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Modulations of Saliency Signals at Two Hierarchical Levels of Priority Computation Revealed by Spatial Statistical Distractor Learning

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Many attention theories assume that selection is guided by a preattentive, spatial representation of the scene that combines bottom-up stimulus information with top-down influences (task goals and prior experience) to code for potentially relevant locations (*priority map*). At which level(s) of priority computation top-down influences modulate bottom-up stimulus signals is an open question. In a visual-search task, here we induced experience-driven spatial suppression (*statistical learning*) by presenting 1 of 2 salient distractors more frequently in one display region than the other. When a distractor standing out in the same dimension as the target was spatially biased in Experiment 1, processing of both the target and another, spatially unbiased distractor standing out in a different dimension was likewise hampered in the suppressed region. This indicates that constraining spatial suppression to a specific distractor feature is not possible, and participants instead resort to purely space-based (distractor-feature-independent) suppression at a supradimensional, overall-priority map. In line with a common locus of suppression, a novel computational model of distraction in visual search captures all 3 location effects with a single spatial-weighting parameter. In contrast, when the different-dimension distractor was spatially biased in Experiment 2, processing of other objects in the suppressed region was unaffected, indicating suppression constrained to a subordinate, dimension-specific level of priority computation. In sum, we demonstrate experience-driven top-down modulations of saliency signals at the overall-priority and dimension-specific levels that do not reach down to the specific distractor features.

Keywords: visual attention, attentional capture, saliency map, dimension-weighting account, statistical learning

Processing all visual information available at any given moment represents a combinatorial problem that is intractable by any limited computational system, including the human brain (Tsotsos, 1990). Selective attention—the highlighting of selected regions or objects within a scene—is thought to be one important means to solve this problem. However, the assumption of selective attention only slightly shifts the problem because it immediately raises the next question of what informs the allocation of selective attention. This function is often thought to be fulfilled by a preattentive, spatial representation of the scene coding for potential behavioral relevance at each location (Li, 2002; Wolfe, 2007). This mental representation is often referred to as a *priority map* because it prioritizes locations and/or (proto-) objects for further processing (Fecteau & Munoz, 2006). The priority-map concept has proven powerful in explaining behavioral and neurophysiological data as well as in building artificial vision systems (Bisley & Mirpour, 2019; Bundesen, Habekost, & Kyllingsbaek, 2005; Chelazzi, Marini, Pascucci, & Turatto, 2019; Fecteau & Munoz, 2006; Itti & Koch, 2000; Liesefeld & Müller, 2019, 2020; Wischnewski, Belardinelli, Schneider, & Steil, 2010).

The priority map is thought to combine bottom-up influences that originate from the stimulus (the visual scene and objects therein) and top-down influences that originate from the observer (e.g., task goals and prior experiences; Awh, Belopolsky, & Theeuwes, 2012; Liesefeld, Liesefeld, Pollmann, & Müller, 2018).[1] Although excellent computational models of bottom-up influences exist, the implementation of top-down influences on the

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[1] Given the inconsistent use of these terms in the visual-search community, it is important to clarify here that we use the term *top-down* to refer to both voluntary (goal-driven) and involuntary (experience-driven) influences on attention (see Liesefeld et al., 2018, Box 1, and Müller, Reimann, & Krummenacher, 2003, in particular). Although we agree it is important to differentiate between the two, both types of influence are brought to the scene by the observer, rather than by the stimulus. Accordingly, they conform to the definition of the term *top-down* as used in the broader attention literature. For an apt discussion of the relevance of adhering to this common definition, see, for example, Gaspelin and Luck (2018b). Also, from our theoretical perspective, goals and experiences exert an influence on visual search via a common mechanism: the modulation of saliency signals (see General Discussion).

priority map is much less well understood (Tanner & Itti, 2017). The crucial architectural question examined here is at which level(s) of the visual processing hierarchy top-down influences can modulate bottom-up signals.

Before we consider at which level(s) top-down influences can *theoretically* affect bottom-up signals, we will first outline the bottom-up part of priority computation: *Saliency*, the major bottom-up influence, is (mainly) determined by *local feature contrast*; even if a preferred stimulus is presented within a neuron's receptive field, the neuron's activity is suppressed if laterally connected neurons coding for similar features receive similar input (*iso-feature suppression*). Consequently, if an object is surrounded by objects differing in one or more feature dimensions, it stands out both in terms of its neuronal activation pattern and phenomenologically (Itti & Koch, 2000, 2001; Li, 2002; Nothdurft, 2000). Most cognitive, computational, and neurophysiological theories of visual attention agree that saliency signals are at some point integrated into a common (overall) priority map so that this single spatial representation can guide attention allocations (e.g., Bisley & Mirpour, 2019; Bundesen et al., 2005; Chelazzi et al., 2019; Itti & Koch, 2000; Koch & Ullman, 1985; Krummenacher, Müller, & Heller, 2001, 2002a, 2002b; Liesefeld & Müller, 2019, 2020; Tsotsos et al., 1995; Wolfe, 2007).

There is less consensus regarding the intermediate processing steps. Our *dimension-weighting account (DWA*) and some computational saliency models stipulate two different hierarchical levels of priority computation after the initial processing of visual features (Failing, Feldmann-Wüstefeld, Wang, Olivers, & Theeuwes, 2019; Found & Müller, 1996; Liesefeld et al., 2018; Müller, Geyer, Zehetleitner, & Krummenacher, 2009; Müller, Heller, & Ziegler, 1995; Zehetleitner, Goschy, & Müller, 2012): the overall-priority map, on the one hand, and subordinate dimension maps explicitly coding for saliency within a given feature dimension (also known as *saliency maps for dimensions* [Navalpakkam & Itti, 2007] or *conspicuity maps* [Itti & Koch, 2000; Walther & Koch, 2006]) on the other hand. Notably, although dimension maps code for feature differences (local feature contrast), they are blind to the specific feature that gave rise to a high saliency value. As information is further integrated on the overall-priority map, information about the feature dimension of the salient object is lost, too. This loss of information is a necessary consequence of integration across features and across dimensions, respectively. However, information on the salient object's location is maintained throughout the processing hierarchy (because this is the information that will eventually guide attention allocations). Thus, top-down influences could *in principle* work at the featural, the dimensional, and/or the overall-priority level, but—based on the architectural assumptions of the DWA—our preregistered hypotheses are that they occur at either the dimensional or the overall-priority level, with the level of implementation depending on the specifics of the attentional-competition scenario (see following discussion).

To determine empirically at which of these levels top-down influences work in a given situation, here we exploit a form of *spatial statistical learning*: When a salient distractor occurs in a particular display region (or location) most of the time (spatial bias), this region is suppressed persistently (i.e., saliency signals are down-modulated), so interference by a distractor presented in this region is reduced (e.g., Di Caro, Theeuwes, & Della Libera, 2019; Ferrante et al., 2018; Goschy, Bakos, Müller, & Zehetleitner, 2014; Sauter, Liesefeld, Zehetleitner, & Müller, 2018; van Moorselaar, Theeuwes, & Olivers, 2019; Wang, Samara, & Theeuwes, 2019; Wang & Theeuwes, 2018; for a review, see Chelazzi et al., 2019). By presenting other, spatially unbiased objects in the suppressed region, we can test at which of the three different levels of priority computation this form of top-down suppression takes place: (a) If it works at the overall-priority map, processing of any object is affected in the suppressed region; (b) if it works at the dimensional level, processing of all objects that stand out (are salient) in the same dimension as the spatially biased distractor is affected; and (c) if it works at the featural level, only processing of the spatially biased distractor itself is affected. Thus, by examining for transfer of spatial suppression to spatially unbiased objects, we can tell at which level of priority computation the spatial top-down modulation is implemented in a given attentional-competition scenario. Also note that the spatial nature of this particular top-down influence is relevant for our purposes: It can only be explained via a map-like representation, rather than some spatially unspecific modulation of saliency signals (which may or may not be implemented on a mental map; see Müller et al., 2009; Zehetleitner et al., 2012).

In the present study, we employ this general approach to demonstrate distractor-experience-induced modulations of saliency signals at two levels of priority computation: the overall-priority map and subordinate, dimension-specific maps. Our prior studies on spatial statistical learning of likely distractor locations have provided first indication that the level of spatial suppression depends on the dimensional relationship of the spatially biased distractor to the target: We assume that if the distractor stands out in the same dimension as the target (*same-dimension distractor*, e.g., an orientation-defined distractor in search for an orientation-defined target), suppression will operate at the overall-priority map; and if the distractor stands out in a different dimension than the target (*different-dimension distractor*; e.g., a luminance-defined distractor in search for an orientation-defined target), suppression will occur at the subordinate, distractor-dimension-specific map. In particular, according to a strong version of the DWA, feature-specific top-down weighting of saliency signals is not feasible (Liesefeld & Müller, 2019, 2020), so only the dimensional and the overall-priority map remain as potential loci for suppression. Suppression at the dimensional level would be optimal with different-dimension distractors because it attenuates only the distractor and not the target. Accordingly, if top-down influences at a subordinate, priority-map level are possible, they should be employed for spatially biased different-dimension distractors. For same-dimension distractors, by contrast, both suppression at the dimensional and suppression at the overall-priority level would impair target processing, so the level of suppression in this case is largely an empirical question. We have argued, though, that observers might perceive suppression of the dimension in which the target is defined to run more strongly counter to their goal of finding the target, and thus they would rather resort to suppression at the overall-priority map (Sauter, Liesefeld, & Müller, 2019; Sauter et al., 2018; for more detailed reflections, see the General Discussion).

Suggestive evidence in line with same-dimension-distractor suppression at the overall-priority map was reported by Sauter et al. (2019), who found that suppression induced by a *spatially biased same-dimension* distractor in a learning phase transferred to a *spatially unbiased different-dimension* distractor in a subsequent test phase. However, despite being significant ($p = .033$), suppression of the different-dimension distractor at testing was weak (compared with that of the same-dimension distractor during learning) and equivocal in terms of Bayes factors (BFs; $BF_{10} = 1.53$). Decreased suppression would be compatible with our hypothesis of suppression at the overall-priority map if suppression fades (over the period) from learning to testing or is rapidly unlearned during testing (with an unbiased distractor distribution). But it would also be consistent with the alternative hypothesis that although being induced on the distractor-specific dimensional map, suppression to some extent spills over to other dimensional maps, and it is this lossy spillover that is seen in the transition from learning with same-dimension distractors to testing with different-dimension distractors (see Sauter et al., 2019, p. 2095).

