Attentional set and explicit expectations of perceptual load determine flanker interference

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Abstract

Task-irrelevant stimuli often capture our attention despite our best efforts to ignore them. It has been noted that tasks involving perceptually complex displays can lead to reduced interference from distractors. The mechanism behind this effect is debated, with some accounts emphasising the ‘perceptual load’ of the stimuli themselves, and others emphasising the role of proactive control. Here, in three experiments, we investigated the roles of perceptual load, proactive control, and reward motivation in determining distractor interference. Participants performed a visual search task of high, low, or intermediate-load, with flanking task-irrelevant distractors. Each trial was preceded by a cue indicating the level of perceptual load (Experiments 1-3) as well as the potential reward that could be earned (Experiments 2-3). In all three experiments, the attentional set induced by preceding trial and cued proactive expectation of perceptual load interacted to determine flanker interference, which was significant for all trial types except trials cued as high-load which were also preceded by high-load. These effects were not modulated by reward motivation, although in the final experiment reward did significantly improve performance overall. Thus, successful distractor exclusion does not depend upon motivation or load per se but does require an expectation of high load.

Significance statement

We all experience distraction by irrelevant stimuli which can impair our performance of tasks, even when we are motivated to ignore them. There has been considerable debate as to which factors determine whether or not a distractor will capture our attention. We investigated several factors, including the prospect of obtaining a reward and proactive preparation based on

the expected task difficulty. Our results suggest that both the expectation of a difficult task, and the recent experience, combine to allow us to effectively and proactively ignore distractors. In contrast, rewards could improve performance overall, but did not affect the extent to which distractors captured attention.

Keywords: Perceptual load theory, distraction, reward, cognitive effort, attention, proactive control

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The limitations of attentional selection are typically demonstrated by paradigms wherein the observer fails to suppress salient but task-irrelevant distractors despite them being detrimental to the task (Eriksen & Eriksen, 1974; Stroop, 1935; Theeuwes, 1991). These failures of selective attention are powerful and pervasive, as people are unable to suppress distractors even when their presence is predictable and participants are motivated to ignore them (e.g. Marini et al., 2015). However, Lavie and colleagues (Lavie, 1995; Lavie & Tsal, 1994) found that flanker interference was reduced in contexts involving high levels of ‘perceptual load’. They demonstrated that more complex displays, such as those which produce inefficient visual search, led to relatively less flanker interference than simpler displays. Initial support for this account came from experiments which combined the Eriksen flanker paradigm (Eriksen & Eriksen, 1974) with a visual search task. In one condition, participants searched for an angular target letter (e.g. Z or N) among heterogeneous, angular non-targets (e.g. M, K, V, Y, X; high perceptual load) or else among homogeneous ‘O’s (low perceptual load). In addition to this visual search task, flanking distractors were presented in the periphery of the display which could be either neutral, incongruent or congruent with regard to the identity of the target letter. In this paradigm, ‘distractors’ are therefore distinguished from the search-relevant stimuli by their spatial location (peripheral to the ring of positions around fixation in which the target can appear). The impact of these distractors upon search response times is then compared between the low- and high-load conditions, wherein ‘non-target’ stimuli in the search-relevant locations either produce an efficient ‘pop-out’ search or an inefficient or serial search respectively (Lavie & Cox, 1997; Roper et al. 2013). In these experiments, flanker interference was significantly reduced in the high-load condition relative to the low-load condition (Lavie, 1995).

These results led to the development of Perceptual Load Theory (PLT) which proposes that a limited-capacity resource underlies perceptual processing and that this resource is allocated automatically provided there is capacity available (Lavie, 1995; 2005). When a demanding task exhausts perceptual capacity, there is none left to ‘spill over’ to flankers, but when an easier task leaves spare capacity available, flankers must be processed. Alternatively, Tsal and Benoni described a ‘dilution’ account, which suggested flankers always receive some level of processing, but that this processing is ‘diluted’ by the presence of more heterogeneous display elements (Benoni & Tsal, 2013; Tsal & Benoni, 2010). The dilution account was based on the observation that stimuli which involve heterogeneous stimuli such as those in high-load, but which nonetheless facilitate efficient ‘pop out’ search, still eliminate flanker interference. Thus, although the PLT and dilution accounts differ in the underlying mechanism of the effect, they both attribute reduced flanker interference in high-load to the perceptual elements of the display.

Alternatively, the effects observed in these experiments could be the result of a proactively set ‘attentional window’: when participants anticipate a more difficult search, they proactively adopt a narrowed focus of attention to focus on the individual elements of the display. This narrowed focus then excludes peripheral flanker stimuli, which would otherwise be encompassed by a wider focus. This account was proposed following the observation by Theeuwes et al. (2004) that the load-induced reduction of flanker interference did not occur when high- and low-load trials were randomly intermixed as opposed to being presented in blocks (as is typical). Indeed, they found that in this case, flanker interference in a given trial depended both on the perceptual load of that trial and on the load experienced in the preceding trial. That is, only high-load trials which were preceded by high-load trials showed a significant reduction in flanker interference, whereas low-load trials or high-load trials preceded by low-load trials had equivalent flanker effects (see also Biggs & Gibson, 2010, 2018). The authors concluded that perceptual-load induced reductions in flanker interference are due to proactive control based on prior experience. When the participant experiences a perceptually demanding task, they narrow their attentional window, thereby excluding the peripheral distractors; this narrowed window is then maintained into the subsequent trial. In contrast, if the perceptual task is relatively easy, a broader attentional window can be employed, which then encompasses the peripheral distractors and is likely maintained in a similar fashion for the upcoming trial.

This attentional window account received further support from studies which manipulated the degree to which a narrowed attentional window can effectively exclude distractors. For example, Chen and Cave (2016) presented participants with a target identification task in which two distinct stimuli had to be attended. They manipulated perceptual load (in blocks) and presented flanker stimuli either peripheral to the two targets (as in previous studies) or in between the two task-relevant locations. Thus, while a narrowed attentional window could exclude the distractors in the former case, it could not in the latter. They found that trials with peripheral distractors exhibited the typical pattern of results, with significant flanker effects under low-load but not high-load. When the flanker was presented centrally, in between the two target locations however, there was no load-induced reduction in flanker interference. Similarly, Biggs and Gibson (2018) found that when a stimulus display involved heterogeneous non-target stimuli (thus constituting high-load and dilution), flanker interference was still observed if the stimuli were arranged in a way that made the boundary between target and flanker locations ambiguous (i.e. if the stimuli moved randomly on each trial). In contrast, if the possible target locations were clearly demarked on each trial, flankers did not cause interference in either high- or low-load.

The degree to which the size of the window is under proactive control of the participant remains unclear. The inter-trial contingencies observed by Theeuwes et al. (2004) and others may simply be due to passive persistence of the attentional set from one trial to the next. Alternatively, participants could have some control over the size of the attentional window they employ for the upcoming task. Indeed, in the original formulation of the attentional window account (Belopolsky et al., 2007; Theeuwes, 1994) the size of the window was assumed to be determined by top-down control. Studies relying on the persistence of an attentional set from the preceding trial (i.e. implicit cueing of the upcoming load condition), or in which high- and low-load trials are presented in blocks, cannot inform us about possible proactive control strategies. The relative contributions of proactive control, stimulus-driven capture and selection history to the allocation of visual attention has been a topic of considerable recent debate (Lamy, 2021; Luck et al., 2021; Theeuwes, 2019).

To our knowledge, only one study has tested the effects of explicit cueing of perceptual load. Sy et al. (2014) used the same visual search task with peripheral flankers as described previously, but also included a pre-cue immediately before each trial to indicate the upcoming level of perceptual load that the participant should expect. When the cues accurately predicted the upcoming load condition (on 84% of trials) no flanker interference was observed, regardless of load. In contrast, when the cues were invalid, the typical pattern of results was observed – significant flanker interference for low-load (cued as high-load) but not for high-load (cued as low-load). These results are difficult to align with any existing account (PLT, dilution or attentional window) but do suggest that participants either cannot or choose not to limit the size of their attentional window based purely on the anticipated level of perceptual load.

Here, we therefore sought to investigate the role of proactive control in the effects of perceptual load. We presented participants with a visual-search-plus-flanker paradigm typical to the PLT literature, but before each trial we presented an explicit cue indicating what level of load to expect in the upcoming trial (low or high). In order to have a strong test of the effects of expectancy, independent of any stimulus-driven effects, we also included a minority of trials with 'intermediate' perceptual load. On these trials, participants still saw a cue, suggesting that the upcoming trial would be either low- or high-load, but the actual display was identical in the two conditions, ruling out any actual effect of load or of dilution per se.

This represents a novel approach to investigating PLT, allowing us to test for the first time the role of participants' expectations on the effects of load in a pure fashion. While previous studies used 'intermediate' load conditions to characterise the threshold between high- and low-load (Lavie & Cox, 1997; Maylor & Lavie, 1998; Roper et al., 2013), such conditions have not been used to test the effects of expected load. Thus, our inclusion of both cues and intermediate-load trials can provide insights into the role of proactive control in the size of the attentional window, independent of any stimulus-driven effects. If the ability to exclude flankers depends on the perceptual load (or dilution) of the display, then it should not matter which cue preceded an intermediate-load trial. In other words, we expect similar levels of flanker interference for invalidly cued, intermediate load trials (with either a high- or low-load cue). However, for validly cued trials, we expect the typical effects of load. On the other hand, if load effects depend on proactive control, then the flanker effect in intermediate-load trials should depend on the pre-cues. In this case, we would expect trials cued as 'high-load' to exhibit a reduced flanker effect relative to trials cued as 'low-load'. Finally, if the inter-trial contingencies observed in prior studies depend on a more passive persistence of the attentional set adopted during the preceding trial, then we should expect to see no effect of the cues, but rather that flanker interference depends on the load of the preceding trial.

