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Contextual Recruitment of Selective Attention Can Be Updated Via Changes in Task Relevance

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CITATION

Contextual Recruitment of Selective Attention Can Be Updated Via Changes in Task Relevance

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Evidence across a wide variety of attention paradigms shows that environmental cues can trigger adjustments to ongoing priorities for attending to relevant and irrelevant information. This context-specific control over attention suggests that cognitive control can be both automatic and flexible. For instance, in selective attention tasks, congruency effects are larger for items that appear in a context associated with infrequent conflict than in a context associated with frequent conflict. Because the to-be-presented context cannot be predicted or prepared for in advance, attention is assumed to be rapidly updated on-the-fly, triggered by the currently presented context. Context-specific control exemplifies how learning and memory processes can influence attention to enable cognitive flexibility. However, what determines the use of previously learned associations remains unclear. In the current study, we examined whether task relevance would influence the learning and use of context cues in a flanker task. Using a secondary counting task, context dimensions associated with differing levels of conflict were made task-relevant or -irrelevant across the experiment. In short, we found that making new contextual information task-relevant caused participants to suppress a previously learned context-attention association and adopt a new context-specific control strategy—all without changing the experimental stimuli. Furthermore, we found participants did not spontaneously learn about context-specific proportion manipulations (Experiment 2) and explicit instructions were insufficient for producing context-specific effects (Experiment 3). These results suggest that task relevance is a key determinant of context-specific control. All data, analyses, article preparation, and experimental design code is available at https://osf.io/ztcyb/.

Public Significance Statement
Contextual cues have been shown to automatically trigger adjustments in selective attention independent of awareness and intention. Here, we find that task relevance plays an important role in determining which context cues are used to direct attention. These findings contribute to a better understanding of how context-dependency might occur in more complex environments and more generally, how learning and memory processes enable flexible control over attention.

Keywords: attention, cognitive control, conflict adaptation, context-specific, task relevance

Selective attention is commonly investigated using interference paradigms like the Stroop (1935) and flanker (Eriksen & Eriksen, 1974) tasks, where participants identify a target while ignoring a response-congruent or response-incongruent distractor. Performance is typically better on congruent versus incongruent trials and the difference—the congruency effect—taken as an index of attentional priorities. Large congruency effects are thought to reflect ineffective filtering of the distracting stimuli whereas small congruency effects are thought to reflect effective filtering. By probing factors that systematically alter congruency effects, we can then make inferences about processes that control attentional filtering. For example, manipulating the frequency of conflict via the proportion of congruent versus incongruent trials influences the size of the congruency effect, such that high proportion congruent conditions produces larger congruency effects than low proportion congruent conditions (Logan & Zbrodoff, 1979; Lowe & Mitterer, 1982; West & Baylis, 1998). This result is usually explained as strategic control, where participants increase attentional control under high conflict demands and relax attentional control under low conflict demands (Logan, 1980; Logan & Zbrodoff, 1979; Logan, Zbrodoff, & Williamson, 1984; Lowe & Mitterer, 1982). Recent work however, has demonstrated that

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attentional control is not only adjusted by top-down regulation, but can also be triggered automatically by environmental cues (Brosowsky & Crump, 2018; Bugg & Crump, 2012; Egner, 2014; Fischer & Dreisbach, 2015; King, Korb, & Egner, 2012; Mayr & Bryck, 2007).

For example, Crump, Gong, and Milliken (2006; see also, Corballis & Gratton, 2003) presented Stroop stimuli in one of two randomly chosen locations and manipulated the frequency of conflict associated with each location. One location was associated with a high frequency of conflict (25% congruent trials) and the other with a low frequency of conflict (75% congruent trials). Overall, the proportion of congruent trials was 50% and trials were randomized such that the upcoming location could not be predicted. Even so, congruency effects were smaller for trials where the stimulus appeared in the high conflict location as compared to the low conflict location. This context-specific proportion congruent (CSPC) effect has now been replicated in a number of different selective attention paradigms (e.g., Alards-Tomalin, Brosowsky, & Mondor, 2017; Blais, Harris, Sinanian, & Bunge, 2015; Bugg, 2014; Crump, 2016; Crump, Milliken, Leboe-McGowan, Leboe-McGowan, & Gao, 2018; Fischer, Gottschalk, & Dreisbach, 2014; Hübner & Mishra, 2016).

Critical evidence however, that CSPC effects reflect context-specific control rather than other noncontrol learning processes (e.g., Schmidt & Besner, 2008), comes from work showing that CSPC effects can transfer to frequency unbiased items (Brosowsky & Crump, 2016; Crump & Milliken, 2009; Weidler & Bugg, 2016; Weidler, Dey, & Bugg, 2020; though, see Hutcheon & Spieler, 2017). For example, Crump and Milliken (2009) divided Stroop items into two mutually exclusive sets (e.g., red/green and blue/yellow). One set was defined as the frequency biased set, and presented with 75% congruency in one location, and 25% congruency in the other. The second set (i.e., the unbiased set) however, was presented with 50% congruency in both locations. Nevertheless, they found smaller congruency effects for unbiased items presented in the high conflict location as compared to the low conflict location.

One explanation for CSPC effects is that the repeated application of attentional priorities in a particular context creates an associative link in episodic memory between the attentional control procedures and contextual information (Abrahamse, Braem, Notebaert, & Verguts, 2016; Brosowsky & Crump, 2018; Crump, 2016; Egner, 2014). Once the associative link is established, processing the context is assumed to trigger the retrieval of previous experiences, automatically reinstating associated attentional priorities. In this way, automatic memory-based retrieval may be an effective means to sidestep an effortful basis for managing attentional priorities appropriate to a task at hand.

The current study extends prior work by addressing two related issues: First, how is attentional control resolved when multiple contextual features could be used as cues? And second, what determines the expression and/or suppression of previously learned attentional associations?

Real-world environments are complex and contain many potential cues for directing attention. For instance, a coffee shop is a rich environment with numerous context cues (e.g., pictures on the wall, chairs, coffee, laptop), each of which someone could have different experiences with and afford different learned associations. A laptop for example, could cue attentional processing

helpful for writing, whereas other features of the coffee shop might cue attentional processing related to other experiences like socializing and people-watching. Given that all these cues are available to set attentional priorities, what determines which associative relationships will be used to guide attention? On the one hand, context-based retrieval of attentional priorities might be completely obligatory: the mere presence of learned cues might trigger attention in a ballistic manner (e.g., Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Attention in this case could be at the mercy of the environment and perhaps commanded by order of strongest association. Alternatively, current task-goals may constrain the expression of previously learned associations. If someone sits down to write in the coffee shop, their current goals may allow the laptop to take priority over other environmental cues that are irrelevant to the task at hand.

Historically, perspectives of automaticity and attentional control have emphasised the role of practice and likened the learning of attentional associations to the development of automaticity in skill-learning (Logan, 1988, 1992; Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Under this view, forming an association requires many repeated experiences pairing a cue with a set of attentional priorities. Once formed it becomes habitual or hardwired and the mere presence of the cue is sufficient to automatically adjust attention. Learned associations are therefore ballistic, difficult to suppress, and difficult to unlearn (e.g., Posner & Snyder, 1975a, 1975b; Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977; Treisman, 1960, 1964, 1969). This view is often referenced in modern work on contextual cuing where it is assumed that learned associations require practice (e.g., Crump, 2016; Lehle & Hübner, 2008), form without awareness or intention, and are triggered automatically by the mere presence of the cue (Crump et al., 2006; Crump & Logan, 2010; Crump, Vaquero, & Milliken, 2008; King et al., 2012; Reuss, Desender, Kiesel, & Kunde, 2014; though, see Brosowsky & Crump, 2016).