Based on these DWA-based theoretical considerations and our previous empirical findings, we predicted that a spatial bias on a same-dimension distractor would induce suppression at the overall-priority map, whereas a spatial bias on a different-dimension distractor would induce suppression at a subordinate, dimensional level. Experiments 1 and 2, respectively, were designed to test these predictions. To obtain unequivocal evidence for either purely space-based suppression at the overall-priority map or suppression at the dimensional map, both experiments employed the following task: Participants searched for an orientation-defined target in search displays that contained either no distractor, an orientation-defined (same-dimension) distractor, or a luminance-defined (different-dimension) distractor (see Figure 1). Critically, trials with an orientation (same-dimension) distractor or a luminance (different-dimension) distractor were randomly intermixed within the same experimental session. Presenting both distractor types unpredictably intermixed removes the problem with Sauter et al.'s (2019) design, namely, that the acquired suppression might fade from learning to testing or is rapidly unlearned during testing. Consequently, the design implemented here makes the measured strength of suppression quantitatively comparable across both distractors. Furthermore, we devised a computational model of visual search to demonstrate that with same-dimension-distractor learning (Experiment 1), the quantitative degree of suppression is indeed comparable for all three object types (target, same-dimension distractor, different-dimension distractor), as would be predicted by purely space-based suppression at the overall-priority map rather than lossy spillover of suppression across dimensional maps. Moreover, to ensure that we would gain strong evidence for either the presence or the absence of each relevant effect, we employed a recently developed sequential testing procedure with preregistered hypotheses that continues data collection until a predefined level of evidence in terms of BFs in favor of or against each preregistered hypothesis is reached (Schönbrodt & Wagenmakers, 2018).

In Experiment 1, the same-dimension distractor was spatially biased, and we observed the predicted evidence for suppression at the overall-priority map (suppression transferred without attenuation to the target and the unbiased distractor). In Experiment 2, the different-dimension distractor was spatially biased, and we observed the predicted evidence for suppression at the subordinate, dimensional level (suppression did not transfer at all to the target or unbiased distractor).

Method

Participants

Altogether, 82 students recruited at Ludwig-Maximilians-Universität participated in this study. Of these, three were excluded (all from Experiment 1), one for not following instructions as indicated by chance performance, one for chance performance in the first two blocks and outlying mean reaction times (RTs), and one for outlying mean RTs (see the Analysis section for our definition of outliers). Including these participants did not change the result pattern and conclusions drawn from the statistical tests (see Appendix A). The final samples consisted of 32 participants in Experiment 1 (median age: 24 years, range: 19–40 years, 27 female, 4 left-handed) and 47 participants in Experiment 2 (median

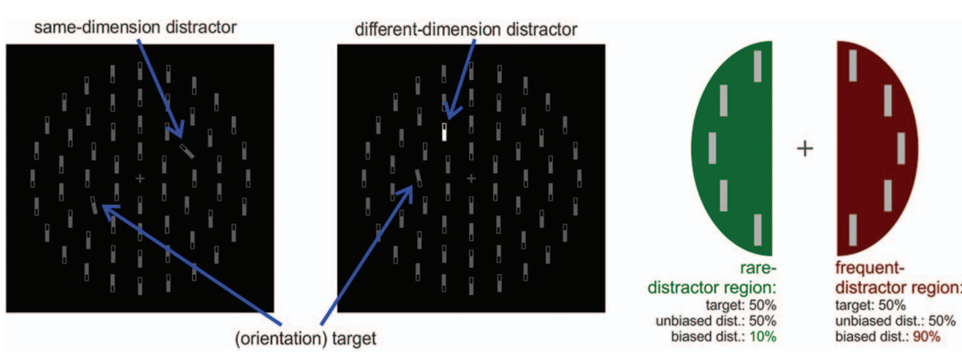

Figure 1. Search displays and target/distractor regions. Observers had to find a bar tilted 12° to the left and indicate the position of the notch (top or bottom) in the (orientation) target bar. On some trials, the displays additionally contained a 45°-tilted (same-dimension [as the orientation-defined target]) distractor (left panel) or a 98%-intensity (different-dimension [to the orientation-defined target]) distractor (middle panel). Distractors were always completely irrelevant and should therefore ideally be ignored. Targets and distractors could appear at any location on the second ring except for the midline vertical (right panel). One type of distractor (but not the other) occurred more often (90%) in one display region than in the other (right panel), thus inducing spatial statistical learning: the same-dimension (orientation) distractor in Experiment 1 and the different-dimension (luminance) distractor in Experiment 2. See the online article for the color version of this figure.

age: 25 years, range: 18–39 years, 35 female, 4 left-handed). The larger sample size in Experiment 2 is an expectable outcome of the sequential testing procedure employed, given that we were looking for convincing evidence for the absence of effects in Experiment 2, which is typically more difficult to obtain than evidence for the alternative. All participants had normal or corrected-to-normal vision. They gave prior informed consent (in writing) and received course credit or were paid for their participation. Procedures were approved by the ethics committee of the Department Psychology and Pedagogics at Ludwig-Maximilians-Universität.

Stimuli

The stimuli and trial procedure were virtually identical to the orientation-target group of Liesefeld, Liesefeld, and Müller (2019). Stimuli were gray bars (0.18 × 0.81°) presented on a thin-film-transistor (TFT) monitor (screen resolution: 1,920 × 1,080 pixels; refresh rate: 60 Hz), at a viewing distance of approximately 70 cm, against a black background. Search displays (see Figure 1) consisted of 60 bars arranged (with a spatial jitter of ± 0.1°) around four concentric rings (radii of 1.1°, 2.2°, 3.3°, and 4.4°, respectively) centered on a central gray fixation cross (0.49°). Each bar contained a notch (~0.25° in height) in its upper or lower part. Most of the bars (homogenous background/nontargets) were vertical (0°) in orientation and at 27% (of maximum) intensity in luminance. The target was tilted by 12° to the left and presented at nontarget intensity. The orientation (same-dimension) distractor was tilted 45° to the left and at nontarget intensity; the luminance (different-dimension) distractor was at 98% intensity and vertical. Targets and distractors always appeared on the second ring from fixation except for the midline vertical (12 and 6 o'clock) locations; that is, there were five possible locations per display side (relevant regions). The exclusion of midline positions and the crucial distractor-location manipulation were the only notable deviations from Liesefeld et al. (2019).

Procedure

Participants performed a classification-search task in which they had to find the 12°-tilted target bar that was present on each trial and press a mouse button with either their right or left thumb indicating the position of the notch in the target bar. Targets and distractors were always presented on the second ring from fixation. The search display was shown until response, which had to be issued within 4 s (*response deadline*). Participants were told to respond as fast as possible without sacrificing accuracy. In the case of an incorrect or delayed response, the fixation cross changed color for 1,000 ms, turning red if the answer was wrong and, respectively, blue if it was too slow. The intertrial interval, which contained only the fixation cross, was jittered between 0.8 and 1.6 s. After the experiment, participants answered a series of questions to probe their knowledge on the spatial probability manipulation (see Appendix E).

Design

Whereas one distractor (luminance in Experiment 1 and orientation in Experiment 2) occurred with equal probability in both relevant regions (balanced), the other distractor occurred on 90% of trials in the left or, respectively, the right region (*spatial bias*; biased region counterbalanced across participants). The target location (frequent vs. rare region) was balanced overall and within each condition, thus ensuring that any location effect observed is due to the experimentally induced bias on the distractor location, rather than to any spatial bias on the target location. Importantly, the target was presented equally often on the same versus the opposite side to the spatially biased distractor, both when the latter occurred in the frequent-distractor region and when it occurred in the rare-distractor region. The search display contained a spatially biased distractor on one half of the trials, an unbiased distractor on one fourth of the trials, and no distractor on the remaining one fourth of the trials. Participants of each group performed 160 (nonanalyzed) training trials without response deadline followed by nine blocks of 160 trials each, thus yielding 1,440 analyzed trials in total, with 360 distractor-absent trials, 180 trials with a nonbiased distractor in each region, 648 trials with the biased distractor in the frequent-distractor region, and 72 trials with the biased distractor in the rare-distractor region.

Analysis

We performed sequential testing with BFs and preregistered hypotheses (Liesefeld, 2020; https://osf.io/ywefp), following the recommendations of Schönbrodt and Wagenmakers (2018). This sampling procedure does not require determining sample sizes in advance and therefore does not rely on (often questionable) prior estimates of effect sizes. Instead, the desired level of evidence for each critical test is determined in advance (preregistered) and (batches of) data are collected and analyzed until this level of evidence is reached (for a detailed explanation and justification, see Schönbrodt & Wagenmakers, 2018). The critical tests determining the stopping rule were whether distractors caused less interference when presented in the frequent-distractor region compared with the rare-distractor region and whether, on distractor-absent trials, RTs were slowed for targets in the frequent-distractor region compared with the rare-distractor region. BFs were calculated using the standard Jeffreys–Zellner–Siow (JZS) prior with a scale factor of $r = \sqrt{2}/2$, but placing zero probability on negative effects (comparable to one-tailed testing). We stopped data collection when sufficient evidence for either the null ($BF_{10} \leq 1/6$) or the alternative hypothesis ($BF_{10} \geq 6$) was reached for all three tests.

Our analyses focus on RTs because these most directly relate to our hypotheses. As reported in Appendix A, tests on error rates corresponding to the relevant tests on RTs resulted in inconclusive results for Experiment 1 ($1/6 < BF_{10} < 6$) and strong evidence for the null in Experiment 2 (all BF_{10}s $< 1/6$). A measure combining error rates and RTs (Liesefeld & Janczyk, 2019) followed the RT pattern. These examinations rule out the possibility that the RT effect patterns reported in the text are driven by differential (condition-specific) speed–accuracy trade-offs.

Trials with incorrect responses or time-outs (5.0% of all trials in Experiment 1 and 2.9% in Experiment 2) or outlying log-transformed RTs per participant × experimental-condition cell (<0.5% in each cell) were excluded from the RT analyses. Outliers were defined as values larger than 1.5 interquartile differences above the third or below the first quartile of the respective empirical distribution.

Instead of unspecific analyses of variance (ANOVAs), we report one-tailed (directed) *t* tests of our specific and directional hypotheses that distractor presence impairs (rather than improves) performance, that same-dimension distractors cause more (rather than less) interference compared with different-dimension distractors, that target processing is impaired (rather than improved) in the frequent-distractor region, and that distractor interference is weaker (rather than stronger) in the frequent-distractor region. When interpreting the respective *p* values, there is no need for multiple-comparison corrections of the α-error level because the tests relate to different hypotheses (all of which must be true in order to confirm our theoretical stance), and all expected effects were significant (Hochberg, 1988). To achieve a good compromise between what is custom in the field and informational richness, we report classical frequentist tests together with the respective BFs.

