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Conflict-induced perceptual filtering: A mechanism supporting location-specific control?

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Abstract

Cognitive control can adapt to the level of conflict present in the environment in a proactive (pre-stimulus onset) or reactive (post-stimulus onset) manner. This is evidenced by list-wide and location-specific proportion congruence effects, reduced interference in higher conflict lists or locations, respectively. Proactive control in the flanker task is believed to be supported by a conflict-induced-filtering (CIF) mechanism. The goal of the present set of experiments was to test if CIF also supports reactive location-specific control in the flanker task. To measure CIF, we interspersed a visual search task with a flanker task. After reproducing evidence for CIF using a two-location, list-wide proportion congruence manipulation (Experiment 1), we examined if a similar pattern emerges using a location-specific proportion congruence manipulation in Experiments 2 - 5. We found minimal evidence that reactive location-specific control employs a CIF mechanism. What was clear, however, is that the location-specific proportion congruence effect is susceptible to disruption from an intermixed task that dilutes the location-conflict signal. This highlights the need for alternative approaches to elucidate whether CIF or another mechanism supports reactive, location-specific control.

Keywords

Cognitive control; flanker task; visual search; list-wide proportion congruence; context-specific proportion congruence

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Cognitive control—a mechanism that enables attending to task relevant information in the face of task-irrelevant information– is necessary for many daily tasks (e.g., Egner, 2017). For example, while reading a research article you must avoid being distracted by facets of the environment such as others' conversations or notifications on your computer. An important aspect of cognitive control that allows us to tackle daily tasks is its flexibility to adapt to the environment. One example of this flexibility is that in the face of conflict, cognitive control is enhanced to filter distracting stimuli more efficiently, thereby shielding current goals and facilitating task completion.

There is now evidence that the upregulation of control in the face of conflict can be elicited in a more proactive (i.e., global, preparatory) manner or in a more reactive (i.e., "on-the-fly," in response to an imperative stimulus) manner, consistent with recent theorising (Braver et al., 2007; Bugg, 2012; Bugg & Crump, 2012). For example, imagine reading an article in a coffee shop that tends to have many distracting conversations. You might employ a proactive control mechanism to upregulate attention in this environment compared to when working in a relatively conversation-free library. Proactive control implies that such a mechanism is preparatory in nature and filters *all* distracting stimuli in the environment (e.g., De Pisapia & Braver, 2006; Gonthier et al., 2016). Alternatively, you might upregulate control reactively as distraction arises. For example, you may have learned that people entering a group study room to your right in the library tend to be engaged in distracting noisy conversations whereas people entering the study carrels on the left tend to enter quietly (see Abrahamse et al., 2016; Egner, 2014, for recent discussions on the role of learning in control). The appearance of a person on the right may then trigger an upregulation of control (to filter the distraction) but the

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appearance of a person on the left would not trigger such filtering. Of note, the term *reactive* implies that control is triggered on the *appearance* of the stimulus (i.e., the person walking past) and not in advance of it (as in proactive control). Broadly, the goal of the current research is to explore the mechanism underlying this reactive form of cognitive control.

List-wide and location-specific manipulations

Before discussing the leading theoretical account for reactive control, we briefly detail how both proactive and reactive adjustments to control have been demonstrated in the laboratory, through list-wide and location-specific manipulations, respectively. One common way to measure attentional control is through the flanker task, in which participants identify the central symbol in a string of symbols (e.g., Eriksen & Schultz, 1979). Sometimes the flanking symbols are *compatible* with the central target symbol whereas sometimes they are incompatible. The magnitude of response time slowing on incompatible compared to compatible trials (referred to henceforth as the *compatibility effect*) indexes attentional control—with reduced compatibility effects illustrating greater ability to control intrusions from distracting stimuli. Returning to the central point of manipulating the level of conflict in this paradigm, in list-wide manipulations that index proactive control, half of the session is comprised of high-conflict and half of the session is comprised of low-conflict test lists (i.e., blocks). Specifically, in the *mostly compatible* (MC) lists, most (e.g., 75%) of the flanker stimuli are compatible whereas in the *mostly incompatible* (MI) lists, few (e.g., 25%) of the stimuli are compatible (cf. e.g., Logan & Zbrodoff, 1979; Wendt et al., 2012). This list-wide manipulation of conflict produces what we henceforth refer to as the *list-wide PC* effect—compatibility effects are smaller in the high conflict MI lists than in the lower conflict MC lists. Thus, in an environment (i.e., a list) that is relatively high in conflict, participants can proactively upregulate control and show less interference from distracting flanker stimuli (Bugg & Gonthier, 2020).

In contrast to list-wide manipulations, in location-specific manipulations that index reactive control, a given list is 50% compatible but certain *contexts* (here, locations) within those lists are biased with different levels of conflict. For example, one location might be associated with a high level of conflict (i.e., MI) whereas the other is associated with a low level of conflict (i.e., MC). A similar behavioural pattern—hereafter referred to as the *location-specific PC* effect – emerges in this scenario: The flanker compatibility effect is reduced (i.e., control is enhanced) in the MI condition, here, the *location* associated with a higher level of conflict (e.g., Corballis & Gratton, 2003; Crump, Gong & Milliken, 2006). Notably, location-specific control (Crump & Milliken, 2009) is necessarily operating post-stimulus onset (reactively) because it is impossible to predict the optimal control setting to engage prior to the stimulus appearing in its location (i.e., stimuli are presented randomly across locations). If control were operating at a proactive (list-wide) level in this paradigm, then one would not expect the differing compatibility effects based on location that have repeatedly been demonstrated.

Accounts of reactive control

The broad goal of this set of experiments was to learn about the mechanism(s) supporting reactive control. At present, the primary extant theoretical account of location-specific control is the episodic retrieval account. It proposes that when a stimulus appears in a certain context (i.e., specific location on screen), that context serves as a cue that triggers retrieval of prior instances of experience within that context. Critically, this episodic record includes the attentional settings that have been applied when interacting with stimuli in that location in the past (e.g., Crump, 2016; Crump & Milliken, 2009). So, for example, presumably a more stringent attentional setting that minimises processing of the distractor (flanking) arrows is retrieved in a high conflict (MI) context than a low conflict (MC) context since most prior experiences necessitated such a setting.

However, at present it remains unclear as to the nature of this "more stringent" attentional setting in the MI location (compared to a MC location). That is, during prior experiences in the MI location, what attentional processes are engaged to deal with conflict in that location that then get associated with that location and are retrieved when a stimulus appears? Recently Wendt et al. (2012) proposed that *conflict-induced-filtering* (CIF) supports proactive adjustments of cognitive control when list-wide PC is manipulated in a flanker task. Briefly, the CIF account posits that a perceptual visual filter is applied to the flanker symbol locations in the high-conflict MI context to reduce the accumulation of the (often conflicting) information presented there. The goal of the current set of experiments was to examine whether CIF might be a mechanism that also supports reactive adjustments of cognitive control when location-specific PC is manipulated in a flanker task. Next we describe CIF and the approach Wendt et al. used to measure it.

Conflict induced filtering (CIF) in list-wide manipulations

To examine the contribution of CIF to proactive control, Wendt et al. (2012) intermixed a *visual search* task with a list-wide PC manipulation in a flanker task. During the visual search task, participants indicated whether the digit 3 or 7 was present in a three-number string. The key manipulation in that task was the position of the search task target relative to the target symbol in the flanker task—whether the search target was central in the string (the location of the target in the flanker task) or in the left or right position of the string (i.e., laterally located, as in the flankers in the flanker task).

As was expected given that the search task was interspersed with a flanker task that necessitated attention to the central target, the researchers found a *target position benefit* – participants performed better in the search task when the target appeared centrally in the number string, compared to when the search target appeared laterally (see also Wendt et al., 2017 for evidence for this pattern in a paradigm that does not include a conflict manipulation). The key comparison, however, rested in how this target position benefit in the search task differed as a function of conflict. More specifically, if a CIF mechanism is operative, one would expect the target position benefit to be exacerbated in the high-conflict MI list in which participants presumably would be filtering information from the flanker location compared to the low-conflict MC list in which that same information is generally facilitative to performance. Indeed, this is what Wendt et al. (2012) found: Larger target position benefits (i.e., lateral—central visual search RT) in the MI compared to the MC list. Thus, Wendt et al. argued that flanker compatibility effects are reduced in the face of an overall high probability of conflict (i.e., in the MI list) because participants maintain a visual filter[1] over the locations of the flankers (that often provide conflicting information in an MI list).

