching for a form singleton, as the color singleton should be blocked from capturing attention when the system is set for form (for a recent review of the long-running debate between Theeuwes and collaborators and Folk and collaborators, see Folk & Remington, 2006).¹⁰ However, the modulation of the capture effect demonstrated in the present study would fit with a modified version of contingent attentional capture, according to which the set does work in a graded, rather than an all-or-nothing, manner, continuously regulating the relative power of competing signals to summon attention. Given that sets can be developed for visual dimensions, this modified version of the contingent-capture hypothesis would reduce to the DWA, which assumes that the weights assigned to competing dimensions are continuous values.

Search-mode account. In the search-mode account (Bacon & Egeth, 1994), distractor interference is dependent on the observers operating in a singleton search mode. That is, as long as the observers search for a (feature) singleton, another singleton will cause interference. This is not wholly consistent with the present data, which revealed the degree of interference to be dependent on prior practice with distractors and the incentive to suppress them. In order to explain this finding in terms of the singleton-search-mode account, one would have to assume that, given relevant practice, observers change from a singleton search mode to a nonsingleton mode, that is, to filtering on the basis of the target-defining (form) feature. However, this fails to explain the present finding that color distractor interference was largest on trials that were preceded by a no-distractor trial, particularly in the XX_20 conditions. Of course, it could be that observers in the XX_20 conditions adopted a singleton search mode by default, changing to a more effortful, feature-based search mode only after encountering a distractor on a trial; this set is then maintained for a trial or so until it reverts to the default singleton search mode. However, dependent on the type of practice, this adjustment would have to have occurred only in the 00_20 group of observers, as the 100_20 group showed much less trial-based adjustment of the search mode

⁹ In this account, it is not the incentive as such (i.e., in terms of a top-down influence) that determines distractor interference but rather the frequency with which a distractor is encountered that determines the acquisition and availability of a rapid-disengagement routine.

¹⁰ In the meantime, there have been several reports that when searching for a feature singleton target in a given dimension, it is harder to suppress a feature singleton distractor defined in the same dimension than a distractor defined in a different dimension (Meeter & Theeuwes, 2006; Olivers & Meeter, in press). This is consistent with the contingent-capture hypothesis (and, of course, with the DWA).

when encountering a distractor. Thus, arguably, the results are more parsimoniously explained in terms of observers exerting top-down control of the degree of distractor suppression, rather than in terms of observers adopting qualitatively different (i.e., mutually exclusive) search sets.¹¹

Ambiguity-resolution account. In terms of the ambiguity-resolution account (Meeter & Olivers, 2006; Olivers & Meeter, 2006), search efficiency is determined by the ambiguity in establishing the presence of a target or its location (as well as by the ambiguity in deciding on a response): the greater the target ambiguity, the longer the search will take. Within this context, the critical question is how to rate the ambiguity in, say, the XX_20 condition relative to the XX_80 conditions of the present experiments. Arguably, the most plausible assumption would be that the (overall) ambiguity is greater when a distractor is nearly always present, compared with when a distractor is hardly ever present. Yet, the greater the likelihood that a distractor was present, the more efficient was the search. This would be hard to explain in terms of target ambiguity. Alternatively, one could assume the opposite: that there is less target ambiguity on a given trial when a distractor is nearly always present (or, rather, when ambiguity has to be resolved consistently). However, this would presuppose that there must be an efficient strategy to reduce the ambiguity—either in terms of adopting a feature-based search mode or in terms of top-down suppression of saliency signals in the irrelevant dimension. Consequently, the ambiguity resolution hypothesis alone would not explain the present pattern of findings; mechanisms specified in other accounts would be needed. This is acknowledged by Olivers and Meeter (2006) who stated that “when the target is not the only unique item in the display, but accompanied by a unique . . . distractor [defined in a nontarget dimension], the exact identity of the target then becomes important and its defining dimension may receive additional activation” (p. 3).