Here, we were interested in persistent spatial suppression of the distractor region as a result of statistical learning. Region effects are, however, also driven by a short-lasting influence (see Sauter et al., 2018): When an object (target or distractor) occurs at a location that was occupied by a distractor on the preceding trial, its priority is likely to be affected by the spatial suppression that was applied to that preceding distractor (location). Problematically, because spatially biased distractors appeared more often in one display region than in the other, they would induce this short-lasting suppression more often, and this—rather than persistent suppression of the frequent-distractor region—could well explain any region effects on mean RTs. To gain a purer measure of persistent statistical learning, the analyses reported herein excluded trials on which a relevant object (target, orientation distractor, or luminance distractor) appeared at a location that was occupied by a spatially biased distractor on the preceding trial (11.5% of all trials in Experiment 1 and 11.2% in Experiment 2). Analyses including these trials were—in line with the preregistration—used for determining the stopping rule during data collection and led to the same pattern of results (see Appendix A).

Results

As displayed in Figure 2, same-dimension (orientation) and different-dimension (luminance) distractors slowed responses by 200 ms, $t(31) = 20.10$, $p < .001$, $d_z = 3.55$, $BF_{10} = 35.74 \times 10^{15}$, and by 44 ms, $t(31) = 7.42$, $p < .001$, $d_z = 1.31$, $BF_{10} = 11.65 \times 10^5$, respectively, in Experiment 1, and by 273 ms, $t(46) = 28.60$, $p < .001$, $d_z = 4.17$, $BF_{10} = 80.25 \times 10^{26}$, and by 28 ms, $t(46) = 6.95$, $p < .001$, $d_z = 1.01$, $BF_{10} = 23.84 \times 10^5$, respectively, in Experiment 2. Overall, same-dimension distractors generated greater interference than different-dimension distractors, $t(31) = 15.66$, $p < .001$, $d_z = 2.77$, $BF_{10} = 39.97 \times 10^{12}$, and $t(46) = 25.66$, $p < .001$, $d_z = 3.74$, $BF_{10} = 8.21 \times 10^{24}$, respectively.

To examine spatial suppression of distractors due to statistical learning of likely distractor locations in Experiment 1, we compared interference in the frequent-distractor region to interference in the rare-distractor region (location effect). There was indeed a strong learning-induced reduction in interference induced by the

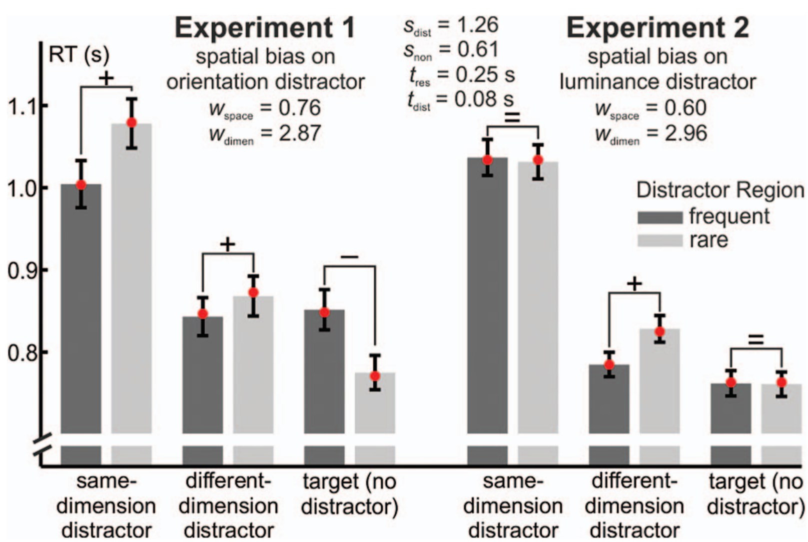

Figure 2. Results and model fit. The spatial bias of the same-dimension distractor caused strong location effects for all object types in Experiment 1 (decreased distractor costs for distractors in the frequent-distractor region and increased response times for targets in the frequent-distractor region), whereas the spatial bias of the different-dimension distractor in Experiment 2 caused a location effect only for the different-dimension distractor itself. Error bars present standard errors of the mean. The gray dots display model predictions, which were within the error bars for each condition. s_{dist} = saliency of the two distractors; s_{non} = saliency of the nontargets; t_{res} = residual time (time for encoding and response, excluding search); t_{dist} = extra time needed for distractor handling for each unit of distractor priority; w_{space} = spatial weight; w_{dimen} = dimensional weight. The first four parameters were constrained to the same value in both experiments, and the latter two (top-down) parameters were allowed to vary between experiments. The excellent model fit indicates (among other things) that a single spatial weighting parameter is sufficient to explain all location effects in Experiment 1, as predicted by purely space-based suppression on the overall-priority map. See the online article for the color version of this figure.

spatially biased same-dimension distractor ($RT_{\text{distractor in rare region}} - RT_{\text{distractor in frequent region}} = 74$ ms), $t(31) = 6.16$, $p < .001$, $d_z = 1.09$, $BF_{10} = 44{,}012.66$. As detailed in Appendix E, despite this strong learning effect, participants were largely unaware of the spatial bias. Turning to our central hypothesis, we found that the effect of learned spatial suppression indeed transferred to the different-dimension distractor (25 ms), $t(31) = 3.89$, $p < .001$, $d_z = 0.69$, $BF_{10} = 120.33$.

An effect of statistical learning also emerged for the spatially biased different-dimension distractor used in Experiment 2 (43 ms), $t(46) = 6.68$, $p < .001$, $d_z = 0.97$, $BF_{10} = 962{,}641.90$. Critically, however, this time the effect did *not* transfer to the other (same-dimension) distractor (−0.5 ms), $t(46) = -0.66$, $p = .744$, $d_z = -0.10$, $BF_{10} = 0.10$. The differences in transfer effects between Experiments 1 and 2 indicate that the spatially biased same-dimension distractor in Experiment 1 was suppressed on the overall-priority map (thus also influencing the different-dimension distractor), whereas the spatially biased different-dimension distractor in Experiment 2 was suppressed on a subordinate dimension-specific map (thus not influencing the same-dimension distractor).

Suppression at the overall-priority map versus a subordinate dimensional map also makes differential predictions for the processing of the target: Suppression on the overall-priority map in Experiment 1 should influence target processing, too, whereas suppression on the subordinate luminance map in Experiment 2 should leave processing of the orientation target untouched. Therefore, we expected processing of targets in the frequent-distractor (vs. the rare-distractor) region to be slowed in Experiment 1 but not in Experiment 2. Following common practice (e.g., Failing et al., 2019; Sauter et al., 2018, 2019; Wang et al., 2019; Wang & Theeuwes, 2018), we confined target-location analyses to distractor-absent trials that would arguably show the purest effect of target suppression (see Appendix B for a detailed explanation and for target-location analyses on distractor-present trials that confirm the approach of focusing on distractor-absent trials). Indeed, responses were slower (by 77 ms) when targets occurred in the frequent-distractor region compared with the rare-distractor region in Experiment 1, $t(31) = 6.59$, $p < .001$, $d_z = 1.17$, $BF_{10} = 137{,}689.20$, and this effect was absent in Experiment 2 (a 1-ms difference), $t(46) = 0.10$, $p = .908$, $d_z = 0.02$, $BF_{10} = 0.17$.

Interim Discussion

The qualitative result pattern reported above supports our prediction that participants learn to suppress (a) the frequent region of a same-dimension distractor on an overarching feature- and dimension-less priority map (in Experiment 1, all three types of objects were suppressed in the frequent- vs. the rare-distractor region) and (b) the frequent region of a different-dimension distractor on a subordinate map coding for local contrast in the respective distractor dimension (in Experiment 2, only the different-dimension distractor but not the same-dimension distractor or target were suppressed in the frequent-distractor region). Thus, our result pattern is indicative of top-down modulations of saliency signals at two hierarchical levels of priority computation. We now turn to a quantitative prediction.

Recall that we interpreted the transfer of spatial suppression to spatially unbiased objects in Experiment 1 to reflect suppression at the overall-priority map. Alternatively, one could argue that suppression is applied at a lower level, and some suppression spills over to the other objects' feature or dimension maps or to the overall-priority map (see Failing et al., 2019). That we have found robust evidence against location effects for the target and the unbiased distractor in Experiment 2 already indicates that suppression does not spill over from one dimensional map to the other: It appears implausible that a spillover would occur from the orientation to the luminance map in Experiment 1 but not in the reverse direction in Experiment 2. In any case, for Experiment 1, overall-map suppression and lossy spillover make the same qualitative predictions (namely, of location effects for all three object types) but different quantitative predictions: If suppression were implemented on the overall-priority map, the exact same amount of spatial weighting would be applied to all three object types; by contrast, lossy spillover across dimensional maps would predict less suppression for the unbiased distractor.

Testing this quantitative prediction is complicated by the fact that—because of up-weighting of the target dimension—the same-dimension distractor necessarily has a higher priority (and therefore causes more interference) than the different-dimension distractor to begin with (Liesefeld et al., 2019; Liesefeld & Müller, 2019). Multiplying with the same (spatial weighting) factor has a larger effect on higher than on lower values.[2] Consequently, the observed location effects are not directly comparable between distractors, and the exact same spatial weighting must have a weaker effect on the different-dimension distractor than on the same-dimension distractor. Indeed, the empirically observed location effect in Experiment 1 was weaker for the different-dimension distractor than for the same-dimension distractor (25 ms vs. 74 ms), $t(31) = 3.81$, $p < .001$, $d_z = 0.67$, $BF_{10} = 100.24$. Another difficulty for our quantitative predictions is that modulations of activation on the overall-priority map do not necessarily (or even likely) linearly affect RTs. Rather, RTs in the present task are determined by an interaction of target and distractor priorities, and even without distractors, RTs are not a linear function of target saliency/priority (Buetti, Cronin, Madison, Wang, & Lleras, 2016; Liesefeld, Moran, Usher, Müller, & Zehetleitner, 2016). Also, the observed target-location effect does not compare directly to the distractor-location effects.

On this background, to test the quantitative prediction that all three objects in Experiment 1 would be spatially suppressed to the same degree, we employed cognitive modeling: If all three location effects (for same-dimension distractor, different-dimension distractor, and target) in Experiment 1 are due to spatial suppression at the same

[2] Consider the following example: Both distractors have the same initial (for the sake of this example: arbitrarily chosen) saliency value of 5 ($d = s = 5$; with d and s standing for the priority-map activation generated by the different- and same-dimension distractor, respectively), and dimensional weighting increases the same-dimension distractor's activation to 150% ($s_{\text{dimen}} = 7.5$; with subscript *dimen* standing for "dimension weighting"). Now, if both distractors are spatially weighted by the exact same factor to 30% ($s_{\text{dimen/space}} = 2.25$ and $d_{\text{space}} = 1.5$; with subscript *space* standing for "spatial weighting"), the difference in priority with and without spatial weighting is smaller for different-dimension distractors ($d - d_{\text{space}} = 5 - 1.5 = 3.5$ vs. $s_{\text{dimen}} - s_{\text{dimen/space}} = 7.5 - 2.25 = 5.25$). Thus, the effect of location on the degree of interference induced by the distractor and, consequently, the effect on overall RTs must be stronger for the same-dimension distractor than for the different-dimension distractor even if spatial weighting is identical.

