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Probability cueing of singleton-distractor locations in visual search:

priority-map- vs. dimension-based inhibition?

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ABSTRACT

Observers can learn the likely locations of salient distractors in visual search, reducing their potential to cause interference. While there is agreement that this involves positional suppression of the likely distractor location(s), it is contentious at which stage the suppression operates: the search-guiding *priority map*, which integrates *feature-contrast* signals (e.g., generated by a red amongst green or a diamond amongst circular items) across dimensions, or the distractor-defining dimension. On the latter, dimension-based account (Sauter et al., 2018), processing of, say, a *shape*-defined *target* should be unaffected by distractor suppression when the *distractor* is defined by *color*, because in this case only color signals would be suppressed. At odds with this, Wang & Theeuwes (2018a) found slowed processing of the target when it appeared at the likely (vs. an unlikely) distractor location, consistent with priority-map-based suppression. Adopting their paradigm, the present study replicated this target location effect. Crucially, however, changing the paradigm by making the target appear as likely at the frequent as at any of the rare distractor locations and making the distractor/non-distractor color assignment consistent abolished the target location effect, without impacting the reduced interference for distractors at the frequent location. These findings support a flexible locus of spatial distractor suppression – priority-map- or dimension-based – depending on the prominence of distractor 'cues' provided by the paradigm.

       Key words: search guidance, attentional capture, statistical (distractor location) learning, distractor suppression

Public Significance Statement: Distraction by a salient visual stimulus outside the 'focus' of the task at hand happens often in everyday life. The present study examined whether and how acquired (implicit) 'knowledge' of where distractors are likely to occur in visual displays helps observers to mitigate distraction. The results confirmed that observers can learn to suppress distracting stimuli at likely locations. Further, they showed that suppression may operate at different levels in the hierarchically organized system of search guidance that determines the priorities with which objects are to be attended in the environment.

INTRODUCTION

Recently, there has been a growing interest in statistical, location-probability learning in visual search. While most of this research has focused on the learning of target locations (e.g., Druker & Anderson, 2010; Geng & Behrmann, 2002, 2005; Jiang, Swallow, & Rosenbaum, 2013; Walthew & Gilchrist, 2006; see also Miller, 1988; Müller & Findlay, 1987; Shaw & Shaw, 1977), more recently, there have been various attempts to extend this to the learning of distractor locations (e.g., Ferrante, Patacca, Di Caro, Della Libera, Santandrea, & Chelazzi, 2018; Goschy, Bakos, Müller, & Zehetleitner, 2014; Leber, Gwinn, Hong, & O'Toole, 2016; Sauter, Liesefeld, Zehetleitner, & Müller, 2018; Sauter, Liesefeld, & Müller, 2019; Wang & Theeuwes, 2018a). Collectively, these studies showed that observers appear to be able to learn, from experience, the spatial distribution of salient but task-irrelevant singleton, or 'pop-out' distractors in the search array, to minimize the interference – or potential for 'attentional capture' – normally caused by such distractors (an effect Goschy et al., 2014, referred to as 'distractor location probability cueing'). This appears to be the case whether the salient distractor occurs consistently at one specific, 'most frequent' location in relatively sparse displays (e.g., 4-item displays in Ferrante et al., 2018; 8-item displays in Wang & Theeuwes, 2018a, b) or within a 'frequent' region encompassing multiple possible locations, such as a whole display half, in dense displays (39-item displays in Goschy et al., 2014, and Sauter et al., 2018). However, even though there is no dispute about the fact that observers can learn the statistical distribution of salient distractors and use this 'knowledge' to minimize the interference of distractors occurring at frequent locations, conclusions differ with regard to the locus, or processing stage, within the functional architecture of search guidance at which the observed reduction of distractor interference (for frequent vs. infrequent locations) is realized.

In principle, there are two possibilities: the level of the search-guiding 'overall-saliency' (Guided Search; e.g., Wolfe & Gancarz, 1997; Wolfe, 2007) or 'priority' (e.g., Fecteau & Munoz, 2006) map of the search array, essentially inhibiting any saliency signals at the frequent distractor location and thus preventing distractors at this location from summoning attention. Or a level below this map, for instance, the feature-contrast signals generated in the distractor-defining dimension, which – according to the Dimension-Weighting Account (DWA, e.g., Found & Müller, 1996; Müller, Heller, & Ziegler, 1995; Müller, Reimann, & Krummenacher, 2003) – may be 'down-weighted' prior to their integration with feature-contrast signals from other dimensions

(e.g., that of the target) at the 'supra-dimensional' priority map (Müller, Geyer, Zehetleitner, & Krummenacher, 2009; Zehetleitner, Goschy, & Müller, 2012)[1]. Recall that saliency-based accounts of search guidance (from Koch & Ullman, 1985, onwards) assume that the priority map is 'feature-' or 'dimension-blind': due to the loss of feature- and dimension-specific information in the signal integration process, priority signals indicate only to what degree an object at a given location differs from its surround, but not in what way it differs. Accordingly, if distractor suppression operates via inhibition of the likely distractor location at the level of the priority map, this would not only impact the potential of distractor-generated signals to summon attention (reducing interference from distractors appearing at the likely, vs. an unlikely, distractor location), but necessarily also that of target-generated signals (slowing detection of and, consequently, responding to targets at the likely vs. unlikely locations). Critically, this would be the case whether the target is defined in a different dimension to the distractor (e.g., color-defined distractor vs. orientation/shape-defined target) or in the same dimension. By contrast, if distractor suppression (stronger at frequent than at rare distractor locations) operates already on signals at the level of the distractor dimension (i.e., prior to their integration on the priority map), as envisaged by the DWA, then target processing should be unaffected at least when the target is defined in a different dimension to the distractor. Thus, with target and distractor defined in different dimensions, whether or not target processing is affected by learnt suppression of the likely distractor location(s) is diagnostic as to the level, in the hierarchical architecture of search guidance, at which learnt positional distractor suppression is realized.[2]

However, two recent investigations, both using an 'additional-singleton' paradigm (Theeuwes, 1992) with a target defined by a unique shape/orientation and, additionally, a more (bottom-up) salient distractor singled out by color from the homogeneous background items,

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[1] 'Down-weighting' means scaling the 'bottom-up saliency' of the distractor by some inhibitory weight, yielding the effective 'selection saliency' (see Zehetleitner, Koch, Goschy, & Müller, 2013), where the acquired inhibitory weight is greater for the frequent vs. the rare distractor locations (see Sauter et al., 2018). Accordingly, the effect of inhibiting a distractor signal depends on how bottom-up salient a distractor is, and inhibition is a matter of degree rather than ever absolute (see, e.g., Müller, Töllner, Zehetleitner, Geyer, Rangelov, & Krummenacher, 2010).

[2] Suppression of the distractor at the likely location might conceivably also operate at the featural level, that is: the coding of distractor features might be inhibited directly, reducing their potential to generate feature contrast in the distractor dimension (e.g., Gaspelin & Luck, 2018a). In this case, too, one would not expect a target location effect. We come back to the issue of dimension- vs. feature-based distractor suppression in Experiment 3 and the General Discussion. – Also note that when the distractor occurs equally likely at each display location, as is typical in studies not examining spatial biases in the distractor distribution, feature- or dimension-based down-weighting would be applied uniformly across the whole display. Both spatial and non-spatial distractor down-weighting may operate voluntarily (according to task goals) or involuntarily (e.g., driven by selection history or statistical learning), or involve a combination of both (cf. Gaspelin & Luck, 2018b).

came to the opposite conclusions. Finding a target location effect (slower reaction times to targets appearing at likely vs. unlikely distractor locations) in their paradigm, Wang and Theeuwes (2018a) concluded that suppression operates at the level of the priority map (see also Ferrante et al., 2018). Sauter et al. (2018), by contrast, interpreted the absence of a target location effect in their paradigm (backed by Bayes factor analysis favoring the null-hypothesis) as evidence for a dimension-based locus of distractor suppression. Notably, Sauter et al.'s (2018) null-finding with 'different-dimension' distractors (color-defined distractor, orientation-defined target) contrasted with a robust target location effect in a condition with 'same-dimension' distractors, in which both distractor and target were defined by a (more or, respectively, less strong) orientation contrast to the background items. Sauter et al. (2018) interpreted this differential effect pattern in terms of a qualitative difference in the level on which distractor suppression operates: dimension-based with different-dimension distractors and priority-map-based with same dimension distractors (see Sauter et al., 2019, for confirmatory evidence from carry-over of distractor location probability cueing acquired, on day 1, as a result of sampling an uneven spatial distribution of *same*-dimension distractors [distractor more likely in frequent vs. rare region] to test, on day 2, with an even distribution of *different*-dimension distractors [distractor equally likely in both regions]).

Given this impasse (Wang & Theeuwes, 2018a, vs. Sauter et al., 2018), the present study was designed to examine why two studies, using at the surface-level similar, additional-singleton paradigms led to fundamentally different *theoretical* conclusions. Specifically, adopting and modifying Wang and Theeuwes' (2018a) original paradigm, we examined what the critical (and potentially uncontrolled or 'confounding') factors might be in their paradigm, vis à vis the Sauter-et-al. (2018) paradigm, that drive the target location effect.

Since our experiments used variations of the original Wang-and-Theeuwes (2018a) paradigm, it is befitting to describe this paradigm and the essential findings in some more detail. Wang and Theeuwes (2018a) presented observers with a ring of 8 shape stimuli (radius: 4° of visual angle), one of which was designated a 'target' item: either the only circle in the array, presented amongst 7 diamond shapes; or the only diamond in the array, presented amongst 7 circular shapes (target-to-non-target assignment was changing randomly across trials). A target was present on all trials. The task was a 'compound-search' task: observers were required to find the target shape and respond to the orientation of a line within it, where a line of the same

orientation (as in the target shape) or a different orientation appeared in each of the non-target shapes. All stimuli – except for possibly one: the additional singleton 'distractor' – were either green or red on a given trial. On distractor-present trials (67% of the total number), one non-target shape appeared in an odd-one-out color: either red (when the other items were green) or green (when the other items were red).

The distractor could appear at any of the 8 possible locations, but, crucially, it was most likely to appear at one, 'frequent' distractor location ($p = .65$ on distractor-present trials, as compared to $p = .05$ for each of 7 the remaining, 'rare' distractor locations), randomly selected (and kept constant) for each observer. Although this was not expressly stated, it is clear from the analyses conducted that the distractor never coincided with the target location on distractor-present trials. On distractor-absent trials, the target appeared with equal likelihood at each location (including the frequent distractor location). The key findings were: (i) a *distractor location effect* on distractor-present trials: distractor interference was markedly reduced when the distractor appeared at the frequent (vs. a rare) distractor location (as well, to a lesser extent when it appeared at a location adjacent to the frequent location); and (ii) a *target location effect* on distractor-absent trials: responding to the target was markedly slowed when it appeared at the frequent (vs. a rare) distractor location. Although also measurable on distractor-present trials, the finding of a target location effect on distractor-absent trials is particularly diagnostic, because it is unaffected by any processes invoked to deal with an irrelevant singleton in the display. This effect pattern was obtained irrespective of the repetition versus swapping (see also Wang & Theeuwes, 2018b), across trials, of the color assignment to the distractor and non-distractor items, arguing against feature-based effects.[3] Wang and Theeuwes (2018a) took this pattern to be indicative of learnt spatial suppression of the likely distractor location on the attention-guiding priority map, thereby reducing the potential of distractors at this location to capture attention.[4]

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[3] By introducing uncertainty with regard to both the distractor- and the target-defining features, thus limiting the use of distractor and target 'templates' to top-down modulate search guidance (distractor templates, e.g. Woodman & Luck, 2007; target templates, e.g., Soto, Heinke, Humphreys, & Blanco, 2005), this paradigm was meant to evoke a saliency-based, 'singleton detection' search mode (Bacon & Egeth, 1994), producing strong 'attentional capture' by the distractor singletons. Going for strong capture effects to start with is reasonable, given the aim of the study was to examine for modulations of the capture effect by statistical learning of the likely distractor location. Of note, these effects are also discernible, though much weaker, when observers are induced to operate a 'feature search' mode (see Wang & Theeuwes, 2018b).