Importantly, the results of Theeuwes et al. (2004) demonstrated that the flanker effect was still present for low-load trials preceded by high-load trials, indicating that the implicit cueing of high-load is not sufficient, but is necessary, for load effects to emerge. This could suggest that a wide attentional window is the default (or preferred) attentional strategy, while adopting and maintaining a narrower window requires additional cognitive effort and is therefore only maintained from one trial to the next if the participant anticipates that the subsequent trial will also involve high-load. In this case, the above-mentioned factors should interact such that the attentional window of the preceding trial is maintained into the subsequent trial, but not when the participant explicitly expects low-load. Alternatively, if high perceptual load on the current trial is indeed a necessary component for load effects to occur (Theeuwes et al., 2004 and in the original PLT), then we should only observe them in validly-cued trials.

As a further test of the degree to which load effects are subject to proactive control, in a second, preregistered experiment we added a reward manipulation, which is known to increase attention and effort allocation to the task, including flanker paradigms (Hübner & Schlösser, 2010; Marini et al., 2015; Walsh et al., 2021). On every trial, in addition to the upcoming perceptual load, the pre-cue indicated whether a relatively large or small cash bonus could be earned on that trial. Cognitive effort is generally assumed to be aversive, and a large body of evidence shows that participants allocate effort in proportion to the available reward (see Shenhav et al., 2013; Westbrook & Braver, 2015 for review). Indeed, previous research has shown that participants can adopt 'distractor filtering' strategies in flanker paradigms, but only do so when they can earn a reward. For example, Marini et al. (2015) found that rewards led to strategic slowing of responses when flankers were expected but not present, but this did not actually reduce the flanker effect on trials where flankers were present. Thus, if adopting and maintaining a narrowed attentional window is effortful then this may only occur when a sufficient reward is available, regardless of the level of perceptual load.

Finally, in a third experiment, we manipulated reward in a blocked design in order to maximise the power of the manipulation while keeping the same cue and trial design as in our initial experiment. Previous experiments have demonstrated that not only the immediate effect of cued reward and load but also the contextual effect of these manipulations can have an important effect on the allocation of cognitive control and attention (e.g. Marini et al., 2013, 2015). Thus, if the effects of anticipated or experienced perceptual load depend on motivation, this should be revealed in our high-reward blocks.

Experiment 1

Methods

Transparency and openness

The data and analysis code for all analyses in this manuscript are publicly available on the Open Science Framework (OSF) at (https://osf.io/xyn6m/). We report all measures, demographics, exclusion criteria and how we determined our sample size in the method section for each experiment.

Participants

100 participants were recruited from Prolific.co to participate in the experiment. Criteria for signing up were that the participant was aged between 18 and 50 years, spoke fluent English and had normal or corrected-to-normal vision. In order to participate in the main experiment, participants were required to provide informed consent and complete two practice sessions (see procedure for details). Participants were reimbursed with £6 for their time. Thirteen participants who failed to achieve 60% accuracy in one or more conditions of the main experimental task were replaced.

Power considerations were based on the effect size of $\eta_p^2 = 0.1$ reported by Biggs & Gibson (2010) for the 3-way interaction of flanker congruence, load and previous-trial load in their experiment. In order to replicate an effect of similar magnitude, a power analysis conducted with Morepower 6.0 (Campbell & Thompson, 2012) suggested that a sample of n = 74 would be sufficient to achieve 80% power. We tested a sample of n = 100, which should provide approximately 91% power to observe a 3-way interaction effect of similar magnitude.

The average age of the final sample was 24.56 years (*SD* = 3.32); 61 participants identified as female, 39 as male. The protocol was approved by the ethical committee of the faculty of psychology and educational sciences at Ghent University.

Stimuli

Participants performed a visual search task with pre-cues indicating the level of perceptual load to expect on each trial. The task was programmed in JavaScript using JsPsych version 6.2.0 (De Leeuw, 2015) and can be downloaded from the Open Science Framework (OSF) repository for the study at https://osf.io/xyn6m/. The experiment was conducted online, as such exact sizes and refresh rates of the stimuli depended upon the participants' home computers, further, the exact time of day and geographic location were not controlled (although these factors are unlikely to affect the results of the experiment). On each trial, participants were presented with a central cue stimulus for 1,000 ms which indicated the level of perceptual load to expect on that trial. The cues were based on those used by Vassena et al. (2019) and consisted of a black circular outline with a horizontal bar across the top or bottom part of the circle (see Figure 1a). The cue indicated the level of perceptual load in the upcoming display with ~67% validity (either high-load or low-load, see below for details). On one third of trials an intermediate-load display was presented instead.

After a one-second blank screen participants were then presented with a circular array of letters centered on fixation with a diameter 1/8th the width of the participants' screen, with an additional flanker letter to either side (with a horizontal offset from fixation of 1.3 times the radius of the visual search array; Figure 1b). The circular array always contained one target letter (either a capital Z or N) along with five other letters. In high-load displays the five non-targets were all unique angular capital letters (from the list: K, Y, M, W, V and X). In low-load displays, the five non-target letters were all capital O's. On intermediate-load trials the circular array instead contained the target plus two unique angular letters and three capital O's (see Figure 1).

To either side of the array a matching flanker was presented which could either be incongruent or neutral with regard to the target letter circle (i.e. either the opposite target or a random angular nontarget). This display remained on screen for 100 ms, followed by a blank screen for 1,500 ms, during which time participants could respond by pressing the keyboard key corresponding to the target letter (Z or N) in the display. Participants were instructed to respond as quickly as possible while maintaining accuracy. A feedback display was then presented for 500 ms, indicating if the response was correct or not.

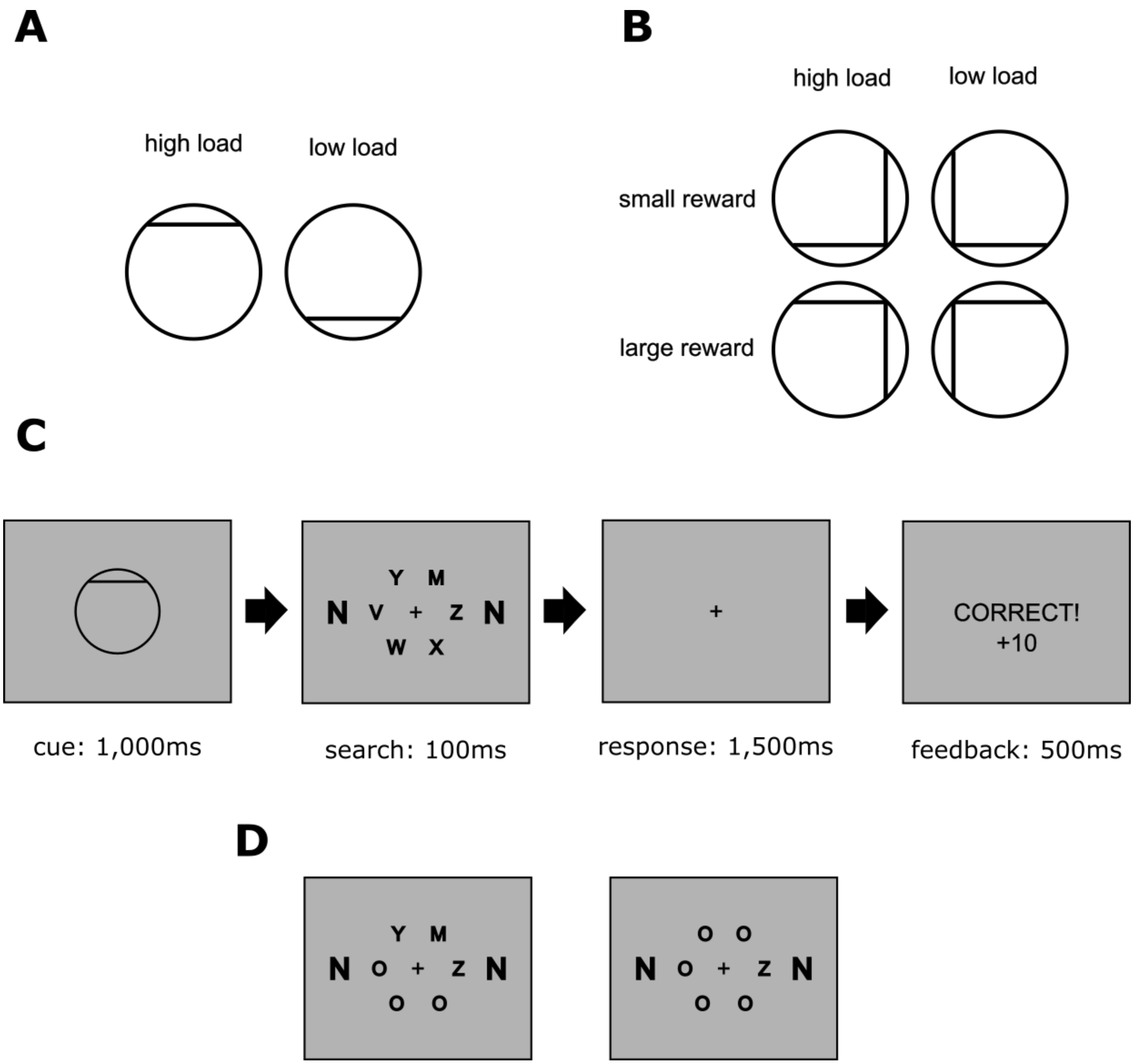
Procedure

Upon clicking the link for the study on the Prolific website, participants were redirected to the Ghent University server where the task was hosted. They first saw an informed consent form and clicked to indicate that they agreed. They then read a series of instructions about the task before completing a familiarization phase in which they learned the meaning of the cue stimuli. To proceed to the next phase of the task the participants had to achieve 80% accuracy in this cue familiarization phase. They then practiced the main task for at least 32 trials or until they achieved 80% accuracy in a block of 32 trials before proceeding to the main task (note: practice blocks did not include intermediate-load trials and so the load cues were 100% valid). Most participants proceeded to the main task after between 2 and 3 blocks of 32 trials (mean = 2.40 blocks, SD = 1.98). In the main task, participants completed six blocks of 48 trials with a self-paced break in between blocks. During the break they saw feedback for their overall accuracy in that block.

