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Attentional priming does not enable observers to ignore salient distractors

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ABSTRACT
It is commonly assumed that we find targets faster if we know what they look like. Such top-down guidance plays an important role in theories of visual attention. A recent provocative proposal is that effects attributed to top-down guidance instead reflect attentional priming. Theeuwes and van der Burg [(2011). On the limits of top-down control of visual selection. *Attention, Perception, and Psychophysics*. *73*(7), 2092–2103. doi:10.3758/s13414-011-0176-9] found that observers could not use top-down set to ignore irrelevant singletons but when priming was maximal such distractors could be successfully ignored, suggesting that feature-based top-down selection is impossible but that this can be overcome when a target feature is constant on consecutive trials. Using a variant of their task, we found that participants were unable to ignore a known colour singleton, but also that repetition priming did not help participants ignore the salient distractor. Our results stand in direct contrast to the results of Theeuwes and van der Burg and cast doubt upon the claim that priming effects can explain top-down effects in visual search. Notably the priming effects we do see are mostly episodic rather than featural which means that they cannot serve as a feature-based selection mechanism.

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A common assumption in the literature on visual attention is that we can steer attention to locations or objects according to our goals at any given moment (Baluch & Itti, 2011; Bundesen, 1990; Wolfe, 1994). So, as you perform a visual search for your child wearing a red striped shirt in a playground you can highlight the relevant features and bias your attention towards red items and striped items (Desimone & Duncan, 1995; Wolfe, Cave, & Franzel, 1989). A recently proposed provocative alternative is that these putative top-down effects can be accounted for by attentional priming – that effects attributed to top-down guidance simply reflect that particular features are highlighted as observers orient in the visual environment, because they are primed to them through selection history (Theeuwes, 2013).

Attentional priming (see e.g., Kristjánsson & Ásgeirsson, 2019, for review) refers to the finding that as observers repeatedly search for the same target, the search becomes faster (Maljkovic & Nakayama, 1994) and stimulus processing becomes more efficient (Ásgeirsson, Kristjánsson, & Bundesen, 2015). Consistent with this proposal, evidence in the literature on visual attention clearly shows that attentional priming makes a large contribution to what have been considered top-down effects. Kristjánsson, Wang, and Nakayama (2002) contrasted blocks of trials of a conjunction visual search where the target was always the same within a block (a red vertical bar; blocked condition) and where the target changed unpredictably between two possibilities (red vertical or red horizontal bars; variable condition) between trials. Performance was overall slower for the variable condition, but interestingly when the same target had repeated for 6–8 trials in a row in the variable condition, search speed became similar to that of the blocked condition suggesting that priming could account for effects that have typically been considered to reflect top-down guidance. This suggested that the ability to orient towards a particular set of features through top-down guidance may have been overestimated in the literature.

Another example comes from the work of Belopolsky, Schreij, and Theeuwes (2010) on attentional capture. Folk, Remington, and Johnston (1992) had proposed that unexpected items must be part of our

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attentional set to capture attention. The degree to which items capture attention is in other words *contingent* on task relevance. But Belopolsky et al. argued that these contingent capture findings reflected that the cues that Folk et al. (1992) used, primed the relevant items that observers searched for and therefore captured attention. Belopolsky et al. tested this possibility by telling observers to adopt a top-down set for a particular attribute on each trial during a cued response time task. They found that priming effects dominated performance even though it went *against* the top-down attentional set of the observers. This finding led them to propose that effects that have been attributed to top-down attention in the literature, actually reflect feature priming (Theeuwes, 2013). This suggests that the ability to ignore salient, yet irrelevant, visual information through top-down guidance may be overestimated.

Potentially the strongest evidence that priming can account for top-down guidance comes from Theeuwes and van der Burg (2011). They tested this using a version of the well-known additional singleton paradigm (Theeuwes, 1991, 1992). In this version of the paradigm observers were to find a predesignated target circle of a particular colour. Observers reported the orientation of a bar enclosed within the target circle. An irrelevant distractor of another colour was also present on the screen and the orientation of the bar of the irrelevant singleton could either match the orientation of the target bar or be orthogonal to it. Crucially, the identity (colour) of the target was only revealed by a pre-cue, without which observers would not be able to distinguish targets from coloured distractors. Older experiments on top-down cueing of targets have been criticized for not controlling whether subjects actually utilize endogenous cues, while this design forces participants to make use of the cue or fail at performing the task. Theeuwes & van der Burg measured the incongruence effect when the orientation of the two bars differed to assess to what degree the irrelevant item captured attention. Their reasoning was that if subjects could make use of the pre-cue to select a target, while ignoring the coloured distractor, there should be no influence of the orientation of the bar enclosed within the distractors. Conversely, if the distractor would capture observers' attention, there should be a response conflict, when bar orientation did not match that of the target, yielding an incongruence effect. They found that observers could not use top-down set (from verbal or symbolic cues) to ignore irrelevant colour singletons (in other words the congruence effect was still present). When the colour of the target singleton remained the same from one trial to the next, however, participants could ignore the irrelevant singleton, reflecting that intertrial priming allowed what Theeuwes & van der Burg called "perfect selection," and was seemingly unique in that respect, since top-down guidance did not have this effect.

Current aims

Theeuwes and van der Burg's (2011) conclusions leave us with two important claims. (1) That perfect feature-based top-down selection is impossible. (2) That this limitation of attention can be completely overcome when a visual target feature is consistent on consecutive trials. This entails the proposal that most if not all effects attributed to top-down feature-based guidance reflect priming (see e.g., Theeuwes, 2013). From a theoretical perspective, it is therefore vital to further explore this issue and to replicate this finding. We report three experiments where we address this issue.