[4] Reduced attentional capture is a plausible explanation, especially given the observation of a P_D component (an electrophysiological index of positional inhibition; Hickey, Di Lollo, & McDonald, 2009; Sawaki, Geng, & Luck, 2012) for the likely distractor location even on distractor-absent trials, i.e., when there was actually no to-be-

The present experiments examined two uncontrolled or potentially 'confounding' factors that might be responsible for the target location effect in Wang and Theeuwes' (2018a) paradigm (influences of these factors were systematically eliminated or ruled out by design in Sauter et al., 2018): (i) carry-over, into a given trial n, of inhibition placed on the distractor location on trial $n-1$ (Experiment 1); and (ii) a reduced likelihood (on distractor-present trials) of the target appearing at the frequent as compared to any of the rare distractor locations (Experiment 2). Experiment 3 went on to examine whether making the color of the distractor, vis-à-vis that of the non-distractor items, predictable (as had been the case in Sauter et al., 2018) would abolish the target location effect. Taken together, the results indicate that learnt suppression of the likely distractor location may actually operate on either the priority map or a (dimension-specific) map of feature contrasts in the distractor dimension, with the level depending on the prominence of distractor-related 'cues' (spatial, feature-definitional) provided by the paradigm.

EXPERIMENT 1

Experiment 1 was designed to replicate the findings of Wang and Theeuwes (2018a) and, additionally, examine whether their pattern of result (in particular, the target location effect) is attributable to an uncontrolled variable: *positional* inter-trial effects, which had not been (or, rather, could not be systematically) examined by Wang and Theeuwes (2018a).

Recall that in Wang and Theeuwes (2018a), a distractor occurred with 65% likelihood at the frequent distractor location, generating a substantial suppression effect centered on this location(s). In our own paradigm (see Supplement in Sauter et al., 2018; see also, e.g., Geyer, Müller, & Krummenacher, 2007; Kumada & Humphreys, 2002; Maljkovic & Nakayama, 1996), such inhibition effects do carry over across trials, that is: if the target on the current trial n appears at the same location as a distractor on the previous trial $n-1$, RTs to the target are significantly increased – ow ing to lingering inhibition placed on the 'rejected' distractor location on the previous trial (a type of cross-trial 'inhibition-of-return', IOR, effect); likewise, if a distractor on trial n falls at the same location as a distractor on trial $n-1$, the interference effect caused by the current distractor is reduced.

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inhibited distractor at this (or any other) location in the display (Theeuwes, 2018). However, to date, potential influences of other mechanisms have not been ruled out, in any of the relevant studies (including Ferrante et al., 2018, and Sauter et al., 2018). We come back to this issue towards the end of the General Discussion.

Thus, with regard to the effect pattern reported by Wang and Theeuwes (2018a), that is, the target location effect (distractor-absent trials) as well as the distractor location effect (distractor present trials): making a distractor (65%) likely to appear at one specific location versus unlikely (5%) at one of the (seven) other locations introduces imbalances, between the frequent and rare distractor locations, in potentially critical inter-trial transitions. For instance, on a given distractor-absent trial, the likelihood for a *target* (on trial n) to follow a distractor (on trial $n-1$) at the frequent distractor location is $.65 \times .125 = .0825$, which compares with a probability of $.05 \times .125 = .00625$ for a *target* to follow a distractor at the exact-same rare distractor location. This means that a target on distractor-absent trial n would have been more likely to fall on an inhibited (i.e., trial $n-1$ distractor) location when it appeared at the frequent distractor location than when it appeared at one of the rare locations, which could account for the slower RTs to targets appearing at the frequent versus the rare distractor locations. Similarly, on a given distractor-present trial, the likelihood for a *distractor* (on trial n) to follow a *distractor* (on trial $n-1$) at the frequent distractor location is $.65 \times .65 = .4225$, which compares with a probability of $.05 \times .05 = .025$ for a distractor to follow a distractor at the exact-same rare distractor location. This means that a distractor (on distractor-present trial n) would have been more likely to fall on an inhibited (i.e., trial $n-1$ distractor) location when it appeared at the frequent distractor location than when it appeared at one of the rare locations, which could go some way to account for the reduced interference caused by distractors appearing at the frequent versus the rare locations.

Thus, it is possible that at least some, if not all, of Wang and Theeuwes' (2018a) critical effects are attributable to passive carry-over across trials of location-based inhibition, rather than to statistical learning of distractor location probabilities. This may apply especially to the target location effect, which is theoretically critical for distinguishing between priority-map- and dimension-based accounts of distractor location probability cueing (see Introduction) and, accordingly, is the critical effect examined in Experiment 1. A role of positional cross-trial inhibition in this effect would be consistent with Sauter et al. (2018), who showed that when positional inter-trial 'confounds' were eliminated, there was no target location effect with different-dimension distractors (in contrast with the distractor location effect, which survived correction for positional inter-trial confounds).

As the number of trials in the Wang and Theeuwes (2018a) experiment (720 trials) was insufficient for estimating inter-trial effects on RTs for relatively rare cross-trial transitions, the number of trials in Experiment 1 was greatly increased to 3000 overall (administered in two separate sessions). This ensured some 29 observations, on average, per participant for the rare transitions with the target on trial n appearing at the exact-same rare (distractor) location as a distractor on trial $n - 1$ (yielding a reasonably reliable measure of cross-trial inhibition for the rare distractor locations). In addition, it permitted us to examine for learning/practice effects in distractor suppression (e.g., Gaspelin & Luck, 2018a; Geyer, Krummenacher, & Müller, 2008; Müller et al., 2009; Zehetleitner et al., 2012; see also Cunningham & Egeth, 2016; Töllner; Conci, & Müller, 2015). In all other respects, Experiment 1 was identical in design and procedure to the study of Wang and Theeuwes (2018a).

Method

Participants. A cohort of 24 participants (mean age: 28.33 years; age range: 18-40 years; 15 female) were recruited at Ludwig-Maximilians-University (LMU) Munich for this experiment. This sample size was determined based on the crucial target location effect reported by Wang and Theeuwes (2018a). Although they did not report effect sizes, we calculated a $d_z = .56$ based on the reported t test. With $\alpha = .05$, $1 - \beta = .80$, and one-tailed testing (the direction of the effect was predicted: RTs to targets appearing at the inhibited, frequent-distractor location were predicted to be slowed, and not expedited!), the sample size needed to replicate this effect is 22 participants. As this is close to the 24 participants in the original Wang and Theeuwes study, we decided to collect the same number of participants, to be on the safe side. As we used a much larger number of trials, thereby reducing the measurement error in each individual average, we actually expected a much higher power. Indeed, post-hoc power calculations indicated a $1 - \beta = .9997$ for Experiment 1 and $1 - \beta = .99$ for session 1 of Experiment 2 (see below).

All participants were right-handed and all reported normal or corrected-to-normal vision, including normal color vision. They received 9 Euro per hour in compensation for their service. The study protocol was approved by the LMU Faculty of Pedagogics & Psychology Ethics Board. Informed consent was obtained from all participants prior to the experiment.

Apparatus. The experiment was conducted in a sound-reduced and moderately lit test room. Stimuli were presented on a CRT monitor at 1280×1024 pixels screen resolution and a

refresh rate of 120 Hz. Stimuli were generated by Psychophysics Toolbox Version 3 (PTB-3) (Brainard, 1997) based on MATLAB R2016a (The MathWorks® Inc). Participants viewed the monitor from a distance of 60 cm (eye to screen) and gave their responses by pressing the leftward- ('horizontal) or upward-pointing ('vertical') arrow on the keyboard with their right-hand index or middle fingers, respectively.

Stimuli. The search displays (see Figure 1 for an example display) were composed of eight outline shapes (circles or diamonds) equidistantly arranged around a virtual circle with a radius of 4° of visual angle. The display items consisted of either one circle (target) and seven diamonds (non-targets), or, alternatively, one diamond (target) and seven circles (non-targets). The diameter of the circle shapes and, respectively, the side length of the diamond shapes was 2° of visual angle. Each outline shape contained a vertical or horizontal gray line inside (0.3° x 1.5°), with half of the internal lines being (randomly) vertical and half horizontal. In a certain percentage of trials (see below), one of the non-target shapes (the distractor) differed in color from all the other shapes, being either green (CIE [Yxy]: 22.5, 0.32, 0.55) amongst homogeneous red shapes (CIE [Yxy]: 8.82, 0.54, 0.36), or red amongst homogeneous green shapes. All search displays were presented on a black screen background (3.58 cd/m^2), with a white fixation cross (1° × 1°) in the center.
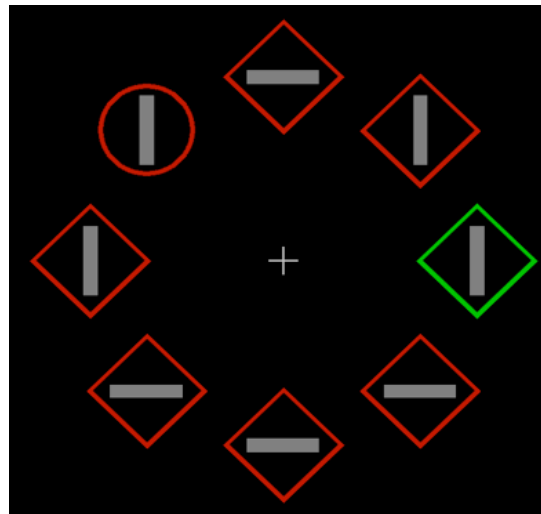
Figure 1. *Example of a visual search display. The search target is the singleton shape (here the only circle), and the distractor is a color singleton (here, the only green, diamond shape). Participants responded to the orientation of the bar inside the target shape (here vertical).*

Design. The target, which was present on all trials, was a singleton, odd-one-out shape amongst the 7 non-target shapes (either a circle or a diamond, randomly assigned on each trial). On trials without a distractor, the target was equally likely to appear at all 8 possible locations. On trials on which a distractor was present in the display, the target appeared equally frequently at all of the remaining 7 non-distractor locations. A singleton distractor, defined by a unique color (red or green, randomly assigned on each trial), appeared in 66% of the trials. If a distractor was present, it appeared with a likelihood of 65% at one, consistent location (frequent distractor location) and with a likelihood of 35%/7 at each of the other 7 locations (infrequent distractor locations). Note that the target and the distractor never appeared at the same location. The frequent distractor location remained the same for each participant, and was counterbalanced across participants. The experiment consisted of 3000 trials in total, subdivided into 2 sessions; each session was subdivided into 25 blocks of 60 trials each. Participants performed the two sessions on separate days.

Procedure. Each trial began with the presentation of a fixation cross for 500 ms, followed by the search array, which was shown until the participant gave a response. The intertrial interval (ITI) ranged between 500 and 750 ms (determined randomly). Participants were instructed to search for the target (the differently shaped item) and identify and respond to the orientation of the line inside – vertical or horizontal – as fast and as accurately as possible. For a vertical line, participants pressed the up arrow on the keyboard; and for a horizontal line the left arrow. At the end of the experiment, participants completed a post-experiment questionnaire, designed to determine whether they were aware of the frequent distractor location. This involved a two-stage procedure: first, participants had to indicate whether the distractor distribution was equal across all locations, or centered on one specific location; second, (even when they had given an equal response in stage 1) participants had to give a forced-choice response at which of the 8 locations the distractor had occurred most frequently (by marking the corresponding location on the 'display' depicted on the answer sheet). Prior to the main experiment (in each session), participants performed 60 unrecorded practice trials to re-/familiarize themselves with the task. Between trial blocks, participants could take a break of a self-determined length. Overall, each session took about one hour and 20 minutes to complete.