(overall-priority) map, they should all be well captured by a single spatial-weighting model parameter. In contrast, lossy spillover across maps would require a different (smaller) spatial-weighting parameter for the spatially unbiased distractor.

Cognitive Modeling of Distractor Suppression

We devised a novel high-level computational model of top-down modulation of saliency signals, based on an established model of visual-search behavior termed *Competitive Guided Search* (CGS; Moran, Zehetleitner, Müller, & Usher, 2013). CGS is an implementation of Wolfe's (2007) popular Guided Search and has proven successful in explaining behavioral results from a range of visual-search tasks, outperforming various competitors (Liesefeld et al., 2016; Moran, Liesefeld, Usher, & Müller, 2017; Moran, Zehetleitner, Liesefeld, Müller, & Usher, 2016; Moran et al., 2013; Narbutas, Lin, Kristan, & Heinke, 2017). Following the basic principle of Guided Search, decisions on where to attend next are based on activations on the (overall-) priority map. Spatial attention is allocated to one object at a time based on a probabilistic selection rule, according to which the object with the highest activation has the highest chance of being attended next, but attention might also be allocated to an object with a lower activation. If the attended object is identified as a distractor, its activation on the priority map is canceled, so it no longer competes for subsequent allocations of spatial attention (Klein, 1988; Koch & Ullman, 1985; Mirpour, Arcizet, Ong, & Bisley, 2009). If the attended object is identified as a target, search terminates, and a response is issued (see Appendix C for details).

CGS was originally devised to explain behavior in search tasks in which participants have to indicate the presence or absence of a target object among homogenous nontargets without any salient distractors and without variation in top-down influences. To adapt it to research on distraction and distractor-handling mechanisms, we removed functionality related to deciding on target absence (in the typical classification/compound search task employed here, a target is present on every trial), added a third object category (salient distractors), and introduced top-down parameters (spatial and dimension weighting; see Appendix C for details).

As can be seen in Figure 2, the fit of the model to the average RTs from Experiments 1 and 2 was excellent, and the resulting parameter estimates were reasonable. Importantly, the model produced all location effects in Experiment 1 using just a single spatial-weighting parameter per experiment (i.e., without the need for separate spatial weights for each object type). As detailed in Appendix C, fitting the same model but with spatial weighting constrained to the same-dimension distractor (implementing feature-based suppression) or target and same-dimension distractor (implementing dimension-based suppression) in Experiment 1 or influencing all objects in Experiment 2 (implementing pure spatial suppression at the overall-priority map) produced considerable misfits. This proves that a single top-down spatial weighting parameter can explain all spatial effects in Experiment 1, as would be expected if spatial statistical learning was indeed implemented as a down-regulation of saliency signals in the frequent-distractor region on the overall-priority map. This result would be unlikely if the various location effects were produced by partly independent spatial weighting mechanisms (including spillover) because it indicates that all three objects are weighted to the same degree. It seems implausible and is nonparsimonious to assume that two (or three) independent weighting mechanisms would be perfectly coupled in their strength.

To directly test for the possibility of lossy spillover, we implemented the respective model versions by adding separate weighting parameters for different dimensions or different features in Experiment 1 (implementing spillover from feature- or dimension-based suppression). Granting the model this extra flexibility did not notably improve the model fit. Furthermore, lossy spillover of suppression from the orientation to the luminance priority map would predict weaker suppression of the different-dimension distractor compared with the same-dimension distractor. However, parameter estimates from the respective model version indicated, if anything, stronger suppression of the different-dimension distractor, thus ruling out lossy spillover (see Appendix C).

Apart from these tests of our main hypotheses, it is also interesting to note that the model indicated stronger space- and dimension-based weighting (= stronger top-down influences) in Experiment 2 than in Experiment 1. Space-based suppression might be stronger because it does not produce the negative side effect of also hampering target processing and can therefore be applied more forcefully. In other words, the visual system had to settle on a compromise between the positive and negative consequences of spatial weighting in Experiment 1, whereas no such trade-off was necessary in Experiment 2, permitting it to make full use of this distractor-handling mechanism. Dimension-based suppression was likely stronger in Experiment 2 because the higher incidence of different-dimension (luminance) distractors provided a stronger incentive for and more learning experience with attenuating distraction from the luminance dimension via dimension weighting (see Müller et al., 2009).

If spatial weighting was identical for both distractors in Experiment 1, location effects should be identical if normalized by the respective location-independent overall-interference effects. This prediction cannot be tested at the level of RTs (i.e., by simply dividing the RT location effects by the respective overall distractor-interference effect on RTs) because it actually relates to activation at the overall-priority map rather than to RTs directly, and the relation between priority activation and RTs is likely highly nonlinear (see previous discussion). Thus, rather than normalizing the location effects in RTs, here we perform the normalization at the level of model-estimated priority activations. In particular, applying the logic and notation from Footnote 2 but using the empirically determined model parameters, both distractors have the same initial model-estimated saliency value of s_{dist} = 1.26 ($d = s = 1.26$; with d and s standing for the different- and same-dimension distractor's priorities, respectively), and dimensional weighting ($w_{\text{dimen}} = 2.87$) increases the same-dimension distractor's activation to 287% of its original strength (s_{dimen} = 3.62). Now, if both distractors are spatially weighted by the exact same spatial factor ($w_{\text{space}} = 0.76$) when they occur in the frequent-distractor region ($d_{\text{space}} = 0.96$ and $s_{\text{dimen/space}} = 2.75$), the location effect for the different-dimension distractor is only about one third of the location effect of the same-dimension distractor ($d - d_{\text{space}} = 1.26 - 0.96 = 0.30$ vs. $s_{\text{dimen}} - s_{\text{dimen/space}} = 3.62 - 2.75 = 0.87$). Thus, the effect of location on the degree of interference induced by the distractor and, consequently, the (nonlinear) effect on RTs is stronger for the same-dimension distractor than for the different-dimension distractor even though spatial weighting is identical. Yet, normalizing the location effect by the original priority activation (before spatial weighting) results in 0.30/1.26 = 0.24 (i.e., $1 - w_{space}$) for the different-dimension distractor and 0.87/3.62 = 0.24 for the same-dimension distractor.

General Discussion

Our goal was to demonstrate top-down modulations of saliency signals at two hierarchical levels of priority computation in the human visual system. To this end, we exploited a recently discovered spatial statistical-learning mechanism. We reasoned that learned spatial suppression of likely distractor locations at the overall-priority map must affect target processing and processing of another distractor and that the effects of spatial suppression on a subordinate, dimensional level must be confined to the dimension of the spatially biased distractor. Further, we speculated that the level of suppression would depend on the dimensional relationship between the biased distractor and target. We found, indeed, that persistent spatial inhibition induced by a spatial bias on a distractor standing out in the same dimension as the target (Experiment 1) affected the processing of other objects in the frequent-distractor region: a spatially unbiased target defined by a different feature within the same dimension (orientation) and another, spatially unbiased distractor defined in a different dimension (luminance). This transfer of spatial suppression demonstrates that suppression is implemented in a purely spatial fashion, independent of the specific distractor feature or dimension. In contrast, the same statistical-learning regime for the different-dimension (luminance-defined) distractor (Experiment 2) did not transfer to the spatially unbiased target or same-dimension distractor. Furthermore, computational modeling provided evidence that a single mechanism is responsible for all three location effects in Experiment 1, namely, spatial suppression at the overall-priority map. As discussed in detail later in the article, this result pattern provides crucial insights into the implementation (and resulting limitations) of top-down influences on priority computation. Overall, these findings strongly support a model in which top-down modulations of saliency signals can occur at two levels of the priority-computation hierarchy (see Figure 3): the overall-priority map (Experiment 1) and, respectively, a subordinate dimension-specific map (Experiment 2).

Reasons to Suppress at a Particular Priority Level

Spatial suppression at the overall priority-map level is often suboptimal because it incurs a cost for target processing (see Liesefeld & Müller, 2019); this is the likely reason why spatial suppression of the different-dimension distractor in Experiment 2 was implemented at the dimensional level. Nevertheless, from our theoretical stance, spatial suppression of different-dimension distractors at the overall-priority map is feasible in principle. Because suppression at the overall-priority map is a viable distractor-handling mechanism, it would also provide a valid strategy to

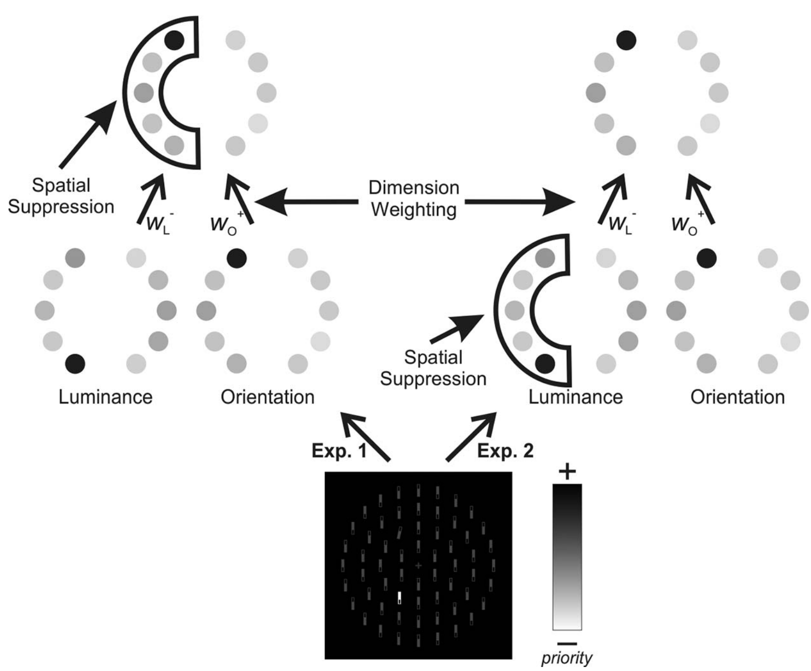

Figure 3. Schematic illustration of priority computation according to the dimension-weighting account (DWA) as applied to the present study. At those positions that contain relevant objects in the visual display (bottom), saliency (local feature contrast) is extracted for each feature dimension (middle) and then integrated on the overall-priority map (top). Dimension weighting modulates the gain from the middle to the top layer (here, up-weighting of signals from the target-defining, orientation dimension) in both experiments. Our data indicate that adaptation to statistical regularities in the environment differed depending on whether a same-dimension (orientation) or a different-dimension (luminance) distractor occurred more frequently in a particular region: Spatial suppression of the frequent-distractor region was applied at the overall map in Experiment 1 and at the luminance-specific map in Experiment 2.

handle spatially biased different-dimension distractors, so—contingent on specifics of the experimental design—some participants might use this strategy some of the time. Indeed, in line with suppression at the overall-priority map, various recent studies have shown effects of statistical learning of frequent different-dimension-distractor locations on target processing (e.g., Ferrante et al., 2018; Wang & Theeuwes, 2018). Looking carefully into the design of Wang and Theeuwes (2018), we identified various factors that support the choice of this suboptimal "strategy" (Allenmark, Zhang, Liesefeld, Shi, & Müller, 2019; Zhang, Allenmark, Liesefeld, Shi, & Müller, 2019). In any case, the aim of the present study was not to show that dimension-based suppression is invariably used for different-dimension distractors but to exploit the fact that it is consistently used in our design (Goschy et al., 2014; Sauter et al., 2018, 2019) to demonstrate top-down influences at two separable levels of priority computation.