Experiment 1

Experiment 1 was a close replication of Experiment 2 in Theeuwes and van der Burg's (2011) original study. A potential weakness of the original study by Theeuwes and van der Burg is that each individual experiment had relatively small sample sizes (9–12 participants). Here, we test the same hypotheses on about three times the number of participants. We hypothesized, based on the results of Theeuwes and van der Burg (2011), that we would find consistent congruence effects manifested in shorter response times when the orientation of the target matched that of the salient singleton distractor (cf. Theeuwes & van der Burg, 2011, Experiments 1–4). Such a result would indicate imperfect feature-based selection in that observers are unable to ignore a salient irrelevant distractor, even with full foreknowledge of the target colour. Secondly, we hypothesized that the congruence effects would disappear, or diminish significantly, when a target colour was repeated on consecutive trials, as claimed by Theeuwes and van der Burg, resulting in selection that is largely or wholly uninterrupted by the irrelevant distractors (cf. Theeuwes & van der Burg, 2011, Experiments 2 and 4). Thirdly, we directly compared the utility of the two cue types (word or colour cue), within-subjects. Based on the original study (Theeuwes & van der Burg, 2011; word cues in Experiments 1 and 2; colour cues in Experiments 3 and 4), we did not expect cue type to interact with priming or congruence effects. A strength of this approach to compare cue effectiveness was that the cues are varied within-subjects, rather than between-experiments, which should yield more reliable data on any potential differences between them. Lastly, we investigated the nature of the priming effects. While the nature of attentional priming has been debated in recent decades, the evidence is increasingly pointing towards a multi-faceted account of priming (see Kristjánsson & Ásgeirsson, 2019; Kristjánsson & Campana, 2010; for reviews). Depending on the task, priming can be purely based on visual features, but also on feature-conjunctions (Ásgeirsson & Kristjánsson, 2011) or episodes – i.e., pairings of visual stimulation and behavioural responses (Huang, Holcombe, & Pashler, 2004). In addition to the analyses performed by Theeuwes and van der Burg (2011), we also analyzed the effect of response repetition in order to determine whether priming effects were based entirely on speeded selection of visual features or on pairings (episodes) of stimulus selection and response selection.

Method

Participants

Thirty-five (32 female) students, aged 21–40 (mean: 27.9, SD = 5.6), participated in the experiment for course credit. Participants had taken an introductory course in the use of the OpenSesame software (Mathôt, Schreij, & Theeuwes, 2012) for research design, and participated using their own computers. The data of participants who reported any disturbance during testing were discarded and are not reported here.

Stimuli and apparatus

The experiment was run in OpenSesame on the students' own hardware. Participants were instructed to perform the experiment in a room where they would be comfortable and would not be disturbed. Exact stimulus sizes, luminance and colour are unavailable, due to the nature of the data collection. Instead, we report a known quantity: percent of monitor height for sizes and RGB-triplets for colours and brightness. In addition, degrees of visual angle are presented in parentheses under the assumption of 57 cm viewing distance, and a range of 13.3″ and 15″ (337 and 381 mm) monitors, i.e., the range of display sizes on most common modern laptop displays. Sizes in degrees of visual angles are, therefore, only plausible estimates of stimulus sizes for a typical observer, rather than measurements.

Stimuli were presented on a dark grey background ($r = 40$, $g = 40$, $b = 40$). Screen resolution was set to 1024×768 pixels. Each individual stimulus consisted of a circle that enclosed a horizontally or vertically oriented bar. The circle diameter was 13% of the monitor height (2.3–2.6°). Six circles were presented on each trial, four regular distractors, a coloured distractor, and a coloured target. Regular distractor circles were coloured light grey ($r = 168$, $g = 168$, $b = 168$), while the target and salient distractor circles were coloured red ($r = 255$, $g = 80$, $b = 80$) or green ($r = 20$, $g = 204$, $b = 40$). The colours were set to have similar perceived brightness by matching their *luma* to 60%, using the coefficients of rec.709 colour space. The salient distractor was always presented in the opposite colour to the target.

The length and width of the enclosed bars was about 61×9 pixels (8×1.2% of monitor height; 1.4–1.6° × .22–.25°), always presented in white ($r = 255$, $g = 255$, $b = 255$). Cues indicating the upcoming target colour were presented before each trial. In word cue blocks, the cue was a colour word: "rauður" or "grænn" (i.e., red or green in Icelandic, respectively). In colour cue blocks, the cues were coloured circles, similar to the targets and salient distractor circles, but of smaller size (10% of monitor height; 1.8–2°). Figure 1 shows the progression within trials.

Procedure

Each trial began when a light grey fixation cross was presented in the centre of the monitor for 900 ms. This was followed by a cue (word or colour) for 850 ms. The fixation cross appeared again, before the presentation of 6 stimuli in a circular configuration, centred on the fixation cross. Participants were instructed to respond quickly and accurately to the orientation of a bar inside the coloured target, while

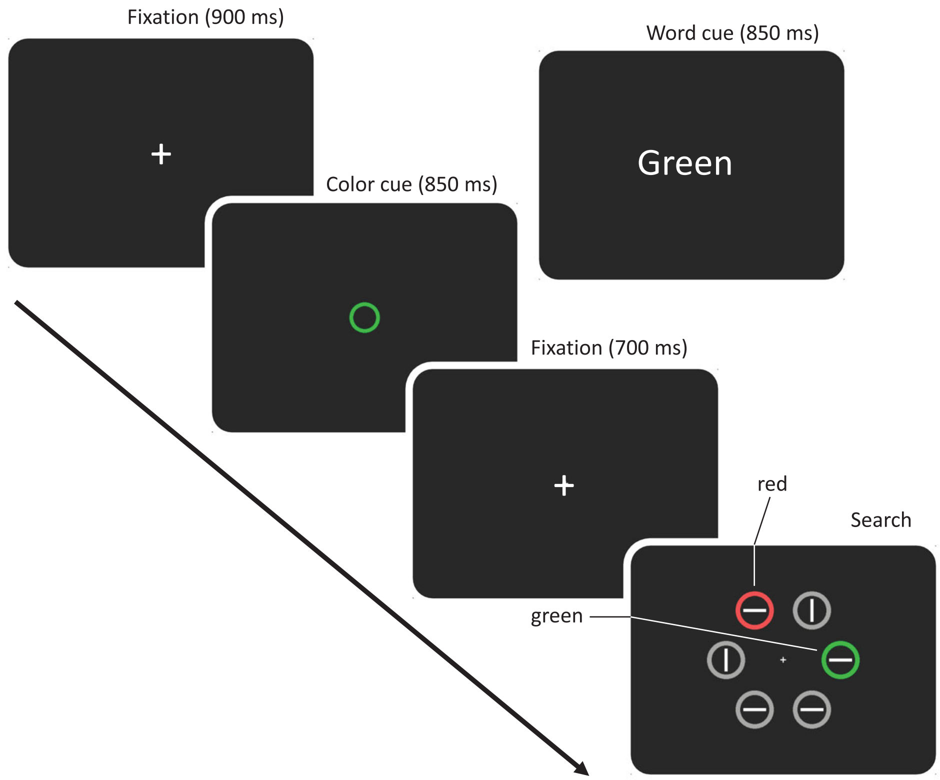

Figure 1. The progression of a typical trial in Experiment 1. As shown, the cue was either a coloured circle or a colour word. The target in this case is the green circle and observers have to report the orientation of the bar within the target circle. The irrelevant distractor is the red circle. The main measure is whether there is a congruence effect between the bars within the target and irrelevant distractor.

ignoring the distractors. Correct responses were made by pressing the "z" or "m" keys on a regular keyboard when a horizontal or vertical bar was enclosed by the target circle, respectively.