Bayes-Factor analysis. Bayesian analyses of variance (ANOVAs) and associated post-hoc tests were carried out using JASP 0.9.0.1 (*http://www.jasp-stats.org*) with default settings. All

Bayes factors for ANOVA main effects and interactions are 'inclusion' Bayes factors calculated across matched models. Inclusion Bayes factors compare models with a particular predictor to models that exclude that predictor. That is, they indicate the amount of change from prior inclusion odds (i.e., the ratio between the total prior probability for models including a predictor and the prior probability for models that do not include it) to posterior inclusion odds. Using inclusion Bayes factors calculated across matched models means that models that contain higher-order interactions involving the predictor of interest were excluded from the set of models on which the total prior and posterior odds were based. Inclusion Bayes factors provide a measure of the extent to which the data support inclusion of a factor in the model. Bayesian *t*-tests were performed using the ttestBF function of the R package 'BayesFactor' with the default setting (i.e., rscale ="medium").

Results and Discussion

All RT analyses below excluded outliers, defined as trials on which RTs were slower than 3 secs or faster than 150 ms (approximately 2% of trials, which is comparable to Wang and Theeuwes, 2018a), as well as trials on which participants made an incorrect response. In the analyses of inter-trial effects, the very first trial in each block was additionally excluded, because of the break between that trial and the last trial in the preceding block.

In the first instance, the data were analyzed analogously to Wang and Theeuwes (2018a), except that, since our experiment consisted of two sessions, we also examined for differences between the sessions reflecting practice effects. See Figures 2 (RTs and error rates as a function of target and distractor condition) and 3 (RTs and error rates as a function of the distance of the distractor from the frequent distractor location) for the results.

Distractor location effects. To examine how distractor presence at the high-frequency position compared to presence at one of the low-frequency positions affected RT performance, and whether the pattern differed between sessions, we performed a repeated-measures ANOVA with distractor condition (distractor absent, distractor at frequent location, distractor at rare location) and session (1, 2) as factors. This ANOVA revealed both main effects to be significant, distractor condition ($F(2,46) = 126.90, p < .001, \eta_p^2 = .85, BF > 100$) and session($F(1,23) = 37.80, p < .001, \eta_p^2 = .62, BF > 100$); the interaction was non-significant ($F(2,46) = 2.83, p = .07, BF = 0.14$). Mean RTs are depicted in Figure 2. The main effect of session reflected faster

RTs in the second compared to the first session (mean RTs: 974 ms vs. 1123 ms). Concerning the main effect of distractor condition, post-hoc *t*-tests revealed that, relative to the distractor-absent baseline (996 ms), there was significant RT interference wherever the distractor occurred (frequent distractor location, $t(23) = 7.24, p < .001, BF > 100$; rare distractor locations, $t(23) = 14.56, p < .001, BF > 100$), but the interference was substantially reduced when the distractor occurred at the frequent location (57 [= 1053 − 996] ms) compared to a rare location (130 [= 1126 − 996] ms), $t(23) = 9.53, p < .001, BF > 100$. The error rates (which were low overall: 3% on average) mirrored the RT pattern, effectively ruling out that the observed RT effects were driven by differential speed-accuracy trade-offs.
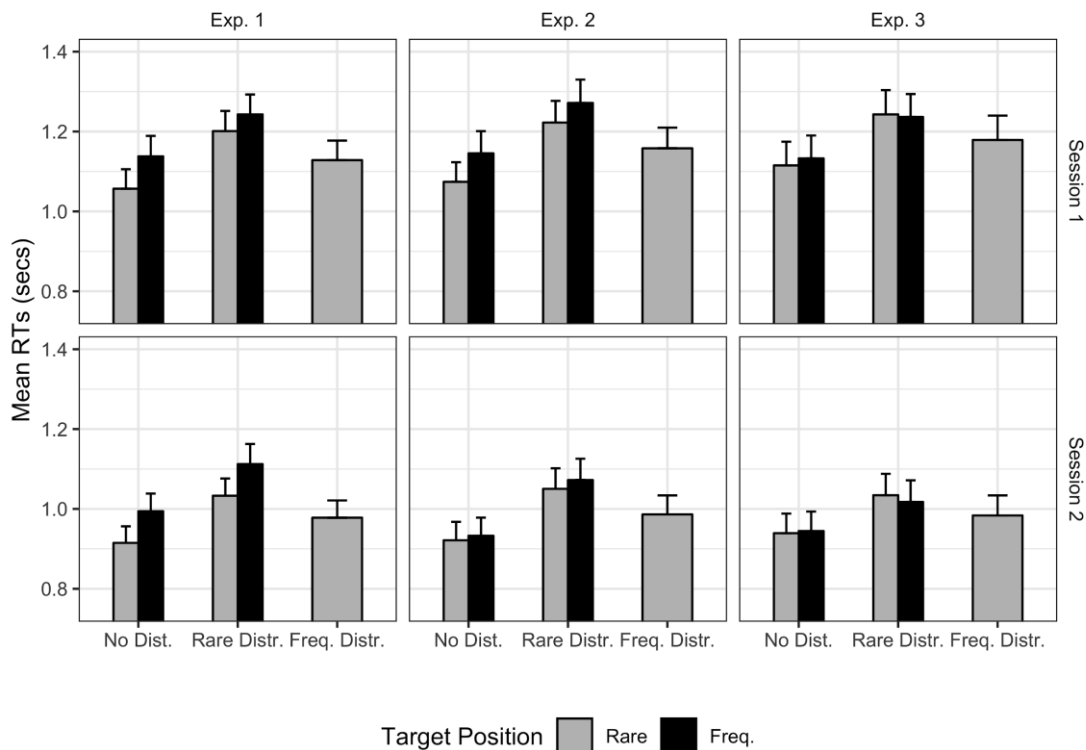
Figure 2. *Mean response times (RTs) for the distractor conditions (No Dist.: distractor absent; Rare Dist.: distractor at rare location; Freq. Distr.: distractor at frequent location), separately for the target positions (grey: target at rare distractor location; black: target at frequent distractor location). The top and bottom panels present the first and the second experimental session, respectively. Error bars denote one standard error. Note that the factor target position is defined only for distractor-absent trials and trials with a distractor at a rare location (on both of which the target could occur at either the frequent or one of the rare distractor locations); on*

trials with a distractor at the frequent location, the target could appear only at one of the rare locations (as the target and distractor positions never coincided).
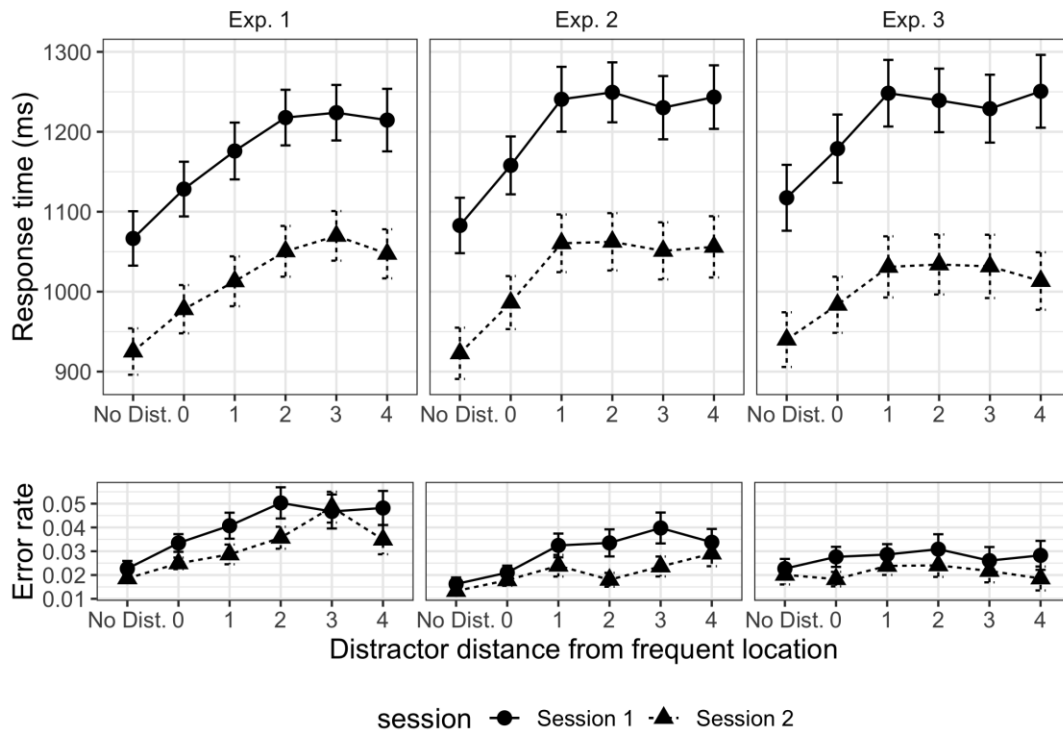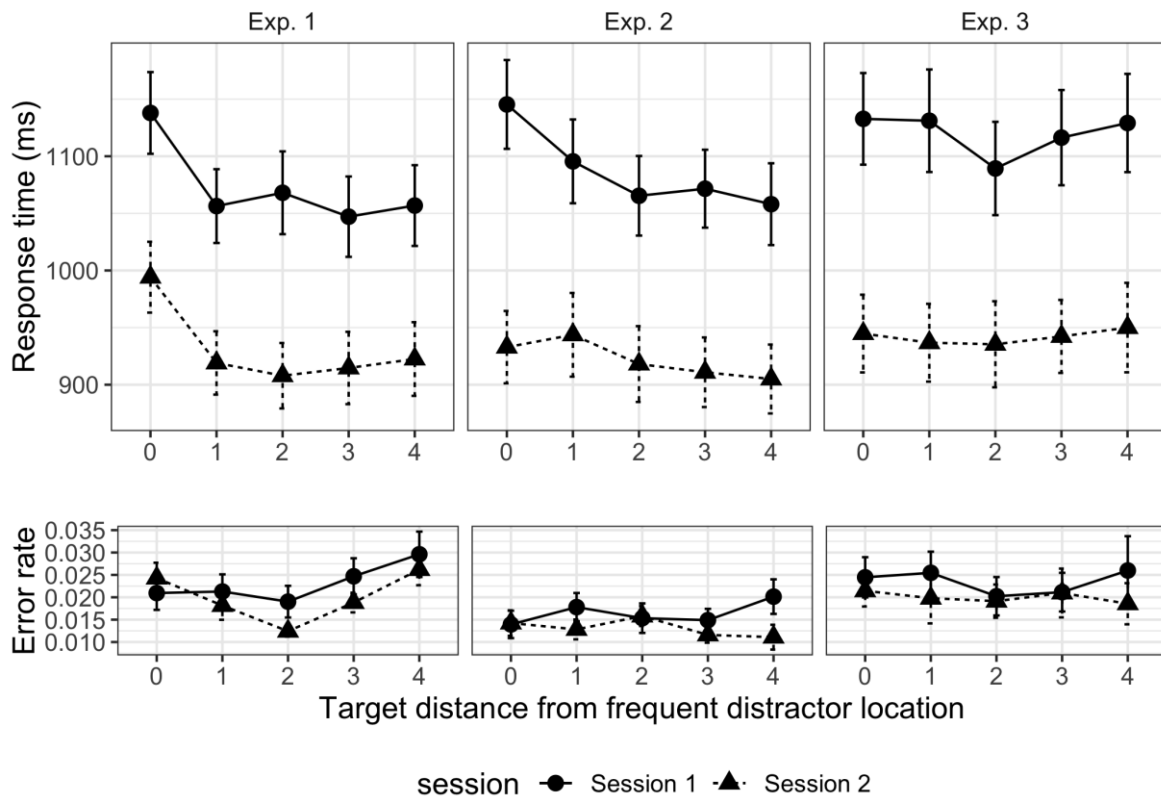