An alternative view—the memory-based perspective—takes a more nuanced view (Abrahamse et al., 2016; Brosowsky & Crump, 2018; Crump, 2016; Egner, 2014; Norman, 1969, 1976). Here, practice is only important to the extent that it improves memory encoding and retrieval. Learning an association can be thought of as the process of encoding experiences where attentional priorities and cues co-occur. The expression of previously learned associations, however, is dictated by memory retrieval—a similarity-based, cue-driven process (e.g., Hintzman, 1984, 1986, 1988; Jacoby, 1978). Presumably, the features of our immediate environment automatically cue memory and reinstate the retrieved attentional priorities (Logan, 1988, 1992). If a few prior experiences are easily retrievable (i.e., memories are distinct or very similar to the retrieval cue), they could support the reinstatement of a learned attentional association despite very little practice (e.g., Brosowsky & Crump, 2018). Conversely, if prior experiences are difficult to retrieve (i.e., memories are indistinct or low in similarity to the retrieval cue), then learned associations may not be expressed, even after much practice. To the extent that you could manipulate memory retrieval, you should also be able manipulate the expression of learned associations.

Here, we examine whether task relevance influences the retrieval of previously used attentional priorities. We hypothesized that the cue-driven, memory-retrieval process might be constrained to the features of our environment that are relevant to our ongoing

task and goals. If this is the case, then a previously learned association should be easily suppressed if that feature was made irrelevant to the task because that feature would no longer contribute to memory-retrieval. Furthermore, it would suggest that the mere presence of a cue is not sufficient for automatically directing attention, but instead requires task-integration (for a similar perspective, see Hommel, 2004, 2019).

Recent work on contextual cueing suggests that task relevance is important for establishing new context-attention associations. Crump et al. (2008) used a Stroop prime-probe task where the distractor word is presented first followed by a colour patch probe. Critically, they used the shape of the color-patch probe, either a square or a circle, as the contextual dimension; one associated with a high frequency of conflict, the other with a low frequency. In one experiment participants were made aware of the shape-proportion contingencies but given no additional instructions. In the second, participants were given a secondary task to count the number of trials that contained a square. The first experiment failed to find evidence for context-specificity, whereas the second did. Although not testing task relevance directly, Cañadas, Rodríguez-Bailón, Milliken, and Lupiáñez (2013) found similar effects. In this study they used images of male and female faces as context cues in a flanker task. When given instructions to think of the faces as members of the gender categories they found CSPC effects, but when given instructions to think of the faces as individuals they did not. These results suggest that making a context dimension relevant to the ongoing task is important for establishing new associations and producing CSPC effects. However, the extent to which task relevance influences the expression of previously learned associations is still unknown.

Experiment 1

Prior work suggests that the task relevance of contextual information could be important for establishing new associations between context cues and attentional priorities. Simply put, CSPC effects tend to emerge when context cues are somehow made relevant to the task, but do not when cues are irrelevant (Cañadas et al., 2013; Crump et al., 2006, 2008). However, what determines the expression of previously learned context-attention associations remains unclear. In the current study, we examined whether manipulating task relevance would allow participants to suppress a previously learned association and adopt a new context-specific control strategy.

A flanker task was used to measure attentional control in an adapted CSPC design (Crump, Brosowsky, & Milliken, 2017; Crump & Milliken, 2009). Similar to previous studies, flanker stimuli were presented in contexts associated with differing levels of conflict (0%, 50%, or 100% congruent; see Figure 1). However, unlike previous CSPC designs where contexts were defined by a single discriminating feature (e.g., upper vs. lower screen locations), contexts were defined by two feature dimensions (see Figure 1, Panel A). In one condition, for example, contexts were defined by object identity (hat or chair) and color (blue or green). Only three out of the four possible feature combinations were presented, each associated with a different proportion of congruent trials (0%, 50%, or 100% congruent). Critically, the frequency unbiased context (50% congruent) always shared a feature with each of the frequency-biased contexts (0% and 100% congruent). The frequency biased contexts (0% and 100% congruent) however, did not share any features (see Figure 1, Panel C for an example).

A secondary counting task was used to manipulate the task relevance of the context dimensions (Crump & Milliken, 2009)
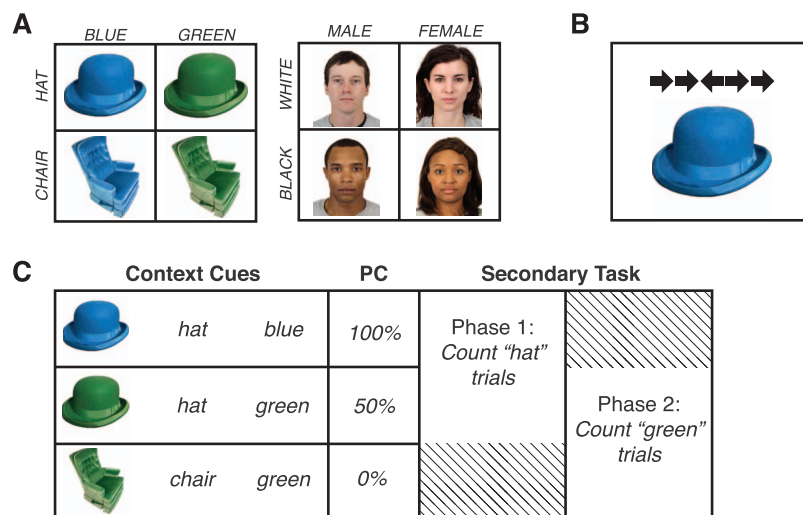

Figure 1. Illustration of the stimuli and trial construction. Panel A shows the feature dimensions for each of the conditions. Panel B shows an example trial stimulus containing both the context and flanker images. Panel C shows an example of how feature dimensions could have been assigned to each proportion congruency condition, and an example of the secondary task assignments. The secondary task order, as well as the feature and congruency assignments were all randomized for each participant. See the online article for the color version of this figure.

which critically, switched halfway through the experiment. Participants were instructed to keep a running count of one of the overlapping features (e.g., "count whenever a hat is presented") which was associated with either a high or low frequency of conflict. Halfway through the experiment they received new instructions to count the other overlapping feature (e.g., "count whenever a green item is presented"). Critically, the set of context images remained the same throughout the experiment (see Figure 1, Panel C for an example) and the task relevance of the context dimensions was the only aspect of the experiment that changed from the first to second phases.

The critical measure of interest is the congruency effect produced in the frequency unbiased context. If, on the one hand, task relevance has no impact on the use of context-attention associations we would expect no differences between the congruency effects when the high conflict context dimension (0% congruent) is made task-relevant or the low conflict context dimension (100% congruent) is made task-relevant. This could occur because all three contexts are treated as individual contexts throughout the whole experiment (e.g., Cañadas et al., 2013). Or it could occur because associations formed in the first block interfere with learning new associations in the second (e.g., Brosowsky & Crump, 2016). On the other hand, task relevance may dictate the expression of previously learned associations. In this case, we would expect smaller congruency effects when the high conflict context dimension is made task-relevant as compared to when the low conflict context dimension is made task-relevant. This would demonstrate that participants were able to suppress a previously learned association and adopt a new context-specific control strategy.

A secondary goal was to conceptually replicate previous findings and test the generalizability of any task relevance effects. Therefore, we included three conditions that differed only in the kinds of stimuli used to create context cues. In one condition we used face images where context dimensions were defined by social categories, such as gender, similar to Cañadas et al. (2013). However, they found that social categorization occurred spontaneously, and it was unclear what impact that would have on the current results. Therefore, we included another condition that used images of simple objects where context dimensions were defined by object features. Finally, Cañadas et al. (2013) also found that context effects generalized to novel face images. In our third condition we used nonrepeating face exemplars to examine whether any task relevance effects would also generalize in our design or if learning would be image-specific.

Method

Participants. All participants were recruited from Amazon Mechanical Turk (MTurk) and were compensated $2.00 for participating. The participant recruitment procedure and tasks were approved by the Brooklyn College Institutional Review Board (IRB). The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hr. For each experiment, the number of HITs (i.e., Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study. Participants were included in the study if they completed all trials. We posted 150 HITs, and 144 participants completed all trials (see Appendix A for demographics).

Apparatus and stimuli. The experiments were programmed using JavaScript, CSS, and HTML. The program allowed participants to complete the task only if they were running Safari, Google Chrome, or Firefox web browsers. Flanker stimuli consisted of images of five arrows pointed left or right presented at 250 pixels × 50 pixels (each arrow was 50 pixels × 50 pixels). Context stimuli were constructed using images selected from Brady, Konkle, Gill, Oliva, and Alvarez (2013) color-rotated to blue and green, and face images from the Chicago Face Database (Ma, Correll, & Wittenbrink, 2015), supplemented with the Nim-Stim Set of Facial Expressions (Tottenham et al., 2009). The object images were displayed at 250 pixels × 250 pixels, while the face images were displayed at 250 pixels × 313 pixels. The experiment ran as a pop-up window that filled the entire screen. The background was white, and stimuli were presented in the center of the screen.