After finishing the experiment, participants were asked to complete two questionnaires: the need for cognition scale (Cacioppo et al., 1983) and the behavioural inhibition/behavioural approach systems (BIS/BAS) (Carver & White, 1994). These questionnaires were included for potential use in future individual-differences studies combining these and other experiments and are not analysed or discussed further here.

Figure 1.

Experimental stimuli for all three experiments



Note. Cue stimuli for Experiment 1 and 3 (A) and Experiment 2 (B). Trial Procedure for all three Experiments (featuring a high-load cue for Experiment 1 or 3; a high-load, incongruent flanker search stimulus and a high-reward feedback screen for Experiment 2 or 3) (C). Example search displays for intermediate and low-load search displays both featuring incongruent flanker conditions (D, left and right respectively).

Results

Intermediate-load trials

We first analysed the invalidly cued, intermediate-load trials in which participants were cued to expect either a high or a low-load search but instead were presented with an intermediate-load display. The search displays following the cues were therefore of equivalent perceptual load and any difference between cued-high-load and cued-low-load displays can only be attributed to proactive expectations of the participant. All analyses were conducted in R (version 4.3.0; R Core Team, 2023) and JASP (version 0.17.2.1; JASP Team, 2023), Bayes factors, where reported, were computed with JASP using default priors and a two-tailed distribution (a Cauchy distribution with r = 1/√2) (Van Doorn et al., 2021).

Response times

Mean response times were analysed via a repeated-measures ANOVA (rANOVA) with three within-subjects factors: Cued perceptual load (low/high), flanker condition (incongruent/neutral) and previous-trial perceptual load (low/high). Trials with response times faster than 200 ms or slower than the condition-wise mean plus 2.5 standard deviations were excluded, as were trials with incorrect responses or trials preceded by incorrect responses (in order to rule out post-error slowing effects). Finally, the first trial of each block was excluded as there is no ‘previous-trial load’ in this case.

The main flanker effect was significant $F(1,99) = 15.27$, $p < .001$, $\eta_p^2 = 0.13$ but the main effect of cued load was not, $F(1,99) = 1.83$, $p = .179$, $\eta_p^2 = 0.02$ and neither was the main effect of previous-trial load, $F(1,99) = 0.02$, $p = .876$, $\eta_p^2 < 0.01$. No interaction effects were significant either (all $F < 2.64$, all $p > .1$).

Accuracy

An equivalent rANOVA on average accuracy in intermediate-load trials revealed a significant flanker effect, $F(1,99) = 20.48$, $p < .001$, $\eta_p^2 = 0.17$ reflecting lower accuracy in trials with incongruent flankers. The main effect of cued load was not significant, $F(1,99) = 0.11$, $p = .739$, $\eta_p^2 < .01$ and neither was the main effect of previous-trial load, $F(1,99) = 0.10$, $p = .755$, $\eta_p^2 < .01$.

The interaction between cued load and flanker was not significant, $F(1,99) = 2.78$, $p = .099$, $\eta_p^2 = 0.03$ but the three-way interaction between flanker, load and previous-trial load was, $F(1,99) = 5.69$, $p = .019$, $\eta_p^2 = 0.05$. All other interactions were non-significant (all $F < 1.8$, all $p > .1$).

Pairwise comparisons (Table 1, Figure 2) showed that the flanker effect was significant in all combinations of load and previous-trial load but not for trials cued as high-load which were preceded by high-load trials. For the latter case, the Bayes factor provides moderate evidence in favour of the null hypothesis (no difference between flanker conditions).

Table 1

Descriptive and pairwise comparisons for intermediate-load trials in Experiment 1.

cued load	previous load	flanker	Mean RT (sd)	Mean accuracy (sd)	comparison (accuracy)
low	low	neutral	684 (90)	92 (9)	$t(99) = 2.46$, $p = .016$, $d = 0.25$, $BF_{10} = 1.94$
low	low	incongruent	702 (110)	88 (13)	
low	high	neutral	692 (110)	92 (10)	$t(99) = 3.50$, $p < .001$, $d = 0.35$, $BF_{10} = 30.91$
low	high	incongruent	704 (105)	87 (13)	
high	low	neutral	679 (108)	92 (10)	$t(99) = 2.94$, $p = .004$, $d = 0.29$, $BF_{10} = 6.21$
high	low	incongruent	707 (112)	88 (14)	
high	high	neutral	680 (109)	89 (10)	$t(99) = -0.54$, $p = .592$, $d = -0.54$, $BF_{10} = 0.13$
high	high	incongruent	693 (103)	90 (11)	

Note. Mean response time (RT, in ms) and accuracy for each cued-load, previous-trial load and flanker condition (standard deviation in parentheses). The comparison column represents the flanker effect for each combination of cued-load and previous trial -load for intermediate-load trials in Experiment 1 (only for accuracy).

Validly cued (high- and low-load) trials

Next, we analysed trials in which the cue correctly indicated whether or not the upcoming display would entail a high- or a low-perceptual load display. Any effects of the cues in these trials are therefore impossible to differentiate from the actual effects of perceptual load.

Response times

As previously, response times for validly-cued trials were analysed with an rANOVA including three within-subjects factors, each with two levels: Perceptual load (low/high), flanker condition (incongruent/neutral) and previous-trial load (low/high). This analysis revealed a significant main effect of load $F(1,99) = 762.03$, $p < .001$, $\eta_p^2 = 0.88$, with slower responses for high-load relative to low-load trials. There was also a significant main flanker effect, $F(1,99) = 42.16$, $p < .001$, $\eta_p^2 = 0.30$, with slower responses for incongruent flankers. The main effect for previous-trial load was not significant, $F(1,99) = 0.\text{-}09$, $p = .764$, $\eta_p^2 < 0.01$ and neither was the interaction between flanker and load, $F(1,99) = 0.40$, $p = .530$, $\eta_p^2 < 0.01$.The interaction between flanker and previous-trial load was significant, $F(1,99) = 8.52$, $p = .004$, $\eta_p^2 = 0.08$, suggesting a smaller flanker effect after high-load trials. The interaction between load and previous-trial load was also significant, $F(1,99) = 9.04$, $p = .003$, $\eta_p^2 = 0.08$. Most important, the three-way interaction between load, flanker and previous-trial load was significant, $F(1,99) = 15.07$, $p < .001$, $\eta_p^2 = 0.13$.

Pairwise comparisons (Table 2) testing for the flanker effect for each combination of load and previous-trial load revealed significant flanker effects for all conditions except for high-load trials which were also preceded by high-load trials. For the latter case, the Bayes factor provided moderate evidence in favour of the null hypothesis.

Accuracy

An equivalent rANOVA on average accuracy for validly-cued trials revealed a significant main effect for load $F(1,99) = 222.62$, $p < .001$, $\eta_p^2 = 0.69$, with lower accuracy observed on high-load trials and a significant main effect for flanker, $F(1,99) = 12.77$, $p < .001$, $\eta_p^2 = 0.11$, with lower accuracy on trials with incongruent flankers. The main effect for previous-trial load was not significant, $F(1,99) = 2.09$, $p = .151$, $\eta_p^2 = 0.02$. The three-way interaction between load, previous-trial load and flanker was marginally significant, $F(1,99) = 3.94$, $p = .05$, $\eta_p^2 = 0.04$. No other effects were significant (all $F < 0.7$, all $p > .4$).

Pairwise comparisons for the flanker effect at each level of load and previous-trial load demonstrated a significant flanker interference effect for all conditions except for high-load preceded by high-load trials (Table 2). Once again, in the latter case the Bayes' factor provided moderate evidence in favour of the null hypothesis (no difference between flanker conditions).

Table 2

Descriptive and pairwise comparisons for validly cued trials in Experiment 1.

load	previous load	flanker	Mean RT (sd)	comparison (RT)	Mean accuracy (sd)	comparison (accuracy)
low	low	neutral	570 (77)	$t(99) = 5.68$, $p < .001$, $d = 0.57$, $BF_{10} > 1000$	94 (6)	$t(99) = 2.30$, $p = 0.24$, $d = 0.23$, $BF_{10} = 1.35$
low	low	incongruent	587 (83)		91 (7)	
low	high	neutral	576 (79)	$t(99) = 6.78$, $p < .001$, $d = 0.68$, $BF_{10} > 1000$	95 (6)	$t(99) = 4.03$, $p < .001$, $d = 0.40$, $BF_{10} = 168.07$
low	high	incongruent	598 (79)		92 (7)	
high	low	neutral	736 (96)	$t(99) = 5.43$, $p < .001$, $d = 0.54$, $BF_{10} > 1000$	82 (11)	$t(99) = 2.15$, $p = 0.03$, $d = 0.22$, $BF_{10} = 1.01$
high	low	incongruent	771 (114)		79 (114)	
high	high	neutral	748 (107)	$t(99) = 0.34$, $p = .737$, $d = 0.03$, $BF_{10} = 0.12$	82 (11)	$t(99) = 0.50$, $p = .621$, $d = 0.05$, $BF_{10} = 0.13$
high	high	incongruent	745 (106)		81 (12)	

Note. Mean response times (RT, in ms) and accuracy with standard deviation in parentheses. Pairwise comparisons are presented for the flanker effect at each level of load and previous-trial load in validly-cued trials in Experiment 1 for both accuracy and response times as both interactions were significant in the rANOVA.