Figure 3*. Mean response times (RTs, upper panels) and mean error rates (lower panels) as a function of the distance of the distractor from the frequent distractor location, separately for the first and the second experimental session. Error bars denote one standard error. Distractor distances 0–4 denote the distance of the distractor from the frequent location (0 = distractor at frequent location; 1 = distractor at location adjacent to frequent location; etc.); 'No Dist.' denotes the distractor-absent baseline.*

Further, RTs to the target increased as the distractor on a given trial was presented further away from the frequent distractor location: an ANOVA with the factors 'distance of distractor from frequent distractor location' (ranging from distance 0 to distance 4) and session revealed the main effect of distance to be significant, $(F(4,92) = 19.60, p < .001, \eta_p^2 = .46, BF > 100)$, without interacting with session $(F(4,92) = 0.38, p = .82, BF = 0.03)$. Importantly, the main effect of distance remained significant when distance 0 (i.e., the frequent location itself) was removed from the analysis: when the distractor was located adjacent to the frequent distractor location (distance 1), the interference effect (99 ms) was larger compared to distance 0 (57 ms)

but smaller compared to greater distances (e.g., 141 ms for distances 2, 3, and 4 combined, which showed little difference amongst each other). This pattern was again mirrored in the error rates. Thus, a distractor appearing in close proximity to the frequent distractor location produced less interference than a distractor further away, consistent with a gradient of inhibition centered on the frequent distractor location.

Taken together, these effect patterns replicate those reported by Wang and Theeuwes (2018a).

Figure 4. Mean response times (RTs, upper panels) and mean error rates (lower panels) as a function of the distance of the target from the frequent distractor location, separately for the first and the second experimental session. Error bars denote one standard error. Target distances 0–4 denote the distance of the target from the frequent location (0 = target at frequent location; 1 = target at location adjacent to frequent location; etc.).

Target location effects. Following Wang and Theeuwes (2018a), to examine for target location effects unaffected by any (processes to deal with the) interference caused by a distractor anywhere in the display (as well as by any unevenness in the target distribution on distractor-present trials; see introduction to Experiment 2), we focused on distractor-absent trials. This analysis revealed that responding to the target was significantly slower, by some 70 ms, when it appeared at the frequent distractor location compared to a rare location (see Figure 4), $t(23) = 5.79$, $p < .001$, $BF > 100$. [This effect was also evident on distractor-present trials (see Figure 2): there was a significant RT disadvantage, of 60 ms, for targets at the frequent versus a rare location, $t(23) = 4.99$, $p < .001$, $BF > 100$]. Figure 4 depicts the RTs (on distractor-absent trials) as a function of the distance between the target location and the frequent distractor location. Although there was a significant effect of distance ($F(4,92) = 12.56$, $p < .001$, $\eta_p^2 = .35$, $BF > 100$), which did not differ between sessions ($F(4,92) = 0.63$, $p = .64$, $BF = 0.04$), there was little evidence of a gradient effect: while RTs were slower for distance 0, they differed little between the larger distances; there was actually no significant effect of distance after removing distance 0 ($F(3,69) = 0.21$, $p = .89$, $BF = 0.03$), and the RTs for distance 1 and distance 4 were virtually the same: 988 ms and 990 ms, respectively.

Again, the slowing of RTs to targets at the frequent distractor location replicate the effect reported by Wang and Theeuwes (2018a). As Wang and Theeuwes (2018a) did not report a distance analysis for distractor-absent trials, we cannot tell whether there was a significant gradient effect in their experiment. In any case, even for distractor-present trials (for which Wang and Theeuwes reported a distance effect), based on Bayesian statistics, the evidence for a distance effect in Experiment 1 was also only weak when distance 0 was removed: $BF = 1.21$.

Positional inter-trial effects. Next, having replicated the presence of a target location effect in the Wang-and-Theeuwes (2018a) paradigm, we examined whether this effect would be (partly) driven by the imbalances in the frequency, between the frequent and rare locations, with which a distractor on trial *n-1* occurred at the exact-same location as the target on trial *n* (i.e., carry-over across trials of inhibition placed on the distractor location, which, as outlined in the introduction to Experiment 1, would occur more often at the frequent distractor location). This analysis – of the effect of the distractor-to-target transition (same vs. different location) from trial *n-1* to trial *n* – again focused on distractor-absent trials *n*, which is the condition that would reveal any carry-over (into trial *n*) of inter-trial inhibition of the distractor location on trial *n-1* in its purest form.
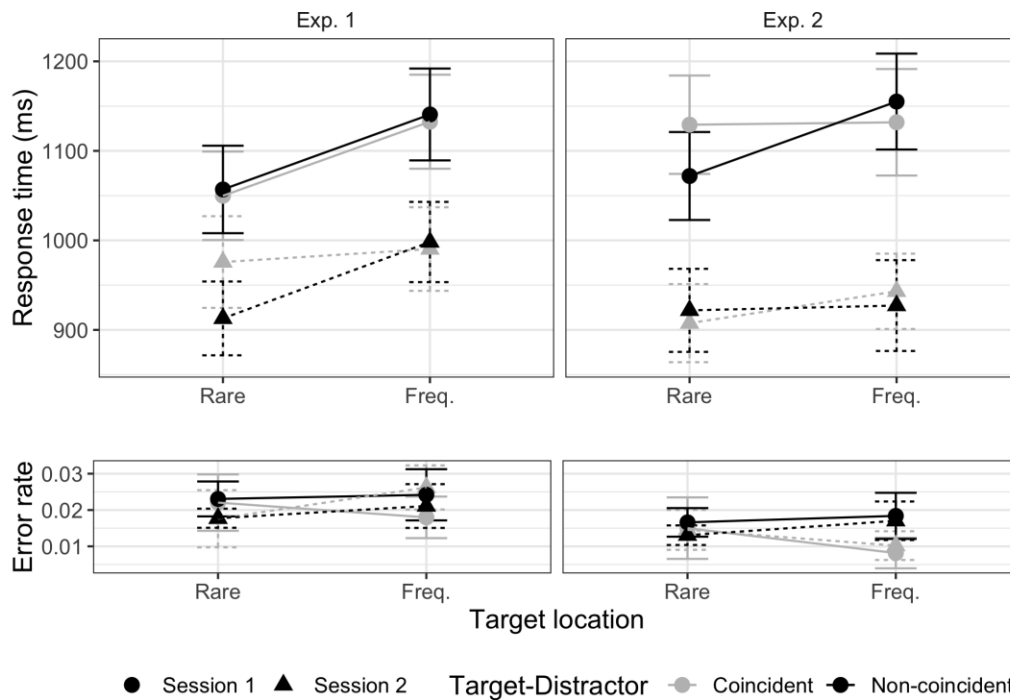
Figure 5. *Mean response times (RTs, upper panels) and mean error rates (lower panels) as a function of the target position (at frequent distractor location, at rare distractor location) and coincidence/non-coincidence of the target position with the previous distractor position, separately for the first and the second experimental session.*

With a distractor absent on a given trial *n*, the target on this trial could appear either at the frequent distractor location or at one of the rare locations. As regards the distractor condition on the previous trial *n-1*, there are then three possibilities: the target and distractor locations are either coincident (i.e., target *n* appears at the same location as the distractor on trial *n-1*) or non-coincident, that is, target *n* appears at a location different to that of the distractor on trial *n-1* or there was no distractor on trial *n-1* (i.e., there were two consecutive distractor-absent trials). As the latter two conditions revealed little difference, we collapsed them into one, 'non-coincident' condition. Figure 5 shows how RTs and error rates depend on target-distractor coincidence for each target condition (target at frequent, at rare distractor location) and session. Overall, there appeared to be some effect of target-distractor coincidence – indicative of cross-trial inhibition – for targets on trial *n* appearing at the location of a rare distractor on trial *n-1* (1013 vs. 985 ms), $t(23) = 1.85$, one-tailed $p = .039$, $BF = 0.93$, but there was no effect whatsoever for targets appearing at the frequent distractor location (1061 vs. 1069 ms, $t(23) = 0.52$, one-tailed $p = .70$,

BF = 0.24). The effect for rare distractor locations appeared to be driven mainly by the second session (second session, coincident vs. non-coincident: 976 vs. 913 ms; $t(23) = 3.15, p = .0045, BF = 9.50$): an ANOVA with the factors target condition (target at frequent, at rare distractor location), target-distractor coincidence, and session suggested the pattern of RTs as a function of target condition and target-distractor coincidence to differ across sessions (three-way interaction: $F(1,23) = 5.66, p = .026, \eta_p^2 = .20, BF = 0.57$). However, this interaction is put into question by the Bayes factor. In any case, the (if anything) larger inter-trial inhibition associated with rare distractor locations is at variance with the hypothesis that the strong overall-inhibition of the frequent distractor location arises as a result of stronger positional (inhibitory) cross-trial dynamics for the frequent location.

Color repetition effects. Because it is conceivable that participants attempt to suppress the distractor based on its color as well as its position, even though the target (i.e., non-distractor) and distractor color changed randomly from trial to trial, we also examined for an effect of repeating versus switching the color assignment between trials. Like Wang and Theeuwes (2018a), we first examined whether the amount of interference caused by a distractor at the frequent distractor location was different when color assignment was repeated compared to when it changed. Contrary to Wang and Theeuwes (2018a), we found the interference effect to be significantly reduced when the color assignment was repeated compared to when it changed (46 ms vs. 70 ms, *t*(23) = -3.19, *p* = .0041, *BF* = 10.40). This color-repetition benefit is indicative of some additional, color-feature-specific component of distractor suppression.

Given this finding, we went on to perform a more detailed analysis of the color-repetition benefits, more precisely: of the color repetition benefit as a function of the distractor condition (distractor absent, at rare location, at frequent location) on the current trial *n*, dependent on the distractor condition of *trial n-1*. As this analysis is exploratory and somewhat tangential to the question at issue in the present study, the results are detailed in a *Supplementary* section. In brief, this analysis revealed a color-repetition benefit on the current trial *n* only when a distractor appeared at one of the rare locations on the preceding trial *n-1* (not when there was no distractor or when a distractor appeared at the frequent location), and a benefit was evident both when the current distractor appeared at a rare location and when it appeared at the frequent location (but not when there was no distractor on the current trial). – This pattern is consistent with the idea that when a distractor at a rare location captures attention (which is more likely to occur in

comparison with a distractor at the frequent, i.e., 'spatially' suppressed, location), the distractor color is inhibited in order to disengage attention from the rare distractor and re-allocate it to the target. If this color set (inhibition of the distractor color) is carried over across trials, it would diminish the potential of a distractor defined by the same color, wherever it appears in the display, to attract attention.

Summary. Thus, overall, our results provide a near-perfect replication of those reported by Wang and Theeuwes (2018a). In particular, there was a significant target location effect (on distractor-absent trials), with targets being responded to slower when they appeared at the frequent distractor location compared to one of the rare locations. Going beyond Wang and Theeuwes (2018a), our analyses of positional intertrial effects revealed that, while there was evidence of cross-trial inhibition (IOR) for the rare distractor locations, there was no evidence of such an effect whatsoever for the frequent location. This pattern is at variance with an account of the target position effect in terms of asymmetric carry-over of inhibition (IOR) across trials between the frequent and rare distractor locations, and it is in line with the interpretation put forward by Wang and Theeuwes (2018a), namely, that there is strong (acquired) positional suppression of the frequent distractor location operating at the level of the priority map. In fact, at least judging from the distractor-absent trials (on trial *n*), suppression appeared to be 'near-saturated' for this location, leaving little room for passive positional inter-trial inhibition to assert itself! Also, there was no evidence that this pattern changed as a result of practice on the task: cross-trial inhibition was essentially absent for the frequent distractor location in both sessions/halves of the experiment (whereas it increased from session 1 to session 2 for the rare locations).