Design. Experiment 1 used a 2 × 2 × 3 mixed design with task-relevant context (high conflict and low conflict) and unbiased-item congruency (congruent and incongruent) as within-subject factors, and context-type (object, social, and social/nonrepeating) as the between-subjects factor.

All three conditions were constructed using the same general method. The experiment was divided into two phases. Each phase consisted of 144 flanker trials (48 trials per context) and 13 count response trials for a total of 314 trials. On the count response trials, participants indicated how many trials they had counted until that point. The counting-response trials occurred once in every block of 12 flanker trials randomly inserted between Trial 6 and 12. Each phase ended with one additional count response trial.

On every flanker trial, participants were presented with flanker stimuli paired with one of three contexts. Each context was associated with a different proportion congruency such that two cues were associated with a biased frequency (0% and 100% proportion congruency), while one was associated with an unbiased frequency (50% proportion congruency). The feature dimensions and corresponding context images assigned to each of the biased and unbiased item sets were randomly determined for each participant. However, context images used for the frequency biased trials never shared features, while the frequency unbiased context image always shared a feature with each of the frequency biased context images (see Figure 1). Additionally, the feature assignments remained the same throughout Phases 1 and 2, and critically, the only change to the task was which feature the participant was instructed to count (see Figure 1, Panel C for an example).

All critical aspects of the task were randomized between participants. This includes the three chosen context images, the features assigned to proportion levels, the features assigned to each counting condition, the secondary task order, and the order of trials.

Procedure. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the direction of the center arrow on each trial as quickly and accurately as possible by pressing *Z* if the arrow pointed left, and *M* if the arrow pointed right. Additionally, they were in-

structed to silently keep count of the number of trials that contained a feature. In the object context condition, they were asked to count trials that contained a certain color (blue or green) or object-identity (hat or chair) and in the social context conditions they were asked to count the number of trials that contained a face resembling a certain gender (male or female) or race (black or white). Periodically throughout the experiment, participants were asked to report how many trials they had counted until that point and to restart their count from 0. Halfway through the experiment participants received new instructions about which feature to count (see Figure 1, Panel C).

Each trial began with a blank interstimulus interval (ISI) of 400 ms, followed by a fixation cross presented in the center of the screen for 200 ms, then a second blank ISI of 400 ms. Next, the flanker and context stimuli appeared in the center of screen (the flanker above the context image; see Figure 1, Panel B) and remained on screen until a response was made. Following a response, accuracy feedback was presented for 1,000 ms. A response automatically triggered the next trial. Halfway through the experiment (157 trials), participants received new instructions about which feature to count and to press the on-screen button when they were ready to continue.

Data analysis. All analyses in this and the following experiments were performed in R (R Core Team, 2019) using a variety of R packages and resources (Aust & Barth, 2018; Wickham, 2016; Wickham, François, Henry, & Müller, 2019; Wilke, 2019). It should be noted that we used the afex and car packages to perform all the null hypothesis significance tests (Fox & Weisberg, 2019; Singmann, Bolker, Westfall, & Aust, 2019), the BayesFactor package to perform Bayesian analyses (Morey & Rouder, 2018), and the conflictPower package to perform the simulation-based sensitivity and power analyses (Crump & Brosowsky, 2019). All data and scripts can be found on the Open Science Framework (https://osf.io/ztcyb/).

We supplemented each null hypothesis significance test with a Bayes factor (BF) analysis (Rouder, Speckman, Sun, Morey, & Iverson, 2009). Using conventional frequentist testing it is not possible to quantify the evidence for a null effect. A BF, however, is a continuous measure of the relative strength of evidence and can quantify the degree to which the data are compatible with the null over the alternative hypothesis (Dienes, 2014; Rouder et al., 2009). All Bayesian analyses were performed using the R package BayesFactor and BFs were calculated using its default settings (Morey & Rouder, 2018). BF_{10} indicates evidence in favor of the alternative hypothesis whereas BF_{01} indicates evidence in favor of the null hypothesis. To simplify the interpretation, we report the Bayes factor in the direction the data supports (e.g., BF_{01} when there is more evidence in favor of the null over alternative hypothesis). As per previous recommendations, we refer to a BF > 3 as "moderate" and BF > 10 as "strong" evidence (Jeffreys, 1961; Rouder et al., 2009).

Results

Participants with mean error rates greater than 25% were excluded from the analyses, eliminating 13 participants. For all remaining participants, the correct RTs from frequency unbiased trials in each condition were submitted to an outlier removal procedure (see Appendix B for the analyses of frequency biased items). The nonrecursive Van Selst and Jolicoeur (1994) outlier removal procedure was applied after removing response times greater than 3,000 ms. This procedure removed 3.39% of the total observations.

First, looking at accuracy in the counting task, participants identified the correct number trials containing the relevant feature on 82% of trials. Furthermore, we can quantify participant accuracy in terms of a difference between the correct answer and their response. Here, we see that the difference between correct answers and participant responses was, on average, less than 1 (0.63). Taken together, we can conclude that participants were generally completing the counting task as instructed.

The primary question of interest was whether the task relevance of context features associated with different levels of conflict would influence the size of the congruency effect for frequency unbiased items (see Figure 2, Figure 3, and Table 1). To that end, congruency effects (incongruent minus congruent performance) from frequency unbiased trials were submitted to a mixed analysis of variance (ANOVA) with task-relevant context (0% and 100% proportion congruency) as the within-subjects factor and context-
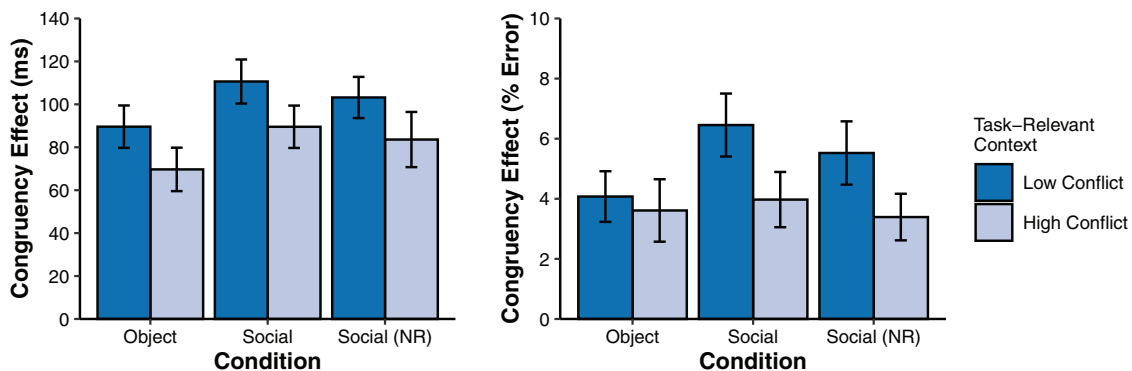

Figure 2. Results from Experiment 1 showing congruency effects (incongruent - congruent) for frequency unbiased items in reaction times (left panel) and error rates (right panel) as a function of the task-relevant context (100% proportion congruent vs. 0% proportion congruent). Task relevance was manipulated by the secondary feature-counting task. Error bars represent standard error of the mean. See the online article for the color version of this figure.
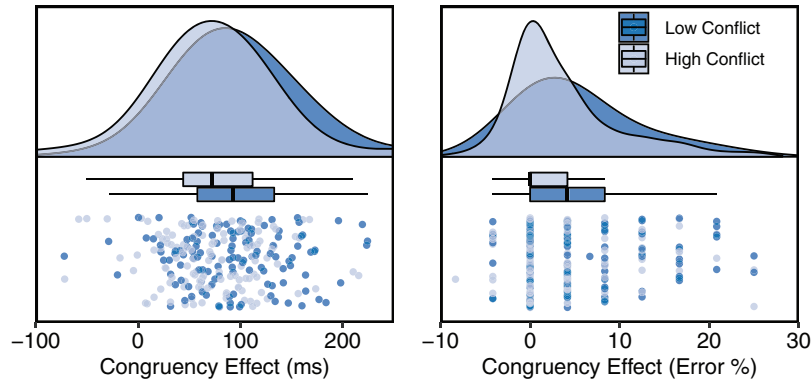
Figure 3. Results from Experiment 1 showing congruency effects (incongruent − congruent) for frequency unbiased items in reaction times (left panel) and error rates (right panel) as a function of the task-relevant context (low conflict vs. high conflict) collapsed over context type. See the online article for the color version of this figure.

type (object, social, and social/nonrepeating) as the between-subjects factor. We supplement each null hypothesis significance test with a corresponding Bayesian analysis.