Experiment 1 showed that participants suppressed a spatially biased same-dimension distractor at the overall-priority map, although, according to our stance, suppression at the subordinate dimensional map would have been possible and equally effective in reducing interference by the spatially biased same-dimension distractor. So, why did participants explicitly or implicitly "select" that level of suppression? A simple answer might be that spatial suppression is cognitively less effortful at the overall level than at the dimensional level. Alternatively, suppressing saliency signals from the target dimension (in this case, orientation) might run too obviously against the goal of searching for the target (see also Sauter et al., 2019). In particular, suppression on the orientation map would mean that observers actively ignore objects that stand out in the orientation dimension, including the very object they are searching for (the orientation target). This might be an explicitly perceived goal conflict or, implicitly, the consequence of repeatedly selecting the orientation target (Awh et al., 2012; Liesefeld et al., 2018; Theeuwes, 2018; Wolfe & Horowitz, 2017; see Appendix E). Pure spatial weighting at the overall-priority map, by contrast, might not be construed as directly working against the goal of finding the spatially unbiased orientation target but, rather, as attending less to a region that often contains distraction.

Architecture of Priority Computation

Our observation of suppression at two different levels of priority computation necessarily implies the existence of these two levels, including the subordinate dimension-specific level. A dimension-specific level of priority computation is a central assumption of the DWA (e.g., Liesefeld et al., 2018; Liesefeld & Müller, 2019, 2020) but is expressly acknowledged only in some theories of visual attention (Found & Müller, 1996; Itti & Koch, 2000; Liesefeld et al., 2018; Müller et al., 1995; Navalpakkam & Itti, 2007; Walther & Koch, 2006; Zehetleitner et al., 2012). In what follows, we attempt to relate this assumption to several recent studies that examined the architecture of priority computation with regard to distractor suppression (for a more general review, see Liesefeld et al., 2018).

Failing et al.'s (2019) Experiment 1 featured two frequent-distractor locations, each likely to contain a distractor of a different color among gray nontargets, during search for a shape-defined singleton target (i.e., in our terms, there were two spatially biased different-dimension distractors). In line with a locus at the dimensional level, Failing et al. (2019) found suppression to transfer from one color distractor to the other. However, the degree of suppression was higher when a given color distractor occurred at its own frequent location rather than that of the other color distractor, indicative of some feature specificity of statistical learning. This finding is in line with the DWA and the conclusions from the present study because we consider color to consist of multiple subdimensions (Liesefeld et al., 2018; Liesefeld & Müller, 2019; see also Nothdurft, 1993; Wolfe, Chun, & Friedman-Hill, 1995).[3] Thus, what Failing et al. (2019) refer to as *feature-specific suppression* might actually be an instance of (sub-)dimension-specific suppression (in terms of the DWA). In line with our predictions, their Experiment 2 showed no transfer of suppression between different-dimension distractors from clearly independent dimensions (color and shape distractors during search for an orientation target).[4]

In apparent conflict with distractor suppression at the dimensional level as predicted by the DWA (Liesefeld & Müller, 2019), Gaspelin and Luck (2018a) recently showed that distractors are only suppressed if participants know the specific distractor feature in advance; knowledge of the distractor dimension was not sufficient. However, the preponderance of evidence speaks for (at least the possibility of) dimension-based suppression, including much work from our lab (Liesefeld, Liesefeld, Töllner, & Müller, 2017, Liesefeld et al., 2019; Sauter et al., 2018, 2019) but also that from other labs (Feldmann-Wüstefeld, Busch, & Schubö, 2020; Feldmann-Wüstefeld, Uengoer, & Schubö, 2015; Sawaki & Luck, 2010; Vatterott, Mozer, & Vecera, 2018; Won, Kosoyan, & Geng, 2019). Won et al. (2019), for example, found that varying the distractor feature within the same dimension did not increase interference with respect to a constant-feature distractor. Their data provide evidence for dimensional (rather than feature-specific) down-weighting that was modulated by distractor prevalence and distractor repetition from one trial to the next.

A crucial aspect of Gaspelin and Luck's (2018a) design is that in contrast to the prototypical additional-singleton task (Theeuwes, 1991), the target does not stand out (a shape target among heterogeneous nontarget shapes). Thus, in contrast to their distractor, their target was not a salient singleton. If the target is not salient, there is no point in using priority signals to guide attention. We believe that in such a situation, participants resort to some kind of systematic scanning of the display, rather than making use of the (noninformative) output of priority computation (Liesefeld & Müller, 2020; see also Kerzel & Burra, 2020). Thus, results from search tasks of the type

[3] That color is multidimensional is not an arbitrary assumption, as evidenced by the fact that at least two numbers are needed to describe a color (even if luminance is kept constant, e.g., in the CIELAB or hue, saturation, lightness [HSL] color spaces), and color coding in early visual processing is multidimensional (e.g., S, L, and M cones in the retina and opponent color coding in the lateral geniculate nucleus). Of note, findings in the visual-search literature can be theoretically consolidated by assuming that the saliency/priority representation of "color" consists of multiple subdimensions that are separate, although nonindependent (*integral*; Garner, 1974; see Liesefeld et al., 2018). We have argued that this renders comparisons between colors nonideal for differentiating dimensional and feature-specific processing as long as the dimensional structure of color in terms of priority computation is unknown (see, e.g., Liesefeld et al., 2019, pp. 255–256).

[4] Regarding their target-location effects, see the preceding section and the extensive discussions and empirical tests in our previous work (Allenmark et al., 2019; Zhang et al., 2019).

used by Gaspelin and Luck are, in our view, noninformative with regard to preattentive priority computation.

Complementary to defending DWA in light of studies from other labs, one can also ask how theories assuming feature weighting would account for our results. As far as we can tell, these theories cannot readily explain why participants failed to use feature weighting in our Experiment 1 or in our previous studies targeted at the question (Liesefeld et al., 2017, 2019; Sauter et al., 2018, 2019). That is, it is difficult to see why participants would refrain from applying feature weighting to selectively decrease same-dimension-distractor interference if they had this useful distractor-handling mechanism at their disposal. The fact that they do not use feature weighting cannot be due to a lack of incentive or training because same-dimension distractors produce massive interference (>200 ms) and robust attentional capture even if they are categorically different from the target (left- vs. right-tilted) and predictably occur on a high number (67%) of trials (Liesefeld et al., 2017). Rather, it appears that people simply do not have the ability to selectively down-weight specific features.

Indeed, the pattern of spatial statistical-learning effects observed here adds further converging evidence against feature weighting: Evidently, participants were unable to confine spatial suppression to the specific orientation of the same-dimension distractor in Experiment 1; instead, they also suppressed the spatially unbiased target that was defined by a different feature within the same dimension (orientation). Moreover, computational modeling showed that suppression was equally strong for the target and the distractor(s). Distractor-specific spatial suppression only occurred for the different-dimension distractor in Experiment 2, where suppression at the level of the dimensional map would not affect target processing. If participants had been able to apply spatial suppression to individual features, they could (and should) have done so in Experiments 1 and 2 alike because this would have left target processing unaffected, even in Experiment 1. Accordingly, the presence of location effects for the two unbiased objects in Experiment 1 argues against the possibility of feature-specific spatial suppression. Instead, adaptation to spatial regularities appears to be confined to either the dimensional or the overall priority level, which further supports the priority-computation architecture proposed by the DWA (see Figure 3) and highlights important limitations in the ability to control or (passively) adapt visual attention.

A Computational Model of Distraction in Visual Search

We developed a computational model of distraction and distractor handling in visual search and used this model to demonstrate that all three location effects in Experiment 1 can be explained by a single spatial-weighting factor. To our knowledge, this model is the first that accounts for data from the additional-singleton task and the first to take the top-down influences of dimensional and spatial weighting into account. Several aspects of the model might prove useful in interpreting existing and future data patterns produced in this frequently employed task, thus fueling theory development.

For example, our model might contribute to clarifying the relationship between experience- and goal-driven influences on priority computation. In particular, the model illustrates why these two types of influence should be considered as belonging to the same category (top-down influences) and are fundamentally different from (bottom-up) saliency (see also Gaspelin & Luck, 2018b): The effects of goals and experiences are both captured by the two weighting parameters, which means that these mechanisms can be conceived as computationally equivalent. As long as this relatively simple model can account for all data patterns, it would be nonparsimonious to assume that goals and experiences affect search by fundamentally different principles. In other words, although, of course, it makes much theoretical and practical sense to differentiate goal- and experience-driven influences, they both influence search behavior via the same mechanism (weighting of saliency signals) and should therefore be subsumed under a common umbrella term (e.g., "top-down influences").

Another interesting insight relates to how verbal labeling affects the interpretation of (top-down) weighting effects. Most researchers, including ourselves, conceive of spatial statistical learning of likely distractor locations as suppression of the frequent-distractor region or location (e.g., Chelazzi et al., 2019; Liesefeld & Müller, 2019; Theeuwes, 2018; van Moorselaar & Slagter, 2020). This idea is intuitively plausible, but there is nothing in the data that provides empirical evidence against alternative interpretations of this effect. For example, rather than suppression of the frequent-distractor region, a spatial bias can be induced equally well by enhancing the rare-distractor region or by biasing the distribution of a limited attentional resource in favor of the rare-distractor region and against the frequent-distractor region (for a similar discussion, see Gaspelin & Luck, 2018a). These possibilities might be differentiable by comparing activation on priority maps in experiments of the type performed here and baseline experiments without a spatial-bias manipulation. However, such a comparison is not trivial because we cannot directly observe activations on priority maps, and the relation between priority activations and RTs is likely nonlinear (see previous discussion). Moreover, according to our model, enhancement and suppression are, in principle, not discriminable based on behavioral data and might not even be discriminable by direct recordings of activations from the neuronal implementation of priority maps because attention allocations are guided by relative rather than absolute priority (see Appendix D for mathematical proofs). Despite this in-principle mimicry of suppression and enhancement, it seems important to design future studies in a way that the results are more or less *plausible* under the assumption of one or the other weighting mechanism.