Also, unlike Wang and Theeuwes (2018a), we found a significant benefit of repeating (vs. switching) the color of the distractor (relative to that of the other display items) across consecutive trials, though only when there was a distractor at a rare location (not when there was one at the frequent location) on the previous trial. This points to an element of color-based suppression of distractors at the frequent location, on top of space-based suppression. However, as color-based suppression works equally for all (potential distractor) locations (i.e., both the frequent and the 7 rare locations), this component cannot account for the overall reduced interference with distractors at the frequent versus the rare locations.

EXPERIMENT 2

Experiment 1 showed that the result pattern of Wang and Theeuwes (2018a) cannot be reduced to positional inter-trial effects. Nevertheless, it still remains a question whether the (near-saturated) suppression of the frequent distractor location can be attributed solely to distractor position learning, that is, learning to ignore the frequent distractor location. This does remain an open question because, in the paradigm of Wang and Theeuwes, not only was a distractor more likely to appear at the frequent distractor location (on 65% of the distractor-present trials), but a target was also, at the same time, less likely to appear at this location. In number terms: on distractor-present trials, while a target appeared with a likelihood of 95%/7 (= 65%/7 + 30%/7) = approx. 14% at an infrequent distractor location, it appeared only with a likelihood of 35%/7 = 5% at the frequent distractor location; in other words, it was nearly three times less likely to appear at the frequent distractor location on distractor-present trials, and almost twice as likely across all trials combined. Accordingly, learning of the likely distractor location is potentially 'confounded' with learning of an unlikely target location, so that we cannot tell whether the suppression effect is due to one or the other or a combination of both. Experiment 2 was designed to examine for this, by making the frequent distractor location as likely to contain a target as any of the infrequent locations not only on distractor-absent trials, but also on distractor-present trials. Note that there was no negative target location bias in the Sauter-et-al. (2018) paradigm, in which the target was as likely to appear in the frequent as in the rare distractor region on both distractor-present and -absent trials.

Method

Methodologically, Experiment 2 was essentially the same as Experiment 1, the only exception being that, on distractor-present trials, a target was equally likely to appear at the frequent distractor location as at any one of the infrequent locations, by increasing the likelihood of a target appearing at the frequent distractor location on the 35% of trials on which a distractor occurred at an infrequent location. On distractor-absent trials, the target appeared equally likely at all locations, in any case. 24 new volunteers (mean age: 24.96 years; age range: 19-34 years; 16 female) participated in Experiment 2, on the same terms and procedural conditions as in Experiment 1. Overall, participants performed 3000 trials in two sessions, which again allowed us to examine for any changes in performance as a function of practice (session effects).

Results and Discussion

Analogously to Experiment 1, we first examined the RTs (and error rates) by a repeated-measures ANOVA with distractor condition (distractor absent, at frequent location, at rare location) and session as factors. See the middle panels of Figure 2 for a depiction of the results. This ANOVA revealed both main effects to be significant: distractor condition ($F(1.5,34.2) = 122.60, p < .001, \eta_p^2 = .84, BF > 100$), and session ($F(1,23) = 45.20, p < .001, \eta_p^2 = .66, BF > 100$); the interaction failed to reach significance ($F(2,46) = 2.83, p = .07, BF = 0.13$).

Distractor location effects. The session effect again reflected faster RTs in the second compared to the first session (mean: 973 ms vs. 1146 ms). Concerning the effect of distractor condition, post-hoc *t* tests revealed that relative to the distractor-absent baseline (1003 ms), there was significant RT interference wherever the distractor occurred (frequent distractor location, $t(23) = 11.99, p < .001, BF > 100$; rare locations, $t(23) = 12.98, p < .001, BF > 100$), but the interference was significantly reduced when the distractor occurred at the frequent location (69 [= 1072 – 1003] ms) compared to one of the rare locations (146 [= 1149 – 1003] ms), $t(23) = 7.63, p < .001, BF > 100$. The error rates (which were low overall: 2% on average) mirrored the RT pattern, effectively ruling out that the observed RT effects were merely driven by speed-accuracy trade-offs. Essentially, this replicates the pattern seen in Experiment 1.

However, different to Experiment 1, in Experiment 2 there was no evidence of increased distractor interference with distance of the current distractor from the frequent location (see middle panels of Figure 3): an ANOVA with the factors 'distance of distractor from frequent distractor location' (distances 1–4, i.e., excluding distance 0) and session revealed neither a significant main effect of distance nor a significant interaction with session (main effect: $F(3,69) = 1.01, p = .39, BF = 0.03$; interaction: $F(3,69) = 0.09, p = .97, BF = 0.06$).

Target location effects. To determine whether, on distractor-absent trials, there is any effect of target condition (at frequent distractor location vs. rare distractor location) after accounting for carry-over effects from distractor inhibition on the previous trial, we analyzed the effect of target condition after removing trials on which the target appeared in the previous distractor position (i.e., we considered the non-coincident condition; see right panel of Figure 5) in an ANOVA with target condition and session as factors. Unlike in Experiment 1, the effect of

target condition differed between sessions ($F(1,23) = 6.11, p < .01, \eta_p^2 = .21, BF = 1.71$). In session 1, RTs were slower when the target appeared at the frequent distractor location compared to any other location (1141 ms vs. 1057 ms; $t(23) = 3.50$, $p < .01$, $BF = 19.60$) – which mirrors the pattern seen in Experiment 1 and in Wang and Theeuwes (2018a). In session 2, by contrast, the difference was not statistically significant (927 ms vs. 922 ms; $t(23) = 0.36, p = .72, BF = 0.23$) – that is, there was no longer a target location effect – a pattern consistent with Sauter et al. (2018a). [Essentially the same pattern was seen for distractor-present trials (see Figure 2): an RT disadvantage, of 49 ms, for targets at the frequent vs. a rare location was evident in session 1 ($t(23) = 3.43$, $p = .002$, $BF = 17$), but not in session 2 (disadvantage of 22 ms; $t(23) = 1.34$, $p = .19$, $BF = 0.47$).] – Like in Experiment 1, there was no evidence of a graded effect of the distance of the target location from the frequent distractor location on distractor-absent trials (see middle panels of Figure 4), not even in session 1, where there was a significant target location effect (an ANOVA including only session 1 and removing distance 0 yielded no significant effect of distance: $F(3,69) = 1.48$, $p = .23$, $BF = 0.27$).

Positional inter-trial effects. The pattern of positional inter-trial effects (carry-over of inhibition of the distractor location on distractor-present trial *n-1* to distractor-absent trial *n*) was overall similar to that seen in Experiment 1 (see right-hand side of Figure 5): collapsed across the two sessions, there was evidence of a carry-over of inhibition (RT coincident > RT non-coincident) for the rare locations (22-ms inhibition), but not the frequent location (3-ms difference in the opposite direction to inhibition). However, there was no significant interaction between coincidence and location (interaction coincident/non-coincident x target at frequent/rare location, $F(1,23) = 1.26$, $p = .27$). This time, though, the effect appeared to be arising in the first session ($F(1,23) = 3.48$, $p = .08$, $\eta_p^2 = .13$, $BF = 1.75$; the three-way, session×coincidence×location, interaction was significant: $F(1,23) = 7.36$, $p = .01$, $\eta_p^2 = .24$, $BF = 1.08$); in the second session, the frequent and rare locations appeared equally (un-)affected by positional cross-trial inhibition. However, looked at in terms of the Bayes factor, the evidence for an interaction involving the factor session is not convincing.

Color repetition effects. As for Experiment 1, we first examined whether the amount of interference caused by a distractor at the frequent distractor location differed depending on the repetition versus change of the color assignment across consecutive trials. Again, and contrary to Wang and Theeuwes (2018a), there was a significant color-repetition (vs. -change) benefit (78 ms

vs. 60 ms, $t(23) = 2.57$, $p = .02$, $BF = 3.10$). A follow-up analysis of the color repetition benefit as a function of the distractor condition (distractor absent, at rare location, at frequent location) on trial n, dependent on the distractor condition of *trial n-1* (see *Supplementary* section for details), revealed a similar picture to that seen in Experiment 1: there was a (numerical) color-repetition benefit on the current trial only when a distractor appeared at one of the rare locations on the preceding trial, and a benefit was evident when the current distractor appeared at a rare location (significant) and when it appeared at the frequent location (numerical), but not when there was no distractor on the current trial). By and large, this is in line with the account sketched for Experiment 1: when a distractor appears at a rare location on trial *n-1* (in which case it is likely to summon attention), its color may be noted and suppressed (to aid re-allocation of attention to the target location); this inhibitory set is then carried over across trials and benefits performance when the color assignment is repeated (by down-modulating the color feature contrast of the distractor on trial n, making it less potent to attract attention).

Summary. Overall, Experiment 2 in many respects replicates the findings of Experiment 1: observers do come to learn, and apply strong inhibition to the frequent distractor location. However, there appears to be a major shift between the two sessions in how this inhibition operates. In session 1 (as in the whole of Experiment 1), it involves a robust target location effect, that is: targets are responded to slower when they appear at the frequent compared to one of the rare distractor locations – consistent with inhibition being applied (to the frequent distractor location) at the level of the supra-dimensional priority map. In session 2, by contrast, the target location effect is no longer evident (in fact, the Bayes factor, $BF = 0.23$, favors the null hypothesis of no target location effect!), and this is so despite the fact that the magnitude of the distractor location effect (i.e., the difference in interference between distractors at the frequent vs. the rare locations) is virtually unchanged (72 ms in session 2 vs. 82 ms in session 1; $BF = 0.13$ for the distractor condition × session interaction). The lack of a target location effect mirrors the results of Sauter et al. (2018) for conditions in which the distractor is defined in a different dimension to the target: it is inconsistent with spatial inhibition of the distractor location operating at the level of the priority map, but consistent with inhibition operating at a dimension-based level, such as the map of (color-) dimension-specific feature contrast signals (which are then integrated across dimensions in the search-guiding priority map).