Current goal

In the current set of experiments, our goal was to examine if a CIF mechanism (Wendt et al., 2012) also underlies reactive location-specific control. Wendt et al. (2012) speculated that CIF may not be observed when location-specific PC is manipulated. In contrast however a past review of the PC literature theorised that stimuli may exogenously trigger the operation of an attentional filter to support stimulus-driven (reactive) adjustments in control (Bugg & Crump, 2012). Indeed, a number of extant patterns support the theoretical possibility of a CIF mechanism supporting reactive location-specific control: First, the location-specific PC manipulation produces a similar behavioural pattern (MI compatibility effect < MC compatibility effect) as the list-wide PC manipulation, which raises the possibility that a similar mechanism may underlie the effects even though the mechanisms operate on different time courses (post- vs. pre-stimulus onset, respectively). Second, a perceptual filtering mechanism such as CIF could accommodate extant patterns in the location-specific PC literature (including "transfer" to novel, 50% congruent diagnostic trials as in Crump & Milliken, 2009; but see Bugg et al., 2020; Hutcheon & Spieler, 2017). Third, there is neuroscientific evidence for reduced activity in brain areas supporting visual processing in the high conflict MI location, which could be explained by the operation of a filter (King, Korb, & Egner, 2012). In addition, there is a theoretical reason to expect that reactive location-specific control could reflect the operation of a perceptual filter (CIF) that is retrieved post-stimulus onset but prior to full perceptual processing of the stimulus, which is that the processing of stimulus location (which triggers retrieval of the setting according to the episodic retrieval account) is rapid and automatic (e.g., Logan, 1998; Mayr, 1996). Yet, it remains an open theoretical question if reactive control recruits a CIF and whether such a CIF mechanism can be detected in an interspersed visual search task.

Experiment 1

Given that our primary goal was to examine if CIF may support location-specific modulations of control, Experiment 1 was intended to first establish that such a mechanism can handle location variability, a necessary component of the location-specific PC manipulation. This is unclear from the prior study that implemented the list-wide PC manipulation that was supported by CIF (Wendt et al., 2012) because all stimuli appeared in a single, fixed location. This leaves open the possibility that CIF may support proactive control only under this condition, that is, when an attentional setting can be applied to a fixed portion of space on all trials within a list. To address this possibility, we attempted to reproduce the findings of Wendt et al. (2012) with one important change—instead of presenting all stimuli centrally, stimuli were presented with equal probability in two locations (one above centre and one below centre) in each list. Critically, we still employed a list-wide PC manipulation in this initial experiment meaning that *both* locations within a list comprised the same set of stimuli with the same probability of conflict (either MC or MI depending on the list).

Method

Participants. Forty students recruited from Washington University's undergraduate subject pool participated for course credit. They all had normal or corrected to normal vision, were aged 18–25, and were naïve to the purpose of the experiment. We chose this sample size to more than triple Wendt et al.'s (2012) N of 13.[2]

Stimuli and apparatus. Stimulus presentation was controlled by Psychopy (Peirce, 2007) and presented against a grey background ("dimgray"). The stimuli in the flanker task were the digits "S" and "H" and the stimuli in the search task were the numerals 0–9. Each letter/digit was light grey ("silver") and 1.1 cm high; one stimulus was presented centrally along the x-axis, one .7 cm to the left and one .7 to the right. The entire 3-symbol stimulus (referred to subsequently as a *string)* in both tasks could appear 10 cm above or below the centre of the screen

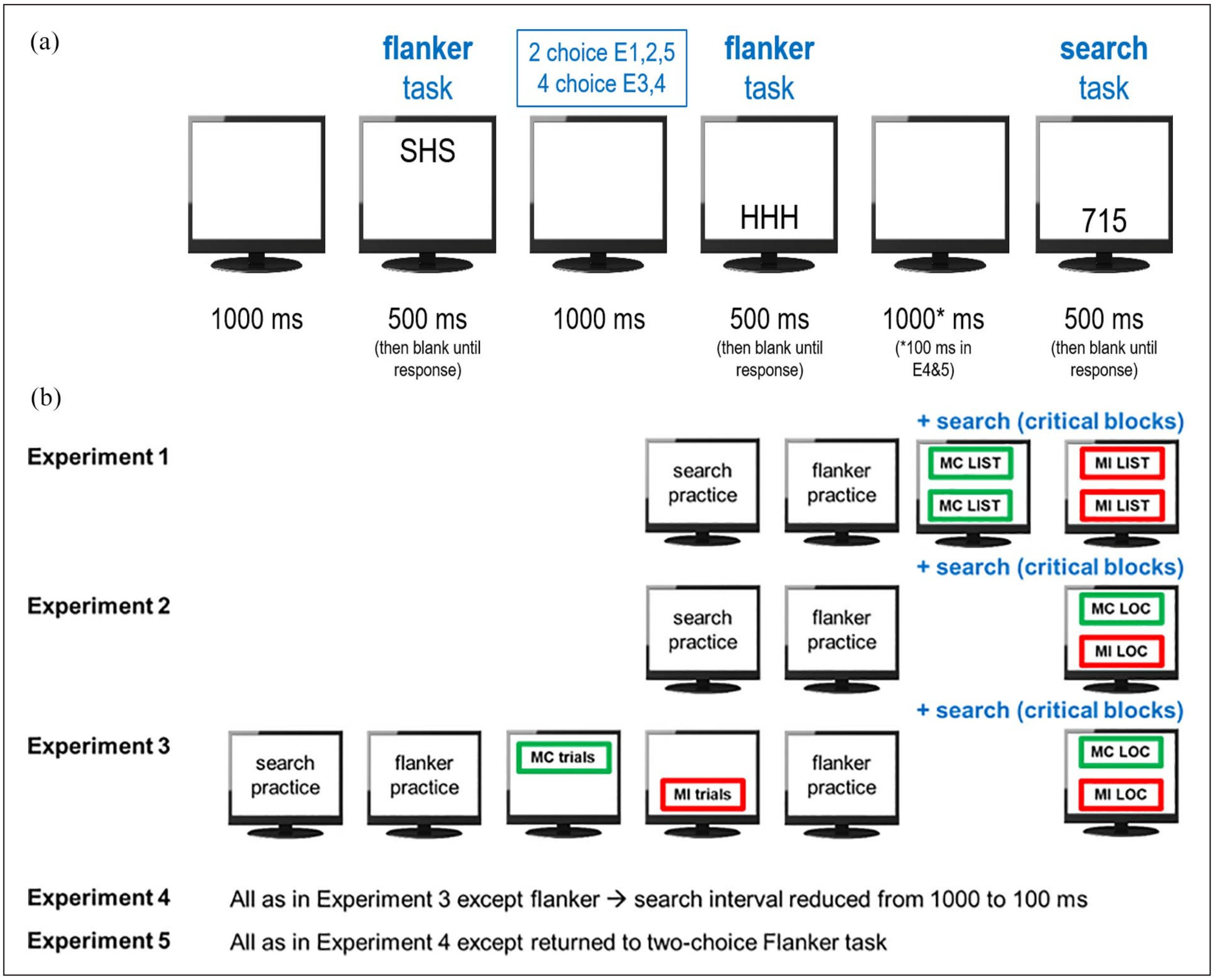

Figure 1. Methods of the experiments. Panel (a) depicts an example three-trial sequence from the critical trials in which the flanker and search task were interspersed. Panel (b) depicts task order details across all five experiments. See text for more detail.

(referred to subsequently as the *upper* and *lower* locations).

Procedure and design. The design and procedure were modelled closely after Wendt et al. (2012). See Figure 1 for more details.