Context

This work represents a substantial step in our ongoing efforts to understand the architecture of priority computation in the visual system. Hermann J. Müller proposed the DWA in the 1990s (Found & Müller, 1996; Müller et al., 1995) and since then has continually collected empirical evidence. We have recently reviewed this evidence and refined this theoretical account (Liesefeld et al., 2018; Liesefeld & Müller, 2020) and are currently mainly working on applying it to distractor handling (Liesefeld & Müller, 2019), including the statistical learning of likely distractor locations (Allenmark et al., 2019; Goschy et al., 2014; Sauter et al., 2018, 2019; Zhang et al., 2019).

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Appendix A

Alternative Analyses of the Present Data Set

Seemingly auxiliary analysis choices can have an impact on conclusions drawn from a given data set (Silberzahn et al., 2018). To demonstrate that our results are robust against these choices, here we report the outcomes of the critical statistical tests on location effects (the respective object presented in the frequent- vs. rare-distractor area) with differently preprocessed data and different outcome measures. In particular, Table A1 contains tests from (a) the original analysis, (b) an analysis that included participants identified as outliers, (c) an analysis that included trials on which a spatially biased distractor had occurred on the previous trial at the same position as a target or distractor in the current trial (this is also the analysis that was used to determine the stopping rule), (d) an analysis on error rates, and (e) an analysis on the balanced integration score (BIS) that combines error rates and RTs to control for condition-specific speed–accuracy trade-offs (Liesefeld & Janczyk, 2019).

Table A1
Location Effects for Each of the Three Object Types With Various Preprocessing Decisions and Outcome Measures

Object	Original analysis	With outlying participants	With position repetitions	Error rates	BIS
Experiment 1 (spatial bias on same-dimension distractor)					
Biased distractor	74 ms, $t(31) = 6.16^{***}$, $BF_{10} = 44{,}012.66$	62 ms, $t(34) = 16.38^{***}$, $BF_{10} = 51.34$	67 ms, $t(31) = 5.87^{***}$, $BF_{10} = 20{,}784.28$	1.1%, $t(31) = 2.00^{*}$, $BF_{10} = 2.11$	0.68, $t(31) = 5.09^{***}$, $BF_{10} = 2{,}615.42$
Unbiased distractor	25 ms, $t(31) = 3.89^{***}$, $BF_{10} = 120.33$,	24 ms, $t(34) = 3.62^{***}$, $BF_{10} = 66.67$	19 ms, $t(31) = 2.95^{**}$, $BF_{10} = 13.70$	0.6%, $t(31) = 2.04^{*}$, $BF_{10} = 2.26$	0.28, $t(31) = 3.21^{**}$, $BF_{10} = 24.04$
Target	77 ms, $t(31) = 6.59^{***}$, $BF_{10} = 137{,}689.20$	78 ms, $t(34) = 7.02^{***}$, $BF_{10} = 660{,}454.5$	85 ms, $t(31) = 7.17^{***}$, $BF_{10} = 622{,}652.9$	−0.1%, $t(31) = -0.57$, $BF_{10} = 0.31$	0.47, $t(31) = 5.76^{***}$, $BF_{10} = 15{,}505.98$
Experiment 2 (spatial bias on different-dimension distractor)					
Biased distractor	43 ms, $t(46) = 6.68^{***}$, $BF_{10} = 962{,}641.90$	N/A[a]	42 ms, $t(46) = 6.65^{***}$, $BF_{10} = 890{,}520.4$	−0.3%, $t(46) = -1.20$, $BF_{10} = 0.08$	0.19, $t(46) = 2.70^{**}$, $BF_{10} = 7.93$
Unbiased distractor	−0.5 ms, $t(46) = -0.66$, $BF_{10} = 0.10$	N/A[a]	−9 ms, $t(46) = -1.13$, $BF_{01} = 0.08$	0.3%, $t(46) = 0.89$, $BF_{01} = 0.09$	0.05, $t(46) = 0.48$, $BF_{01} = 0.11$
Target	1 ms, $t(46) = 0.10$, 0.02, $BF_{01} = 0.17$	N/A[a]	−2 ms, $t(46) = 0.17$, $BF_{01} = 0.08$	−0.1%, $t(46) = 0.70$, $BF_{01} = 0.10$	−0.03, $t(46) = -0.47$, $BF_{01} = 0.11$

Note. BIS = balanced integration score; BF = Bayes factor; N/A = not applicable. All values were calculated such that positive values indicate effects in the predicted direction (less interference and easier search for distractors and targets in the frequent-distractor region, respectively).
[a] No participant was excluded from the analysis of Experiment 2.
$^{*} p < .05$. $^{**} p < .01$. $^{***} p < .001$.

(*Appendices continue*)

Appendix B

Target Location Effects on Distractor-Present Trials

Following common practice (Allenmark et al., 2019; Failing et al., 2019; Sauter et al., 2018, 2019; Wang et al., 2019; Zhang et al., 2019), our examination of the target-location effect focused on distractor-absent trials. There are at least two problematic issues with examining this effect on distractor-present trials: (a) The average distance between the target and biased distractor is smaller for targets in the frequent-distractor region, thus potentially confounding any effect of target location with an effect of distractor proximity (although see Sauter et al., 2018); (b) distractor presence might trigger an ad hoc (i.e., transiently elicited by the search display) change of spatial weights and thereby distort the (persistent) effect of interest here. As evident from Table B1, this distortion was indeed present.

From these analyses, it is apparent that the target-location effect was overestimated when the biased distractor was present (by 53 ms for Experiment 1 and by 20 ms for Experiment 2) and underestimated when the unbiased distractor was present (by 13 ms in Experiment 1 and by 18 ms in Experiment 2). The latter distortion cannot be explained by differential spatial proximity of the unbiased distractor to the target but is in line with the second issue presented in the previous paragraph. Potentially, when the biased distractor is detected early on, extra suppression of the frequent region is induced ad hoc on the overall-priority map (thereby causing a slight tendency for a target-location effect, even in Experiment 2; see Failing et al., 2019, for a similar speculation). By contrast, when the unbiased distractor is detected, persistent suppression of the frequent-distractor region might be reduced or counteracted by transient suppression of the rare-distractor region or enhancement of the frequent-distractor region on the overall-priority map (thereby even causing a slight tendency for a negative target-location effect in Experiment 2). These are, of course, mere speculations regarding the underlying mechanisms and must be backed up by dedicated studies. Nevertheless, the take-home message of this supplemental analysis on this particularly large data set is clear: The common practice of focusing on distractor-absent trials for an analysis of "pure" target-location effects in studies of spatial statistical learning of distractor locations is well founded and to be encouraged because in distractor-present trials, various additional spatial effects might contribute to (and thus distort) the target-location effects induced by spatial learning.

Table B1
Target-Location Effect in the Various Distractor Conditions

Experiment	Distractor absent	Biased distractor	Unbiased distractor
1	77 ms, $t(31) = 6.59^{***}$, $BF_{10} = 137{,}689.20$	130 ms, $t(31) = 8.22^{***}$, $BF_{10} = 8{,}715{,}062.00$	64 ms, $t(31) = 5.49^{***}$, $BF_{10} = 7{,}508.363$
2	1 ms, $t(46) = 0.10$, $BF_{10} = 0.17$	21 ms, $t(46) = 2.29^{*}$, $BF_{10} = 3.30$	−17 ms, $t(31) = -1.29$, $BF_{10} = 0.17$

Note. BF = Bayes factor.
$^{*}p < .05$. $^{***}p < .001$.

Appendix C

Details on the Computational Modeling

To test whether a single spatial-weighting parameter can explain all three location effects observed in Experiment 1, we developed a computational model for the present paradigm, which is based on the successful CGS model of visual search (Liesefeld et al., 2016; Moran et al., 2013, 2016, 2017). Here we (briefly) describe both models, with particular emphasis on how the new model relates to and deviates from the original CGS. The main text contains a brief verbal description of the model and discussion of the results (Fig. 2); for a concise overview of all parameters of the original CGS model, see, for example, the Appendix to Liesefeld et al. (2016).

The core idea of CGS is that each attention allocation is determined by a probabilistic selection process implemented by a choice axiom widely employed in mathematical psychology and other fields (Luce, 1986; see Appendix D): An item's selection probability depends on its current overall-priority-map activation relative to the activation of all remaining items in the scene. Once an item is selected, it is attended and identified. If this item is the target, a response is issued; alternatively, the item's activation on the priority map is suppressed, and the choice axiom is applied again with the remaining activations to determine the next attention allocation. To account for target-absent responses, the model assumes a "quitting unit" that gains weight after each attention allocation and enters the competition for attention on each trial. Once this quitting unit is selected, search ends with a target-absent decision. Various parameters determine error rates and the shape of predicted RT distributions.

(*Appendices continue*)

First, we adapted CGS to the typical additional-singleton task (Theeuwes, 1991) as employed here. Rather than deciding on the presence/absence of a target, the target was present on every trial, and participants had to indicate whether a notch was placed in the lower or upper part of the target bar (a *compound* or *classification task*; see Liesefeld et al., 2018; Töllner, Rangelov, & Müller, 2012). Removing the uncertainty regarding distractor presence made the parameter related to the decision on distractor absence (Δw_{quit}) superfluous, and it was consequently dropped. Because we were only interested in mean correct RTs, we also dropped the motor-error parameter and replaced the four parameters used for modeling RT distributions with two parameters modeling mean times: t_{res} captures residual (= non-search-related) times needed for initial encoding (in particular, creation of the priority map) and response generation (including the time needed to identify the position of the notch); and t_{shift} captures times for identifying the attended object and shifting attention. To simplify the model, we did not consider different residual or identification times for the different object types (see Liesefeld et al., 2016; Moran et al., 2013) and intentionally decided against modeling identification of the notch position in detail (e.g., as a drift-diffusion process; Ratcliff & McKoon, 2008).