The results of the reaction time (RT) analysis revealed a significant effect of the task-relevant context, $F(1, 128) = 8.30$, $MSE, = 3,217.08$, $p = .005$, $\hat{\eta}_p^2 = .061$, 90% CI [0.01, 0.14], with moderate evidence in favor of the alternative, $BF_{10} = 6.87$. Specifically, we found smaller congruency effects when the context dimension associated with high conflict was made task-relevant as compared to when the low conflict dimension was made task-relevant. The main effect of context-type, however, was nonsignificant, $F(2, 128) = 1.51$, $MSE = 6,368.18$, $p = .225$, $\hat{\eta}_p^2 = .023$, 90% CI [0, 0.07], with moderate evidence in favour of the null hypothesis, $BF_{01} = 3.96$. The two-way interaction between the task-relevant context and context-type was also nonsignificant, $F(2, 128) < 0.01$, $MSE = 3,217.08$, $p = .996$, $\hat{\eta}_p^2 < .001$, 90% CI [0, <.001],

with moderate evidence in favour of the null hypothesis, $BF_{01} = 7.9$. Finally, the model containing only the task-relevant context was preferred over the model containing the both factors and the interaction by a factor of 54.25. Therefore, congruency effects did not vary across context-types and, more importantly, the effect of task relevance did not vary across context-types.

Similarly, the corresponding error analysis also resulted a significant effect of the task-relevant context, $F(1, 128) = 6.72$, $MSE = 27.87$, $p = .011$, $\hat{\eta}_p^2 = .050$, 90% CI [0.01, 0.12], with moderate evidence in favour of the alternative evidence $BF_{10} = 3.13$; A nonsignificant effect of context-type, $F(2, 128) = 0.81$, $MSE = 51.40$, $p = .449$, $\hat{\eta}_p^2 = .012$, 90% CI [0, 0.05], with moderate evidence for the null hypothesis, $BF_{01} = 7.54$; And a nonsignificant interaction between task-relevant context and context-type, $F(2, 128) = 0.92$, $MSE = 27.87$, $p = .400$, $\hat{\eta}_p^2 = .014$, 90% CI [0, 0.05], with strong evidence in favour of the null

Table 1
Reaction Times and Error Rates From Experiment 1

Task-relevant context	Condition	PC	Congruent		Incongruent	
			RT	ER	RT	ER
Low conflict	Object	0%			693 (26)	4.17 (0.73)
		50%	585 (23)	0.74 (0.27)	675 (28)	4.81 (0.82)
		100%	587 (25)	0.69 (0.23)		
	Social	0%			745 (25)	5.86 (0.77)
		50%	612 (21)	0.39 (0.19)	722 (26)	6.78 (1.04)
		100%	616 (20)	0.63 (0.2)		
	Social (NR)	0%			813 (25)	5.04 (0.74)
		50%	689 (23)	0.19 (0.14)	793 (27)	5.72 (1.06)
		100%	689 (25)	0.48 (0.17)		
High conflict	Object	0%			649 (16)	3.84 (0.6)
		50%	587 (17)	0.56 (0.21)	657 (17)	4.17(1.02)
		100%	634 (18)	2.69 (0.58)		
	Social	0%			722 (23)	5.77 (0.92)
		50%	633 (20)	0.78 (0.5)	722 (25)	4.75 (0.9)
		100%	673 (21)	1.84 (0.47)		
	Social (NR)	0%			807 (26)	4.55 (0.8)
		50%	735 (28)	0.87 (0.36)	818 (30)	4.26 (0.8)
		100%	779 (34)	2.03 (0.4)		

Note. Standard errors are presented in parentheses. PC = proportion congruent; RT = reaction time (in ms); ER = error rate (in percentages); NR = nonrepeating.

hypothesis, BF_{01} = 15.04. Therefore, the congruency effects in error rates were also smaller when the context dimension associated with high conflict was made task-relevant and effects did not vary across context-type, corroborating the results of the RT analysis.

Exploratory Analyses: Order Effects

Previous work has shown that shifting between list-wide proportion congruent phases produced asymmetrical effects. Abrahamse, Duthoo, Notebaert, and Risko (2013) found that shifting from a mostly congruent to mostly incongruent list produced a large change in the size of the congruency effect, whereas shifting from a mostly incongruent to mostly congruent list produced a relatively small change in congruency effects. The authors posited a learned attentional modulation in the mostly incongruent block had carried over into the mostly congruent block reducing what, typically, would be a large congruency effect. In our study, we were primarily interested in whether (a) participants could use context cues despite the presence of competing, contradictory cues, and (b) whether participants could switch from the use of one context cue to another. We therefore randomized the order across participants to control for the effects of order. However, given that we did find systematic shifts in the CSPC effect for unbiased items, we can also ask whether the shift between phases was asymmetrical.

To address this question, we collapsed over context-type and categorised participants according to the order of the counting task: low conflict to high conflict (low-high) or high conflict to low conflict (high-low). We again removed participants with error rates greater than 25% and adopted the same outlier removal procedure as described above. We submitted congruency effects for frequency unbiased items to a 2x2 mixed ANOVA with task-relevant context (0% and 100% proportion congruency) as within-subject factors and phase order (high-low vs. low-high) as the between-subjects factor and supplemented each test with a corresponding Bayesian analysis.

Consistent with the original analysis, the effect of task-relevant context was significant, $F(1, 129)$ = 8.92, MSE = 3,255.69, p = .003, $\hat{\eta}_p^2$ = .065, 90% CI [0.01, 0.14], with moderate evidence in favour of the alternative, BF_{10} = 8.11. However, main effect of phase order was nonsignificant, $F(1, 129)$ = 0.29, MSE = 6,753.61, p = .593, $\hat{\eta}_p^2$ = .002, 90% CI [0, 0.03], with moderate evidence in favour of the null hypothesis, BF_{01} = 4.92 and the two-way interaction between task-relevant context and phase order was nonsignificant, $F(1, 129)$ = 2.56, MSE = 3,255.69, p = .112, $\hat{\eta}_p^2$ = .019, 90% CI [0, 0.07], with moderate evidence in favour of the null hypothesis, BF_{01} = 4.92. Therefore, we did not find statistical support for a list-shift asymmetry.

We also conducted an additional, more severe, test of the list-shift hypothesis. Given that the asymmetry is interpreted as resulting from a carry-over of learned attentional modulation across phases, we might expect that any asymmetry would be exaggerated near the point of transition. We therefore repeated the above analysis but restricted it to the 24 trials immediately before the transition and 24 trials immediately after the transition. Additionally, this analysis will also allow us to determine whether the shift in context-dependent attentional control we observed occurs immediately after the change in task instructions (within 24 trials).

Again, the interaction between task-relevant context and phase order was nonsignificant, $F(1, 129)$ = 0.02, MSE = 5,732.31, p = .902, $\hat{\eta}_p^2$ < .001, 90% CI [0, 0.01], with moderate to strong evidence in favour of the null BF_{01} = 8.88. The main effect of phase order was nonsignificant, $F(1, 129)$ = 0.09, MSE = 10,680.62, p = .765, $\hat{\eta}_p^2$ = .001, 90% CI [0, 0.02], with moderate evidence in favour of the null, BF_{01} = 5.24. Interestingly, the main effect of task-relevant context was still significant, $F(1, 129)$ = 6.65, MSE = 5,732.31, p = .011, $\hat{\eta}_p^2$ = .049, 90% CI [0.01, 0.12], with moderate evidence in favour of the alternative, BF_{10} = 3.22. This result supports the conclusion that there was no asymmetry. Additionally, the shift in context-dependent effects appears to have taken place almost immediately after the transition.