We propose that this reflects an adaptive shift of the processing level at which distractor inhibition is applied, which is adaptive to the distractor and target location probabilities prevailing in particular task scenarios. Of note, the beginning of a shift can be discerned already within the first session (see Figure 6): splitting the first session of Experiment 2 in halves, the target location effect on distractor-absent trials turns out smaller in the second compared to the first half (first vs. second half of session 1: 91 vs. 51 ms; $t(23) = 2.69$, $p = .013$, $BF = 3.85$[5]; the 51-ms effect in the second half is significantly different from zero: $t(23) = 3.03$, $p = .006$, $BF = 7.51$), without a corresponding decrease of the interference reduction for distractors at the frequent versus the rare locations (first vs. second half: 78 vs. 85 ms; $t(23) = 0.50$, $p = .62$, $BF = 0.24$). That is, the transition from priority-map- to dimension-based suppression of the likely distractor location occurs more gradually, but may need some 1500-plus trials to be completed.
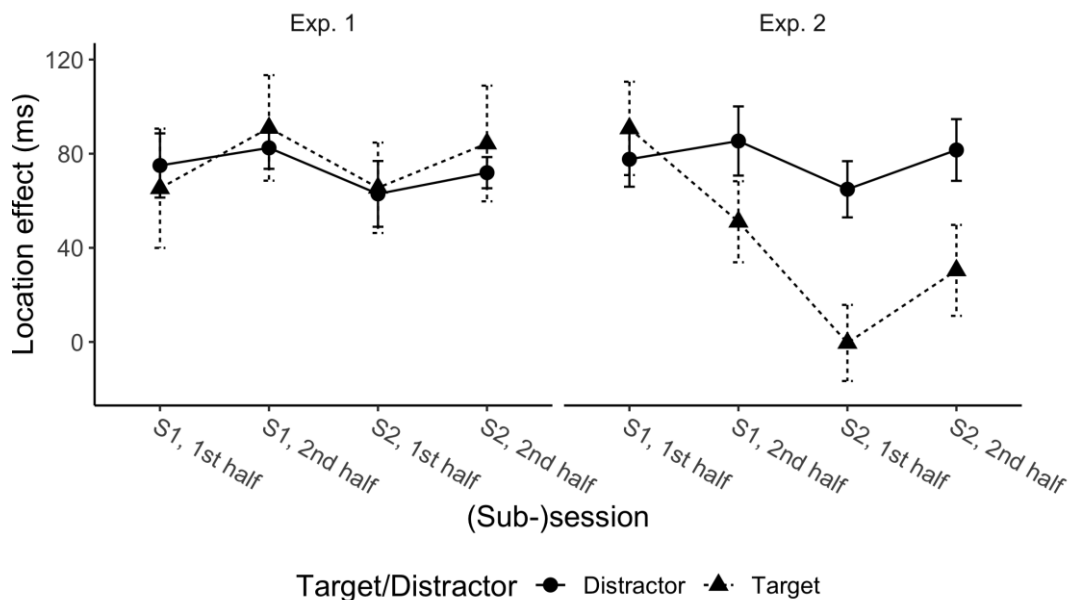
Figure 6. *Distractor location effect (RT difference between conditions with a distractor at a rare vs. the frequent distractor location), and target location effect (RT difference between conditions with the target at the frequent vs. a rare distractor location) on distractor-absent trials, across*

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[5] The reduction of the target location effect is numerically similar when removing trials from the analysis on which the target position on trial *n* coincided with the distractor position on trial *n-1* (first vs. second half: 100 vs. 62 ms), but not statistically significant, $t(23) = 1.44$, $p = .16$, $BF = 0.53$. Note though that estimating the carry-over of inhibition of the previous distractor location to the current target location is inherently more noisy (especially for the rare locations) when the estimates are based on only two thirds of the trials.

the course (first and second half of each session) of Experiment 1 and Experiment 2. In Experiment 2 the target location effect reduces towards zero, while the distractor location effect remains virtually unchanged. (Data uncorrected for cross-trial inhibition of distractor locations.)

EXPERIMENT 3

The results of Experiment 2 may be taken to be indicative of an adaptive shift – in response to the (relative to Experiment 1 unbiased) target location probabilities – of the processing level at which distractor location inhibition is applied, from a priority-map-based level to a dimension-based level. One reason why this shift was seen to develop fully only in the second session (while emerging already in the first session) may be as follows: Under the conditions of Experiment 2 (and 1), with the constant (random) swapping of the distractor and non-distractor colors across trials (which participants reported, in post-experimental debriefing, to be 'irritating') and with the target appearing at a non-predictable location, the most 'salient' regularity that observers would come to extract first is the likely location of the distractor. In response to this (and lacking other 'cues'), they start inhibiting this location at a global spatial level: the priority-map. However, over time they come to realize that this actually harms processing of the target when it appears at the likely distractor location – especially since, in Experiment 2, the target was equally likely to occur at the frequent distractor location on distractor-present trials (whereas this was less noticeable in Experiment 1, in which the target was much less likely to occur at the frequent compared to any of the rare distractor locations). This, together with the eventual realization that, despite the color swapping, the distractor is invariably color-defined[6], drives the shift to a dimension-based suppression strategy: inhibit the frequent distractor location within the color dimension.

On this account, any 'cue' that would help observers realize the definition of the distractor – namely, as being defined in a different dimension to the target – before (more gradually) learning its frequent location of appearance would encourage them to (more or less immediately) adopt an *efficient* inhibitory strategy, that is, one that minimizes distractor interferences *without*

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[6] Of course, participants were told, in the instruction, that one, task-irrelevant item would differ from the others by being the only red amongst green items or the only green amongst red items. However, expressly realizing this regularity and translating it into an effective dimension-based inhibitory strategy would take time – given that, compared to a direct feature-based suppression strategy (e.g., suppress 'red'), implementing a dimension-based strategy (suppress any color difference signal) involves a level of abstraction. This is in line with (and motivated by) informal reports of subjects complaining that the permanent color swapping was perceived as "irritating".

harming target processing. This would be the case in the Wang-and-Theeuwes (2018a) paradigm when there is no unpredictable color swapping between the distractor and non-distractor items in the display, that is, when the color assignment is kept constant. Müller et al. (2009) have shown that with a just few trials of distractor practice under these conditions, distractor interference is very substantially reduced, to a barely significant level. These are also the very conditions under which Sauter et al. (2018; see also Sauter et al., 2019) failed to observe a target location effect in distractor location probability learning, that is: with a constant, color-defined distractor, RTs to the orientation-defined target were not slowed when the target occurred in the frequent versus the rare distractor region. Accordingly, we predicted that by making the color assignment constant in the Wang-and-Theeuwes (2018a) paradigm (with a single likely distractor location), we would observe a robust distractor location learning effect (reduced interference of a distractor occurring at the frequent vs. an infrequent distractor location) without any (substantial) target location effect (slowed RTs to targets at the frequent vs. a rare distractor location) even in the first session of the experiment. This hypothesis was tested in Experiment 3.

Method

Experiment 3 was virtually the same as Experiment 2, except that the distractor/non-distractor color assignment was kept constant throughout the experiment: for half the participants, the singleton distractor was red and the non-distractor items green, and vice versa for the other half. Twenty-four new volunteers (mean age: 24.96 years; age range: 19-34 years; 16 female) took part in Experiment 3, on the same terms and procedural conditions as in the previous experiments. Overall, participants performed 1920 trials in two sessions, which (although fewer than the 3000 trials in Experiments 1 and 2) again allowed us to examine for any changes in performance as a function of practice (session effects). The trial number was reduced because our focus was no longer on the positional inter-trial dynamics, which had been conclusively ruled out to be a factor (in the target location effect) in Experiments 1 and 2.

Results and Discussion

Distractor location effects. As for the previous experiments, we first examined the RTs (and error rates) by a repeated-measures ANOVA with distractor condition (distractor absent, at frequent location, at rare location) and session as factors. See the right-hand side of Figure 2 for a depiction of the results. This ANOVA revealed both main effects to be significant: distractor

condition $(F(1.5,34.2) = 80.9, p < .001, \eta_p^2 = .78, BF > 100)$, and session $(F(1,23) = 84.30, p < .001, \eta_p^2 = .79, BF > 100)$; the interaction was also significant $(F(2,46) = 6.19, p = .004, \eta_p^2 = .21, BF = 0.23)$.

The session effect again reflected faster RTs in the second compared to the first session (mean: 979 ms vs. 1173 ms). Concerning the effect of distractor condition, post-hoc t-tests revealed that relative to the distractor-absent baseline (1029 ms), there was significant RT interference whether the distractor occurred at the frequent $(t(23) = 6.37, p < .001, BF > 100)$ or one of the rare locations $(t(23) = 10.93, p < .001, BF > 100)$, but the RT cost (relative to the distractor-absent condition) was significantly lower for the frequent (vs. the 'rare') location(s) $(52 [= 1081 - 1029]$ ms vs. $105 [= 1134 - 1029]$ ms, $t(23) = 7.89, p < .001, BF > 100)$. The error rates (which were low overall: <3% on average) were near-equivalent for the two distractor location (and the distractor-absent) condition, arguing against the differential interference effect being confounded by differential speed-accuracy trade-offs. When the distractor occurred at one of the rare locations, RTs did not differ as a function of its distance to the frequent location $(F(3,69) = 1.011, p = .39, BF = 0.03)$, that is: reduced interference was confined to the frequent distractor location. Thus, the essential distractor location (probability learning effect) is exactly the same as in the previous experiments.

Also, note that the overall RT speed, the strength of distractor interference, and the magnitude of the learning effect differ only little from Experiments 1 and 2 (see Figure 2). While an ANOVA with the factors experiment (1, 2, 3) and distractor condition (distractor absent, at frequent location, at rare location) failed to reveal a significant main effect of experiment, $(F(2,69) = 0.063, p = .94)$, the interaction turned out significant $(F(3.6,125) = 2.93, p = .027, \eta_p^2 = .078)$: both the distractor interference and the distractor location probability-cueing effects were somewhat smaller in Experiment 3 (interference from distractor at rare location: 105 ms in Experiment 3 vs. 130 and 146 ms in Experiments 1 and 2; cueing effect: 53 ms in Experiment 3 vs. 73 and 77 ms in Experiments 1 and 2), but the interaction was not supported by the Bayes factor analysis $(BF = 0.06)$. That is, the task as such did not become much easier by the constant color assignment in Experiment 3.

Target location effects. Next, and critically for the question at issue in Experiment 3, we examined for the presence of an effect of target condition (i.e., the target appearing at the

frequent vs. a rare distractor location) on distractor-absent trials. A preliminary analysis of positional inter-trial effects revealed no evidence for carry-over of distractor location inhibition from a previous (*n-1*) distractor to a current (*n*) no-distractor trial for either the frequent or the rare locations (if anything, RTs were faster, rather than slower, to targets at the previous distractor location). Accordingly, the critical analysis was conducted on the uncorrected (for cross-trial inhibition) data. An ANOVA with the factors target condition and session failed to reveal any significant effects; in particular, there was no evidence of slower RTs (on distractor-absent trials) to targets occurring at the frequent versus one of the rare distractor locations (overall: 1039 ms vs. 1027 ms; main effect of target location, $F(1,23) = 1.03, p = .32, BF = 0.26$), in either session (session 1: 1133 ms vs. 1115 ms, $t(23) = 1.28$, $p = .21$, $BF = 0.44$); session 2: 945 ms vs. 939 ms, $t(23) = 0.45$, $p = .66$, $BF = 0.24$). [The same was true for distractor-present trials (see Figure 2): overall, 1127 ms vs. 1139 ms; main effect of target location, $F(1,23) = 1.09$, $p = .31$, $BF = 0.27$; session 1, 1237 ms vs. 1243 ms, $t(23) = -0.42$, $p = .68$, $BF = 0.23$; session 2, 1017 ms vs. 1034 ms, $t(23) = -1.25$, $p = .22$, $BF = 0.43$.] Note also that (on distractor-absent trials) there were no effects of the distance of the target to the frequent distractor location in either session 1 or session 2 (ANOVA of the distance effect, with distance 0 = target at frequent distractor location, and the additional factor session: main effect of distance, $F(2.4,55.3) = 0.733$, $p = .51$, $BF = 0.034$; distance $\times$ session interaction, $F(4,92) = 1.16$, $p = .33$, $BF = 0.05$). In other words, the distance functions for the target are essentially flat (see Figure 4), whereas those for the distractor (distance of current distractor from the frequent distractor location) show a narrow trough for the frequent distractor location (see analysis above and Figure 3).