Trial level details. There was a 1,000 ms blank screen preceding each trial. Then, the string appeared for 500 ms, followed by a blank screen until the participant made a response. In the flanker task, participants indicated the identity of the central letter in the string (pressed the left arrow key for "H" and the right arrow key for "S"); in the search task they indicated whether a 3 or 7 was presented in the string (left arrow key for "3," right arrow key for "7"). Incorrect trials were followed by the word "Incorrect!" printed centrally on the screen in red for 800 ms. In the flanker task the central target letter was chosen randomly between S and H on each trial (then flanking letter stimuli were determined by trial compatibility). In the search task the target number (3 or 7), the other two digits in the string (from the set of 1,2,4,5,6,8,9, and 0), and which non-target location they each occupied were chosen randomly on each trial.

Task order details. The experiment began with instructions about the search task, followed by a practice block of 12 search trials (2 repetitions of 6 unique trials comprised of 2 on screen locations—upper or lower, hereafter referred to as *location*—and three within string positions of the target– left, centre, or right, hereafter referred to as *position*). This was followed by another 48-trial search practice (8 repetitions of the same 6-trial list). Next, participants received instructions about the flanker task and completed a practice block of 24 trials (3 repetitions of 8 unique trials comprised of 2 locations and 4 trials necessary per location to obtain 25%/75% compatibility balance; i.e., 3 compatible stimuli and 1 incompatible stimulus were presented in each location in the MC list whereas 1 compatible and 3 incompatible stimuli were presented in each location in the

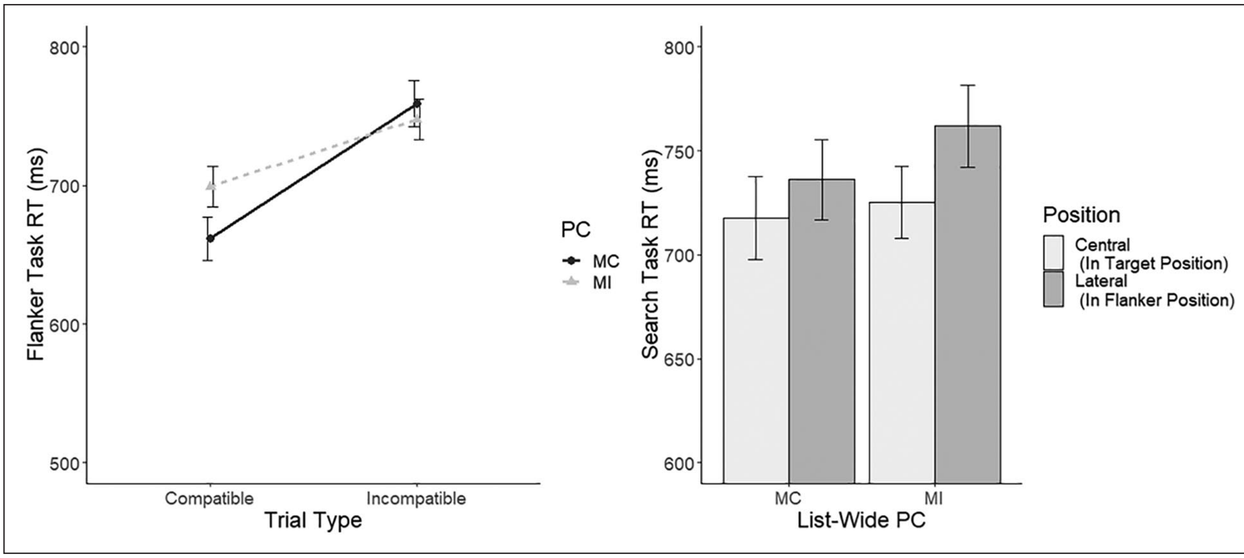

Figure 2. Results from the flanker task (left panel) and search task (right panel) of Experiment 1. In the flanker task, there was a typical list-wide PC effect with larger compatibility effects in the MC than MI list. In the search task we reproduced Wendt et al. (2012) in that *Target Position Benefits* (faster RT for target position, lighter bar, relative to flanker position, darker bar) are larger in the MI than MC lists in spite of the fact that we had two locations in each list.

MI list). This list maintained the PC bias that each participant would experience for the first half of the experimental session; the second half was the opposite PC (order of PC was counterbalanced across participants).

The experimental session was comprised of 14 *mixed* blocks of 72 trials (48 flanker and 24 search trials). Twenty-four of the flanker trials (12 in each location, 9 in the dominant and 3 in the non-dominant compatibility of the block) were followed by another flanker trial whereas 24 identical flanker trials were followed by a search trial (search position balanced equally within the location by compatibility combinations). The search trial was always in the same location as the preceding flanker trial (cf. Hutcheon et al., 2017 who only found spillover of reactive control across consecutive trials when location repeated in a location-specific PC paradigm). During the first seven blocks, both locations had one compatibility bias (e.g., MC). In the following seven blocks, both locations had the opposite compatibility bias (e.g., MI). The first block in each PC bias was treated as warmup and not analysed.[3]

Results

We present results from the 12 non-warmup mixed trial blocks. For RT analyses on both the flanker and search tasks, trials with RTs below 200 ms were excluded, then RTs more than 2.5 standard deviations away from each participant's mean of correct trial RTs were removed (2.1% of flanker trials, 2.4% of search trials). For error rate analyses, no trims were applied (i.e., all trials were included), and error rate was calculated as the number of error trials divided by total trials.[4] If any participant had an error rate above 33.3% in the flanker task on incompatible trials they would have been excluded from analyses (cf. Weidler & Bugg, 2016), but none did. We applied this exclusion criterion of 33.3% to lateral position trials in the search task as well, but no participants met that error rate cutoff either. See Figure 2 for flanker and search task RT results. The raw trial data for this and all subsequent experiments can be found at https://osf.io/yxrs7.

In addition to the standard ANOVAs reported below, we also calculated Bayes factors using JASP statistical software, jasp-stats.org (see for example Marsman & Wagenmakers, 2017) for any expected interaction effects in the flanker and search tasks which did not reach statistical significance at $\alpha = .05$. Bayes factors may be interpreted as providing more continuous evidence for the null (factors < 1) versus alternative (factors > 1) hypothesis, in contrast to the dichotomous retain-or-reject approach of null hypothesis significance testing (Marsman & Wagenmakers, 2017). There are approximate effect sizes with Bayes factors, where a value between 1/3 and 1 indicates anecdotal evidence for the null hypothesis, and a value lower than 1/3 indicates more substantial evidence for the null hypothesis (Wagenmakers et al., 2011). The reverse is also true: a value between 1 and 3 indicates anecdotal evidence for the alternative hypothesis, and a value higher than 3 indicates more substantial evidence for the alternative hypothesis (Wagenmakers et al., 2011). For example, BF = 10 indicates that the observed pattern of data is 10 times more likely under the alternative than null hypothesis, whereas BF = 0.10 (1/10) indicates that the observed pattern of data is 10 times more likely under the null than alternative hypothesis, with both examples falling in the range

Table 1. Error rates across experiments.

Experiment	Task	List/location PC	Compatibility	Position	Prop. error
1	Flanker	MC	Compatible		.055
			Incompatible		.122
		MI	Compatible		.062
			Incompatible		.081
	Search	MC		Central	.058
				Lateral	.075
		MI		Central	.032
				Lateral	.061
2	Flanker	MC	Compatible		.067
			Incompatible		.122
		MI	Compatible		.067
			Incompatible		.106
	Search	MC		Central	.061
				Lateral	.094
		MI		Central	.059
				Lateral	.089
3	Flanker (critical blocks)	MC	Compatible		.038
			Incompatible		.060
		MI	Compatible		.040
			Incompatible		.056
4	Flanker (critical blocks)	MC	Compatible		.072
			Incompatible		.085
		MI	Compatible		.074
			Incompatible		.076
5	Flanker (critical blocks)	MC	Compatible		.049
			Incompatible		.078
		MI	Compatible		.038
			Incompatible		.082

PC: proportion compatible; MC: mostly compatible; MI: mostly incompatible; Experiment 1: list-wide PC manipulation; Experiments 2–5: location-specific PC manipulation.

of substantial or decisive evidence for their respective hypotheses. We henceforth report Bayes Factors 10 Inclusion ($BF_{inclusion}$), a ratio of expressing evidence for the alternative hypothesis that averages all models including that factor or interaction against the null model.