Discussion

The results of Experiment 1 replicated the inter-trial contingency effects observed by Theeuwes et al. (2004) and Biggs and Gibson, (2010; 2018). For validly-cued trials, the flanker effect was eliminated for high-load trials preceded by high-load trials but was still observed for all other trial types. This is in contradiction to the results of Sy et al. (2014), in that the valid cues did not eliminate the flanker effects entirely, although this may simply be due to a difference in statistical power (see General Discussion). Similarly, for intermediate-load trials (invalidly-cued as either high- or low-load), the same pattern was observed, but only for response accuracy; whereas the effects of load and the inter-trial contingencies observed by Theeuwes et al., (2004) and Biggs and Gibson (2010; 2018) were observed in response times. Regardless, this represents a novel finding within the PLT literature, as it is the first time that differential load effects have been observed to depend purely on expectations and not on differences in stimuli (i.e. our intermediate-load trials were identical between the cued-high-load and cued-low-load conditions).

The absence of a main effect for cued-load speaks against the notion that proactive control based on cues would eliminate the flanker effect. Instead, we only observed a reduced flanker effect with a combination of explicit expectations and actual load experienced on the preceding trial. Importantly, the results replicated earlier effects of inter-trial contingencies despite the sequence of events being broken up by feedback and cue stimuli, indicating that the core effects in this set-up are preserved. In Experiment 2 we sought to further investigate the role of proactive control by including a ‘reward’ factor, wherein either a relatively small or large bonus was available for fast and accurate performance on each trial. This time, the pre-cue preceding each trial included information both about the likely upcoming perceptual load as well as the level of reward available (large or small). If it is possible to adopt a flanker-excluding attentional setting proactively (based on expected load alone), but this depends upon motivation (e.g. Marini et al., 2015; Schevernels et al., 2014; Shenhav et al., 2013), then this might be observed in the 'large-reward' condition. On the other hand, the effects of reward motivation on perceptual load are not well studied in general, so it is possible that reward itself may be sufficient to modulate load effects regardless of cue validity.

Experiment 2

Method

Preregistration

The experiment design, hypotheses and power analyses for Experiment 2 were pre-registered on OSF prior to data collection for either Experiment 1 or Experiment 2. As such the hypotheses and power analyses reported below are based on prior literature rather than on the results reported in Experiment 1. The pre-registration also indicates that analyses would be conducted via mixed-effects analyses, however due to the complexity of these models when factors for flanker, load, previous-trial load and reward are all included, these models did not converge satisfactorily. As such, analyses were conducted using repeated-measures ANOVA instead (as in Experiment 1). The data, analysis code and experimental materials are all available on OSF at https://osf.io/whdx2.

Participants

For Experiment 2 we collected a new sample of n = 100 participants. Participants provided informed consent before participating and were reimbursed with £6 for their time, plus up to £3 bonus depending on their performance. The sample size was based on simulations conducted using the superpower package in R (version 0.2.0; Lakens & Caldwell, 2021)) using the means reported in Theeuwes et al. (2004) and approximate hypothetical reward effects of the magnitudes observed by Walsh et al. (2020) in a flanker-type task which did find significant reward-flanker interactions. Note, this pre-registration and accompanying power analysis were completed prior to data collection for either Experiment 1 or Experiment 2, and as such did not make use of the effects observed there. For completeness, based on the effect sizes reported in Experiment 1, a similar power analysis suggests that a sample of n = 100 would provide approximately 97% power for the 3-way interaction between load, previous trial load and flankers in validly-cued trials ($\eta_p^2 = 0.13$). Once again, participants were replaced if they failed to achieve at least 60% accuracy in all conditions of the main experimental task, this led to replacement of 20 participants.

Stimuli and procedure

The task was identical to the one used in Experiment 1 with the exception that cues now indicated both the perceptual load and reward condition on each trial. Cues (Figure 1b) were similar to those used by Vassena et al. (2019), depicting a circle outline containing two line segments, one vertical and one horizontal. The mapping between vertical and horizontal lines was counterbalanced between participants such that for a given participant one of the lines always indicated the reward condition and the other indicated the load condition. On large-reward trials participants could earn 10 points and on small-reward trials they could earn 1 point, these points were then converted to bonus cash payment at the end of the experiment. The maximum possible bonus was £3 and participants were informed of this prior to starting the experiment and were given feedback at the end of each block how much bonus reward they had earned. As in Experiment 1, perceptual load cues in each cued-load condition were 66% valid, with the other 1/3rd of trials entailing intermediate-load. The reward cues were always 100% valid and predictive of the reward for that trial, participants were informed that they would receive the proffered reward on each trial provided their response was both accurate and within the 1.5 s time-limit. After completing a cue-familiarisation task and practice task as in Experiment 1, participants then performed eight experimental blocks of 48 trials. As in Experiment 1, participants were required to achieve 80% accuracy in at least one block of 32 trials in the practice phase before continuing. Most participants required between two and three blocks to proceed (mean = 2.38, SD = 1.75).

Results

Intermediate-load trials

As in Experiment 1, we first analysed intermediate-load trials, in which the cues preceding the search stimulus indicated either high- or low-load but the actual stimulus entailed an intermediate level of load. Thus, any effect of ‘cued load’ can only be attributed to proactive control based on the expectation of the participants. In contrast, reward cues were 100% valid in all trial types.

Response times

A repeated-measures ANOVA was conducted on response times for intermediate-load trials; this was identical to that reported in Experiment 1 but with the additional factor of reward. Thus the rANOVA had four factors, each with two levels: perceptual load (low/high), reward (low/high), flanker condition (incongruent/congruent) and the perceptual load of the preceding trial (low/high). The main effect of flanker was significant, $F(1,97) = 27.33$, $p < .001$, $\eta_p^2 = 0.22$, with slower responses to trials with incongruent flankers. All other main effects were non-significant, including the main effect of reward, $F(1,97) = 0.92$, $p = .34$, $\eta_p^2 < 0.01$. The only significant interaction was the three-way interaction between cued-load, flanker and previous-trial load, $F(1,97) = 4.46$, $p = .037$, $\eta_p^2 = 0.04$ (all other $F < 1.6$, all $p > .2$).

Pairwise comparisons confirmed that flanker effect was significant for all cued-load and previous-trial load conditions except for trials which were cued as high-load and were preceded by high-load trials (Table 3).

Accuracy

The same ANOVA conducted on accuracy in intermediate-load trials revealed only a significant flanker effect, $F(1,97) = 21.50$, $p < .001$, $\eta_p^2 = 0.18$, with lower accuracy on trials with incongruent flankers. All other effects were non-significant (all $F < 2.6$, all $p > .1$).

Table 3

Descriptive and pairwise comparisons for intermediate-load trials in Experiment 2.

reward	cued load	previous load	flanker	Mean RT (sd)	comparison (RT)	Mean accuracy (sd)	comparison (accuracy)
large	low	low	neutral	637 (92)	t(97) = -2.15, p = 0.034, d = -0.22, BF_{10} = 1.01	92 (10)	t(97) = 1.81, p = 0.074, d = 0.18, BF_{10} = 0.53
large	low	low	incongruent	653 (87)		88.8 (14.4)	
small	low	low	neutral	642 (92)		90.7 (12.5)	
small	low	low	incongruent	656 (99)		89.6 (12.2)	
large	low	high	neutral	639 (90)	t(97) = -4.37, p = < .001, d = -0.44, BF_{10} = 551.49	93.8 (9.8)	t(97) = 3.12, p = 0.002, d = 0.32, BF_{10} = 10.17
large	low	high	incongruent	663 (95)		89.8 (12.1)	
small	low	high	neutral	637 (88)		92.8 (10.3)	
small	low	high	incongruent	669 (104)		90.4 (10.5)	
large	high	low	neutral	635 (92)	t(97) = -3.28, p < 0.001, d = -0.33, BF_{10} = 16.12	92.2 (12)	t(97) = 2.49, p = 0.015, d = 0.25, BF_{10} = 2.07
large	high	low	incongruent	663 (97)		89.4 (14.3)	
small	high	low	neutral	649 (99)		92.6 (9.8)	
small	high	low	incongruent	663 (86)		89.4 (12.8)	
large	high	high	neutral	647 (92)	t(97) = -0.73, p = 0.468, d = -0.07, BF_{10} = 0.15	93.3 (10.8)	t(97) = 1.31, p = 0.192, d = 0.13, BF_{10} = 0.26
large	high	high	incongruent	652 (83)		90.8 (12.2)	
small	high	high	neutral	646 (95)		90.5 (12.4)	
small	high	high	incongruent	651 (102)		90.4 (12.6)	

Note. Mean response time (RT, in ms) and accuracy for each cued-load, previous-trial load and flanker condition (standard deviation in parentheses). The comparison column represents the flanker effect for each combination of cued-load and previous trial -load for intermediate-load trials in Experiment 2 for RT and accuracy.

Validly cued (high- and low-load) trials

Next, we analysed validly cued trials, in which cues correctly predicted the level of perceptual load in the following search stimulus on each trial.

Response times

Response times for validly cued trials were similarly entered into a repeated measures ANOVA with four factors (reward, perceptual load, previous trial perceptual load and flanker condition).

The main effect of load was significant, $F(1,97) = 1095.32$, $p <.001$, $\eta_p^2 = 0.92$, with faster response times for low-load trials. There was also a significant flanker effect, $F(1,97) = 20.61$, $p = .001$, $\eta_p^2 = 0.17$, with slower responses to trials with incongruent flankers. The main effect of reward was not significant, $F(1, 97) = 2.40$, $p = .125$, $\eta_p^2 = .02$ and neither was the main effect of previous-trial perceptual load, $F(1,97) = 0.47$, $p = .829$, $\eta_p^2 < 0.01$.