Experiment 2

In Experiment 1, we found that manipulating the task relevance of a shared contextual feature was associated with predictable changes in context-specific proportion congruency effects for frequency unbiased context items. Namely, we found larger congruency effects when the context was associated with low conflict than when the context was associated with high conflict. This suggests that participants learned the association between the context cues and conflict-level in the first block (despite multiple context cues), and subsequently suppressed this association in the second block, allowing participants to learn a new association. So, task relevance then, may be important for establishing new associations (e.g., Crump & Milliken, 2009) *and* for determining which learned associations are expressed in the present moment.

We made two assumptions in the design of Experiment 1: First, we assumed that task relevance was required for participants to learn about the associations between context cues and the biased proportion congruency manipulations. Our task was inspired by Crump and Milliken (2009) who failed find context-specific effects using a task-irrelevant shape cue until they used a counting task to make the shapes relevant. However, location is the most-often used context cue in these kinds of paradigms (e.g., see Bugg & Crump, 2012 for a review). In these cases, no task relevance manipulation is necessary to produce CSPC effects, though, it is arguable whether location is truly task-irrelevant when localizing the target is necessary for responding. Regardless, it is unclear in our paradigm whether participants required the counting task to learn about the biased proportion congruency manipulations.

Second, we assumed that the biased proportion congruency manipulations would have no systematic effect on the unbiased items without the counting task. Although we randomized all aspects of the task across participants to control for potential confounds, it still remains unclear whether the biased items would influence how participants responded to the unbiased items. Experiment 2 served as a control experiment to test these two assumptions directly. Experiment 2 was identical to Experiment 1 with two important changes: First, we removed the counting task, giving no explicit instructions about how to use the context cues. We only informed participants that images would be present throughout the task. This allowed us to determine if there was any systematic effect of biased items on the unbiased items absent the counting task. Second, we altered the proportion congruency of the biased item sets from 100%/0% to 75%/25%. This allowed us to measure the congruency effect for the biased item sets to deter-

mine whether participants learned about the associations between proportion congruency and the context cues without the counting task (e.g., Crump & Milliken, 2009). If this were the case, we would expect larger congruency effects for the 75% proportion congruent items as compared to the 25% proportion congruent items.

Method

Participants. All participants were recruited from MTurk and compensated $2.00 for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study. Participants were included in the study if they completed all trials. We posted 50 HITs and 50 participants completed all trials (see Appendix A for demographics).

Apparatus and stimuli. The apparatus and stimuli were identical to Experiment 1. The context stimuli were identical to those used for the object condition in Experiment 1.

Design. Experiment 2 used a 2x3 within-subjects design with proportion congruent (25%, 50%, and 75%) and congruency (congruent and incongruent) as factors. The experiment consisted of 288 flanker trials (96 per proportion congruent condition) and did not include any counting trials or instructions informing participants about the proportion manipulations. The design in every other respect was identical to Experiment 1.

Procedure. All participants were MTurk workers who found the experiment using the MTurk system. The participant recruitment procedure and tasks were approved by the Brooklyn College IRB. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the direction of the centre arrow on each trial as quickly and accurately as possible by pressing 'z' if the arrow pointed left, and 'm' if the arrow pointed right.

Each trial began with a blank ISI of 400 ms, followed by a fixation cross presented in the centre of the screen for 200 ms, then a second blank ISI of 400 ms. Next, the flanker and context stimuli appeared in the centre of screen (the flanker above the context image; see Figure 1B) and remained on screen until a response was made. Following a response, accuracy feedback was presented for 1000 ms. A response automatically triggered the next trial.

Results

Participants with mean error rates greater than 25% were excluded from the analyses, eliminating 3 participants. For all remaining participants, the correct RTs from frequency unbiased trials in each condition were submitted to an outlier removal procedure. The nonrecursive Van Selst and Jolicoeur outlier removal procedure was applied after removing response times greater than 3000 ms (Van Selst & Jolicoeur, 1994). This procedure removed 3.14% of the total observations.

The questions of interest were whether participants would learn the associations between the context cues without the task rele-

vance manipulation (i.e., the counting task) and whether responding to the unbiased cues would be influenced by the presence of the biased cues. To that end, congruency effects (incongruent minus congruent performance) for RTs and error rates were submitted to an ANOVA with proportion congruency (25%, 50%, and 75%) as the sole within-subject factor (see Figure 4 and Table 2).

The results of the RT analysis revealed the effect of proportion congruency to be nonsignificant, $F(2, 92) = 0.85$, $MSE = 997.11$, $p = .431$, $\hat{\eta}_p^2 = .018$, 90% CI [0, 0.07], with moderate evidence in favour of the null, $BF_{01} = 6.99$. Similarly, the corresponding error analysis also resulted in a nonsignificant effect of proportion congruency, $F(2, 92) = 0.45$, $MSE = 9.23$, $p = .641$, $\hat{\eta}_p^2 = .010$, 90% CI [0, 0.05], with strong evidence in favour of the null, $BF_{01} = 9.98$. Therefore, there was no evidence that the context cues, absent the counting task, systematically influenced the congruency effects in any way.

Experiment 3

The goal of the current study was to examine the importance of task relevance for the contextual recruitment of selective attention. In Experiment 1, we found that task relevance allowed participants to exploit context cues in the presence of a competing, contradictory cue and subsequently suppress a previously learned association in favour of a different context cue. In Experiment 2, we conceptually replicated previous work showing that task relevance was necessary for participants to learn about the biased proportion congruency associations (e.g., Crump et al., 2008) and ruled out possible confounding effects of the biased items.

Experiment 3 tests the boundaries of the task relevance effect found in Experiment 1. The evidence thus far suggests that context-cues, rather than being irrelevant to the task (as their namesake would suggest), need to be integrated into the task-representation to guide attentional control. There is some evidence that instructions alone can be sufficient to produce list-wide proportion congruency effects (Desender, 2018; Entel, Tzelgov, & Bereby-Meyer, 2014), but not item-specific proportion congruency effects (Desender, 2018; Entel et al., 2014), or context-specific proportion congruency effects (Crump et al., 2008). Nevertheless, it is possible that explicit instructions alone could make context-cues relevant to the ongoing task and serve the same purpose as the counting task. In Experiment 3, we test whether participants were
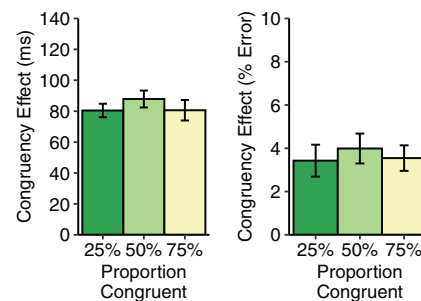

Figure 4. Results from Experiment 2 showing congruency effects (incongruent–congruent) across proportion congruency (25%, 75%, and 100%) in reaction times (left panel) and error rates (right panel). Error bars represent standard error of the mean. See the online article for the color version of this figure.

Table 2
Reaction Times and Error Rates From Experiment 2

	Congruent		Incongruent	
PC	RT	ER	RT	ER
25%	518 (16)	0.35 (0.17)	598 (17)	3.78 (0.71)
50%	515 (15)	0.27 (0.1)	603 (17)	4.26 (0.7)
100%	523 (18)	0.18 (0.07)	603 (19)	3.72 (0.61)

Note. Standard errors are presented in parentheses. PC = proportion congruent; RT = reaction time (in ms); ER = error rate (in percentages).

able to explicitly use the context-cues to guide attentional control through an instruction manipulation.

Method

Participants. All participants were recruited from Amazon Mechanical Turk (MTurk) and compensated $2.00 for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study. Participants were included in the study if they completed all trials. We posted 50 HITs and 50 participants completed all trials (see Appendix A for demographics).

Apparatus and stimuli. The apparatus and stimuli were identical to Experiment 1. The context stimuli were identical to those used for the object condition in Experiment 1.