Participants performed two blocks of 40 practice trials (40 with word cues, 40 with colour cues), and two blocks of 160 experiment trials. A break was introduced midway through each block of the experiment. All practice data, as well as the first 3 trials of each block and after a break, were discarded.

Results

The data for 5 participants were discarded due to error rates in excess of 10%. The data of the remaining 30 participants were analyzed after discarding RT outliers (> 2500 ms). These exclusion criteria were adopted from Theeuwes and van der Burg (2011).

A repeated measures ANOVA with the factors *cue type*, *congruence* and *colour repetition* revealed a strong main effect of congruence, $F(1, 29) = 32.73$, $p = <.001$, $\eta_p^2 = 0.53$; and of colour repetition, $F(1, 29) = 28.34$, $p = <.001$, $\eta_p^2 = 0.494$ (Figure 2). No other effects were significant. Most importantly, repetition did not interact with congruence in either the two-way or the three-way interactions (p's = 0.65 and 0.077, respectively). This shows that even when the target colour is primed there is still a strong congruence effect in opposition to what Theeuwes and van der Burg (2011) observed.

The analysis presented above, is at the same level of detail as Theeuwes and van der Burg (2011) with regards to repetitions of target and distractor colours. However, it is well documented that priming effects accumulate over several trials, reaching a maximum after 5–8 trials (Brascamp, Pels, & Kristjánsson, 2011; Maljkovic & Nakayama, 1994; Martini, 2010; Wang, Kristjansson, & Nakayama, 2005; see Kristjánsson & Ásgeirsson, 2019 for review). Therefore, an additional analysis was run where target repetitions were included as a three-level factor, with no repetition, a single repetition and two or more repetitions. It was deemed impractical to add further precision to the repetition analysis, since this would yield conditions with less than 10 trials per subject, in the three or more repetitions condition. The purpose of

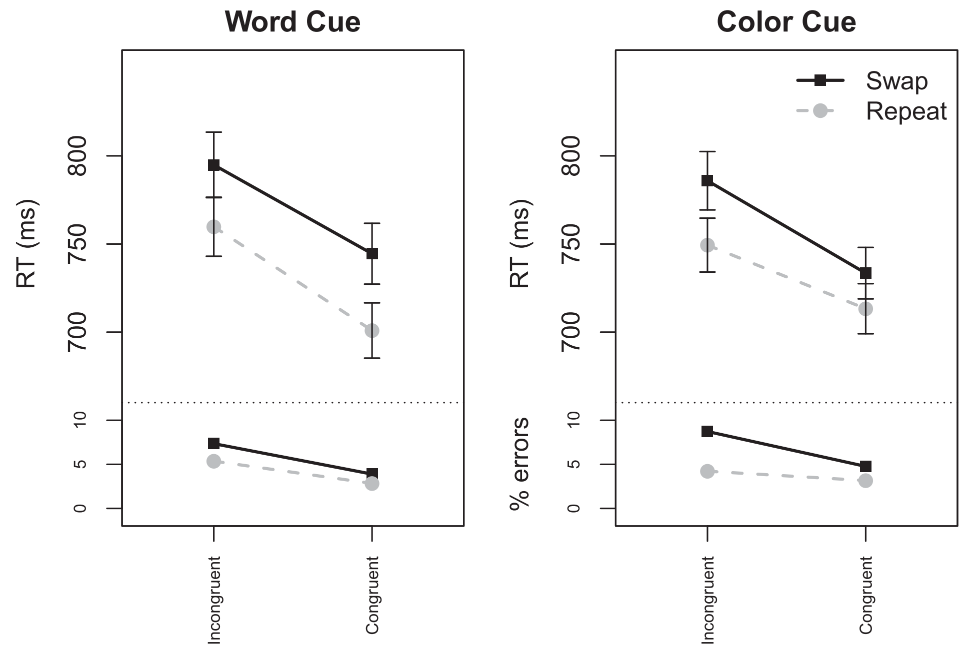

Figure 2. Top: Mean response times from Experiment 1. Error bars show 95% within-subject confidence intervals, calculated with the method of Morey (2008). Bottom: Error rates by condition.

the analysis was to investigate whether the congruence effects would subside or disappear as priming approached maximal influence on response times.

The additional repeated measures ANOVA included all the same factors as the previous one, with the addition of the third level of the colour repetition factor. The analysis confirmed main effects of congruence, $F(1, 29) = 28.32$, $p = <.001$, $\eta_p^2 = 0.494$; and colour repetition, $F(2, 58) = 23.98$, $p = <.001$, $\eta_p^2 = 0.453$; but not of cue type, $F(1, 29) = 0.01$, $p = 0.933$, $\eta_p^2 = <.001$. There was a significant interaction between cue type and congruence, reflecting a slightly larger congruence effect in the word cue condition, $F(1, 29) = 4.35$, $p = 0.046$, $\eta_p^2 = 0.13$. But most importantly, there were no two-way or three-way interactions between colour repetition and congruence ($ps \geq 0.439$), again showing that colour priming did not diminish the congruence effects, in contrast to what Theeuwes and van der Burg observed, even at levels where priming can be considered to be maximized.

To check for signs of speed-accuracy trade-offs, the first ANOVA was repeated with error rates as the dependent variable. There was a main effect of congruence, $F(1, 29) = 24.0$, $p < .001$, and of colour repetition, $F(1, 29) = 23.9$, $p < .001$. However, these effects were both positively related to response times, suggesting that there was no confounding speed-accuracy trade-off.

To ensure that the two colours were perceived as equally salient, we compared response times when the target was green, compared to when it was red. The difference in mean response times was 1 ms on average, and not statistically reliable, $t(44) = -.12$, $p = .904$.