The interaction between load and flanker was significant, $F(1,97) = 11.66$, $p < .001$, $\eta_p^2 = 0.11$, replicating the typical pattern from PLT literature (the flanker effect was reduced for high-load trials). The interaction between flanker, load and previous-trial load was not significant, $F(1,97) = 1.70$, $p = .195$, $\eta_p^2 < .02$. This may suggest that the more complex cue between the preceding trial and the current trial eliminates the impact of the preceding trial. No other interactions were significant, including any interaction with previous-trial perceptual load (all $F < 2.6$, all $p > .1$).

Accuracy

An equivalent rANOVA was conducted on accuracy for validly cued trials. Once again, the main effect of load was significant, $F(1,97) = 221.89$, $p < .001$, $\eta_p^2 = 0.70$, with more accurate responses on low-load trials. The flanker effect was also significant, $F(1,97) = 33.77$, $p < .001$, $\eta_p^2 = 0.26$ with less accurate responses to trials with incongruent flankers. The main effect of reward was not significant, $F(1,97) = 2.63$, $p = .108$, $\eta_p^2 = 0.03$ and neither was the main effect of previous-trial load, $F(1,97) = 0.07$, $p = .79$, $\eta_p^2 < .01$.

The only significant interaction was between reward and load, $F(1,97) = 4.33$, $p = .040$, $\eta_p^2 = .04$. Pairwise comparisons revealed that the effect of reward was significant within the high-load condition (p = .018), with slightly more accurate responses in large reward trials, whereas the difference between small and large reward conditions was not significant in the low-load condition ($p = .46$). All other interactions were non-significant (all $F < 2.81$, all $p > .09$).

Table 4

Descriptive and pairwise comparisons for validly cued trials in Experiment 2.

reward	load	previous load	flanker	Mean RT (sd)	comparison (RT)	Mean accuracy (sd)	comparison (accuracy)
large	low	low	neutral	536 (70)	t(97) = -5.50, p < .001, d = -0.56, BF_{10} > 1000	95 (8)	t(97) = 3.68, p < .001, d = 0.37, BF_{10} = 52.90
large	low	low	incongruent	550 (59)		93 (9)	
small	low	low	neutral	539 (65)		95 (6)	
small	low	low	incongruent	554 (62)		92 (9)	
large	low	high	neutral	527 (64)	t(97) = -9.34, p < .001, d = -0.94, BF_{10} > 1000	95 (7)	t(97) = 3.31, p = 0.001, d = 0.34, BF_{01} = 17.61
large	low	high	incongruent	561 (60)		93 (8)	
small	low	high	neutral	536 (59)		94 (7)	
small	low	high	incongruent	552 (65)		92 (9)	
large	high	low	neutral	724 (97)	t(97) = -0.61, p = 0.547, d = -0.06, BF_{10} = 0.13	83 (12)	t(97) = 2.61, p = 0.011, d = 0.26, BF_{10} = 2.74
large	high	low	incongruent	724 (99)		81 (12)	
small	high	low	neutral	720 (92)		86 (11)	
small	high	low	incongruent	727 (91)		83 (11)	
large	high	high	neutral	714 (96)	t(97) = -0.48, p = 0.635, d = -	84 (12)	t(97) = 3.28, p = 0.001, d =
large	high	high	incongruent	721 (99)		82 (12)	

small	high	high	neutral	729 (98)	0.05, BF_{10} = 0.13	86 (10)	0.33, BF_{10} = 15.99
small	high	high	incongruent	729 (89)		82 (13)	

Note. Mean response time (RT, in ms) and accuracy for each load, previous-trial load and flanker condition (standard deviation in parentheses). The comparison column represents the flanker effect for each combination of cued-load and previous trial load for validly cued trials in Experiment 2 for RT and accuracy.

Discussion

For intermediate-load trials, flanker interference was observed in response times for all conditions except trials cued as high-load which were preceded by high-load trials, replicating the pattern of results in Experiment 1. However, for Experiment 2 this pattern arose in response times instead of accuracy. This is more in-line with previous literature, in which load effects and interactions with preceding trial load are typically strongest in response time.

The reward manipulation had little impact on overall performance however and did not affect flanker interference or interact with either the effects of perceptual load or previous-trial perceptual load on flanker interference. It therefore seems that the attentional set adopted in response to expectations of high-load is not dependent upon the sort of cost-benefit analysis that often underpins allocation of effortful control (Shenhav et al., 2013; Westbrook & Braver, 2015). Alternatively, it is possible that participants simply did not have time or were not motivated to use all of the information available in the cues on every trial. For example, Vassena et al. (2019) found that the same type of two-dimensional (reward and difficulty) cues can lead to participants prioritising one type of information over the other, depending on the task. This could mean that participants attended primarily to the cued perceptual load and only then to the reward information as a secondary consideration (or not at all). However, the fact that accuracy was slightly but significantly higher in validly cued, large-reward trials suggests that participants did attend to the reward condition.

The relatively complex cues could also explain the other key difference between the results of Experiment 1 and Experiment 2 – the effect of previous-trial load in validly-cued trials. Specifically, in Experiment 1 the flanker effect was eliminated only for high-load trials preceded by high-load trials (replicating the pattern observed by Theeuwes et al., (2004) and Biggs & Gibson, (2010). In Experiment 3, we therefore reverted to the same cues as used in Experiment 1 and instead used a blocked manipulation of reward. This should both maximise the power of the reward manipulation while reducing the chance that processing the cues on each trial could interfere with the effects of preceding trial load on the upcoming trial.

Experiment 3

Method

Participants

100 new participants were recruited from Prolific.co to participate in Experiment 3. The average age was 27.57 (*SD* = 6.82), 42 identified as female, 58 as male. Participants provided informed consent before participating and were reimbursed with £6 for their time, plus up to £3 bonus depending on their performance. As in Experiments 1 and 2, participants were replaced if they failed to achieve at least 60% accuracy in one or more conditions of the experimental task, this led to the replacement of 17 datasets.

Based on the effect sizes for reward in Experiment 2 this new sample of 100 would only provide 52% power to detect the effect if present. However, a recent study by Kukkonen et al. (2023) found no effect of reward in trial-by-trial manipulations involving a perceptually demanding task but medium sized effects (w = 0.2) for the interaction of reward and difficulty in a blocked design. Power analysis using MorePower 6.0 (Campbell & Thompson, 2012) suggested our sample should provide > 99% power to detect such an effect in a blocked design.

Stimuli and procedure

The task was the same as that used in Experiment 2 except that high- and low-reward trials were presented in blocks. Block order was counterbalanced in an ABBABAAB pattern, half of the participants starting with a high-reward block and half with a low-reward block. Reward condition was indicated at the start of each block via a text instruction screen. Reward amounts were the same as in Experiment 2: 10 points versus 1 point for high- and low-reward conditions respectively with a maximum possible bonus of £3. As in previous experiments, perceptual load was cued with ~66% reliability using the same cues as in Experiment 1. As previously, practice phase prior to beginning the main experiment and had to achieve at least 80% accuracy on a block of 32 trials before proceeding, the mean number of practice blocks was 2.69 (SD = 1.8).

Results

Intermediate-load trials

Response times

As previously, a four-way rANOVA was conducted on the average response times for invalidly cued (intermediate-load) trials in Experiment 3. This revealed a significant flanker effect, $F(1,99) = 57.06$, $p < .001$, $\eta_p^2 = 0.37$, with slower responses to trials with incongruent flankers, and a significant effect of reward, $F(1,99) = 18.69$, $p < .001$, $\eta_p^2 = 0.16$ with slower responses for low-reward trials. The main effects of cued load and previous-trial load were not significant (all $F < 0.1$, all $p > .7$).

The Interaction between cued-load and flanker condition was significant, $F(1,99) = 17.21$, $p < .001$, $\eta_p^2 = 0.15$ but crucially, so was the three-way interaction between cued load, previous-trial load and flanker, $F(1,99) = 17.23$, $p < .001$, $\eta_p^2 = 0.15$. Pairwise comparisons for the flanker effect in all combinations of load, reward and previous load showed a significant flanker effect of load for all conditions except cued-high-load trials preceded by high-load trials (Table 5). Bayes factors provided moderate evidence in favour of the null hypothesis for cued-high-load trials preceded by high-load and against the null for all other conditions (Table 5). No other effects were significant (all F < 2.4, all p > .1).

Accuracy

Average accuracy for invalidly cued (intermediate-load) trials in Experiment 3 were analysed with an equivalent rANOVA, which revealed significant main effects of flanker, F(1,99) = 28.21, p < .001, $\eta_p^2 = 0.22$, reflecting lower accuracy on trials with incongruent flankers, and reward, *F*(1,99) = 14.41, *p* < .001, $\eta_p^2 = 0.13$, with lower accuracy on low-reward trials. No other effects reached significance (all *F* < 2.6, all *p* > .1).

Table 5

Descriptive and pairwise comparisons for intermediate-load trials in Experiment 3.

reward	cued load	previous load	flanker	Mean RT (sd)	comparison (RT)	Mean accuracy (sd)
large	low	low	neutral	624 (82)	t(99) = -4.58, p < .001, d = -0.46, BF_{10} > 1000	91 (12)
large	low	low	incongruent	650 (94)		90 (12)
small	low	low	neutral	650 (81)		92 (10)
small	low	low	incongruent	672 (85)		87 (16)
large	low	high	neutral	609 (76)	t(99) = -7.43, p < .001, d = -0.74, BF_{10} > 1000	94 (10)
large	low	high	incongruent	653 (88)		90 (12)
small	low	high	neutral	646 (88)		91 (13)
small	low	high	incongruent	677 (90)		87 (15)
large	high	low	neutral	633 (92)	t(99) = -3.80, p < .001, d = -0.38, BF_{10} = 77.87	92 (12)
large	high	low	incongruent	649 (90)		90 (12)
small	high	low	neutral	642 (81)		91 (13)
small	high	low	incongruent	666 (86)		88 (13)
large	high	high	neutral	640 (87)	t(99) = 0.64, p = 0.527, d = 0.06, BF_{10} = 0.14	93 (11)
large	high	high	incongruent	642 (87)		89 (13)
small	high	high	neutral	662 (85)		89 (13)
small	high	high	incongruent	653 (86)		89 (12)

Note. Mean response time (RT, in ms) and accuracy for each reward, cued-load, previous-trial load and flanker condition (standard deviation in parentheses). The comparison column represents the flanker effect for each combination of reward, cued-load and previous trial load for intermediate-load trials in Experiment 3 for RT and accuracy.