Design. Experiment 3 used a 2 × 3 mixed design with task-relevant context (low conflict and high conflict) and unbiased-item congruency (congruent and incongruent) as the within-subject factors. The experiment consisted of 288 flanker trials (96 per proportion congruent condition) and did not include any counting trials. However, participants were explicitly informed about the proportion congruency manipulations and instructed to make used of the context cues (see the Procedure section below for more details). Therefore, unlike Experiment 1, context images were made task-relevant through explicit instructions rather than the counting task. The design in every other respect was identical to Experiment 1.

Procedure. All participants were MTurk workers who found the experiment using the MTurk system. The participant recruitment procedure and tasks were approved by the Brooklyn College IRB. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the direction of the centre arrow on each trial as quickly and accurately as possible by pressing 'z' if the arrow pointed left, and 'm' if the arrow pointed right.

Importantly, participants were given further instructions to exploit the proportion congruency associated with context cues. Halfway through the experiment, participants received new instructions about which context features were relevant and associated with high/low conflict. For example:

"On every trial you'll see five arrows (pointed left or right) paired with an image.

The images will help you perform this task.

The arrows will either be congruent (the centre arrow is the same as the flanking arrows) or incongruent (the centre arrow is different from the flanking arrows). Incongruent trials are typically harder because you need to ignore the flanking arrows, while the congruent trials are easier, because you do not need to ignore the flanking arrows.

In the first block of the experiment, trials that include a chair (regardless of whether it is blue or green) will be mostly incongruent. That is, most of the trials that include a chair will be hard.

This will be helpful to perform the task, so try to proactively use this information when you perform the task."

Each trial began with a blank ISI of 400 ms, followed by a fixation cross presented in the centre of the screen for 200 ms, then a second blank ISI of 400 ms. Next, the flanker and context stimuli appeared in the centre of screen (the flanker above the context image; see Figure 1B) and remained on screen until a response was made. Following a response, accuracy feedback was presented for 1000 ms. A response automatically triggered the next trial.

Results

Participants with mean error rates greater than 25% were excluded from the analyses, eliminating 4 participants. For all remaining participants, the correct RTs from frequency unbiased trials in each condition were submitted to an outlier removal procedure. The nonrecursive Van Selst and Jolicoeur outlier removal procedure was applied after removing response times greater than 3000 ms (Van Selst & Jolicoeur, 1994). This procedure removed 3.42% of the total observations.

The primary question of interest was whether the task relevance of context features (made relevant through explicit task instructions) associated with different levels of conflict would influence the size of the congruency effect for frequency unbiased items. Mean correct RTs and mean error rates from frequency unbiased trials were submitted to an ANOVA with task-relevant context (low conflict vs. high conflict) as the within-subject factors (see Figure 5 and Table 3).

The results of the RT analysis revealed the effect of task relevance to be nonsignificant, $F(1, 45) = 0.02$, $MSE = 4,030.54$, $p = .876$, $\hat{\eta}_p^2 = .001$, 90% CI [0, 0.04], with moderate evidence in favour of the null, $BF_{01} = 4.42$. Similarly, the corresponding error analysis also resulted in a nonsignificant effect of task relevance, $F(1, 45) = 0.66$, $MSE = 28.52$, $p = .420$, $\hat{\eta}_p^2 = .014$, 90% CI [0, 0.11], with moderate evidence in favour of the null, $BF_{01} = 3.39$.

Estimating Replicability by Simulation-Based Sensitivity and Power Analyses

In Experiment 1, we found no evidence for differences in the task relevance effect across context-type. Similarly, Experiments 2 and 3 failed to find CSPC effects. To determine the limitations in our ability to draw inferences from these results, we need to assess the extent to which our experiments could have detected effects if they were present. Although replicability has become an important
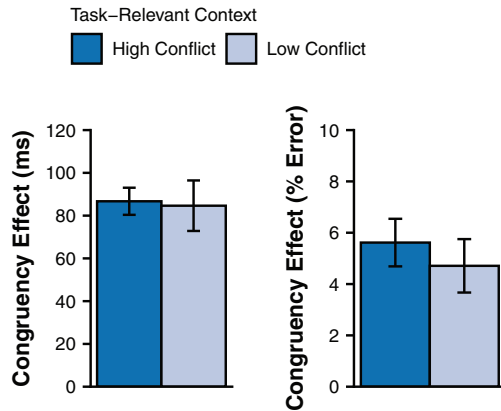
Figure 5. Results from Experiment 3 showing congruency effects (incongruent–congruent) for frequency unbiased items in reaction times (left panel) and error rates (right panel) as a function of the task-relevant context (low conflict vs. high conflict). Task relevance was manipulated through explicit instructions. Error bars represent standard error of the mean. See the online article for the color version of this figure.

and salient issue in Psychology at large (e.g., Camerer et al., 2018; Open Science Collaboration, 2015), this issue has also arisen in the context-specific proportion congruency literature (Crump et al., 2017; Hutcheon & Spieler, 2017).

To address this issue in the current study—and to do so transparently—we conducted simulation-based power and sensitivity analyses (e.g., Crump et al., 2017; see Maxwell, Kelley, & Rausch, 2008) to determine the range of effect sizes our experimental design was sensitive to detect and the required sample sizes to detect a range of effects. We used a Monte Carlo simulation approach with a statistical model of the distributions that underlie the effect of interest (Maxwell et al., 2008). Rather than estimating power based on observed effect sizes from prior experiments (typically, overestimates), we instead estimated the properties of the base RT distributions from our current data and sampled from these distributions using a Monte Carlo simulation approach to estimate the replication success rate across a range of effect sizes and sample sizes.

We created the conflictPower R-package to conduct the Monte Carlo simulations (Crump & Brosowsky, 2019). The conflict-Power package samples simulated RT data for subjects at the

trial-level using base RT distributions for the conflict and no-conflict conditions. To generate the base RT distributions, we fitted ex-Gaussian functions to the RT data from individual participants and averaged across all three experiments using the retimes R-package (Massidda, 2013). We then used the conflict-Power package to simulate the results across a range of effect and sample sizes, calculating the proportion of experiments, across 10000 simulations, resulting in a p value less than .05 (see Figure 6; for a more detailed explanation of the sampling procedure see Crump & Brosowsky, 2019).

First, the results of these analyses show that our experimental design was reasonably powered to detect CSPC effects. In particular, with 150 participants, we estimated 89% power to detect effects as small 10 ms; With only 50 participants, we were still reasonably powered to detect CSPC effects as small as 15 ms at an estimated 78%. Understandably, our design was not as sensitive to detecting changes in the CSPC effect between groups (e.g., Experiment 1). We estimated only 75% power to detect the difference between a 20 ms and 0 ms CSPC effect with 50 participants per group. That is, the experimental design could detect the presence/absence of the CSPC effect moderately well, but likely does not reliably detect small changes in the CSPC effect across conditions. To reliably detect a 15 ms change in the CSPC effect at roughly 80% power, for example, we estimate would require 100 participants per group.

General Discussion

The aim of the current study was to determine whether task relevance plays a role in the contextual recruitment of selective attention (see Table 4 for a summary of hypotheses and results). In Experiment 1, we tested whether manipulating the relative task relevance of context cues could cause participants to suppress a previously learned context-association and apply a new association. We created a frequency unbiased context cue that shared features with two frequency biased contexts and used a feature-counting task to manipulate the task relevance of context dimensions across two blocks of trials. Critically, halfway through the experiment participants received new instructions changing the task-relevant feature from one frequency biased cue to the other.