Finally, to investigate the nature of the priming effects, we ran a repeated measures ANOVA on response times with the factors: response repetition, colour repetition and congruence. If the priming effects reflect independent facilitation of visual features (e.g., Maljkovic & Nakayama, 1994; Ásgeirsson & Kristjánsson, 2011, experiments 3 & 4; Ásgeirsson, Kristjánsson, & Bundesen, 2014), we expected response repetition to speed up responses, independently of colour repetition. However, if the priming effects are based on episodes (e.g., Huang et al., 2004), we expect the effects of response and colour repetition to interact: repeating responses and colour simultaneously should magnify the resulting priming effects. The analysis revealed main effects of colour repetition, $F(1,28) = 20.9$, $p < .001$, $\eta_p^2 = .512$, and congruence, $F(1,28) = 31.6$, $p < .001$, $\eta_p^2 = .53$, but not of response repetition, $F(1,28) < .01$, $p = .99$. Importantly, there was a clear interaction between colour repetition and response repetition, $F(1,28) = 22.6$, $p < .001$, $\eta_p^2 = .447$, Colour priming effects were much larger when the responses were also repeated,

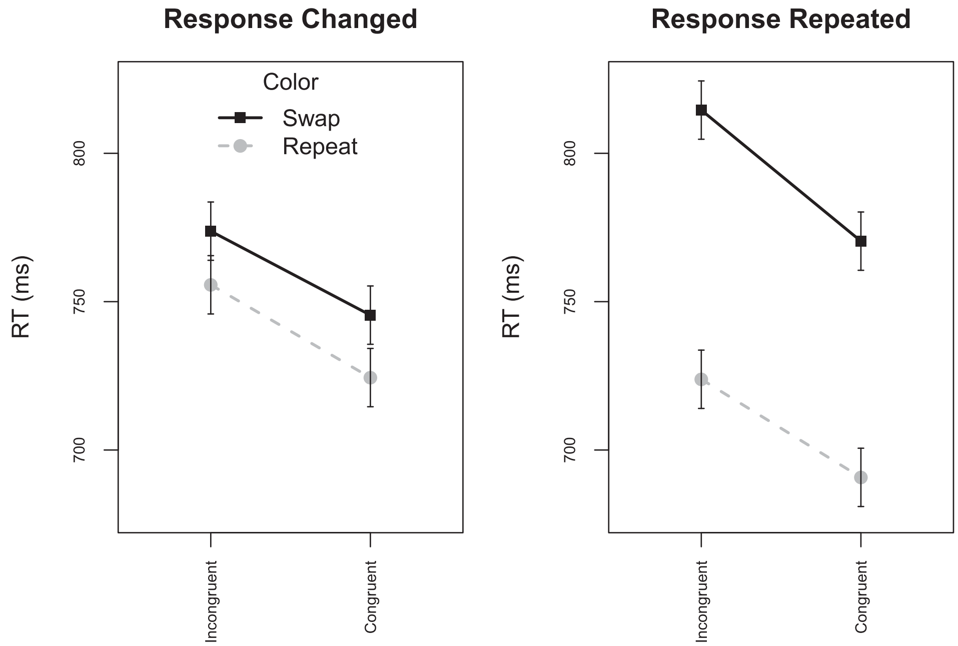

Figure 3. Mean response times by response repetition, colour repetition and congruence. Error bars show 95% within-subject confidence intervals, calculated with the method of Morey (2008).

compared to when the response changes on subsequent trials (85 and 20 ms, respectively; Figure 3). Other interactions did not reach statistical significance (*ps* > .33). This shows that the priming effects must be largely episodic – rather than perceptual – in nature, and most likely explained by decision-making processes. Notably, the primed episodes did not included congruence, which was unaffected by including response repetition in the analysis.

Discussion

Experiment 1 clearly demonstrated a congruence effect between a target bar orientation and the orientation of a bar within the salient singleton distractor. This indicates that the participants were unable to ignore the salient distractor, even when they had full foreknowledge of the target colour and knew that attending to the distractor would hinder their performance. This result is compatible with the original study of Theeuwes and van der Burg. But in contrast to the results of their Experiments 2 and 4, repetition priming did not enable participants to ignore the salient distractor. They found no congruence effects on colour repetition trials and argued that selection was perfect when observers were primed to the target colour. Conversely, the congruence effects we found were robust, regardless of colour repetition. This suggests that participants were not able to effectively ignore the salient stimulus with the aid of colour priming, suggesting that the limitations to top-down feature-based attention are even more severe than reported by Theeuwes and van der Burg (2011). Finally, the significant interaction between colour and response repetition suggests that the priming effects we do see were largely episodic rather than featural which means that the priming cannot serve as an effective feature-based selection mechanism.

At this point we should note that Theeuwes and van der Burg raise the possibility that participants may effectively select a target first, suggesting perfect selection, but then attend to the distractor, before responding to the target. This situation may lead to a decision conflict revealed by a congruence effect, identical to what one would expect if the salient distractor had captured attention. To exclude such an explanation, they ran a control experiment (Experiment 5, Theeuwes & van der Burg, 2011), where they presented the same stimuli, but with brief exposures (8–300 ms) and unspeeded responses. The results showed that congruence effects were still present. The experiment was a welcome addition to the response time experiments, since it demonstrated that overt attention to both target and the salient distractor cannot fully account for the congruence effects since overt attentional orienting would not be possible within the limited stimulus durations. Theeuwes and van der Burg (2011, p. 2100) concluded that the congruence effect is most likely the result of "[…] mandatory processing of the distractor singleton *before* attention is switched to the target singleton." Our experiment 2 here was designed to examine whether this explanation holds for the congruence effect in the response time version of the additional singleton task.

Experiment 2

The results of Experiment 1 are consistent with an even stronger view against feature-based attention than that of Theeuwes and van der Burg (2011), in that even the additional bias towards certain features that comes with colour priming did not enable participants to ignore salient distractors. A plausible reason for the tenacious distraction by a colour singleton, is that the relatively large bars enclosed in the stimulus circles can be processed without overt attentional orienting. This would also have been the case in the original tasks of Theeuwes and van der Burg (2011). This property of the experimental design may lead to a situation where observers are able to encode information about the salient distractor in addition to the target to an extent where the orientation of the distractor can affect responses to the target. In fact, this must have been the case in Theeuwes and van der Burg's Experiment 5, since there was not enough time for overt attention shifts towards both a target and a distractor. This distinction between covert and overt attention shifts is important, since attentional capture and oculomotor capture have been shown to be dissociated under certain circumstances (Theeuwes, De Vries, & Godijn, 2003).