After calculating the probability of each possible sequence of attention allocations within a display and adding up the probabilities (p_i) for those sequences in which the target was attended as the *i*th out of *S* items, we can apply the following formula to determine the average time spent searching in each condition:

$$t_{search} = t_{shift} \sum_{i=1}^{S} (p_i \cdot i)$$

We assume that in addition to capturing attention, distractors produce additional activation-dependent interference (Sauter, Hanning, Liesefeld, & Müller, 2020). Potential reasons for the extra interference include that their suppression (after identification) requires time and/or that target processing is delayed (e.g., as a result of heightened response caution after capture). Again, we calculate the probabilities (p_j) for those sequences in which the distractor is attended as the *j*th item (which is zero if the target was attended beforehand). The contribution of the extra interference to RTs is then captured by the following formula:

$$t_{interf} = t_{dist} \sum_{j=1}^{S} p_j$$

Adding to the times for search and interference the times for encoding and response selection (t_{res}) results in our prediction of RTs in each condition:

$$t_{pred} = t_{res} + t_{search} + t_{interf}$$

Because the original CGS is devoid of any top-down mechanisms, it does not differentiate between priority (= a combination of bottom-up and top-down influences) and saliency (= mere bottom-up influences). Our model version reflects the two postulated top-down influences, spatial and dimension (feature-based) weighting, as two additional parameters, w_{space} and w_{dimen}. w_{dimen} is implemented as an up-regulation of same-dimension distractors as well as target saliencies. w_{space} is implemented as a down-regulation of targets and distractors in the rare-distractor region. Whereas top-down influences might differ across conditions, saliencies should be constant for each object. Thus, rather than directly entering the competition for attention according to Luce's (1986) choice axiom as in the original CGS, saliency values in our modified version are but one contributor to the overall-priority-map activations that enter this competition. Because the specific luminance and orientation distractors employed here cause comparable levels of interference when top-down weighting is controlled for (Liesefeld et al., 2019),[5] we used a single distractor-saliency parameter (s_{dist}; individual saliency parameters would only increase the flexibility of the model and mimic the effects of dimension weighting). As a final deviation from the original CGS that is inconsequential for the model fit, we used the target saliency (s_{targ}) rather than the nontarget saliency (s_{non}) as a scaling factor set to 1 so that distractor saliencies can be interpreted with respect to the target (rather than the nontargets), with a saliency (s_{dist}) > 1 indicating distractors (s_{dist} times) more salient than the target.

Parameters were estimated by fitting the model to the average RT data plotted in Figure 2. To determine model fit, we divided the difference between predicted and empirical RTs by the RT standard error (calculated across participants) per condition (Schwarz, 1993). Squaring this weighted difference resulted in an approximately χ^2 distributed variate with 1 degree of freedom (*df*). Summing χ^2 values across conditions yielded a measure of goodness of fit with *df* = number of conditions – number of estimated parameters. This χ^2 value was minimized by the optimization algorithm implemented in MATLAB as *fminsearch* (Nelder & Mead, 1965).

[5] In particular, when Liesefeld et al.'s (2019) participants were searching for a luminance target, the luminance distractor caused the same amount of interference as the orientation distractor when searching for an orientation target. Equivalently, when searching for an orientation target, the luminance distractor caused the same amount of interference as the orientation distractor when searching for a luminance target.

(*Appendices continue*)

Table C1
Parameter Estimates and Goodness of Model Fit (χ^2) for Various Model Versions

Suppression level						Experiment 1		Experiment 2			
Experiment 1	Experiment 2	s_{dist}	s_{non}	t_{res}	t_{dist}	w_{space}	w_{dimen}	w_{space}	w_{dimen}	*df*	χ^2
Overall	**Dimension**	**1.26**	**0.61**	**0.25**	**0.08**	**0.76**	**2.87**	**0.60**	**2.96**	**4**	**0.22**
Overall	Overall	0.46	0.95	0.00	0.78	0.84	2.01	0.98	2.13	4	3.85
Dimension	Dimension	2.32	0.69	0.34	0.02	0.70	4.77	0.51	4.81	4	1.28
Feature	Dimension	665.55	199.30	0.00	0.00	0.55	394.85	0.48	437.35	4	7.13
Individual dimension	Dimension	1.64	0.59	0.30	0.04	0.74/0.68[a]	3.34	0.55	3.47	3	0.18
Individual feature	Dimension	12.45	4.16	0.00	0.00	0.60/0.61[a]/0.83[b]	8.91	0.50	9.15	2	0.15

Note. "Individual" refers to separate spatial weights for each dimension or each feature, respectively. Experiment 2 did not differentiate between weighting at the dimension level and weighting at the feature level. The preferred model is printed in bold (first row).
[a] Separate weight for unbiased distractor. [b] Separate weight for unbiased target.

During fitting, we found that within a reasonable range (100–500 ms/item), search speed (t_{shift}) was mimicked by a combination of other parameters, and therefore, we fixed it to a reasonable value (200 ms/item). The final model version included eight free parameters, four parameters constant for all conditions and for both experiments (s_{dist}, s_{non}, t_{dist}, t_{res}), and only two parameters varying between experiments but constant within each experiment (w_{space} and w_{dimen}). The upper part of Table C1 compares various versions of this eight-free-parameter model, assuming suppression at various levels for Experiment 1 and Experiment 2, respectively. The goodness of fit for the preferred model was virtually perfect with $\chi^2 = 0.22$. When assuming that spatial suppression occurs at the overall-priority map also in Experiment 2 (i.e., influencing all objects), model fit decreased to $\chi^2 = 3.85$, and residual time estimates were unrealistically low ($t_{res} = 0.00$ ms; i.e., no time for encoding or responding). When suppression was implemented at the dimensional level in Experiment 1, the model fit decreased to $\chi^2 = 1.28$ because the model had no means to account for the effect of different-dimension-distractor location. When suppression was implemented at the featural level in Experiment 1, model fit was even worse ($\chi^2 = 7.13$) because the model could not account for location effects for the different-dimension distractor and the target, and many parameter estimates (s_{dist}, s_{non}, t_{res}, w_{dimen}) became highly unrealistic.

Conversely, granting the model more flexibility did not considerably improve model fit (lower part of Table C1): With separate spatial weights for both dimensions, model fit improved only slightly to $\chi^2 = 0.18$, and the algorithm selected different spatial weights for the two dimensions in Experiment 1. Interestingly and in contrast to what would be predicted by a lossy spillover, the different-dimension distractor was more strongly down-weighted in this model version. Finally, using individual spatial-weighting parameters for each object in Experiment 1 yielded a little extra improvement to $\chi^2 = 0.15$, but this improvement came at the cost of unrealistic residual times ($t_{res} = 0.00$; i.e., no time for encoding and responding) and nontarget saliencies ($s_{non} = 4.16$, i.e., nontargets more salient than the target).

These unrealistic parameter estimates and the need to fix the t_{shift} parameter indicated that some aspects of the model were not sufficiently constrained by the present data; the absolute value of the respective parameter estimates should therefore be interpreted with caution. However, the comparison of top-down weighting parameters between Experiments 1 and 2 was stable throughout: In almost all model versions, spatial weighting was considerably stronger in Experiment 2 than in Experiment 1. The second row of Table C1 deviates from this pattern because spatial weighting was forced to apply to all objects, so that the fitting algorithm decided to essentially switch this parameter off (i.e., it selected a value close to 1). The degree of dimension weighting varied across model versions, but it was always somewhat stronger for Experiment 2 than for Experiment 1 (see the main text for an interpretation of these patterns with regard to the top-down weighting parameters).

As a final remark on our modeling efforts, we would like to stress that our aim here was to show that a single top-down spatial-weighting factor is sufficient to explain all three location effects in Experiment 1, rather than establishing a biologically or computationally plausible holistic account of visual-search behavior including details on bottom-up processing. It is, however, easy to imagine how this model could be combined with existing biologically and computationally plausible ideas on saliency computation: One would simply have to replace the fitting-derived estimates of object saliencies with values predicted from computational models based on the visual input; models that already contain the notion of intermediate dimension maps (e.g., Navalpakkam & Itti, 2007; Walther & Koch, 2006) would be natural choices.

(*Appendices continue*)

Appendix D

Nondiscriminability of Enhancement and Suppression

It appears that spatial statistical learning of likely distractor locations is unanimously interpreted as suppression of the frequent-distractor region or location (Chelazzi et al., 2019; Failing, Wang, & Theeuwes, 2019; Liesefeld & Müller, 2019; Theeuwes, 2018; van Moorselaar & Slagter, 2020). However, suppression is not the only option for implementing a spatial bias. Alternatives would be to enhance the rare region/location(s) or to concurrently up- and down-weight the rare and frequent region/location(s), respectively. In mathematical terms, a spatial bias s_x on priority-map activation for a set of k objects $A = a_1, \ldots, a_m$ in the frequent-distractor region and objects $\bar{A} = a_{m+1} \ldots a_k$ in the remaining part of the display (referred to as the *rare-distractor region* here) might be implemented as suppression of the frequent region:

$$A_s = A \cdot s_1 \text{ and } \bar{A}_s = \bar{A}; \text{ with } 0 < s_1 < 1; \tag{1}$$

or enhancement of the rare region:

$$A_s = A \text{ and } \bar{A}_s = \bar{A} \cdot s_2; \text{ with } s_2 > 1; \tag{2}$$

or concurrent enhancement and suppression:

$$A_s = A/s_3 \text{ and } \bar{A}_s = \bar{A} \cdot s_3; \text{ with } s_3 > 1. \tag{3}$$

In CGS, the probability that object i is attended next (p_i) is determined by its relative activation at the overall-priority map via Luce's choice axiom (LCA; Luce, 1986):

$$p_i = \frac{a_i}{\sum_j a_j}, \tag{4}$$

where the numerator is the priority of object i, and the denominator is the sum of priorities for all objects in the display. LCA is a selection rule routinely applied to many choice problems ranging from high-level economic decisions to low-level perceptual choices (Pleskac, 2015).

For ease of understanding, the following proof focuses on only two objects (say, the target and the salient distractor on a trial with the two singletons occurring on opposite sides of the display), namely, object i with priority activation a_i from set A and object j with activation a_j from set $\bar{A}$, but the same applies for each object from the respective sets when all objects in the display are taken into account. What we aim to demonstrate is that the probability p_i of choosing object i is the same independently of whether we implement the spatial bias as suppression (Eq. 1), enhancement (Eq. 2), or concurrent enhancement and suppression (Eq. 3) and that the specific activations do not matter (drop out of the equation). We therefore enter Equations 1 and 2 into the right side of Equation 4 and equate the two terms:

$$\frac{a_i s_1}{a_i s_1 + a_j} = \frac{a_i}{a_i + a_j s_2},$$
$$\Leftrightarrow a_i^2 s_1 + a_i a_j s_1 s_2 = a_i^2 s_1 + a_i a_j,$$
$$\Leftrightarrow \ s_1 s_2 = 1 \ \Leftrightarrow \ s_2 = 1/s_1.$$

Thus, the two terms are equivalent if $s_2 = 1/s_1$, independently of the specific activations a_i and a_j; in plain words, suppressing the frequent region has the same effects as enhancing the rare region by the inverse of the suppression weight.