The key finding was that congruency effects for the frequency unbiased items were significantly larger when the low conflict context was made task-relevant as compared to when the high conflict context was made task-relevant. This result is consistent

Table 3

Reaction Times and Error Rates From Experiment 3

Task-relevant context	PC	Congruent		Incongruent	
		RT	ER	RT	ER
Low conflict	0%			658 (28)	5.66 (0.94)
	50%	579 (32)	0.54 (0.31)	664 (29)	5.25 (1.03)
	100%	571 (26)	0.45 (0.24)		
High conflict	0%			643 (26)	6.43 (0.87)
	50%	550 (21)	0.36 (0.28)	637 (23)	5.98 (0.99)
	100%	547 (22)	0.45 (0.17)		

Note. Standard errors are presented in parentheses. PC = proportion congruent; RT = reaction time (in ms); ER = error rate (in percentages).
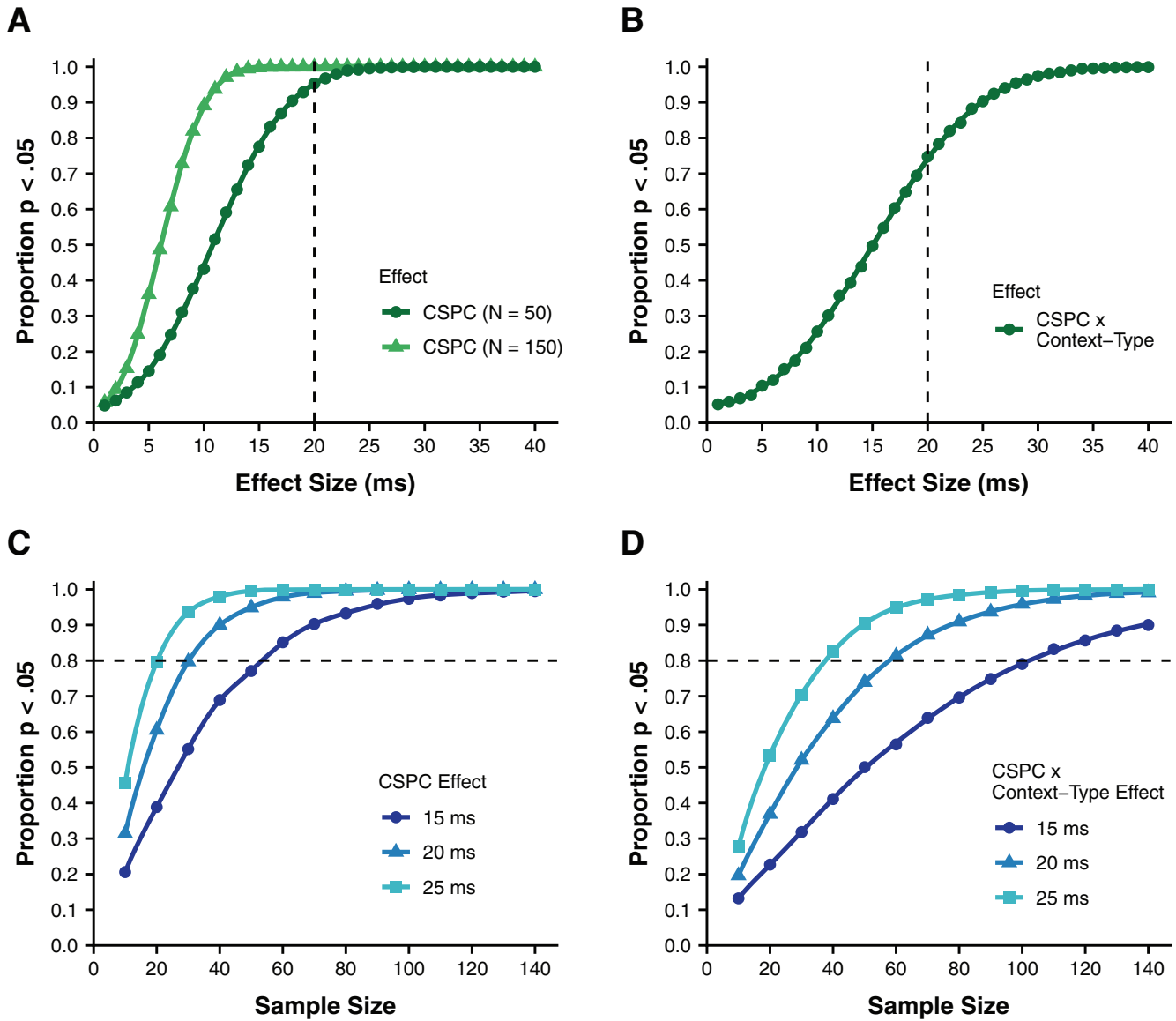
Figure 6. Results from the simulation-based sensitivity and power-analyses. Panel A shows the sensitivity of our experimental design to detect a range of effect sizes for the context-specific proportion congruency (CSPC) effect with 50 (e.g., Experiments 2 and 3) and 150 (e.g., Experiment 1) participants. Panel B shows the sensitivity to detect the CSPC by context-type interaction with 50 participants per group (e.g., Experiment 1). Panels C and D show the estimated power curves for the CSPC and CSPC by context-type effects across sample sizes. Each data point represents the proportion of *p* values less than .05 from 10,000 simulations. See the online article for the color version of this figure.

with prior CSPC effects and, like the previous work, suggests that context cues triggered rapid adjustments to attentional control (Crump & Milliken, 2009). However, unlike prior studies, we were able to experimentally manipulate the CSPC effect across blocks of trials without changing any of the physical properties of the stimuli. This novel finding demonstrates that participants were able to learn and apply one context-attention association in the first phase, and subsequently suppress that association to learn a new association in the second phase.

This result implicates an important role for task relevance in producing CSPC phenomena. Crump et al. (2008) used shapes as

context cues in a prime-probe Stroop task and did not find CSPC effects until the context cues were made task-relevant. Similarly, Cañadas et al. (2013) eliminated the CSPC effect by making the contextual cue effectively unrelated to the task. These studies suggest that task relevance plays an important role in establishing new associations between contextual information and attentional priorities to produce CSPC effects. Our finding extends this work in two important ways. First, we show that changing the task relevance of the presented cues corresponded with a change in attentional control in the predicted direction. This demonstrates that task relevance is also a key determinant the previously learned

Table 4

Summary of Hypotheses and Conclusions From Experiments 1 Through 3

Hypothesis (H)	Supported	Unsupported
Experiment 1		
H1. Task relevance dictates the expression/suppression of learned attention-context associations	X	
H2. In the presence of multiple context cues, task relevance dictates which cue guides attention	X	
H3. CSPC and task relevance effects generalize across different stimuli (faces and objects)	X	
H4. Context-specific attentional control generalizes across novel exemplars	X	
Experiment 1 (Exploratory)		
H1. Learned attentional modulations carry-over across blocks producing asymmetrical list-shifting effects		X
H2. Shifting context-specific attentional control strategies occurs rapidly (within 24 trials)	X	
Experiment 2		
H1. Context-dependency can be learned without a task relevance manipulation		X
H2. Proportion biased items influence responses to unbiased items absent task relevance manipulations		X
Experiment 3		
H1. Context cues can be explicitly used to guide attentional control via instructions		X

Note. CSPC = context-specific proportion congruent.

attention-context association is expressed or suppressed. Second, we show that task relevance allowed participants resolve competition between two competing contextual cues, responding on the basis of one at the expense of the other. To our knowledge, this is the first demonstration that CSPC effects can be produced when there are multiple, overlapping contextual cues available.

In Experiment 2, we found that participants did not spontaneously learn the associations between proportion congruency and context-cues without the secondary counting task. This replicates previous work (Crump et al., 2008) and demonstrates further that task relevance is necessary for establishing associations between contexts and attentional priorities. Moreover, this should serve as a cautionary tale for future work using the context-specific proportion congruent design. Prior studies have had difficulty replicating CSPC effects using the typical location-based CSPC design. The inconsistency of participants to learn associations between attentional priorities and location cues might arise because location is a poor cue in terms of task relevance. Additionally, in Experiment 3 explicit instructions to make use of the cues also failed to produce context-specific effects. This result also tracks well with previous work that found instruction manipulations could successfully produce list-wide proportion congruency effects, but not item-specific (Entel et al., 2014) or context-specific (Crump et al., 2008) effects. Explicit instructions are another method for inducing task relevance further illustrating that weak task relevance manipulations are not sufficient for producing CSPC effects.