Validly cued (high- and low-load) trials

Response times

Average response times for validly cued trials were entered into a repeated measures ANOVA with four within-subjects factors, each with two levels: Perceptual load (low/high), reward (high/low), flanker condition (incongruent/ neutral) and previous-trial perceptual load (low/high).

The main effect for load was significant $F(1,99) = 1740.06$, $p < .001$, $\eta_p^2 = 0.95$, with slower responses for high-load trials as was the main effect for flanker $F(1,99) = 111.04$, $p < .001$, $\eta_p^2 = 0.53$, with slower responses on trials with incongruent flankers. Unlike the previous experiments, the main effect for previous-trial load was also significant, $F(1,99) = 22.56$, $p < .001$, $\eta_p^2 = 0.19$, with slower responses on trials preceded by high-load trials than those preceded by low-load trials. The main effect of reward was also significant, $F(1,99) = 29.56$, $p < .001$, $\eta_p^2 = 0.23$, reflecting faster responses for large-reward trials.

The interaction between load and flanker was significant, $F(1,99) = 14.57$, $p < .001$, $\eta_p^2 = 0.13$ but so was the three-way interaction between load, flanker and previous-trial load, $F(1,99) = 8.61$, $p = .004$, $\eta_p^2 = 0.08$. No other interaction was significant (all $F < 1.33$, all $p > .2$)

Pairwise comparisons for the flanker effect at all combinations of load and previous-trial load showed a significant flanker effect for all conditions except for high-load trials preceded by high-load trials (Table 6). Bayes factors provided moderate evidence in favour of the null hypothesis for hard trials preceded by hard trials and against the null for all other trial types (Table 6).

Accuracy

The same repeated measures ANOVA applied to average accuracy for Experiment 3 revealed a significant main effect for load, $F(1,99) = 44.52$, $p < .001$, $\eta_p^2 = 0.31$, with less accurate responses for high-load trials. The main effect of flanker was also significant, $F(1,99) = 41.72$, $p < .001$, $\eta_p^2 = 0.30$, reflecting less accurate responses on trials with incongruent flankers, as was the main effect for reward, $F(1,99) = 16.05$, $p < .001$, $\eta_p^2 = 0.14$, reflecting less accurate responses on small-reward trials. No other effects were significant (all $F < 3.4$, all $p > .07$).

Table 6

Descriptive and pairwise comparisons for validly cued trials in Experiment 3.

reward	load	previous load	flanker	Mean RT (sd)	comparison (RT)	Mean accuracy (sd)
large	low	low	neutral	523 (68)	$t(99) = -10.74$, $p < .001$, $d = -1.07$, $BF_{10} > 1000$	96 (5)
large	low	low	incongruent	550 (65)		93 (10)
small	low	low	neutral	551 (67)		93 (9)
small	low	low	incongruent	573 (60)		88 (11)
large	low	high	neutral	529 (69)	$t(99) = -9.44$, $p < .001$, $d = -0.94$, $BF_{10} > 1000$	95 (6)
large	low	high	incongruent	559 (72)		90 (10)
small	low	high	neutral	555 (65)		93 (8)
small	low	high	incongruent	579 (67)		90 (11)
large	high	low	neutral	700 (83)	$t(99) = -5.96$, $p < .001$, $d = -0.60$, $BF_{10} > 1000$	90 (10)
large	high	low	incongruent	720 (90)		87 (10)
small	high	low	neutral	718 (85)		88 (9)
small	high	low	incongruent	741 (87)		86 (10)
large	high	high	neutral	718 (94)	$t(99) = 0.16$, $p = 0.873$, $d = 0.02$, $BF_{10} = 0.11$	88 (10)
large	high	high	incongruent	717 (85)		88 (10)
small	high	high	neutral	739 (89)		87 (10)
small	high	high	incongruent	738 (78)		86 (11)

Note. Mean response time (RT, in ms) and accuracy for each reward, cued-load, previous-trial load and flanker condition (standard deviation in parentheses). The comparison column represents the flanker effect for each combination of reward, cued-load and previous trial load for validly cued trials in Experiment 3 for RT only.

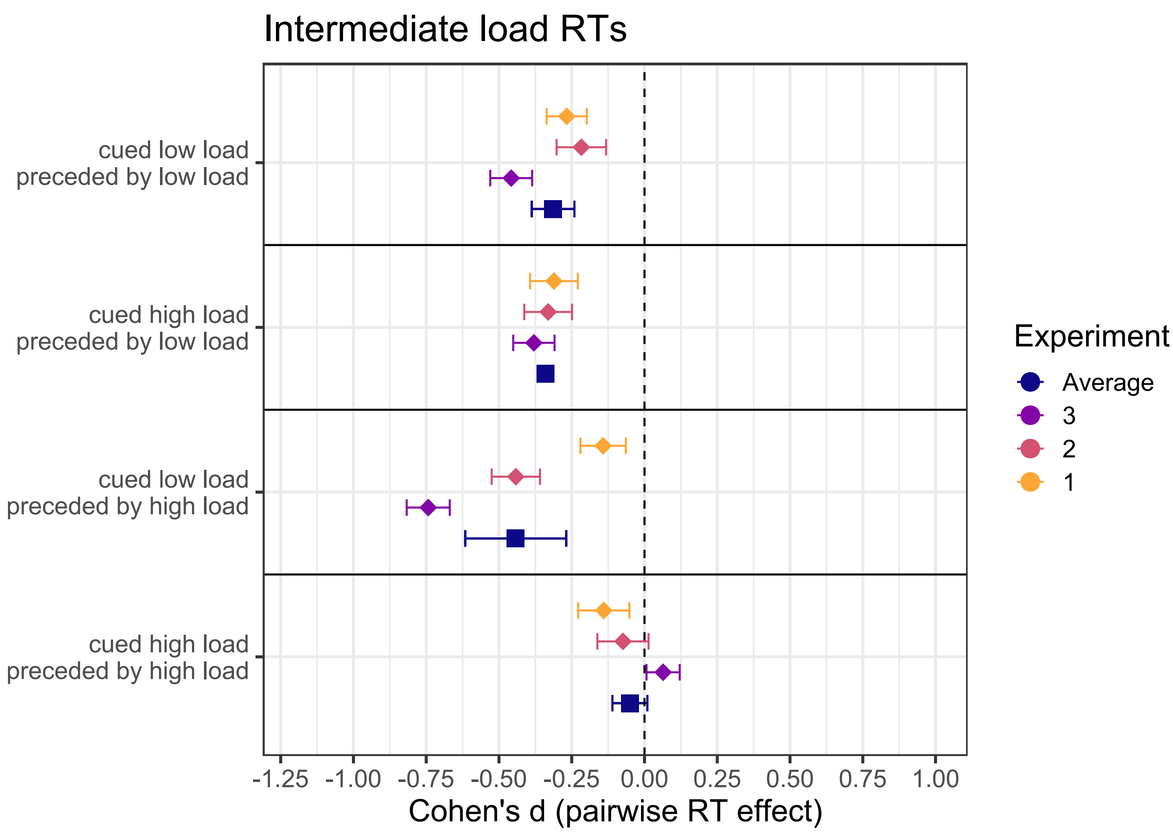
Meta-effects across experiments

In order to concisely summarise the interacting effects of (cued) load and previous-trial load across all three experiments, we visualised these effects and their meta-effect in forest plots. The forest plots present the effect sizes (Cohen's *d*) for each of the pairwise comparisons between flanker conditions (incongruent – neutral response times) for intermediate-load trials (Figure 2) and validly cued trials (Figure 3) respectively. A random-effects meta-analysis fit using the metafor package (Viechtbauer, 2010) for intermediate-load (Figure 2) and validly cued (Figure 3) conditions separately. As can be seen from Figure 2, the average effect size was strongly negative for all combinations of cued load and previous trial load except for high-load preceded by high-load, where the average effect size was near zero (d-estimate = 0.03, p = .837). The meta-analysis of the flanker effect across all conditions was significant, d-estimate = 0.29, p < .001; the Q-statistic indicated significant heterogeneity in the effect between conditions, Q(11) = 99.68, p < .001, I^2 = 87.39%, τ^2 = 0.04. When excluding trials cued as high-load which were preceded by high-load, the flanker effect remained significant, d-estimate = 0.37, p < .001 and the heterogeneity in the effect was no longer significant, Q(8) = 3.21, p = .921, I^2 <1%, τ^2 < 0.01

Similarly, for validly cued trials (Figure 3), the average effect size was strongly negative for all conditions except for high-load trials preceded by high-load trials, for which the average effect was almost exactly zero (d-estimate = 0.0006, p = .997). The meta-analysis over all conditions revealed a significant overall flanker effect, d-estimate = 0.49, p < .001 and the Q-statistic indicated that there was significant heterogeneity in the effect between conditions, Q(11) = 457.20, p < .001, I^2 = 98.12%, τ^2 = 0.15. When excluding high-load preceded by high-load trials, the flanker effect remained significant, d-estimate = 0.67, p < .001 and the heterogeneity of the effect was no longer significant, Q(8) = 12.93, p = .114, I^2 = 36.24%, τ^2 = 0.029 , suggesting that the flanker effect did not vary significantly between the other trial types.