We can do the same for any combination of Equations 1–3, for example, for suppression (Eq. 1) and concurrent enhancement and suppression (Eq. 3):

$$\frac{a_i s_1}{a_i s_1 + a_j} = \frac{a_i/s_3}{a_i/s_3 + a_j s_3},$$
$$\Leftrightarrow \ \frac{a_i^2 s_1}{s_3} + a_i a_j s_1 s_3 = \frac{a_i^2 s_1}{s_3} + \frac{a_i a_j}{s_3},$$
$$\Leftrightarrow \ s_1 s_3 = 1/s_3 \ \Leftrightarrow \ s_3 = \sqrt{1/s_1}.$$

Thus, again, the two terms are equivalent, independently of the specific activations a_i and a_j. This equivalence holds for $s_3 = \sqrt{1/s_1}$, or in plain words, suppressing the frequent region has the same effects as concurrently enhancing the rare region and suppressing the frequent region by the square root of the inverse of the suppression weight. To summarize, we can arbitrarily convert suppression and enhancement weights from Equations 1–3 according to the following rules without any consequences for model predictions:

$$s_1 = 1/s_2 = 1/s_3^2,$$
$$s_2 = 1/s_1 = s_3^2,$$
$$s_3 = \sqrt{1/s_1} = \sqrt{s_2}.$$

A neuronal interpretation of the formula presented here as LCA (Eq. 4) makes this point even more pervasive, implying even more profound nondiscriminability of enhancement and suppression: In their influential review article, Carandini and Heeger (2012) suggest that normalization is a canonical neural computation affecting many neuronal representations, with particularly strong evidence for visual representations and with explicit mention of attentional mechanisms. They define the normalization process as one "in which the responses of neurons are divided by a common factor that typically includes the summed activity of a pool of neurons" (p. 51). More specifically, the normalized response R_j of neuron j is given by their normalization equation (Eq. 10, p. 54):

$$R_j = \gamma \frac{D_j^n}{\sigma^n + \sum_k D_k^n}, \tag{5}$$

(*Appendices continue*)

with D_j = neuron's (nonnormalized) driving input; $\sum_k D_k^n$ = normalization pool; and the constants γ, σ, and n determining the specifics of the neuron's behavior. For $\gamma = n = 1$ and $\sigma = 0$, and assuming that each item in the search display is represented by a set of neurons that all contribute to the normalization pool, this formula reduces to the form of LCA (Eq. 4).

Thus, we can interpret the (normalized) response R_j of a (set of) neuron(s) coding a particular object i at the overall-priority map as reflecting the probability p_i that object i is selected for the next allocation of attention. This further supports the plausibility of our model's assumption that LCA is involved in visual search and means that the equality relationships shown previously might apply already for priority maps (rather than the readout of these maps). In other words, if the neuronal implementation of priority maps is already normalized, enhancement and suppression are not even discriminable by direct measurement from the neurons implementing the priority maps.

Appendix E

Are Participants Aware of the Bias in Distractor Location?

Most researchers in the field, including us, assume that suppression of the frequent-distractor region/location is (by and large) an implicitly acquired bias, rather than a conscious strategy to ignore a particular region or location (Chelazzi et al., 2019; Sauter et al., 2018; van Moorselaar & Slagter, 2020). This question is tightly related to the recent interest in differentiating volitional and involuntary/experience-driven influences on search behavior (Awh et al., 2012; Theeuwes, 2018). In order to voluntarily suppress the frequent-distractor region, participants must be aware of the spatial bias. It is therefore common to collect data on participants' awareness regarding the distractor's spatial bias after the experiment, and so we did in this study. Showing that participants are unaware of the spatial bias is evidence that the suppression mechanism is involuntary. By contrast, showing that participants are aware does not necessarily imply that suppression was voluntary; rather, they might, for example, have become aware of their implicitly acquired bias after the experiment (maybe even triggered by the postexperimental question). Furthermore, together with others, we believe that both types of influence should be subsumed under the label "top-down" (Gaspelin & Luck, 2018b; Liesefeld et al., 2018), and this differentiation does not matter for the current study. Nevertheless, given the high interest in the question, here we also report and exploratively analyze and interpret the results from our relatively detailed postexperiment questions regarding awareness of the distractor bias. In particular, we asked the following questions (translated from German; with response options in square brackets; note that instructions introduced the salient distractors as "irrelevant bars"):

> *Question 1 (Q1):* Do you think that irrelevant bars appeared more often in one region of the display screen compared to others? [yes/no]

> *Question 2 (Q2):* Even if you have to guess, in which display half were irrelevant bars presented more frequently? [up/down/left/right]

> *Question 3 (Q3):* Which irrelevant bar appeared more often in this half? [bright bar/strongly tilted bar/both]

> *Question 4 (Q4):* How certain are you regarding the last two answers? [1–5, 1 = *perfectly certain*, 5 = *perfectly uncertain*]

The questions were successively posed on the computer screen, in this order; that is, Questions 2–4 were shown only after a response to the respective previous question had been given. Participants had the possibility of changing their answers (as they would on a physical questionnaire), but we tracked these changes and could therefore examine whether revealing additional questions affected their previous choices. Only a few participants made use of the possibility to change one or more of their responses (3 out of 25 in Experiment 1 and 8 out of 47 in Experiment 2). Only three participants in Experiment 2 (none in Experiment 1) changed their response to the first question (the answer to which was revealed by the second question). All three changed from the incorrect "no" to the correct "yes" (i.e., they "cheated" on that question). Otherwise, we focused our analyses on participants' final answers. Because of an oversight by Heinrich R. Liesefeld, seven participants from Experiment 1 who were included in the main analyses were not queried for the frequent region (Q2) and were thus excluded from the following analyses.

Analyzing responses to the various questions separately (Table E1) indicated that participants responded clearly above chance to the question regarding the frequent-distractor location in Experiment 2 (featuring a spatially biased different-dimension distractor) and somewhat above chance to that question in Experiment 1 and, in both experiments, to the question regarding which distractor was spatially biased.

(*Appendices continue*)

Table E1
Results of the Postexperiment Questions on Awareness of the Spatial Distractor Bias in Experiment 1 (Exp. 1; n = 25), Experiment 2 (Exp. 2; n = 47), and Experiment 2 Without Those Participants Who Cheated on the First Question (Exp. 2′; n = 44)

(Combination of) questions	Chance	Exp. 1	Exp. 2	Exp. 2′
Q1: General	50.0%	6 (24.0%)	25 (53.2%)	22 (50.0%)
Q2: Spatial	25.0%	9 (36.0%)	24 (51.1%)	22 (50.0%)
Q3: Distractor type	33.3%	12 (48.0%)	20 (42.6%)	19 (43.2%)
Q1 $\wedge$ Q2	12.5%	3 (12.0%)	12 (25.5%)	10 (22.7%)
Q1 $\wedge$ Q3	16.7%	3 (12.0%)	09 (19.2%)	08 (18.2%)
Q2 $\wedge$ Q3	8.3%	5 (20.0%)	14 (29.8%)	13 (29.6%)
Q1 $\wedge$ Q2 $\wedge$ Q3	4.2%	0 (00.0%)	07 (15.9%)	06 (13.6%)
Q2 $\mid$ Q1	25.0%	3 (50.0%)	12 (48.0%)	10 (45.5%)
Q2 $\mid \neg$Q1	25.0%	6 (31.5%)	12 (54.5%)	12 (54.5%)
Q3 $\mid$ Q1	33.3%	3 (50.0%)	09 (36.0%)	08 (36.3%)
Q3 $\mid \neg$Q1	33.3%	3 (15.8%)	11 (50.0%)	11 (50.0%)
Confidence (C; *perfectly certain* = 1; *perfectly uncertain* = 5)				
C Overall		3.8	3.6	3.6
C $\mid$ Q2		3.4	3.5	3.5
C $\mid \neg$Q2		4.0	3.7	3.7
C $\mid$ Q3		3.7	3.5	3.4
C $\mid \neg$Q3		3.9	3.7	3.8

Note. Q1 = Question 1; Q2 = Question 2; Q3 = Question 3. Upper part: Number (and percentage) of participants who responded correctly on Q1–Q3. Results are given for correct responses on individual items, conjunctive correct responses, and separated by whether participants claimed to have noticed a spatial bias in general. Lower part: Response confidence overall and contingent on whether Q2 and Q3 were answered correctly.

However, correctly answering the question on where a distractor appeared most often does not necessarily imply that a given participant was aware of the spatial bias during the experiment. Those who are aware should also respond (without cheating) that there was a spatial bias (Q1). Only three participants in Experiment 1 claimed to have perceived a spatial bias and also correctly indicated its polarity (Q1 $\wedge$ Q2); none of these also correctly indicated that only the orientation distractor was spatially biased (Q1 $\wedge$ Q2 $\wedge$ Q3). Thus, participants in Experiment 1 were largely unaware of the spatial bias, and maximally 3 out of the 25 participants analyzed here could have voluntarily employed their knowledge to produce the general spatial suppression observed in Experiment 1. In Experiment 2, 10 participants (without "cheating") claimed to have perceived a spatial bias and got the biased region right (Q1 $\wedge$ Q2). To voluntarily suppress only the different-dimension distractor, they also needed to be aware that this specific distractor was spatially biased (Q1 $\wedge$ Q2 $\wedge$ Q3), which was the case for only 6 out of 47 participants. Thus, all in all, the results of the postexperiment questions indicate that participants did not voluntarily suppress the frequent-distractor region but that the observed effects were purely experience driven. Those few participants who appeared to possess the knowledge required to voluntarily produce the observed pattern of suppression (three in Experiment 1 and six in Experiment 2) might also have guessed correctly or become aware of their knowledge when being asked the postexperimental questions.

That participants in Experiment 1 performed below chance on Q1 and that most participants did not change their response to Q1 indicates that they did not guess on that question but largely answered truthfully. Indeed, in contrast to Q2 and Q3, we did not ask them to guess. In line with this, in Experiment 1, performance on Q2 and Q3 was higher for those participants who claimed to have perceived a bias. This pattern was reversed in Experiment 2 (although differences were small): Here participants claiming to have perceived a spatial bias actually performed worse than those who admitted their ignorance. This might indicate that Q 2 and Q3 often brought that knowledge to awareness.

Another indication for awareness and voluntary, strategic use of knowledge regarding the spatial bias would be that participants are confident that their responses are right. However, response confidence regarding Q2 and Q3 on a scale from *perfectly certain* (1) to *perfectly uncertain* (5) was low on average (3.8 in Experiment 1 and 3.6 in Experiment 2). For both experiments and questions, participants getting the respective answer right were only slightly more confident than those getting it wrong. Those three participants who could potentially have used their knowledge to produce the spatial bias observed in Experiment 1 were not considerably more confident than the rest (3.3); only one of these participants was perfectly certain of their responses (but incorrectly claimed that the luminance distractor was spatially biased). The six participants in Experiment 2 were somewhat more confident than the rest (2.8) but far from being highly confident, as one would expect if they had used that knowledge as a voluntary strategy throughout the experiment; only one of these participants was perfectly certain of their responses. In our opinion, these data convincingly show that the suppression observed in the main analyses could not have been driven by a voluntary strategy but was instead due to an implicitly acquired spatial bias (experience).

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