In light of prior work, we take these results as evidence that the contextual recruitment of selective attention, although likely implicit, is not obligatory (e.g., Brosowsky & Crump, 2016), requiring that environmental information be incorporated into the task representation. Similarly, there appears to be flexibility in which environmental features are selected and used to guide attention, which can be rapidly updated depending on the task relevance of those features (for a similar perspective, see Hommel, 2004, 2019). Such a result lends some insight into how context-specific control might operate in more complex, real-world environments, where there is an overabundance of environmental features that afford many different learned associations. From a theoretical perspective, this result is consistent with memory-based accounts of CSPC phenomena (Brosowsky & Crump, 2018; Bugg & Hutchison, 2013; Crump et al., 2017, 2018; Crump & Milliken, 2009). Under

this view, a memory process encodes attentional priorities in the representation of individual experiences and, as a result, becomes associated with the environment where they were used. The subsequent reoccurrence of a prior context triggers the retrieval and reinstatement of those attentional priorities. Our results show however, that all the features of the environment may not be treated equally and that only task-relevant features are used to probe memory and guide attention.

Finally, another key result of this study concerned the different stimuli used as context cues in Experiment 1. Across the three conditions, we varied the type of context image and dimensions. We manipulated the type of image presented, including both objects (identity and colour dimensions) and faces (gender and racial dimensions). We also manipulated whether a single set of three repeating images were presented (object and social) or a set of nonrepeating images were presented (social/nonrepeating). Across all three conditions, we found no evidence that using different stimuli had an influence on the size or direction of the CSPC demonstrating generalizability of this phenomenon. Furthermore, CSPC effects were present even when using nonrepeating images which suggests that context-dependency did not rely on image-specific associations but higher-order, learned categorical information. We should also note an important limitation is that the experimental design, while moderately powered to detect the presence versus absence of the CSPC effect between groups, was likely underpowered to detect small changes in the CSPC effect (see the Replicability section above).

Traditional models of person perception posit that social categories are automatically activated in the presence of social stimuli (e.g., Brewer, 1988; Devine, 1989; Fiske & Neuberg, 1990). Cañadas et al. (2013) however, found that directing participants to think about faces in terms of individual features eliminated context-specific attention effect and suggested that momentary motivations may influence the automaticity of social categorisation. Our results add to this literature by observing the influence of momentary motivations (i.e., task relevance) when there is competition between two salient social categories. Specifically, we found that participants categorised and responded on the basis of one social cue at the expense of the other, and flexibly switched between them. These findings may speak to issues of automaticity in social categorisation (Macrae & Bodenhausen, 2001) as well as

understanding how the situational context can prime one social identity over another (Crisp & Hewstone, 2007). Furthermore, we found no evidence for differences between the social and object conditions. This suggests that categorisation within the CSPC task is quickly and easily learned when the task supports such learning and is nonunique to social stimuli.

In sum, our results provide new evidence that changes in task relevance can update the contextual recruitment of selective attention in a CSPC flanker task. We demonstrated that making one context cue task-relevant produced a CSPC effect, even in the presence of a competing contextual cue. More important however, we found that changing the task relevance of the contextual cues across blocks of trials was accompanied by predictable changes in the congruency effects. These effects were found to be generalizable across two different kinds of stimuli and occurred even when using nonrepeating images and implicate an important role for task relevance in producing context-dependency and are consistent with memory-based accounts of CSPC phenomena.

Résumé

Les données recueillies dans le cadre d'une grande variété de paradigmes d'attention montrent que les indices environnementaux peuvent déclencher des ajustements des priorités en cours pour traiter des informations pertinentes et non pertinentes. Ce contrôle de l'attention spécifique au contexte suggère que le contrôle cognitif peut être à la fois automatique et souple. Par exemple, dans les tâches d'attention sélective, les effets de congruence sont plus importants pour les items qui apparaissent dans un contexte associé à des conflits peu fréquents que dans un contexte associé à des conflits fréquents. Étant donné que le contexte à présenter ne peut être prédit ou préparé à l'avance, on suppose que l'attention sera rapidement mise à jour sur-le-champ, déclenchée par le contexte actuel. Le contrôle spécifique au contexte illustre comment les processus d'apprentissage et de mémoire peuvent influencer l'attention pour permettre une flexibilité cognitive. Toutefois, ce qui détermine l'utilisation des associations acquises auparavant reste flou. Dans l'étude en cours, nous avons examiné si la pertinence de la tâche avait une incidence sur l'apprentissage et l'utilisation d'indices de contexte dans une tâche d'accompagnement. En utilisant une tâche de comptage secondaire, les dimensions contextuelles associées aux différents niveaux de conflit ont été rendues pertinentes ou non pertinentes à la tâche tout au long de l'expérience. En bref, nous avons découvert que le fait de rendre la nouvelle information contextuelle pertinente à la tâche a incité les participants à supprimer une association contexte-attention apprise précédemment et à adopter une nouvelle stratégie de contrôle spécifique au contexte – tout cela sans changer les stimuli expérimentaux. De plus, nous avons constaté que les participants n'avaient pas spontanément appris au sujet des manipulations de proportions spécifiques au contexte (expérience 2) et que les instructions explicites étaient insuffisantes pour produire des effets spécifiques au contexte (expérience 3). Ces résultats suggèrent que la pertinence de la tâche est un déterminant clé du contrôle propre au contexte. Toutes les données, les analyses, la préparation des articles et le code de conception expérimentale sont disponibles à l'adresse https://osf.io/ztcyb/.

Mots-clés : attention, contrôle cognitif, adaptation aux conflits, spécifique au contexte, pertinence de la tâche.

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(Appendices follow)

Appendix A

Demographics From Experiments 1 Through 3

Characteristic	Experiment 1		Experiment 2		Experiment 3	
	n	*%*	*n*	*%*	*n*	*%*
Age						
>59	10	6.99	3	6.00	2	4.00
50–59	21	14.69	7	14.00	8	16.00
40–49	16	11.19	8	16.00	5	10.00
30–39	55	38.46	18	36.00	23	46.00
18–29	40	27.97	14	28.00	12	24.00
No response	1	0.70	0	0.00	0	0.00
Gender						
Female	66	45.83	20	40.00	18	36.00
Male	76	52.78	29	58.00	32	64.00
No response	2	1.39	1	2.00	0	0.00
Handedness						
Left	21	14.58	5	10.00	11	22.00
Right	118	81.94	44	88.00	38	76.00
Both	3	2.08	1	2.00	1	2.00
No response	2	1.39	0	0.00	0	0.00

Appendix B

Experiment 1: Biased Item Supplementary Analysis

For the sake of completeness, we also include an analysis of the biased items. We follow all the same analysis procedures as noted in the results section of Experiment 1. We should note, however, that the effects of congruency and task relevance are confounded with the counting task, so we have no a priori hypotheses about these analyses and do not include them in our general interpretation of the results.

The congruency effects (incongruent minus congruent performance) from frequency biased trials were submitted to a mixed ANOVA with task-relevant context (high conflict versus low conflict) as the within-subjects factor and context-type (object, social, and social/non-repeating) as the between-subjects factor.

The results of the RT analysis revealed a significant effect of the task-relevant context, $F(1, 128) = 89.09$, $MSE = 5,868.79$, $p < .001$, $\hat{\eta}_p^2 = .410$, 90% CI [0.3, 0.5]. Specifically, we found smaller congruency effects when the context dimension associated with high conflict was made task-relevant as compared to when the low conflict dimension was made task-relevant. The main effect of context-type, however, was non-significant, $F(2, 128) = 3.02$, $MSE = 6,017.35$, $p = .052$, $\hat{\eta}_p^2 = .045$, 90% CI [0, 0.11]. The two-way interaction between the task-relevant context and context-type was also non-significant, $F(2, 128) = 0.23$, $MSE = 5,868.79$, $p = .793$, $\hat{\eta}_p^2 = .004$, 90% CI [0, 0.02]. Similarly, the corresponding error analysis also resulted a significant effect of the task-relevant context, $F(1, 128) = 11.66$, $MSE = 0.00$, $p = .001$, $\hat{\eta}_p^2 = .083$, 90% CI [0.02, 0.17]; A nonsignificant effect of context-type, $F(2, 128) = 2.72$, $MSE = 0.00$, $p = .070$, $\hat{\eta}_p^2 = .041$, 90% CI [0, 0.1]; And a nonsignificant interaction between task-relevant context and context-type, $F(2, 128) = 0.59$, $MSE = 0.00$, $p = .555$, $\hat{\eta}_p^2 = .009$, 90% CI [0, 0.04].