Flanker task. Flanker task RTs were analysed with a 2 PC (MC or MI) x 2 compatibility (compatible or incompatible) repeated-measures ANOVA. An interaction of the factors, $F(1,39)=118.48$, $p<.001$, $\eta^2p=.75$, revealed the list-wide proportion congruence effect: the flanker compatibility effect was reduced in the MI list (49 ms) compared to the MC list (97 ms; henceforth all RT measures are reported in ms though the "ms" is omitted); see Figure 2. There was also a reliable main effect of compatibility, $F(1,39)=536.41$, $p<.001$, $\eta^2p=.93$ ($M_{compatible}=680$, $M_{incompatible}=753$) and a marginally significant main effect of PC, $F(1,39)=3.33$, $p=.076$, $\eta^2p=.08$. The same analysis on error rate revealed a similar pattern of: an interaction, $F(1,39)=20.82$, $p<.001$, $\eta^2p=.35$, with a larger compatibility effect in error rate in MC (.067) than MI (.019) lists; a main effect of compatibility, $F(1,39)=27.94$, $p<.001$, $\eta^2p=.42$ ($M_{compatible}=.058$, $M_{incompatible}=.101$); and a main effect of PC, $F(1,39)=4.75$, $p=.035$, $\eta^2p=.11$, with more errors in the MC list (.088) than the MI list (.071). Please see Table 1 for error rates across experiments.

Search task. Search task RTs were analysed with a 2 PC (MC or MI) x 2 position (central or lateral; lateral = pooling of left and right positions[5]) repeated-measures ANOVA. The analysis revealed a main effect of position, $F(1,39)=26.53$, $p<.001$, $\eta^2p=.41$, with RTs faster when the search target was in the central location ($M=721$) compared to the lateral locations ($M=749$), as is expected when a task is interleaved with the flanker task that requires attention to the central position (cf. Wendt et al., 2012). Importantly, PC modulated that effect, $F(1,39)=7.61$, $p=.009$, $\eta^2p=.16$. To decompose the interaction here and henceforth we calculated target-position-benefits (RT in search task when target was in lateral positions − RT in search task when target was in central position) as a function of PC. The interaction arose because the target-position benefit was larger in the MI list (37 ms), $t(39)=5.56$, $p<.001$, than in the MC list (18 ms), $t(39)=3.07$, $p=.004$,

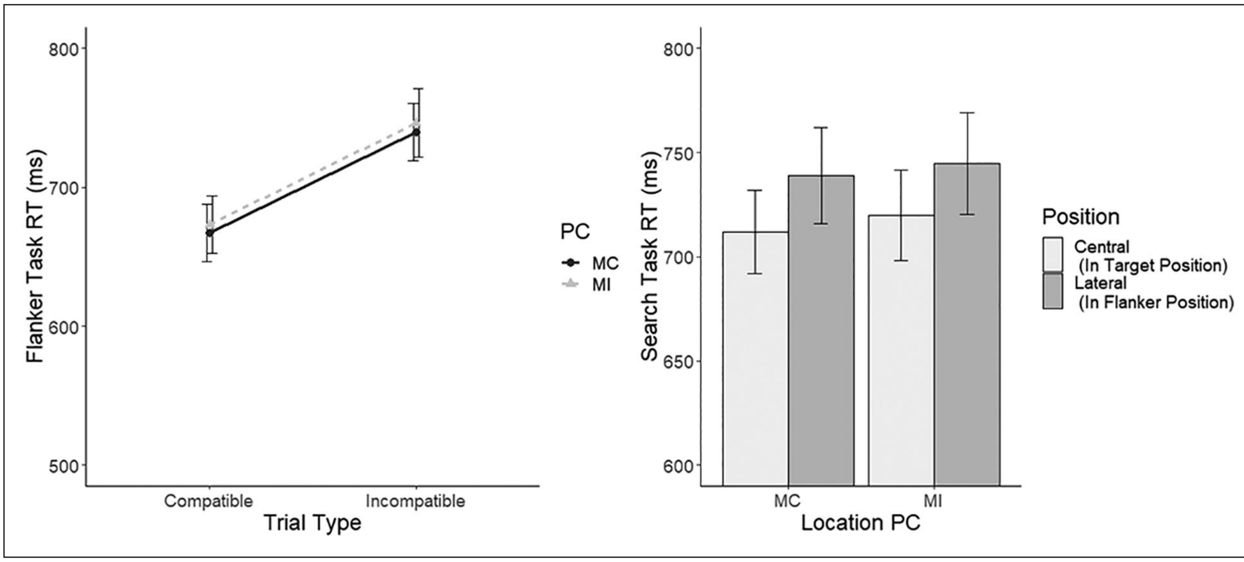

Figure 3. Results from the flanker task (left panel) and search task (right panel) in Experiment 2. There was no Location-PC effect in the flanker data, and no modulation of the target position benefits in search task RT data.

as is expected if CIF is operating during the flanker task. There was a marginally significant main effect of PC in the search task RTs, $F(1,39)=3.40$, $p=.073$, $\eta^2 p=.08$.

The same analysis on error rate also revealed an effect of position, $F(1,39)=21.36$, $p<.001$, $\eta^2 p=.35$, with more errors in the lateral ($M=.068$) than central ($M=.045$) positions. PC did not modulate this effect in error rate, $F(1,39)=2.19$, $p=.147$, $\eta^2 p=.05$, $BF_{inclusion}=2.09$. There was a main effect of PC, $F(1,39)=17.85$, $p<.001$, $\eta^2 p=.31$, with more errors in the search task in the MC (.067) than MI (.046) list.

Discussion

The goal of Experiment 1 was to reproduce the key finding of Wendt et al. (2012)—increased CIF in MI compared to MC lists—in the presence of location variability. Despite the location variability, we indeed found increased CIF in the MI list compared to the MC list. To our knowledge, this is the first study to demonstrate that CIF is not dependent on the stimuli being presented in a single fixed location, which could make it easier to maintain a conflict-induced filter in the MI condition. Importantly, this pattern also gives us licence to explore our primary question of interest in Experiment 2, by employing a location-specific PC manipulation to examine if CIF supports reactive, location-specific control.

Experiment 2

The goal of Experiment 2 was to address the following question—when manipulating the proportion compatibility (PC) on a location-by-location basis as opposed to a list-wide basis, will there be evidence for CIF?

Method

Participants. Forty additional undergraduates at Washington University that met the same criteria as in Experiment 1 participated.

Stimuli, apparatus, procedure and design. The method was identical to that of Experiment 1 except for one important change: instead of manipulating PC during the flanker task in a *list-wide* fashion, PC was manipulated in a *location-specific* fashion (e.g., for one participant the upper location on screen was always MC—75% compatible—whereas the bottom was always MI—25% compatible—across all blocks; location of PC was counterbalanced across participants). All participants completed 13 critical blocks (i.e., flanker and search tasks intermixed) blocks (1 warmup block that was not included in analyses followed by 12 test blocks).

Results

No participants committed errors on more than 33.3% of the flanker task incompatible trials, but one participant was removed for committing errors on over 33.3% of flanker-position search task trials in the test blocks. The flanker and search RT trims based on 2.5 SDs excluded 2.0% of trials and 2.3% of trials in the remaining participants, respectively. See Figure 3 for flanker and search task RT results.

Flanker task. The 2 PC (MC or MI) x 2 compatibility (compatible or incompatible) repeated-measures ANOVA on flanker task RTs from the critical blocks revealed a main effect of compatibility, $F(1,38)=218.95$, $p<.001$, $\eta^2 p=.85$

($M_{compatible}$=670, $M_{incompatible}$=743), but no PC x compatibility interaction, $F<1$, $BF_{inclusion}$=.245,[6] or main effect of PC, $F<1$. For error rate, the interaction between PC and compatibility was in the right direction but marginally significant, $F(1,38)=3.54$, $p=.068$, $\eta^2 p=.09$, $BF_{inclusion}$=.634. There was a significant main effect of compatibility, $F(1,38)=53.48$, $p<.001$, $\eta^2 p=.59$ ($M_{compatible}$=.067, $M_{incompatible}$=.114), but no significant main effect of PC, $F(1,38)=2.60$, $p=.116$, $\eta^2 p=.06$.