Figure 2

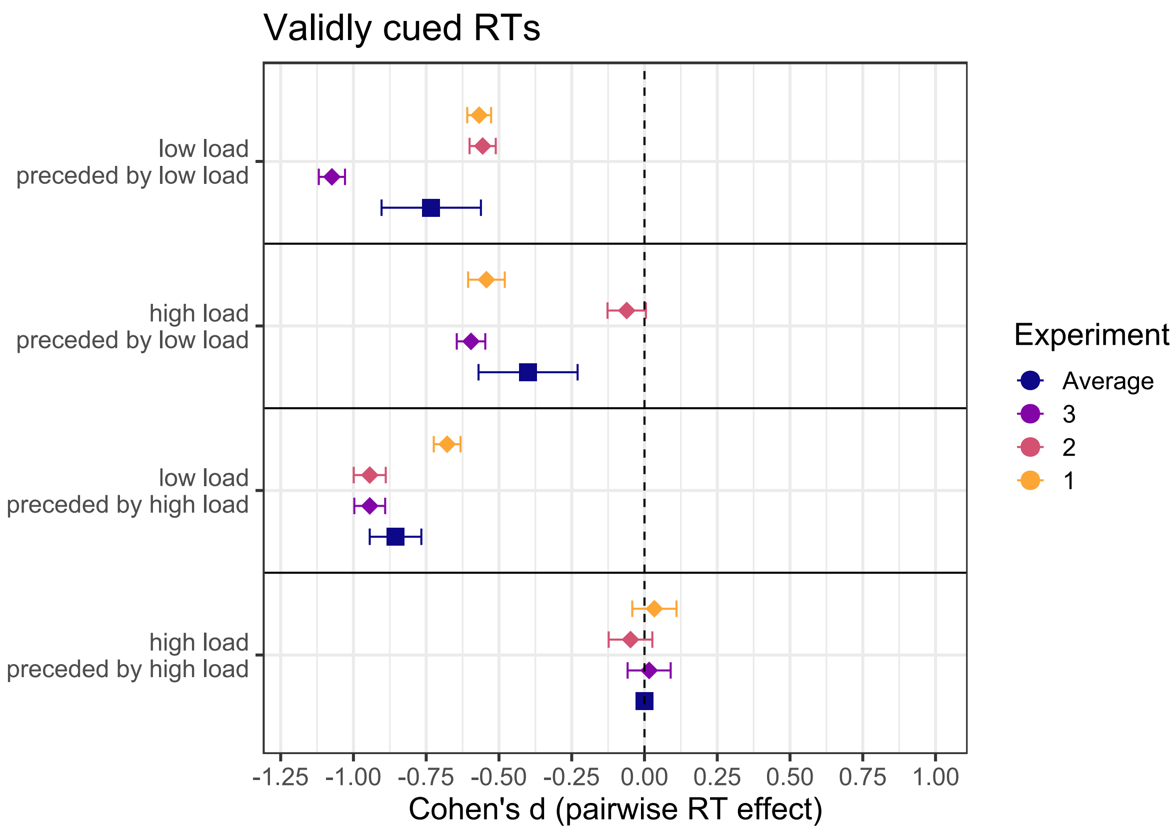
Forest plot of the flanker effect for intermediate-load trials of each condition in all three experiments



Note. Diamonds represent the pairwise effect size (Cohen's d) and standard error of the flanker effect (neutral-incongruent) for each combination of previous-trial and current-trial (cued) load for intermediate-load trials for each of the three experiments. Squares represent the averaged effect across Experiments.

Figure 3

Forest plot of the flanker effect for validly cued (high- and low-load) trials of each condition in all three experiments



Note. Diamonds represent the pairwise effect size (Cohen's d) and standard error of the flanker effect (neutral-incongruent) for each combination of previous-trial and current-trial load for validly cued trials in all three experiments. Squares represent the averaged effect across Experiments.

General discussion

In three experiments, we investigated the role of perceptual load and explicit expectations of load on flanker interference, as well as the potential effects of motivational factors (Experiments 2 and 3). In all three of our experiments, distractor interference depended on both the explicit (cued) expectation of perceptual load as well as the load of the preceding trial. That is, with some exceptions, a significant flanker effect was observed for all trial types except for high-load trials preceded by high-load trials. In Experiment 1 this was true both for trials in which perceptual load was validly cued and intermediate-load trials, which were cued as high or low-load but actually entailed an intermediate level of load. In Experiment 2, which included an additional trial-wise reward manipulation, the same pattern was observed for intermediate-load trials in response times. In contrast, however, this time the flanker effect was eliminated in all validly cued high-load trials, even if they were preceded by a low-load trial. Furthermore, the reward manipulation itself did not interact with flanker effects, only eliciting a small improvement in response accuracy for high-load trials. Finally, in Experiment 3 a blocked reward manipulation was implemented. The results replicated the interactions between current-trial and previous-trial load for both validly cued and intermediate-load trials. However, reward did not modulate flanker interference, but improved performance across all conditions.

Two key aspects of these results are incompatible with the capacity-based account as proposed by PLT (Lavie, 1995, 2005) or with dilution (Benoni et al., 2014; Benoni & Tsal, 2013; Tsal & Benoni, 2010). These models cannot account for the different effects of cues in our intermediate-load trials (for which the stimuli and therefore the perceptual load and dilution were identical), or for the influence of the preceding trial on flanker interference. Instead, our results are more compatible with models such as the attentional window account proposed by Theeuwes et al. (2004; see also Chen & Cave, 2016) which proposes that the effects of perceptual load can be attributed to the attentional set adopted in the preceding trial and maintained after the stimuli are no longer present. Note, however, that it remains possible that perceptual load or dilution could have effects in addition to those of expectation. For example, Benoni et al. (2014) used a similar manipulation to Theeuwes et al. (2004), intermixing high-load, low-load and high dilution displays. While they did not replicate the exact pattern of results reported here or by Theeuwes (2004), they did replicate the observation that perceptual load only eliminated flanker interference in blocked high-load conditions. In blocks of intermixed trials (with any pairwise combination of high-load, low-load and high dilution), they observed no difference in flanker interference between high- and low-load but flanker interference was still eliminated by high dilution. These results therefore indicate that expectation-driven attentional window effects and stimulus-driven dilution effects are not mutually exclusive.

These results therefore extend previous findings by demonstrating that perceptual load is neither sufficient nor necessary to eliminate flanker interference. Prior work established that flanker effects are reduced only for high-load trials which are preceded by high-load trials (Biggs & Gibson, 2010, 2018; Theeuwes et al., 2004). However, given that in these previous studies low-load trials still exhibited significant flanker effects regardless of the preceding trial-type, the authors concluded that both the expectation of high-load as well as high-perceptual load in the current trial were necessary to reduce flanker interference. Our inclusion of intermediate-load trials, however, eliminates the possibility that a high-load display is a necessary component to eliminate flanker interference. In our experiments, intermediate-load displays elicited opposite results depending purely on the cued perceptual load.

This is in contrast to the results of Sy et al. (2014), who used a similar design to ours, including explicit pre-cues to indicate the upcoming load condition, but without the addition of intermediate-load displays. They found that valid cues eliminated flanker effects in both high- and low-load displays, whereas typical load effects were observed only for invalidly cued trials (or those with uninformative cues). However, the effect sizes associated with the null flanker interference effects reported by Sy et al. (2014) were comparable to the (significant) effects in our own data (and those reported by others). They did not report Bayesian analyses, and so the strength of their evidence in favour of the null cannot be ascertained. Further, given the relatively small sample sizes employed by Sy et al. (2014) ($n = 14$ and $n = 13$ in their two experiments) it seems likely that their analyses lacked the necessary statistical power to observe these effects.

Possibly the most intriguing aspect of our results is the fact that for intermediate-load trials, both cued load and previous-trial load interacted to determine flanker interference. This suggests that neither the explicit expectation (from cues) nor the persistence of the attentional set from the preceding trial could independently reduce flanker interference in this task. That is, trials which were cued as high-load but were preceded by low-load still elicited significant flanker interference, possibly suggesting that narrowing the attentional window did not happen on these trials despite the cues. Similarly, trials cued as low-load which were preceded by high-load also did not reduce the flanker effect – suggesting that the previous-trial's attentional window does not necessarily persist into the subsequent trial. Instead, it seems from our results that the flanker-excluding attentional set is only adopted reactively, in response to high-load trials. Once adopted however, this narrowed window can then be volitionally maintained or abandoned depending on the participants' explicit expectations about the upcoming trial. As mentioned in the introduction, Theeuwes et al. (2004) and others who have reported trial-sequential dependencies in load effects found significant flanker interference for low-load trials preceded by high-load trials. It is possible that, in the absence of intermediate-load trials or explicit cues, in these experiments participants simply adopted a strategy of rapidly 'zooming out' upon encountering a low-load display after adopting a narrowed attentional window. Perhaps because doing so is either an easier or faster way to find the target. This being the case, our results would still suggest that participants are either unable or very unwilling to adopt a narrowed focus of attention based solely on cued expectations. One possibility is that the 1.5 second response window and reward context (in Experiments 2 and 3) encouraged participants to use a wider window which may facilitate faster target identification.

Recently, Theeuwes (2023) elaborated on the attentional window account, proposing that it can potentially explain capture of visual attention broadly. Somewhat similarly to the original proposal of PLT, Theeuwes (2023) suggests that the difficulty of a visual search task determines the size of the attentional window. According to this account, harder search tasks, which elicit a serial search strategy, will necessarily involve a narrowed attentional window, while easier tasks support parallel search within a broader window. Theeuwes (2023) further notes that while the original conception of the attentional window account assumed that the size of the window could be determined strategically by top-down control (Belopolsky et al., 2007; Theeuwes, 1994), the existing evidence rather suggests that it could instead be “induced by the search display without much, if any, top-down control” (Theeuwes, 2023). Here, our results are particularly informative, suggesting that both explicit expectations and selection-history effects interactively contribute to the exclusion of flankers from attention.