Experiment 2 involved the repetition of the word-cue condition in Experiment 1 with an additional condition where the bars inside the stimulus circles were of much smaller size. If the congruence effects in Experiment 1, and the response time-based experiments in Theeuwes and van der Burg (2011) were largely – or wholly – caused by processing that occurs before attentional orienting; as they must have been in their experiment 5, there should be clear differences in the results between the large-bar and small-bar conditions. We expect the small bars to slow down performance, but response times should not depend on the orientation of a bar inside the distractor, if it is too small to be covertly attended.

The large-bar condition of Experiment 2 was an exact replication of the word-cue condition in Experiment 1 on new subjects. Therefore, we predicted that we would replicate the main results of Experiment 1: robust main effects of colour repetition and congruence. In the small-bar condition, we predicted that there would be a main effect of priming, but no congruence effect. The small size of the response feature should be too small to affect responses, if observer performance is, in fact, influenced during pre-selective processes. At the very least, the congruence effects should be significantly diminished with small bars, since these would most likely only occur on trials where distractors were overtly attended before a response decision was made. Alternatively, the simple fact that the small bars are more demanding on perceptual processes could also reduce or eliminate congruence effects. According to *load theory* (Forster & Lavie, 2008), an increase in perceptual load reduces distractions by irrelevant stimuli. In this case, our measure of distraction is the congruence effect, which should shrink or vanish if perceptual load is sufficiently increased by the task of discriminating the small-bar orientations. Therefore, both of these hypotheses yield essentially the same predictions for Experiment 2.

Method

The methods of Experiment 2 were identical to Experiment 1, with the following exceptions: Forty-five students (42 female, one did not specify), aged 20–39 (mean = 27.5, SD = 5.4) participated for course credit. The stimuli and timing were exactly as in Experiment 1, with the exception of the enclosed bars, which were smaller in the *small-bars* condition. In this condition, the bars were only 14 by 7 pixels instead of 61 by 9 pixels ($1.8 \times .9\%$ of screen height; .32–.36°; Figure 4). The small size was expected to force observers to make saccades towards the centre of the stimuli, rather than covertly discriminate the bar orientation. The target was cued by a word cue, identical to the one in the colour cue condition of Experiment A final difference between Experiments 1 and 2, was that on 1/6 of trials, there was no salient distractor, but only a coloured target and five grey distractors. This was included to obtain a measure of the response time costs accounted for by the presence of a salient distractor.

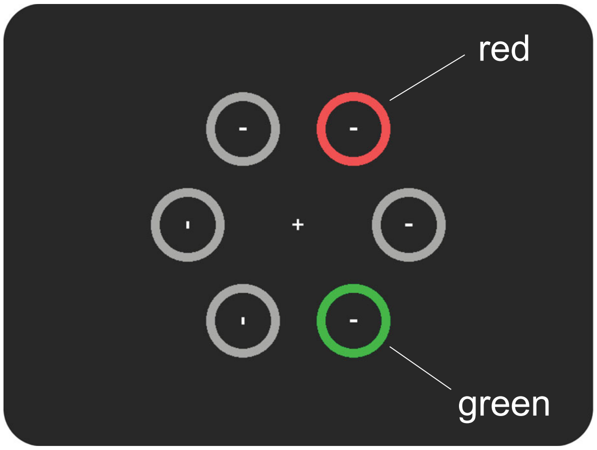

Figure 4. An illustration of a typical stimulus display in the small-bar condition of Experiment 2. See Figure 1 to compare the relative sizes of large bars. The "red" and "green" labels were not displayed. Circle and bar stimuli are drawn to scale.

Each participant performed a total of 40 practice trials, followed by 158 trials in each block of the experiment (small or large bar), totaling 316 experimental trials. Block order was counterbalanced, based on a random number in the observers' national identification number.

Results

A repeated measures ANOVA was performed on the data from all trials that included a salient distractor. There was a main effect of bar size accounting for slower response times (61 ms) when the bars were smaller, $F(1, 44) = 17.8$, $p < .001$, $\eta_p^2 = .29$. There were also main effects of congruence, $F(1, 44) = 24.1$, $p < .001$, $\eta_p^2 = .35$, and colour repetition, $F(1, 44) = 39.4$, $p < .001$, $\eta_p^2 = .47$. No interactions were significant ($ps \geq .3$, $\eta_p^2 s \leq .02$), suggesting that observers were not able to use a feature-based strategy to ignore the salient distractor. The average response times are shown in Figure 5.

An analogous repeated measures ANOVA on the error rates in each condition revealed main effects of congruence, $F(1, 44) = 17.1$, $p < .001$, and colour repetition, $F(1, 44) = 22.9$, $p < .001$, but not of bar size ($p = .199$). However, both main effects had a positive relationship with response time, ruling out confounding speed-accuracy trade-offs. Finally, there was a significant interaction between congruence and colour repetition, which accounted for larger congruence effects on errors, when target colour was swapped, compared to when it was repeated.

A repeated measures ANOVA was also performed on the data from trials without a salient distractor. The analysis revealed analogous effects to the previous analysis: main effects of bar size, $F(1, 44) = 4.66$, $p < .036$, $\eta_p^2 = .096$, and of colour repetition, $F(1, 44) = 12.9$, $p < .001$, $\eta_p^2 = .226$, but no interaction, $F(1, 44) = 3.3$, $p = .076$, $\eta_p^2 = .070$. The response time costs