Search task. A 2 PC (MC or MI) x 2 position (central or lateral) repeated-measures ANOVA on search task RT revealed a main effect of position as in Experiment 1, $F(1,38)=16.64$, $p<.001$, $\eta^2 p=.31$ ($M_{central}$=716, $M_{lateral}$=742). Importantly however, this effect was not modulated by location-specific PC, $F<1$, $BF_{inclusion}$=.251; as can be seen in Figure 3 the target position benefits were equivalent in the MC (27 ms) and MI (25 ms) locations. There was no effect of PC, $F<1$. The analysis on error rate mirrored that of RT with only a reliable main effect of position, $F(1,38)=14.78$, $p<.001$, $\eta^2 p=.28$ ($M_{central}$=.060, $M_{lateral}$=.092); there was no significant PC by position interaction, $F<1$, $BF_{inclusion}$=.161, nor a main effect of PC, $F<1$.

Discussion

To our knowledge, this experiment was the first to explore if CIF supports reactive location-specific control. To examine this question, Experiment 2 was a reproduction of Experiment 1 with one important change—likelihood of conflict was manipulated in a location-specific instead of a list-wide manner. Unexpectedly, there was no reliable location-specific PC effect in the RT data (and although there was marginally reliable evidence in the error rate data, the Bayes factor indicated weak evidence for the null). There was also no evidence for CIF—specifically, there was not a difference in the target position benefits in the search task based on whether the search stimulus appeared in the location associated with a high or low probability of conflict. One interpretation is that CIF does not support on-the-fly control. However, a comparison of the flanker task results between Experiments 1 and 2 suggests caution in endorsing that interpretation. Specifically, the list-wide PC manipulation produced a large list-wide PC effect in Experiment 1 in RT and error rate, but the location-specific manipulation in Experiment 1 produced no effect in RT and only a marginal effect in error rate. Therefore, it is unclear whether CIF cannot support reactive location-specific control, or if we were unable to elicit a substantial enough modulation of control to observe evidence of CIF. Thus, in Experiment 3 we employed three empirically–driven changes to attempt to increase the magnitude of the location-based PC effect.

Experiment 3

In the current experiment we made three changes to the method of Experiment 2 to attempt to increase the magnitude of the location-specific PC effect to provide a more rigorous test of the research question. First, we presented blocked-context training of flanker trials prior to the mixed blocks. Specifically, in the first block a participant experienced a whole list of MC trials in the location that would subsequently be the MC location, and in the second block the participant experienced a whole list of MI trials in the location that would subsequently become the MI location. Such a manipulation has been successfully used to elicit context-based effects for more stubborn features like colour in the past (Lehle & Hübner, 2008; see also Crump, Vaquero, & Milliken, 2008; Fischer et al., 2014). Second, following this blocked training and prior to the critical blocks (i.e., in which the flanker and search task were intermixed), participants received additional practice with the flanker task in which the location-specific PC manipulation was active. The idea here was that participants may learn about location-specific PC more quickly, and therefore show larger location-specific PC effects for the flanker task in later blocks if they initially encountered the flanker stimuli without the search task. Finally, we changed the flanker task from a two-choice to a four-choice task because we had obtained sizable location-specific effects with a four-choice task in earlier work (e.g., Diede & Bugg, 2017; Weidler & Bugg, 2016). If these changes are effective, then a more robust location-specific PC effect will be observed in the flanker task. Moreover, if CIF can support reactive, location-specific control, then we should additionally find that search task performance varies as a function of whether search trials appear in the MI or MC location.

Method

Participants. Forty additional undergraduates at Washington University that met the same criteria as in Experiment 1 participated.

Stimuli, apparatus, procedure and design. The method was as in Experiment 2 except as follows. There were four letters used in the flanker task (S, H, C, and K; participants used the number pad keys 4, 8, 6, and 2 respectively to respond to each target letter, and the number pad 4 and 6 to respond in the search task). Flanker target letters were again chosen randomly on each trial, and then the flanking letters were chosen randomly in incompatible trials.

After the two search task practice blocks and the flanker task instructions participants received 24 trials (6 instances of each letter) of practice in which a single letter appeared centrally on the screen until response to learn the key

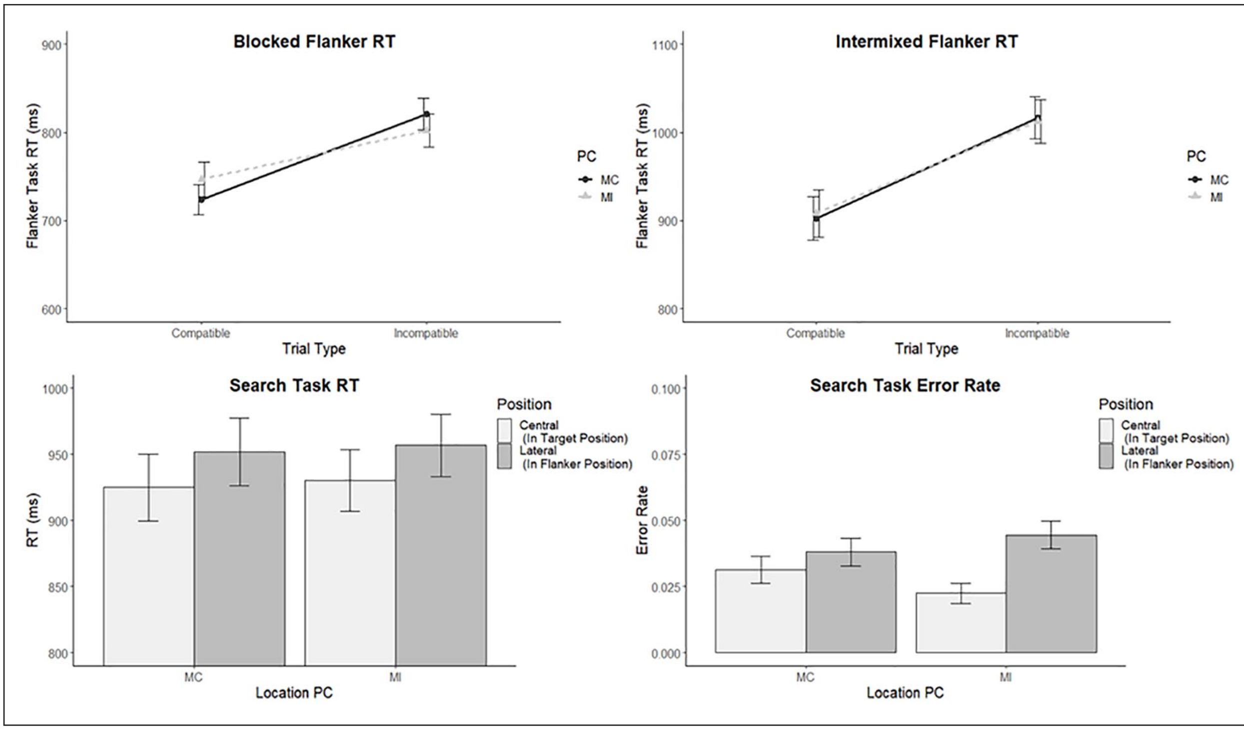

Figure 4. Results from Experiment 3. Top panel depicts RT results from the flanker task: In the blocked training trials (left) there was a PC effect but this effect was eliminated during the intermixed critical blocks (right panel). Bottom panel depicts results from the search task: there was no evidence for CIF in the RT data (left panel) but there was evidence for CIF in the error rate data (right panel), with a target position benefit in the MI, but not MC, location.

mappings. Next they had a 24-trial flanker practice block (with strings and the stimuli appearing in both locations) as in Experiment 2. After that, the blocked PC manipulation was employed – participants completed two 160-trial flanker blocks. Importantly, in each block the location *and* PC was kept constant (i.e., a participant with MC on the top might see the whole list of MC trials, on top, in the first block and then the whole list of MI trials, on the bottom, in the second block; order of MC vs. MI presentation was counterbalanced across participants and crossed with location of MC or MI). Next, participants completed a 96-trial "warmup" block of flanker trials alone (48 in each location, 36 in the dominant compatibility and 12 in the non-dominant compatibility per location). Then participants completed 13 intermixed critical blocks as in Experiment 2.