Positional-priming account. According to the positional-priming account (Kumada & Humphreys, 2002), singleton distractor interference on Trial *N* is dependent on the presence of a distractor on the immediately preceding Trial *N-1*, rather than on the presence of a distractor on Trial *N*. This proposal is based on the finding of slowed RTs when the form singleton target on Trial *N* was presented at the location (or in the hemifield) occupied by a color singleton distractor, rather than the target, on Trial *N-1*. This inhibitory cross-trial effect was observed in both “pure” distractor blocks (in which a distractor was present in 100% of the trials, as in Theeuwes, 1992), and “mixed” blocks (in which distractor and no-distractor trials were presented randomly intermixed). It is interesting that in the latter blocks, RTs on Trial *N* were independent of whether the display did or did not contain a distractor. In this condition, each type of Trial *N*, distractor or no-distractor, was equally likely preceded by a distractor or a no-distractor trial, with positional inhibitory priming causing interference only with a distractor on Trial *N-1*. As a result, the overall effect of positional inhibitory priming from Trial *N-1* onto Trial *N* would be balanced between distractor and no-distractor trials. This is in contrast with pure blocks, in which a distractor Trial *N* was always preceded by a distractor Trial *N-1*. Kumada and Humphreys (2002) therefore surmised that automatic positional (inhibitory) priming may well account for the pattern of distractor interference effects on Trial *N*, that is, the absence of interference with mixed blocks and its presence with pure blocks.

According to this account, search RTs should become slower overall as the proportion of distractor trials increases, because of an increasing number of trials affected by positional inhibitory priming generated by the presence of a distractor on Trial *N-1*. However, the results of the present experiments are inconsistent with this prediction. Rather, the Trial *N* RTs became faster as the proportion of distractor trials increased (669 ms, 666 ms, and 631 ms for the 20%, 50%, and 80% distractor conditions, respectively; data combined across all relevant conditions in Experiments 1 and 2). This was true for (distractor or no-distractor) Trials *N* that were preceded by a distractor on Trial *N-1* (680 ms, 662 ms, and 650 ms for 20%, 50%, and 80% distractor conditions, respectively). Given this finding, the only way to explain the present findings within the positional-priming framework would be to assume that the degree to which a distractor location is inhibited or the amount of inhibition carried over across trials is under top-down control: the greater the likelihood of a distractor, the lower the inhibition is. However, this additional assumption would be at odds with the original motivation for proposing this account: to explain singleton distractor interference under conditions of consistent distractor presentation. Thus, although a singleton distractor on Trial *N-1* may well cause locational inhibition on Trial *N*, cross-trial positional priming would have been only a minor source of interference in the present study. (This is so not least of all because displays in the present study contained seven items/item positions, as compared with only four in the study of Kumada and Humphreys, thus diluting cross-trial positional priming effects.) Consequently, it is fair to conclude, as acknowledged by Kumada and Humphreys (2002), that search performance may not only be influenced by—relatively immutable—positional priming but also by “strategic aspects of subjects” (p. 501)—such as top-down suppression of the distractor dimension when the incentive is sufficiently high.

In summary, the DWA, as well as the contingent-capture and singleton-search-mode accounts, provides a plausible explanation for the present results; the two latter accounts, however, explain the results only when additional assumptions are incorporated into the theoretical framework (the same applies to the positional-priming account). In contrast, the automatic-capture account is clearly challenged by the present findings.

Relation to Accounts of Task Switching

The presence of a salient distractor in the search display may be considered to generate a “response conflict” as to where to allocate focal attention, which would have to be prevented or resolved (e.g., de Fockert, Rees, Frith, & Lavie, 2004; Lavie & de Fockert, 2006). Given this point, the present results exhibit a clear resem-

¹¹ Bacon and Egeth (1994) found that once their observers had adopted a feature search mode in response to other, non-target-form singletons being introduced in the display, they persisted in using this mode even on those infrequent trials in which the target happened to be the only form singleton (i.e., even on those trials, there was no interference by a color singleton distractor). That is, once a feature search mode was set, observers continued to use it throughout a block of trials (see also Leber & Egeth, 2006). This is in contrast with the present findings, which suggest that the search mode is highly variable across trials (at least for the 00_20 groups of observers).