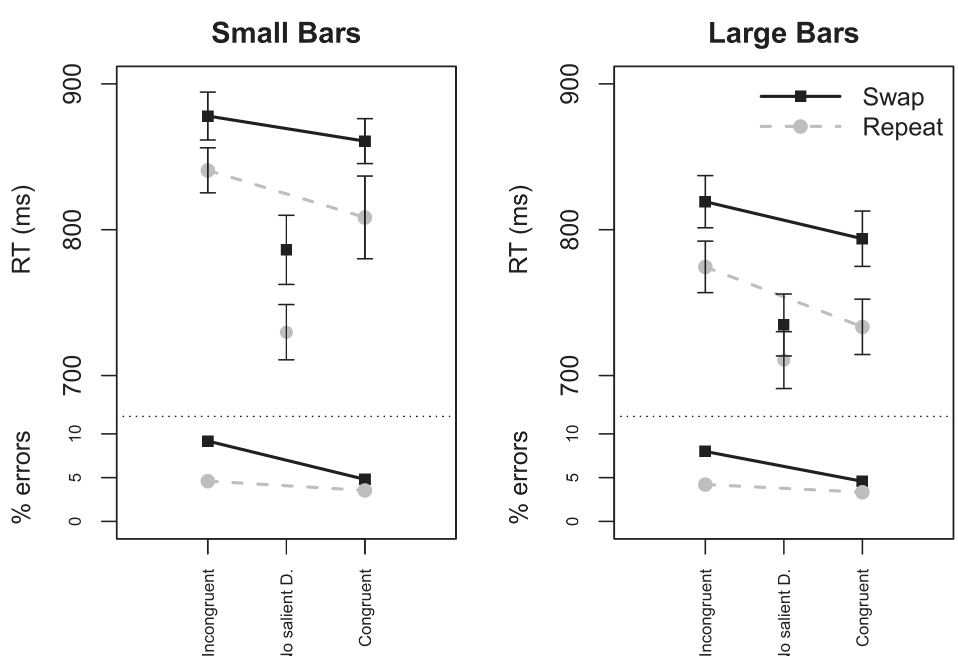

Figure 5. Top: Mean response times from Experiment 2. Error bars show 95% within-subject confidence intervals, calculated with the method of Morey (2008). Bottom: Error rates by condition.

associated with a salient distractor were 56 and 90 ms in the large and small bar conditions, respectively. This main effect was significant, $F(1,44) = 6.78$, $p = .012$, $\eta_p^2 = .134$. There was no main effect of colour repetition nor an interaction between colour repetition and bar size on response time cost (*p*s = . 468 and .079, respectively).

To ensure that the two colours were perceived as equally salient, we compared response times when the target was green, to when it was red. Consistent with the results of Experiment 1, the 6 ms difference in mean response times was not statistically reliable, $t(44) = .94$, $p = .35$.

Finally, we ran a repeated measures ANOVA on response times with the factors response repetition, colour repetition and congruence, to see if Experiment 2 supported the episodic priming account, suggested by Experiment 1. The results of the analysis were largely in agreement with the analogous analysis of Experiment 1: there were significant main effects of congruence and colour repetition (*p*s < .001), but not of response repetition ($p = .277$). Furthermore, there was an interaction between colour repetition and response repetition, $F(1,44) = 15.0$, $p < .001$, $\eta_p^2 = .254$, accounting for a much larger colour priming effect when response was also repeated (68 vs 26 ms), and of congruence and response repetition, $F(1,44) = 5.79$, $p = .02$, $\eta_p^2 = .116$, explaining the larger congruence effect when response was repeated (44 vs 12 ms). This is in contrast to the results of Experiment 1, where congruence was not modulated by response repetition. Regardless of this discrepancy, both analyses suggest that a large chunk of the variation in response times is explained by episodic priming, where benefits of repetitions are maximal when an episode of colour and response are repeated on subsequent trials.

Discussion

Experiment 2 confirmed the main effects found in Experiment 1 in an even larger sample ($n = 45$). There were large colour priming- and congruence effects. However, the critical interaction between priming and congruence, reported by Theeuwes and van der Burg (2011), did not appear. This suggests that observers were equally distracted by a salient colour distractor, regardless of the colour of the previous target. This is perhaps not surprising if the majority of the priming effects are episodic, since they should then not induce feature-based biases.

The smaller sized bars enclosed within target circles did slow responses down, but there was no interaction with congruence. This suggests that congruence effect in the response time experiments were most likely due to overt orienting of attention towards distractors, or that the method of orienting is unimportant in this type of task.

The analysis of trials without a salient distractor show that the effects of bar size and colour repetition are not limited to salient distractor trials. In fact, the pattern suggests that a large chunk of the variance explained by repetition priming, is probably not related to faster orienting of attention towards a target. If that had been the case, priming effects should shrink – or vanish – when the salient distractor is not present alongside the target (see Ásgeirsson et al., 2014; Goolsby & Suzuki, 2001, for examples). In addition, the robust interaction between colour repetition and response repetition in both experiment 1 and 2, suggests that the priming effects seen here are in large part due to decision processes (e.g., Huang et al., 2004; see also Hommel, Müsseler, Aschersleben, & Prinz, 2001).

Experiment 3

The results of Experiment 1 and 2 lend strong support to the idea of severely limited top-down feature-based attention but did not give any indication that colour priming can remove or attenuate these limitations. Again, this latter result is in direct contrast with the original study by Theeuwes and van der Burg (2011), where a salient distractor did not hinder performance when a target colour was repeated on consecutive trials. Experiment 3 was an eye-tracking study run to better understand how observers' orient attention in the experimental task.

One of the limitations of previous experiments is that we have no information on observers' eye movements while they perform the task. Experiment 1, as well as Theeuwes and van der Burg's (2011) experiments could have been performed by overtly or covertly orienting attention onto the visual stimuli. Experiment 2 would make this more difficult, due to finer grained orientation judgments necessary to perform the task, but we have no empirical evidence for how the task was actually performed. If minor differences between the experiments of Theeuwes and van der Burg (2011) and the current study encouraged different strategies – such as covert orienting in the former, but overt in the latter – this could explain discrepancies between the results.

Experiment 3 was a replication of Experiment 2 run on 8 observers in the laboratory. Eye movements were monitored during the experiment and observers were initially allowed to perform the task without any constraints on eye movements, followed by a session where they were instructed to fixate a central cross throughout. The purpose was to answer several questions regarding Experiment 2: (1) Can observers perform the task without moving their gaze onto targets? (2) Do observers prefer to remain fixated or move their gaze when given freedom to perform the task as they like? (3) Are response time effects – i.e., congruence and colour priming effects – modulated by constraints on gaze shifts?

Method

Participants

Eight volunteers (6 female), aged 19–40 (mean = 28.4, SD = 7.2) participated in the experiment.

Stimuli and apparatus

The experiment was run in MATLAB, using the Psychophysics Toolbox (Kleiner, Brainard, & Pelli, 2007) on a desktop computer running Windows 7. Eye movement data was collected using a Cambridge Research Systems eye tracker, sampling eye position at 250 Hz. Eye movements were monitored monocularly (right eye) while participants rested their head on a chinrest. The distance from eye to monitor was 57 cm. Stimuli were sized to match the retinal sizes of stimuli in Experiment 2, if viewed on a 15 inch monitor from the distance of 57 cm (i.e., the upper limits of the stimulus size estimates in Experiments 1 and 2). Stimuli were presented on a 27 inch Philips LCD monitor, running at a 100 Hz refresh rate.