Results

No participants were excluded based on the flanker or search task error rate trims. The RT trim based on 2.5 SDs removed 2.5% of flanker trials in the blocked condition and 2.5% of trials in the intermixed condition, and 2.2% of search trials. See Figure 4 for blocked and intermixed flanker RT results, and search task RT and error rate results.

Flanker. The blocked (i.e., list + location) manipulation was successful in establishing a robust PC effect: A 2 PC x 2 compatibility analysis resulted in a significant interaction, $F(1,39) = 38.30$, $p < .001$, $\eta^2 p = .50$, because compatibility effects were larger in the MC block/location (97 ms) than the MI block/location (55 ms). Turning to flanker RT in the intermixed critical blocks, the same 2 x 2 analysis revealed only a main effect of compatibility, $F(1,39) = 352.76$, $p < .001$, $\eta^2 p = .90$ ($M_{compatible} = 906$, $M_{incompatible} = 1015$). There was no interaction, $F < 1$, as the compatibility effects were equivalent in the MC (114 ms) and MI (104 ms) locations, $BF_{inclusion} = .174$, nor was there an effect of PC, $F < 1$.[7] The error rate analysis revealed the same pattern—only an effect of compatibility, $F(1,39) = 24.68$, $p < .001$, $\eta^2 p = .39$ ($M_{compatible} = .039$, $M_{incompatible} = .058$), and no interaction between the factors, $F(1,39) = 1.19$, $p = .283$, $\eta^2 p = .03$, $BF_{inclusion} = .181$, or a main effect of PC, $F < 1$.

Search. The 2 PC x 2 position ANOVA on RTs from the search task revealed only a main effect of position, $F(1,39) = 24.27$, $p < .001$, $\eta^2 p = .38$ ($M_{central} = 928$, $M_{lateral} = 954$). As in Experiment 2, there was no effect of PC, $F < 1$, or interaction of PC and position, $F < 1$, $BF_{inclusion} = .166$. The same analysis on error rate revealed a main effect of position, $F(1,39) = 13.10$, $p = .001$, $\eta^2 p = .25$ ($M_{central} = .027$, $M_{lateral} = .041$), and no effect of PC, $F < 1$. However, the factors interacted in the error rate analysis, $F(1,39) = 4.86$, $p = .033$, $\eta^2 p = .11$. As can be seen in

Figure 4, this interaction arose because the decrement in performance when the search target appeared in the lateral locations compared to central was significant in the MI location (.022), t (39) = 3.67, p = .001, but not significant in the MC location (.007), t (39) = 1.52, p = .136, consistent with CIF.

Discussion

In Experiment 3 we attempted to increase the magnitude of the location-specific proportion compatibility effect with three changes to the method (blocked training, extra practice, use of a four- instead of a two-choice flanker task; see Figure 1). We indeed established a robust PC effect in the flanker task during the blocked training trials. However, when the search task was interspersed with the PC manipulation to index CIF, there was no evidence for the location-specific PC effect. Turning to search task performance, as in Experiment 2, in the RT data we saw no modulation of the target position benefit as a function of conflict (i.e., no evidence for CIF). However, there was evidence of enhanced CIF in the high-conflict location in the error rate data—target position benefits were evident in the MI, but not MC, location. We note that this pattern—finding an "aftereffect" of control without detecting the shift of control is not unprecedented (see e.g., Bejjani et al., 2018, Experiment 1). Given this preliminary evidence for CIF, we sought additional corroborative evidence in Experiment 4.

Experiment 4

In Experiment 4, we implemented the same three changes to the method as in Experiment 3 to again attempt to boost the magnitude of the location-specific PC effect. Although this effect weakened dramatically (and was no longer significant) once we interspersed the search task with the flanker task in Experiment 3, possibly these changes were influential in producing the first evidence that CIF might support location-specific control (as evidenced by the larger target position benefit for MI locations than MC locations in the error rate data). This is the expected pattern if participants are more heavily "filtering" information from the flankers when stimuli appear in the high-conflict location.

One possible theoretical explanation for the thus far weak evidence of CIF is based on a property of reactive control itself. Specifically, as noted earlier, reactive control is presumed to be engaged rapidly post flanker stimulus onset to resolve interference, *and* additionally, reactive control is transient (e.g., Braver et al., 2007). That is, the time course of reactive control differs from proactive control—whereas there is evidence that proactive control is sustained across trials, reactive control is triggered as needed and then quickly dissipates after a response is made (De Pisapia & Braver, 2006). Thus, it is possible that CIF underlies location-specific control in the flanker task, but by the time the search stimulus appears (~2,000 ms after the flanker onsets; flanker RT + 1,000 ms ITI) the filtering has faded in the case of reactive control but not proactive control (Wendt et al., 2012; Experiment 1 of current study). To assess this possibility, in Experiment 4 we reduced the ITI from 1000 ms to 100 ms and reasoned that a transient CIF mechanism, if operative, should be more likely to be observed under the present conditions.

Method

Participants. Forty undergraduates were recruited from the subject pool at the University of Toronto. They all were naïve to the purpose of the experiment, aged 18–25 and had normal or corrected-to-normal vision.

Stimuli, apparatus, procedure and design. The method was identical to that of Experiment 3 except that the blank screen before search trials was reduced to 100 ms (instead of 1,000 ms).

Results

Two participants were excluded from analyses; one for committing errors on over 33.3% of the flanker task incompatible trials, and the other for committing errors on over 33.3% of the search task trials. The RT trim based on 2.5 SDs removed 2.7% of flanker trials in the blocked condition and 2.7% of trials in the mixed condition, and 2.9% of search trials. See Figure 5 for blocked and intermixed flanker RT results, and search task RT and error rate results.

Flanker. During the blocked trials, PC and compatibility interacted, $F(1,37) = 8.46$, $p = .006$, $\eta^2 p = .19$. The compatibility effects were larger in the MC list/location (105 ms) than the MI list/location (66 ms). Turning to flanker RT in the intermixed critical condition, as in Experiment 3, the 2 x 2 analysis revealed only a main effect of compatibility, $F(1,37) = 178.13$, $p < .001$, $\eta^2 p = .83$ ($M_{compatible} = 975$, $M_{incompatible} = 1101$). There was no effect of PC, $F < 1$, nor did the factors interact, $F < 1$, $BF_{inclusion} = .147$, as the compatibility effects were 130 ms in the MC location and 123 ms in the MI location.[8] The same analysis on error rate revealed only a marginally significant main effect of compatibility, $F(1,37) = 3.83$, $p = .058$, $\eta^2 p = .09$, with no main effect of PC, $F < 1$, or interaction, $F(1,37) = 1.14$, $p = .293$, $\eta^2 p = .03$, $BF_{inclusion} = .111$.