Summary. Thus, when the distractor is defined by a constant color relative to the non-distractor items (in contrast to the random color assignments in Experiments 1 and 2), although we observe the development of the 'standard' distractor location probability cueing effect (of a similar magnitude as in Experiments 1 and 2), this effect is *not* accompanied by a target location effect in either session 1 (in contrast to both Experiments 1 and 2) or session 2 (in contrast to Experiment 1). The lack of a target location effect in Experiment 3 replicates Sauter et al. (2018) using the Wang-&-Theeuwes paradigm and implies that the frequent location was not inhibited at the global level of the priority map: if it had been, processing should have been slower when the target appeared at the frequent as compared to a rare distractor location – in addition to distractor

interference being reduced when the distractor appeared at the frequent versus a rare location. The fact that only distractor processing, but not target processing, was impacted argues in favor of the idea that the distractor location was inhibited at some level below the priority map, such as a color-based level, leaving target (i.e., shape) signals unaffected. Based on the present data alone, it is not clear whether this level is dimension-specific (inhibit any color difference signals) or feature-specific (inhibit blue or, respectively, red signals). However, given that the result pattern is the same as in the second session of Experiment 2 (in which the distractor color was non-predictable), dimension-based inhibition is more likely than feature-based inhibition. Also, it is not clear from Experiment 3 whether, and to what extent, the immediate 'abolishment' of the target location effect was due to the constancy of the distractor (vis-à-vis the non-distractor) color or the balanced target location probabilities (on distractor-present as well as -absent trials). Given that many trial samples are required for observers to learn and utilize the target distribution (see Experiment 2), it is likely that the constant color assignment was the more decisive factor, which was then secondarily reinforced by the even target distribution.

GENERAL DISCUSSION

Three experiments designed to examine the target location effect in Wang and Theeuwes' (2018a) paradigm revealed their pattern of effects to be highly replicable. In particular, in all experiments, there was strong suppression of the frequent distractor location: a distractor at this location caused substantially less interference than a distractor at a rare location (on distractor-present trials). In addition, in Experiment 1 (which was an exact replication of Wang and Theeuwes', 2018a, experiment, the only difference being an increased number of trials), we also found a target location effect on distractor-absent trials: RTs were substantially slowed when the target appeared at the frequent distractor location compared to a rare location. Going beyond a mere replication, we also examined for a potential confound: carry-over of positional inhibition of the distractor location from one (distractor-present) trial to the next (distractor-absent) trial. Contrary to our initial hypothesis, however, the target location effect could not be reduced to positional inhibition being cumulatively stronger for the frequent (i.e., statistically frequently inhibited) distractor location compared to the rare locations. If anything, the effect pattern was the other way round: the frequent distractor location was inhibited (tonically) to such a degree that cross-trial positional inhibition made little difference. This overall effect pattern was essentially the same in both experimental sessions – thus ruling out a potential confound and

supporting Wang and Theeuwes' (2018a) conclusion that suppression of the frequent distractor location operated at the level of the priority map.[7]

Experiment 2 went on to examine whether this strong inhibition was influenced not only by the distractor location probability, but also by the target location probability. In Wang and Theeuwes' (2018a) original paradigm, the frequent distractor location was actually nearly three times less likely to contain a target than any of the rare locations on distractor-present trials (and some two times less likely across all trials), providing participants with a secondary incentive to ignore the frequent distractor location. This target location bias was removed in Experiment 2. The results revealed that in the first experimental session (averaged across the two session halves), the effect pattern essentially mirrored that obtained in Experiment 1. In particular, a distractor at the frequent (vs. one of the rare) location(s) caused less interference, and responding was significantly slowed when the target appeared at the frequent (vs. a rare) distractor location on distractor-absent trials. This pattern was changed in the second session: while distractor interference was still reduced – by an equal amount! – on distractor-present trials, there was no longer a target location effect on distractor-absent trials (in fact, the Bayes factor argues in favor of a null effect). This is the very pattern observed by Sauter et al. (2018) for conditions with a distractor defined in a different dimension (color in both studies) to the target (shape in the present study, orientation in the Sauter-et-al. study). Experiment 3, which was identical to Experiment 2 except that there was no random swapping, across trials, of the color assigned to the distractor and non-distractor items, yielded essentially the same result pattern as that seen in session 2 of Experiment 2 – however, this time, this pattern was obtained right from the start, in session 1: while there was a significant distractor location probability-cueing effect (of a comparable magnitude to Experiments 1 and 2), this was not associated with a target location effect: RTs were not slowed to targets at the frequent as compared to the rare distractor locations (again, the Bayes factor argues in favor of a null effect).

The effect pattern seen in Experiment 1 and session 1 of Experiment 2 is consistent with the notion, advocated by Wang and Theeuwes (2018a; see also Ferrante et al., 2018), that (spatial) suppression of the frequent distractor location operates at the level of the search-guiding priority

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[7] This is supported by a complementary analysis of distractor location repetition effects on distractor-present trials: while a distractor falling at a previous distractor location causes reduced interference, the distractor-location probability-cueing effect survives correction for positional inter-trial inhibition (see Supplementary 2 for details.)

map.[8] This is beneficial in that it brings about a substantial reduction of distractor interference; at the same time, it is costly in that targets appearing at the frequent distractor location fall into the inhibitory trough: they take much longer to be detected and processed. By contrast, the effect pattern seen in session 2 (and already emerging during the second half of session 1) of Experiment 2 and in both sessions of Experiment 3 is consistent with the notion of dimension-based (spatial) inhibition, advocated by Sauter et al. (2018): strongly inhibiting color signals at the frequent distractor location effectively reduced the interference of (color-defined) distractors at this location, while leaving the processing of shape/orientation-defined (target) signals unaffected. Thus, the present results argue that removal of the target location bias in Wang and Theeuwes' (2018a) paradigm (Experiment 2) and making the distractor/non-distractor color assignment consistent (Experiment 3) can bring about an adaptive shift from priority-map-based to dimension-based suppression.

There are at least two questions to be discussed as regards this interpretation: (i) Why was the target location effect fully abolished only in session 2 of Experiment 2, but not already in session 1 (even though it started to decrease in the second half of session 1; see Figure 6), whereas the effect was never evident, in any session, in Experiment 3? (ii) Is the mode of suppression applied (i.e., the level, in the functional architecture, at which suppression operates) flexible, a matter of strategic set?

Concerning question 1, one plausible answer is that, in Wang and Theeuwes' (2018) original paradigm, observers first pick up the more striking distractor location 'regularity' (as also evidenced by the fact that most observers became consciously aware of the likely distractor location[9]), and this makes them operate a purely spatial, priority-map-based inhibitory strategy: suppress any stimulus at this location because it is likely to be a distractor. However, over time,

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[8] A similar, 'spatial-filtering' account was recently proposed by Ruthruff and Gaspelin (2018), to explain the lack of interference caused by a salient onset 'pre-cue' stimulus presented at one of two invariable, i.e., known, non-target locations in a variant of the 'contingent-capture' paradigm (cf. Folk & Remington, 1996).

[9] In Experiments 1, 2, and 3, 12 (of 24), 6 (of 24), and, respectively, 11 (of 24) participants correctly pointed to the likely distractor location in an eight-alternative forced-choice test at the end of the second session. Thus, overall, more than three times as many participants had precise knowledge of the likely distractor location (40.28%) than would be expected by random guessing (12.50%). This is indicative of a degree of above-chance knowledge of the likely distractor location, consistent with Wang and Theeuwes (2018a). However, Wang and Theeuwes (2018a) had found no difference in performance between observers who could vs. could not correctly tell the frequent distractor location. That is, suppression of the likely distractor location reflects, by and large, an implicit learning effect (see also Sauter et al., 2018).

they come to realize that this strategy harms detection of (and responding to) the target when it appears at the frequent distractor location, especially when they come to learn more slowly, in Experiment 2, that the target is (actually) not less likely to be located at the frequent distractor position as at any of the rare locations. In this situation, switching to dimension-based inhibition is adaptive: it minimizes distractor interference while not harming target processing at the frequent location.

In Experiment 3, by contrast, the most immediately apparent regularity is that the distractor is distinguished by a constant color from the other, themselves consistently colored items. Accordingly, participants adopt a color-based suppression strategy right from the start – that is, even before they learn more slowly that the distractor is most likely to appear at one particular location, in response to which they come to focus color-based suppression on this location over time. According to Sauter et al. (2018, 2019), this is the default strategy to deal with 'different-dimension' distractors, which minimizes (color) distractor interference without harming (shape/orientation) target processing. However, while the absence of a target location effect (supported by Bayes factor analysis) strongly argues against a priority-map-based account, we cannot tell from Experiment 3 alone at what level the inhibition of the color distractor was implemented: a feature-specific level (inhibition of a specific color feature, e.g., red) or a dimensional level (inhibition of any color feature contrast signal). However, there are at least two pieces of evidence in favor of the latter account. First, the result pattern for the whole of Experiment 3 is the same as for the second session of Experiment 2 – an experiment in which the distractor (and non-distractor) color was completely unpredictable, so that observers could not have developed a consistent 'distractor feature template' (e.g., Woodman & Luck, 2007). Under these conditions, to effectively reduce distractor interference, observers would have had to resort to a color-, that is, dimension-based inhibition strategy. Second, while (some) feature-based effects were resolvable in Experiments 1 and 2, they contributed only very little to the probability-cueing effect (see below and *Supplementary* section). This is broadly consistent with Wang and Theeuwes (2018a) as well as Experiments 3 and 4 of Wang and Theeuwes (2018b): in these experiments, the distractor was made more likely to appear in one versus the other color (80% vs. 20%), and yet there was no feature-specific effect. Note, though, that these experiments were set up to make observers operate in 'feature search', as opposed to 'singleton detection', mode (cf. Bacon & Egeth, 1994). As a result, the distractor interference and distractor location probability-cueing effects were greatly reduced compared to Wang and Theeuwes' (2018a)

original study (e.g., overall interference: 27 ms vs. 117 ms), which would have made it harder to resolve any feature-based effects. – Thus, given the evidence from Experiments 1 and 2, there is no reason to assume that a different (namely: feature-based) suppression strategy was at work in the present Experiment 3 than in Experiment 2.[10] In any case (whatever the precise level): both feature- and dimension-based inhibition would act on signal coding processes *below* the priority map.

Concerning question 2, it appears that observers adapt their mode of suppression to the prevailing positional distractor *and* target probabilities. When target and distractor colors swap randomly across trials, as in Wang and Theeuwes' (2018a) paradigm, the default set appears to be priority-map-based, which immediately brings about a strong reduction of distractor interference; a shift to dimension-based suppression is set in motion only later, when it is realized (over the course of the first session) that this set is associated with a substantial cost in processing targets at the frequent location. In contrast, in the paradigm of Sauter et al. (2018), the distractor color stays constant and is therefore perfectly predictable. In this case, the default may be dimension-based suppression. The random swapping of the (distractor, non-distractor) color assignment across trials in Wang and Theeuwes' (2018a) original paradigm (in contrast to the consistent assignment in the Sauter-et-al., 2018, paradigm and our variation of Wang & Theeuwes', 2018a, paradigm in Experiment 3) may also retard adoption of a dimension-based set, as observers could not tell by the color of a stimulus that it is likely a distractor, rather than a target (they could tell this more reliably based on its position at the frequent location). Thus, if spatial information is perceptually dominant over dimensional (or featural) information, observers may (first) come to operate a purely spatial (priority-map-based) distractor inhibition set.

More generally, this is to say that different default sets, or strategies, may be suggested by specifics of the individual paradigms, and overcoming these default sets may take time and additional learning of more subtle (e.g., target location) probability cues entailed in these paradigms. Thus, the mode of suppression applied is in principle flexible. This does not necessarily mean that adopting a specific set or changing set involves a conscious decision;

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[10] This is consistent with Won, Kosoyan, and Geng (2019), who – in the absence of color swapping (similar to Experiment 3) – found the interference reduction in a high vs. a low distractor prevalence condition (cf. Müller et al., 2009) to be equally effective when the distractor could be defined, variably across trials, by 192 different colors (randomly chosen from the CIE Lab color wheel) as compared to one, fixed color.

rather, it may simply be an adaptive process, driven by the availability of various distractor- (and target-) related probability cues. Also, it is conceivable that the two sets do not operate in an all-or-nothing fashion; rather (as suggested by the roughly halved, though still significant target location effect in the second compared to the first half of session 1 of Experiment 2), priority-map-based suppression may coexist with dimension-based suppression. However, more work is necessary to examine how this 'mixture' comes about: do the two sets operate in parallel within a given trial, or can only one set be effective on a trial (yielding a statistical mixture of the two sets across trials)?