Procedure

Observers read the same instructions as those for the small bar task in Experiment 2. This was followed by additional instructions explaining the monitoring of eye movements during the task. To avoid affecting their choice of eye movement behaviour, they were not informed about the restrictions on eye movements that would be introduced in the second half of the experimental session Observers were asked to keep their head still in the chinrest during experimental blocks and told that they must fixate on the central fixation point at the beginning of each trial. These were the only additional restrictions on their behaviour, relative to Experiment 2, and they were instructed to perform the task as felt most comfortable to them.

Each observer performed 40 practice trials, before two 96 trial blocks of the task. A 9-point calibration was performed before the practice block and each block of the experiment. The first two blocks were the *free* condition of the experiment, where no restrictions were made on eye movements, other than the required central fixation at the beginning of each trial.

After completion of the two blocks of *free* visual search, observers were instructed that this time, they would have to perform the same task without making any saccadic eye movements. They were instructed to fixate the centre of the monitor from the beginning of each trial, until they made a keyboard response. This was the *fixed gaze* condition of the experiment. Each observer performed 40 practice trials in the fixed gaze condition, before performing two blocks of 96 experiment trials. The full experiment amounted to 80 practice trials and 384 experiment trials.

A typical trial progressed as in Experiment 2. The only exception was that the experiment was halted until the eye tracker registered a fixation on the fixation cross. In the fixed gaze condition, a warning tone and the message: please fixate the fixation cross, were displayed if the eye tracker registered an eye movement more than 2° outside the central area of the display.

Results

Eye tracking data

Out of a total of 3072 trials, data from 152 were lost due to recording errors (4.9%). Of the remaining 2920 trials, further 6 trials were rejected due to eye blinks or other temporary losses of signal.

First, we examined how observers performed the experimental task when their eye movements were not restricted. Seven out of eight observers clearly preferred making saccades onto the targets and did so on 85%–100% of trials. Three subjects fixated the target on all valid trials. A single subject (#4) only fixated the target on 11% of trials, showing a strong preference to remain fixated on the fixation cross. On average, subjects fixated the target on 84% of trials. In the fixed gaze condition, observers fixated the target on 1% of trials. The data suggest that most observers prefer to overtly move their attention towards a target, when they are free to choose their strategy.

Second, we examined how often observers fixated the distractor stimulus. In the free condition, seven out of eight observers fixated the salient distractor on 9%–31% of trials, while a single subject (#4) never fixated the distractor. Overall, the observers fixated the distractor on 15.5% of trials in the free gaze condition. Only a single subject ever fixated the distractor in the fixed gaze condition. These accounted for 2% of the subject's valid trials.

Behavioural data

Data from a single observer (#1) did not contain a valid congruence factor and was discarded from analyses where this factor was included. Furthermore, all incorrect responses and the first trial of any block were discarded before response time analyses.

The constraints on eye movements in the fixed gaze condition did not slow down observers' responses (793 and 782 ms, in the free and fixed gaze conditions, respectively), nor did it lead to more errors (7.2% and 6.1%). Thus, the preference for eye movement did not yield better performance, and the subjects were clearly able to perform the task while fixating on the fixation cross.

A repeated measures ANOVA of response times with the factors *gaze condition* (free, fixed), congruence and colour repetition yielded significant main effects of congruence, $F(1,6) = 13.6$, $p = .01$, $\eta_p^2 = .69$, and of colour repetition, $F(1,6) = 6.1$, $p = .048$, $\eta_p^2 = .50$, but not of gaze condition, $F(1,6) = 1.6$, $p = .70$, $\eta_p^2 = .026$. There were no indications of any reliable interactions between factors (*ps* > .58; see Figure 6).

A repeated measures ANOVA of error rates, with all the same factors as the previous analysis did not reveal any statistically reliable effects (*ps* ≥ .11).

Discussion

Experiment 3 did not reveal meaningful differences in performance based on whether observers were allowed to overtly attend the stimuli, but clearly showed that they were capable of performing the task under both conditions. It was also clear that observers preferred to perform the task by overtly attending stimuli, even if such a strategy did not enhance their performance. This result suggests that

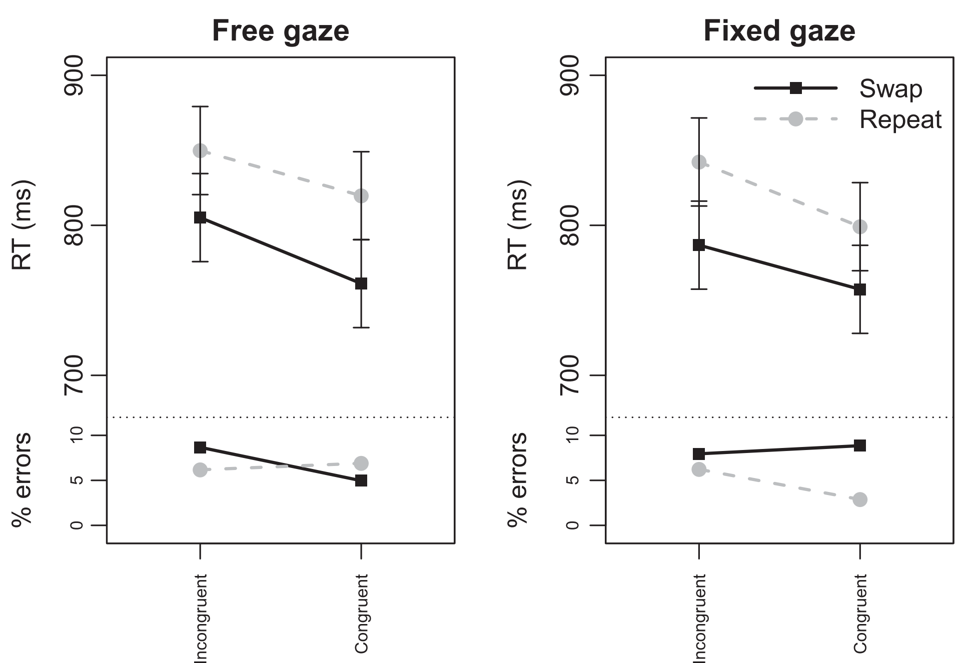

Figure 6. Mean response times from Experiment 3. Error bars show 95% within-subject confidence intervals, calculated with the method of Morey (2008). Bottom: Error rates by condition.

most observers in Experiment 2 would have fixated the targets. However, the results of Experiment 3 indicate that this is of no consequence: feature-based attention seems equally limited, regardless of gaze.