Search. The 2 PC x 2 position analysis on RT revealed a main effect of position, $F(1,37) = 5.25$, $p = .028$, $\eta^2 p = .12$, with RTs to central targets ($M = 1,013$) faster than lateral targets ($M = 1,031$). There was no effect of PC, $F(1,37) = 1.25$,

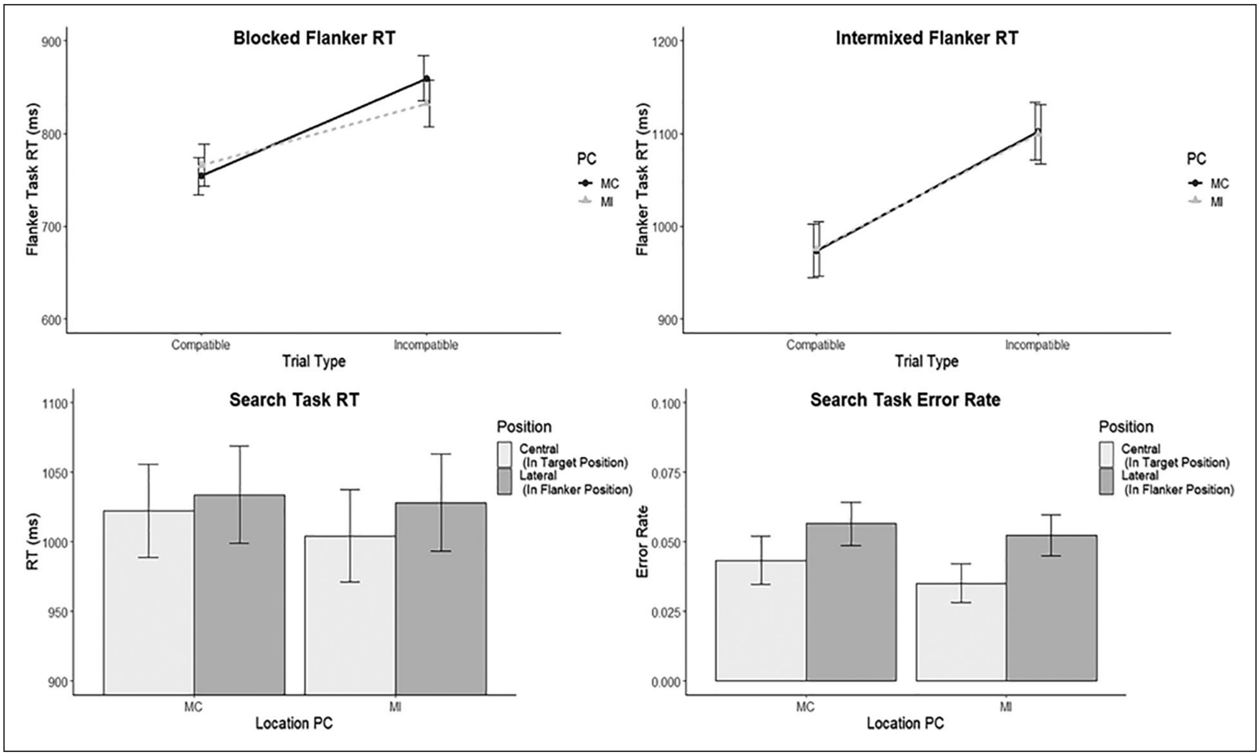

Figure 5. Results from Experiment 4. Top panel depicts RT results from the flanker task: In the blocked training trials (left) there was a PC effect that was eliminated during the critical blocks (right panel). Bottom panel depicts results from the search task: there was no evidence for CIF in the RT data (left panel) or error rate data (right panel), as the target position benefits were similar in the MI and MC locations for both.

$p=.271$, $\eta^2p=.03$, and PC did not modulate the position effect, $F<1$, $BF_{inclusion}=.182$. The same analysis on error rate revealed only a main effect of position, $F(1,37)=9.17$, $p=.004$, $\eta^2p=.20$ ($M_{central}=.039$, $M_{lateral}=.054$). There was no effect of PC, $F(1, 37)=1.93$, $p=.173$, $\eta^2p=.05$, and PC did not modulate the position effect, $F<1$, $BF_{inclusion}=.264$.

Discussion

In Experiment 4 we reduced the time interval between the response to the flanker stimulus and the search array. Given that reactive control is believed to be triggered upon stimulus onset and its activation is assumed to be transient, our interval between signals in previous experiments may have been too long to assess the after-effects of a conflict-triggered filter (De Pisapia & Braver, 2006). However, after shortening this interval, there was still no evidence for CIF in the RT data nor was there evidence for a location-specific PC effect with the interspersed search task.

Experiment 5

Across the three preceding experiments we sought evidence for CIF supporting reactive location-based shifts of control. The only evidence we observed for such a mechanism was in error rate for the search task in Experiment 3. However, across these three experiments we have seen a surprising but consistent pattern in the flanker task – interspersing a visual search task seems to have abolished the location-specific PC effect. Of course, this presents a barrier to investigating the primary research question and interpreting our findings: Are we seeing very limited evidence for CIF because reactive control does not employ such a mechanism? Or is it because the tool used to assess CIF (i.e., the search task) dilutes the location signal (i.e., locations no longer serve as strongly predictive signals of conflict) and concomitantly location-based adjustments in control? Thus far, the only hint of the typical location-specific PC pattern on flanker trials in the presence of the search task was in the error rate data in Experiment 2, which used a two-choice flanker task like Wendt et al. (2012; see also Experiment 1). Therefore in Experiment 5, in addition to continuing to implement the changes we incorporated into Experiments 3 and 4 (to increase the magnitude of the location-specific PC effect and maximise chances of observing effects of a transient CIF), we returned to a two-choice flanker task.

Method

Participants. Thirty-seven undergraduates were recruited from the undergraduate subject pool at Towson University.

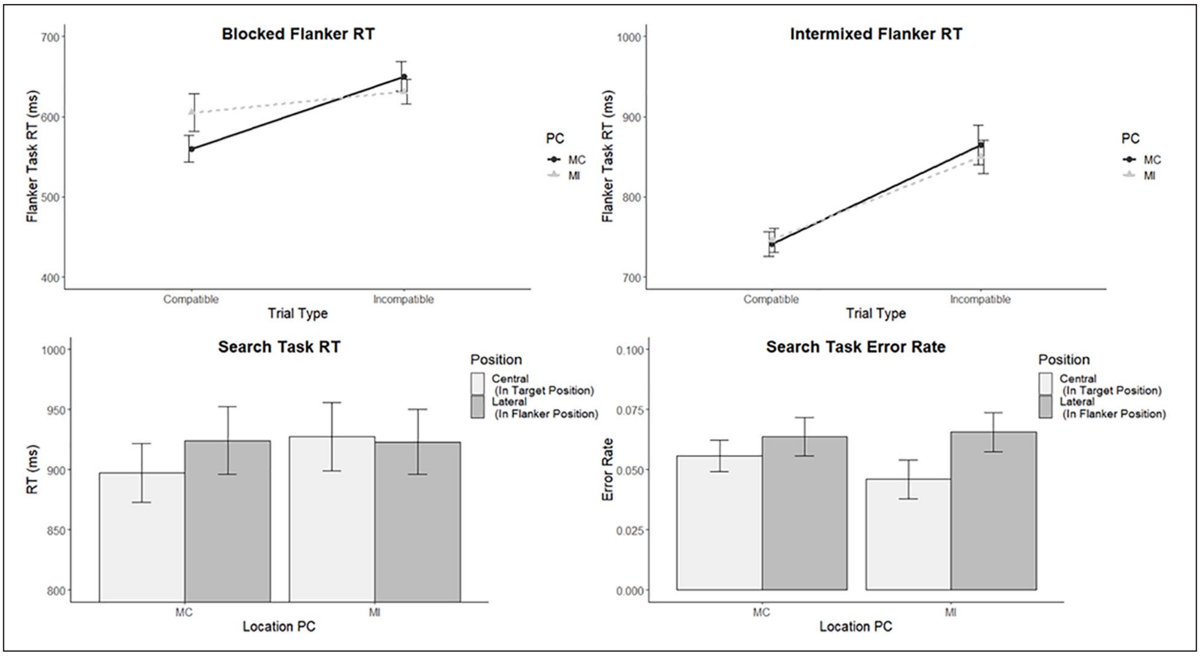

Figure 6. Results from Experiment 5. Top panel depicts RT results from the flanker task: In the blocked training trials (left) there was a robust PC effect that was also significant in the critical blocks (right panel). Bottom panel depicts results from the search task: there was no evidence for CIF in the RT data (left panel) or error rate data (right panel): in RT, target position benefits were large in the MC location but absent in the MI location (opposite of CIF predictions) and in error rate, target position benefits were similar in the MI and MC locations.

They all were naïve to the purpose of the experiment, aged 18–25 and had normal or corrected-to-normal vision.