In any case, the 'locus' of inhibition is flexible: priority-map- or dimension-based. And: just because one finds a distractor location effect, one cannot conclude from this finding alone that inhibition operates at the level of the priority map. Ultimately, of course, it is the priority map via which the inhibition is always expressed in search guidance, but the true level, at which it is instantiated at least in certain conditions, may be below the priority map. This is as envisaged by the Dimension-Weighting Account, according to which selection is ultimately based on the priority map which, however, is itself shaped by the weighting applied to the various, target- and distractor-defining feature dimensions (e.g., Liesefeld, Liesefeld, & Müller, 2019a; for a recent review, see Liesefeld, Liesefeld, Pollmann, & Müller, 2019b).

Finally, a few remarks are in order concerning other influences in the present paradigm, in particular, inter-trial effects as well as spatial gradient effects.

The first concerns *positional cross-trial inhibition of (previous) distractor locations*. We did find evidence of passive carry-over of inhibition, however the effect tended to be small and relatively larger for rare distractor locations. The fact that there was hardly any effect for the frequent location supports the argument that distractors at this location are effectively prevented from capturing attention by other means, limiting the room for passive cross-trial inhibitory effects to influence performance: the less often a distractor occurring at this location captures attention, the less often would this location have to be inhibited (e.g., in order to re-allocate attention to the target location), and the less often would such inhibition be carried over across trial. Thus, with strong persistent inhibition, (cross-trial) transient inhibition becomes rare and therefore, contrary to our initial hunch, the latter has next to no influence in the present paradigm – which may not be entirely surprising given that transient inhibition tends to be small with cross-dimensionally defined targets and distractors to begin with (see Sauter et al., 2018, who found

these effects to be larger by a factor of 4 with distractors defined in the same vs. a different dimension to the target).

The second concerns *color-based cross-trial repetition effects*. We did find *color-based repetition effects* (see Figure 7, which presents the effect pattern combined across Experiments 1 and 2), which were however relatively weak and tended to reflect, in the main, carry-over of (inhibition) of the distractor color from the previous trial, which aids performance if a same-colored distractor is present on the current trial (while it makes little difference with regard to where the current distractor appears, at the frequent vs. the rare locations).[11] This contrasts with Wang and Theeuwes (2018a), who probably did not have the power to resolve these effects (due their smaller number of trials). In any case, the fact that there is a significant color repetition benefit also for (current) trials with a distractor at the frequent location would indicate that the suppression of the distractor at this location is not entirely space-based, but involves some element of color-based suppression. However, given that such an effect is seen only in the relatively infrequent event that there is a distractor at a rare location on the preceding trial (i.e., $p = .66 \times .35 \times .66 \times .65 \approx .10$, and $p = .05$ for color repetition trials), and also given that the effect is equally seen when the distractor on trial *n* occurs at a rare location (i.e., it is a spatially non-specific, parallel effect), carry-over of inhibition of the distractor color (from trial *n-1* into trial *n*) cannot account for the interference reduction with distractors at the frequent versus one of the rare locations. This is the reason why we also find no difference (or only a numerical difference) when we compare simply the interference reduction for the frequent versus the rare locations between trials with a color repetition versus a switch (from the preceding trial): Experiment 1, 78 versus 67 ms, $t(23) = 1.27$, $p > .05$, $BF = 0.44$; Experiment 2, 70 versus 83 ms, $t(23) = -1.34$, $p > .05$, $BF = 0.48$. Overall, this is also consistent with Wang and Theeuwes's (2018b) recent report that making one specific distractor color more likely than the alternative color (80% vs. 20%) in their Experiments 3 and 4 failed to produce a significant main effect of color feature (numerically, there was a small effect, of the order of 5 to 10 ms, in both experiments), or interaction of color feature with distractor location.

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[11] It should be noted that it could conceivably also, or in addition, reflect carry-over of 'facilitation' of the target color from the previous trial – though, arguably, given there are 7 items of the same color (including the target in the display) in the display, carry of inhibition of the unique distractor color would represent a more effective strategy. More work, varying distractor and target colors independently across trials, would be necessary to dissociate these alternatives.
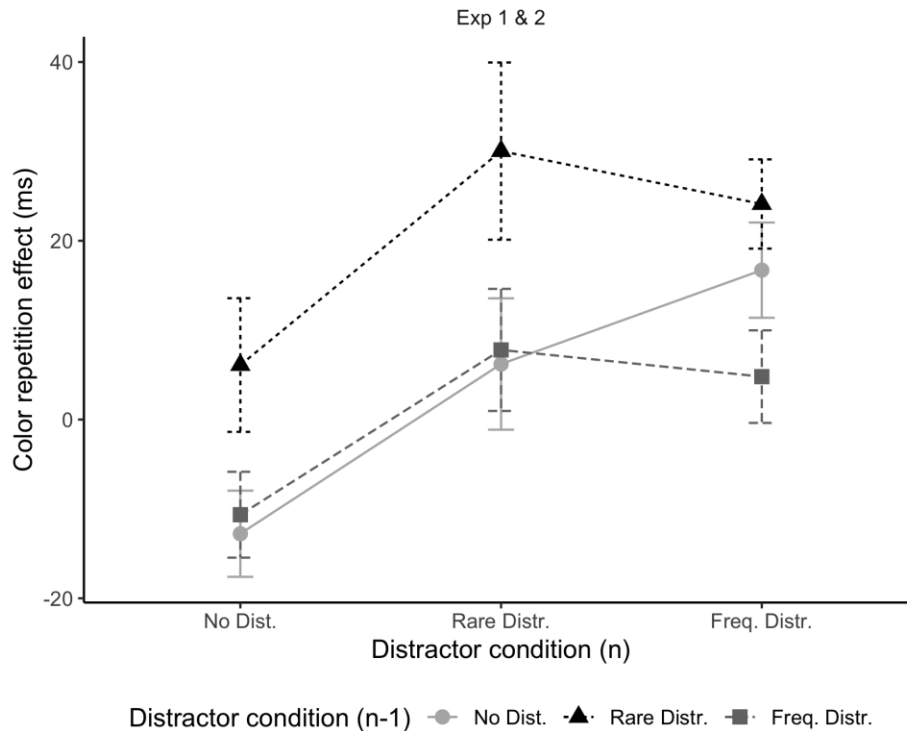
Figure 7. Color repetition effects (mean difference in RT between trials with a cross-trial change vs. repetition of the color assignment) as a function of the distractor condition on trial n, dependent on the distractor condition on trial n-1, combined across Experiments 1 and 2. Positive values mean RTs were faster when the same color was repeated (color repetition benefits).

With regard to the dimension-weighting account, one interesting issue in this context is why a color-feature-based modulation (spatially parallel carry-over of inhibition of the distractor color from the previous trial) appears to coexist with a dimension-based modulation (dimension-based suppression of the likely distractor location) in the second session of Experiment 2. As argued elsewhere (see, e.g., Sauter et al., 2018a), the two may not be incompatible: one may de-prioritize (down-weight) some specific feature at a feature-based level, while also de-prioritizing (down-weighting) the respective feature dimension at a higher level, prior to the integration of the dimension-specific feature contrast signals into the search-guiding (feature-less and supra-

dimensional) priority map. Alternatively, different colors might effectively be treated as different (though related) dimensions (see Liesefeld et al., 2019b).

In sum, our finding (in two experiments) of a feature-based component of distractor suppression provides evidence in favor of all three levels – the featural, dimensional, and priority-map level (see Gaspelin & Luck, 2018a, for a similar distinction) – being of importance. However, the feature-based color repetition effect is additive to the distractor location effect (affecting the frequent and rare distractor locations equally), that is, it cannot explain the distractor location probability-cueing effect.

A third point concerns the notion of an inhibitory gradient centered on the frequent distractor location, which Wang and Theeuwes (2018a) took to be indicative of the inherently 'spatial' nature of the distractor location learning effect. While we found some evidence of a gradient of inhibition (on distractor-present trials) in Experiment 1 (consistent with Wang & Theeuwes, 2018a, 2018b), there was little evidence of a gradient effect on either distractor-present or -absent trials in Experiments 2 and 3 – despite the fact that the respective distractor location effects (the difference in RTs between frequent and rare locations) were comparable in magnitude to those of Experiment 1 and those reported by Wang and Theeuwes (2018a). This is not problematic for their (and our) conclusion, however: it might simply mean that the suppression is tightly centered on, rather than being fuzzily distributed around, the frequent distractor location.

Last, but not least, there is a more general point to be addressed: Although the present (as well as previous) results on distractor-location probability cueing are all coherently interpretable in terms of learnt suppression of the likely distractor location(s) at some level in the architecture of saliency computation, there are other, conceivable mechanisms that could also underlie the distractor location and target location effects, which have not been systematically investigated as yet. In particular, rejection of distractors and, consequently, disengagement of attention might be expedited at the frequent distractor location (Geng & Diquattro, 2010), and/or selection of responses to targets at the likely distractor location might be slowed (Huang, Holcombe, & Pashler, 2004). For instance, assume that what observers learn in distractor-location probability-cueing paradigms is a decision bias towards an item occuring at the likely distractor location being a distractor and against it being a target. In terms of a drift-diffusion model (Ratcliff & McKoon, 2008), the starting point of evidence accumulation might be shifted towards the

'distractor' boundary and away from the 'target' boundary. This would generate both a distractor location effect (faster disengagement from distractors at frequent vs. rare locations), and, on the flipside, a target location effect (slower responses to targets at the frequent vs. rare locations). However, while this account has the virtue of simplicity, it could not explain why the target location effect vanishes under certain conditions, while the distractor location effect remains fully intact. Of course, other accounts assuming differential 'attentional-disengagement' and 'response selection' processes may be feasible, but these would have to be more complex. These possibilities need to be examined in future work. However, given the finding of a P_D component (Hickey, Di Lollo, & McDonald, 2009; Sawaki, Geng, & Luck, 2012) for the likely distractor location even on distractor-absent trials (Theeuwes, 2018, March, TeaP; see Footnote 4), we would expect these processes to operate alongside the modulation of attentional capture envisaged by saliency-based accounts.

CONCLUSION

We conclude that in the Wang and Theeuwes' (2018a; see also Wang & Theeuwes, 2018b) paradigm, the learnt distractor location inhibition is not necessarily based on the priority map (as assumed by Wang & Theeuwes, 2018a; see also Ferrante et al., 2018). Instead, it may also be implemented at (or, over the course of learning, shifted to) a lower, dimension-based level in the functional architecture of saliency computation – especially when the featural predictability of the distractor (vis-à-vis the non-distractor items) and/or the target location distribution affords a dimension-based inhibitory set: implementing inhibition at a dimension-based level would leave target processing unaffected in scenarios in which the distractor is defined in a different dimension to the target (see Sauter et al., 2018). On the other hand, observers do not mandatorily operate a dimension-based suppression strategy with different-dimension distractors (as implicitly assumed by Sauter et al., 2018). Rather, at least when the distractor and non-distractor (including the target) colors swap randomly across trials, suppression at the level of the priority map may provide a ready 'default' strategy which is only slowly adapted in response to more subtle target location probability cues. Thus, both strategies are feasible in principle, and which one is adopted depends on the various, distractor and target probability cues acquired over the course of practice on the task.

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