General discussion

In two experiments, we were unable to uncover any feature-based control of attention in a version of the additional singleton paradigm (Theeuwes, 1991, 1992) where the task cannot be performed successfully, without foreknowledge of the target colour. These results support Theeuwes's notion (Theeuwes, 2010, 2013; Theeuwes & van der Burg, 2011) that feature-based attention is much more limited than has been assumed in a number of theories of visual attention (e.g., Baluch & Itti, 2011; Wolfe, 1994). But notably, Theeuwes and van der Burg also hypothesized that repetition priming may cause illusory feature-based top-down attention (Theeuwes, Reimann, & Mortier, 2006); that through repeated selection of target colours, selecting becomes more efficient to the extent that a salient distractor will not interfere with search for the target. However, we were unable to find any evidence of this in our replication and extension of Theeuwes and van der Burg's (2011) own paradigm. Here, colour priming was robust throughout all conditions of the experiments, but importantly, it did not interact with congruence effects in any way.

The result that there were no conditions where a salient distractor does not interfere with target processing (Experiments 1 and 2) does not demonstrate conclusively that there is no such thing as top-down feature-based attention, in the early *sweep* of stimulus processing (Theeuwes, 2010). For example, a probabilistic weighting account of attention, such as TVA (Bundesen, 1990), acknowledges that feature-based attention is imperfect, but that it still plays an important role in the weighting of the visual environment (see Nordfang & Bundesen, 2010). According to this theory, top-down attention can be directed to a certain visual feature. This results in an increased weight of any stimulus with that feature, which increases the probability that those stimuli will be encoded in a probabilistic race for representation by the neural architecture. Similarly, while we do not adhere to the strong interpretation that top-down feature-based attention must be completely ineffective, our results certainly support claims that it plays a limited role in target selection.

The second main finding, that priming does not eliminate – or even attenuate – distractor interference is more troublesome. While we are in many ways sympathetic to the claims of Theeuwes and van der Burg that priming contributes to top-down guidance effects (check Ásgeirsson et al., 2015; Kristjánsson et al., 2002) the strong version of their argument that intertrial priming causes perfect selection and that it is unique in doing so is not supported by our results here. While there is good evidence that priming can account for large proportions of effects attributed to top-down guidance, there is seemingly little evidence for the claim that priming and only priming can allow perfect selection that is unaffected by a salient distractor. Lamy and Kristjánsson (2013) reviewed the literature to ask whether top-down effects could simply be described as intertrial priming and they concluded that "priming accounts for considerable portions of effects attributed to top-down guidance, but that top- down guidance can be independent of intertrial priming." More generally, it seems that replacing top-down attention with priming is an unsatisfactory account of how we orient in our visual environments. As we interact with our visual environment such as when we walk down a busy street with our child in the striped shirt looking for the ice cream store, we have many goals – keeping our child safe, watching cars and other pedestrians and keeping an eye out for the store, street signs and traffic lights. It is a highly dynamic situation and priming alone cannot explain how we keep track of all these goals, although we have no wish to deny the large effects that priming has on attention in tasks such as those tested here. We also note that our results are consistent with the dimension-weighting account of priming (see Liesefeld, Liesefeld, & Müller, 2019), since according to this account, distractor interference should be relatively impervious to across-trial priming effects when the distractor is unique on the same dimension as the target since the primed dimension (in this case colour) is weighted above other dimensions, although this need not be quite categorical (Zhang, Allenmark, Liesefeld, Shi, & Müller, 2019). Follow-up research on the limitations of top-down control could include a version of Theeuwes and van der Burg's (2011) task, where target identity can only be known by dimensional cueing – rather than within-dimension cueing – to investigate whether behaviour results in similar congruence and priming effects, as they do here.

We do not have a clear idea why our results differ from those of Theeuwes and van der Burg, but we note that our design has high power and large effect sizes. There were several subtle differences between the two studies. (1) Observers performed the experiment at their own convenience on their own computers in our study, while the original study was a laboratory study. The decision to use a perform-at-home design was a compromise between internal control and statistical power, which would likely introduce new sources of error. An obvious by-product of such a design is that stimuli are not exactly the same size, brightness and colour for all observers. The same problem occurs in all web-based studies of behaviour, which have nevertheless been shown to sufficiently reliable (e.g., Crump, Mcdonnell, & Gureckis, 2013; Germine, Nakayama, Duchaine, & Wilmer, 2012). The current study should, if anything, be more reliable, since it is run on dedicated presentation and measurement software (OpenSesame, Mathôt et al., 2012), and the observers were students of psychology looking to get course credit, rather than anonymous visitors to a website (e.g., Germine et al., 2012) or anonymous volunteers seeking a minor compensation (e.g., Crump et al., 2013). Experiments 1 and 2 in the current study were of similar duration to those in the original study (close to 300 trials per session), while Experiment 3 was approximately twice as long, but the original study varied fewer properties within subject. Notably, we varied cue type (Experiment 1), bar size (Experiment 2) and gaze constraints (Experiment 3) within participant. Finally, the stimulus displays in the current study were always of set size 6, while the original study presented 7 stimulus circles on each trial. We can, of course, not rule out that any or all of these design differences contributed to the discrepancies between the two studies, but none of them would be expected to produce systematic effects that interact with colour priming or congruence. Systematic effects of design subtleties seem even more unlikely in light of the data from Experiment 3; an experiment in a laboratory setting, where we also found that colour priming did not lead to perfect selection.

While power issues or subtle design issues can possibly account for discrepancies between our results and theirs, at this point, our results must be considered a non-replication of their results, at least of the aspects critical for the argument that priming replaces top-down guidance. Finally we note that there were significant interaction between colour and response repetition in experiments 1 and 2, which suggests that the priming effects we do see are largely episodic rather than featural which means that the priming *cannot* serve as a feature-based selection mechanism.

In sum, our results cast doubt upon the claim that priming effects can explain top-down effects in visual search, and even if they would in certain tasks, that would not mean that top-down attention reduces to priming effects. Our interactions with the visual world seem to be far too multifaceted for such a conclusion.

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No potential conflict of interest was reported by the authors.

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