Theeuwes’ (2023) proposal has been met with some criticism on various fronts, however (e.g. Gaspelin et al., 2023; Lien & Ruthruff, 2023). For example, Gaspelin et al. (2023) point out that in order for an attentional window account to explain diverse capture phenomena, it would first have to be assumed that the window is narrowed prior to the onset of the stimulus in order to already exclude the distractor when it first appears and second, that the stimulus itself does not cause an automatic adoption of a wide attentional window. Finally, attention would have to be guided to the target by feature-based information despite not being affected by distractor information. These criteria are harder to explain in other visual attention paradigms, such as capture by ‘additional singletons’ in a visual search task (Bacon & Egeth, 1994; Folk et al., 1992; Theeuwes, 1991). However for the case of our search-plus-flanker paradigm, these assumptions do seem reasonable – the attentional window is narrowed in response to the preceding stimulus, this is only maintained when participants believe it is necessary (due to cueing of high-load), and search can be guided within the smaller window while excluding the more peripheral distractors.

Some aspects of our stimuli lend themselves particularly to the use of a narrowed attentional window, as we used two flankers to either side of fixation rather than one flanker in an unpredictable location. The presence of two flankers on every trial may have served to increase the potential size of the interference effect as well as providing extra incentive to narrow the attentional window rather than shifting from one side of the screen to another in order to exclude the flankers. The fixed locations of both flankers and search-relevant stimuli have also been shown to lend themselves to a narrowed attentional window (Biggs & Gibson, 2018). Although note that our design meant that both flankers were consistently in the same locations, which has been shown to reduce the potency of the flanker effect relative to unpredictable flanker locations, potentially interacting with perceptual load (Marciano & Yeshurun, 2011; Yeh et al., 2014).

Direct effects of perceptual load on flanker interference cannot account for our results, however, previous research has demonstrated that alternative forms of task load can produce similar effects to those of perceptual load, even after the stimuli are no longer present. Specifically, Konstantinou and colleagues (Konstantinou et al., 2014; Konstantinou & Lavie, 2013) demonstrated that visual working memory load could also reduce flanker interference and increase rates of inattentional blindness for stimuli appearing during the memory delay. It is thus possible that in our paradigm, participants actively maintained the stimuli from the preceding (high load) trial when expecting a new high-load trial, thus loading visual working memory. While this is a possibility, in our paradigm there would be no benefit for the participant in actively remembering the preceding stimuli once a trial is finished (whereas in the experiments of Konstantinou and colleagues memory maintenance was a necessary part of the task). This interpretation therefore seems unlikely to account for our results. In fact it seems possible that a narrowed attentional window could parsimoniously account for the results described by Konstantinou and colleagues, especially given the nature of their stimuli, which involved a small memory array presented in a central area surrounded by a wider visual search array similar to ours.

It should be noted however, that while our results are compatible with the attentional window account (Chen & Cave, 2016; Theeuwes et al., 2004), our experiments were not designed to test the specific mechanism underlying this effect. Rather, our results could be compatible with any flanker-filtering/exclusion mechanism which can be adopted in response to high-load and maintained in the subsequent trial. Further work will be necessary in order to properly test whether the effects observed here can be attributed to a narrowed attentional window (and indeed in the earlier work of Biggs & Gibson, 2010 and Theeuwes et al., 2004). For example, Chen and Cave (2016) demonstrated that high perceptual load only caused a reduction in the flanker effect when flanker stimuli were presented to the periphery of the display and not in between two attended locations (thus a narrowed attentional window could not encompass them while excluding the flanker). However, the experiments reported by Chen and Cave (2016) necessitated a blocked manipulation of load and cannot therefore inform as to the inter-trial contingencies or cueing effects observed here.

In Experiment 1, the 3-way interaction between flanker, load and previous-trial load was only observed in accuracy and not in response time. In contrast, for the other experiments, and in the broader literature, load effects are commonly observed in response time. It is unclear from the present results exactly what caused this discrepancy, as the instructions to participants were always to respond as quickly and as accurately as possible, and the trial structure and timing was the same in all three experiments. It is possible that the combination of the online setting and the absence of a ‘reward context’ informed the way in which participants approached the task. Relatedly, it is also possible that the additional reward-related points-feedback on each trial in Experiments 2 and 3 provided additional sustained motivation to respond quickly. Regardless though of whether the effects in our experiments were observed in accuracy or in response times, the effect was always for reduced flanker interference for high-load trials preceded by high-load trials.

It is also interesting that reward had no impact upon flanker interference effects or any higher-order interactions involving the flankers. In Experiment 2, reward effects were largely absent except for a small effect of reward on response accuracy for high-load trials. This could simply be due to the fact that the two-dimensional cues led participants to prioritise the potentially more useful difficulty information. Indeed, Vassena et al. (2019) demonstrated, using identical cues to ours, that participants tend to prioritise either the reward or difficulty information depending on the nature of the task. Given the small effect sizes associated with reward in our experiments in general, it could also be the case that more subtle effects exist which we lack the statistical power to detect. Perhaps because reward sensitivity varies considerably between individuals (Carver & White, 1994).

Relatedly, we recently found that the effects of cued reward can depend upon the prediction horizon to which the cues pertain (Kukkonen et al. 2023). In a random-dot kinematogram (RDK) paradigm, participants were cued to expect either high- or low-reward and difficulty (as here). In a first experiment, where cues were presented before each trial, reward expectations had little effect upon performance. But in a second experiment, when cues predicted the reward condition for the next six trials, high-reward was associated with significant improvements to performance (but only in the low-difficulty condition, suggesting a strategic allocation of resources in contrast to our results). This effect is interpreted to reflect a cost entailed by frequently adjusting effort, which discouraged participants from doing so on a trial-by-trial basis. This interpretation could similarly apply to our results, where evaluating the relatively more complex cues in Experiment 2 and reconfiguring the attentional set on a trial-by-trial basis may be associated with the same cost and may have interfered with the lingering attentional set from the preceding trial. Thereby both limiting the effect of reward cues and leading to reduced influence of the load from the preceding trial on the attentional set. In Experiment 3, where simpler cues were used and a blocked reward manipulation adopted, this explanation should no longer hold. Indeed, while we did observe a markedly stronger main effect of reward, reward did not interact with either flanker interference or on the load/flanker interaction.

Our preregistered hypotheses for Experiment 2 were somewhat agnostic regarding the possible effects of reward. On one side, there is ample evidence that effortful allocation of cognitive resources depends on an evaluation of the costs and benefits of doing so (Schevernels et al., 2014; Shenhav et al., 2013; Westbrook & Braver, 2015), and so adoption of a flanker-excluding strategy might depend on a similar cost/benefit analysis. Relatedly, there is some evidence that reward effects can reduce the impact of distracting stimuli in a flanker-like task (Padmala & Pessoa, 2011; Walsh et al., 2021). On the other hand, several studies have also failed to show any reward-induced reduction of flanker interference. For example, Hübner & Schlösser, (2010) found that monetary incentives led to a general speeding of participants' response times in a flanker task, but this improvement did not interact with flanker congruence. That is, participants performed faster when motivated by a cash bonus, but were just as susceptible to flanker interference as compared to the no-reward condition (mirroring the results of our Experiment 3). Similarly, in a study by Marini et al. (2015), participants were informed by a cue before each trial whether a reward would be available and whether the trial was likely to include a flanker stimulus. Once again, responses were faster on reward trials compared to no-reward trials, but there was no reduction in flanker interference. This was despite the fact a strategic slowing was observed when participants were cued to expect a distractor than when they expected no distractor, but only in the rewarded condition. The authors therefore suggested that participants could adopt a distractor filtering strategy when they expected a conflict trial and were sufficiently motivated, but this still was not sufficient to reduce interference when distractors were actually present. Furthermore, the distractor filtering mechanism is presumed to be somehow costly or aversive to implement because participants only did so when motivated by a reward incentive.

Similar to the abovementioned studies, in Experiment 3 a main effect of reward was observed in response times - participants were significantly faster for high-reward than for low-reward blocks, but no reduction of the flanker effect was observed. However, unlike Marini et al. (2015) we did not observe a strategic slowing of responses. While Marini et al. (2015) provided participants with cues which directly informed them about the flanker condition of the upcoming trial, our cues were only informative about the perceptual load (and reward in Experiment 2). It would therefore appear that the ‘distractor filtering’ mechanism described by Marini et al. (2015) is a separate mechanism from that which underlies our cued-perceptual load effects. Furthermore, given that our cued-load effects were not influenced by the reward condition it would seem that the mechanism underlying this effect (e.g. narrowing the attentional window) is not dependent upon a particularly strong motivation or economic cost/benefit analysis (Shenhav et al., 2013; Westbrook & Braver, 2015).

It is also possible that some element of our specific task design made it less possible for participants to use the cue information to full effect. In all three of our experiments cues were presented for 1,000 ms before the visual search stimulus, which may not have provided sufficient time to adopt a different attentional strategy. Bugg & Smallwood (2016) found that cues which were informative about flanker congruence were only effective when a relatively long (around two seconds) inter-stimulus-interval separated the cues from the stimulus. This could suggest that processing the cues and translating that information into an altered proactive attentional set requires more time than was available to our participants.

In conclusion, our experiments provide valuable insights into the complex interplay between perceptual load, top-down expectations, and distractor interference. Our findings suggest an interplay between explicit expectations and attention set as both the attentional set adopted in the preceding trial as well as proactive control based on the cued expectations in the current trial played a critical role in modulating distractor interference. While our results are consistent with the attentional window account, future research is needed to elucidate the specific mechanisms underlying this effect.

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