blance to recent work on task switching, in which the adjustment of cognitive control as a result of response conflict (for example, in a Stroop color-vs.-word task in which observers are pre-cued to respond to either the word meaning or the print color of the upcoming color word) has received a great deal of interest. Two different hypotheses have been proposed. The first derives from the conflict monitoring theory of Botvinick, Braver, Carter, Barch, and Cohen (2001), who assumed that in the case of a response conflict, the anterior cingulate cortex signals an increased demand for cognitive control, which leads to an enhanced mobilization of top-down control on the next trial, presumably mediated by the dorsolateral prefrontal cortex. This model predicts less interference from distracting sources of information on trials following a response conflict, because of “high conflict on incongruent trials leading to the recruitment of greater cognitive control on the subsequent trial” (Kerns et al., 2004, p. 1024). An alternative hypothesis has been proposed by Goschke (2000, 2005a): *conflict-triggered goal shielding*. This hypothesis holds that a response conflict leads to increased shielding of the currently active goal and an enhanced inhibition of distracting stimulus dimensions (and/or competing task sets) on the current trial, which is carried over to the next trial (see also Koch & Philipp, 2005, and Schuch & Koch, 2003). Note that both accounts may be compared with regard to the mechanisms regulating task interference in that Goschke’s (2000, 2005a) facilitation (shielding) of the relevant and inhibition of the irrelevant task could also be considered as instantiations of Botvinick et al.’s (2001) cognitive control mechanism. Both accounts predict reduced interference (i.e., a reduced incongruency effect) on task-repeat trials, which has been confirmed empirically by Kerns et al. (2004) and Goschke and Koch (as reported in Goschke, 2005b). It should be noted that Goschke and Koch also found evidence for inhibition of the distracting stimulus dimension: both the costs of switching from one to the other task and the incongruency costs on task-switch trials were larger after incongruent than congruent trials. After an incongruent trial ($N-1$), the stimulus dimension response-relevant on the task-switch trial (N) is inhibited, giving rise to a larger switch cost. Also, as the stimulus dimension task-relevant on Trial $N-1$ is not inhibited, it causes a greater cost on the switch Trial N when it specifies an incongruent response to that required by the relevant dimension. (While this pattern was found for short task-cue-to-stimulus SOAs, it was nonsignificant at longer SOAs. Along the lines of Botvinick et al. (2001), this finding may be taken to suggest that response conflict on Trial $N-1$ leads to increased mobilization of top-down control on Trial N that, given sufficient time, attenuates carryover effects of conflict-triggered distractor inhibition.) The analogy to the present results is clear: assuming that the registration of a distractor on a given trial causes a response conflict as to where to allocate focal attention, the ensuing demand for increased top-down control would attenuate distractor interference on the next trial, for instance by inhibiting (i.e., down-modulating the weight of) the distractor dimension.

Brain Mechanisms Involved in the Modulation of Distractor Interference

Recent results from functional magnetic resonance imaging (fMRI) studies of distractor interference in visual search are generally consistent with the modulation of the RT effects demon-

strated in the present experiments. Observers in a study by de Fockert et al. (2004; see also Lavie & de Fockert, 2006) were presented with a compound-search task adapted from Theeuwes (1992), with displays either containing or not containing a color singleton distractor. To identify the neural correlates of attentional capture by an irrelevant distractor, de Fockert et al. compared activity in the presence (vs. absence) of a color singleton distractor to activity in the presence (vs. absence) of a color singleton target (examination of this interaction required a factorial design that was realized through the introduction of, among others, trials in which the form-defined target could also be a color singleton). Behaviorally, there was a distractor interference effect of nearly 100 ms (according to Lavie & de Fockert, 2006, this large effect was likely due to the fact that the experiment included trials on which the form-defined target was also a color singleton). The fMRI data revealed differential activity associated with the presence of a color singleton distractor (compared with the presence of a color singleton target) in bilateral superior parietal cortex (BA 7) and in left lateral precentral gyrus (BA 6) of frontal cortex. The RT interference effect was significantly negatively correlated with activity in left frontal cortex, whereas no such correlation was found with activity in the superior parietal cortex. Lavie and de Fockert (2006) interpreted this pattern as follows:

The activity in superior parietal cortex may reflect stimulus-driven shifts of spatial attention toward the irrelevant singleton distractor. . . . As such, attention may always be captured by the more salient distractor . . . , thus precluding any correlation with behavioural interference effects. The extent to which the irrelevant singleton distractor . . . will produce interference on behaviour, however, may be determined by the extent to which frontal cortex exerts a strong or weak top-down control signal (in order to resolve the competition between the target and the capturing distractor). (pp. 870–871)

In findings consistent with the involvement of frontal lobe control mechanisms in determining the extent of distractor interference, Lavie and de Fockert (2006) also reported that when (frontal) working memory (WM) functions were occupied by a high-load WM task, the interference effect caused by a distractor in a concurrent visual search task was significantly reduced. (See also Dreisbach & Goschke, 2004, and Goschke & Reimann, 2005, who found that the distractor interference was increased when background monitoring was encouraged by the presentation of a positive emotional stimulus prior to the search display and was reduced when it was discouraged by the presentation of a negative emotional stimulus before the search display.)

While Lavie and de Fockert’s interpretation of the frontal activation as reflecting top-down control is not contentious, their interpretation of the superior parietal (SPL) activation, which did not correlate with behavioral interference measures, appears at odds with the present proposal that the power of the salient color singleton distractor to capture attention is under top-down control. However, other interpretations are equally feasible. In particular, given the temporally sluggish nature of the blood oxygenation level dependent (BOLD) signals, the SPL activation may also be explained by SPL mechanisms simply registering two saliency signals, rather than just one, in a map determining the priorities for the allocation of focal attention. (See also Colby, Duhamel, & Goldberg, 1996, and Gee, Ipata, Gottlieb, Bisley, & Goldberg, 2008, who were able to decompose signals of neurons in lateral

intraparietal area into a fast, bottom-up driven signal component and a later, cognitively modulated component, which discriminates between target and distractor activity: the former is sustained, the latter declines.) In other words, it does not rule out that the signal for the color distractor singleton is rapidly assigned a lower priority than that for the target singleton. According to the DWA, this would be the case when the weight of the distractor dimension is top-down reduced and that of the target dimension increased. (See also Hopfinger, Buonocore, & Mangun (2000), who emphasized the involvement of inferior, rather than superior, parietal cortex in spatial-attention shifts.)

Indeed, fMRI studies of dimension weighting in visual singleton search have revealed a frontoparietal network of brain areas to be involved in the readjustment of dimensional weights following a switch in the target-defining dimension, such as from color to motion and vice versa (Pollmann, Weidner, Müller, & von Cramon, 2000, 2006; Weidner, Pollmann, Müller, & von Cramon, 2002). The frontal components of this network (partially) overlap with brain structures involved in top-down controlled task switching, including activation of the (pregenual) anterior cingulate cortex (BA 32/24). One effect of these frontal control mechanisms is to modulate the perceptual sensitivity of dimension-specific coding mechanisms in posterior cortical areas. For example, an analysis of trial epochs with successive targets defined in the same dimension revealed tonically increased activation in the posterior fusiform gyrus, which contains human area V4, for color target epochs and in the lateral occipital cortex, which contains the human motion complex (hMT+), for motion target epochs (Pollmann et al., 2006). This represents a dimension-specific perceptual set that biases the system toward detecting signals in the respective dimension (i.e., signals in this dimension tend to reach threshold faster, either due to a baseline shift or a greater gain in activation).

In summary, the DWA can provide a coherent account for the top-down modulation of the salient color distractor interference effects demonstrated in the present study that is both consistent with accounts of task control and plausible in terms of the network of frontoposterior brain mechanisms involved.

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