Stimuli, apparatus, procedure and design. The method was as in Experiment 4 except that we employed a two-choice flanker task using the letters S and H (as in Wendt et al., 2012).

Results

Three participants were excluded from analyses; all three had an error rate over 33.3% in both the flanker task incompatible condition, and the search task flanker position. The RT trim removed 2.4% of flanker trials in the blocked condition and 2.6% of trials in the intermixed condition, and 2.7% of search trials. See Figure 6 for blocked and intermixed flanker RT results, and search task RT and error rate results.

Flanker. In the blocked trials, PC and compatibility again interacted, $F(1,33)=20.48$, $p<.001$, $\eta^2p=.38$. The compatibility effect was larger in the MC list/location (90 ms) than the MI list/location (26 ms). Turning to flanker RT in the intermixed critical blocks, unlike in Experiment 4, the 2 x 2 analysis revealed a significant interaction between PC and compatibility, $F(1,33)=4.29$, $p=.046$, $\eta^2p=.12$; there was a larger effect of compatibility in the MC condition (123 ms) than the MI condition (104 ms). There was also a main effect of compatibility, $F(1,33)=110.36$, $p<.001$, $\eta^2p=.77$ ($M_{compatible}=744$, $M_{incompatible}=857$), but there was no effect of PC, $F<1$. For error rate, the PC x Compatibility interaction was marginally significant though not in the right direction, $F(1,33)=3.30$, $p=.079$, $\eta^2p=.09$, $BF_{inclusion}=.432$.[9] There was a significant main effect of compatibility, $F(1,33)=28.13$, $p<.001$, $\eta^2p=.46$ ($M_{compatible}=.080$, $M_{incompatible}=.043$), with no main effect of PC, $F<1$.

Search. The 2 PC x 2 position analysis on RT revealed a main effect of position, $F(1,33)=4.21$, $p=.048$, $\eta^2p=.11$, with RTs to central targets ($M=912$ ms) faster than lateral targets ($M=925$ ms). There was no effect of PC, $F(1,33)=2.73$, $p=.108$, $\eta^2p=.08$. In this analysis, PC modulated the position effect, $F(1,33)=6.49$, $p=.016$, $\eta^2p=.16$. However, the target position benefit was 27 ms in the MC location; $t(33)=3.51$, $p<.001$, and absent (-4 ms) in the MI location $t(33)=-0.51$, $p=.616$), which is opposite that expected by a CIF mechanism.

The same analysis on error rate revealed only a main effect of position, $F(1,33)=6.37$, $p=.017$, $\eta^2p=.16$ ($M_{central}=.051$, $M_{lateral}=.065$). There was no effect of PC, $F<1$, and PC did not modulate the position effect, $F(1,33)=1.05$, $p=.312$, $\eta^2p=.03$, $BF_{inclusion}=.253$.

Discussion

In Experiment 5, for the first time, we observed a significant location-specific PC effect in RT for the flanker task in the presence of the search task.[10] In Experiment 2, which used the same two-choice flanker task, the effect size for the location-specific PC effect was 0 whereas here it was 0.12. This suggests the methodological changes were effective in increasing the size of the effect. Critically, however, in spite of this now significant PC effect, we did not find evidence for CIF in the search task. Rather, we found a pattern that was opposite to that which was predicted by CIF– there was a robust target position benefit for the MC location and no benefit for the MI location. This interaction is neither consistent with a perceptual account of reactive control (CIF), nor with the post-perceptual accounts discussed below.

General discussion

The goal of this series of experiments was to assess whether reactive location-specific control is supported by a perceptual filtering mechanism (CIF) that has been reported to support proactive control as induced by a list-wide proportion congruence manipulation in a flanker task (Wendt et al., 2012). After establishing that proactive control did indeed result in CIF when location varied but PC remained constant within a block (as this would be a necessary precursor to support location-based reactive control) in Experiment 1, we sought evidence for reactive control eliciting CIF in four additional experiments. Experiment 2 was essentially a direct replication of Experiment 1 with the important difference of eliciting modulations of control in a reactive location-specific manner instead of a proactive list-wide manner. We found no evidence for CIF in Experiment 2. On the assumption that the lack of evidence for CIF may be due to the lack of location-based modulations of control in the flanker task in Experiment 2, we aimed to boost the size of the location-specific effect in the flanker task through multiple empirically-supported changes (blocked training, extra flanker practice trials, and a four-choice flanker task) in Experiment 3. We established a robust PC (list + location) effect at the beginning of the session, but it disappeared once the search task was intermixed. And, while we again found no RT evidence for CIF, target position benefits in error rate were weakly modulated by location (i.e., conflict) in a manner consistent with CIF. Given this preliminary evidence for CIF we made one additional, theoretically-motivated change to the method in Experiment 4: We reduced the interval between the flanker task and the search task in an attempt to better capture transient adjustments of reactive control (e.g., Braver et al., 2007; De Pisapia & Braver, 2006), with the expectation that any effect of CIF based on location-specific PC may be short-lived. However, there was again no evidence for a PC effect in the intermixed blocks and there was no evidence for CIF. Finally, in Experiment 5, we returned to the two-choice task in which we (in Experiment 1) and Wendt et al. (2012) found evidence for CIF, and in which we found a hint of a location-specific PC effect in error rate when the search task was intermixed (in Experiment 2). In Experiment 5, for the first time we found a reliable location-specific PC effect when the search task was intermixed. However, we again found no evidence for CIF; in fact, the pattern of results in the search task was opposite that predicted by CIF.

Implications for CIF supporting reactive control

As mentioned in the introduction, the episodic retrieval account postulates that reactive shifts in control occur because the appearance of a stimulus in a certain location retrieves memories of past instances in that location (e.g., Crump et al., 2017; Crump & Milliken, 2009). This memory trace includes the relevant control setting associated with the location (i.e., heighten control in a high-conflict MI location). Our primary goal was to investigate whether heightened control in the MI location might reflect retrieval of a perceptual filter. We had hypothesised that reactive location-specific control could employ a perceptual filter that was triggered upon the appearance of a stimulus in the MI location but before full perceptual processing of the individual flanker stimuli. However, across four experiments (Experiments 2 – 5) we found minimal evidence for CIF supporting location-specific PC effects. Only the error rate data in Experiment 3 showed greater filtering in the MI than the MC location. Thus, it appears that reactive control does not readily employ a perceptual CIF, at least when assessed via a paradigm that successfully elicited CIF in the presence of proactive shifts of control (Wendt et al., 2012; Experiment 1).

Given the minimal evidence for reactive location-specific control employing a CIF mechanism, the next question is—what mechanism *is* location-specific control employing? One possibility is that the control setting triggered by stimulus onset in a certain location is not perceptual, and instead lies at a post perceptual (i.e., response or decision) stage (for discussion of post-perceptual mechanisms as related to proactive control, see Wendt et al., 2012). One post-perceptual account postulates that experience with conflict in a certain location (i.e., the MI location) increases how readily the cognitive system resolves conflict between a stimulus and a response. That is, instead of the appearance of a stimulus in a high conflict location triggering a perceptual filter (i.e., CIF) that reduces the accumulation of perceptual information the flanker location, the appearance of a stimulus triggers a process that more efficiently resolves conflict between the flankers and the incorrect response they automatically trigger (cf. Botvinick et al., 2001). An alternative post-perceptual account, based on modelling across a number of reactive control data sets, posits that the location-specific PC effect results from changes in post-perceptual decision criteria instead of any alterations to attentional processing of the stimulus (King, Donkin, et al., 2012).

Another possibility is that the location-specific PC effect does not reflect a control mechanism, perceptual or post-perceptual, but instead is attributable to compound-cue contingency learning (Schmidt & LeMercier, 2019; cf. Schmidt & Besner, 2008). According to this view, participants learn to predict the correct target response based on location + distractor conjunctions, which is possible for both locations in a two-choice task (e.g., in the MI location, one learns to respond H when S distractors are shown but in the MC location, one learns to respond S when S distractors are shown). On this view, the weak evidence for CIF in the present study may reflect that the mechanism supporting location-specific PC effects is compound-cue contingency learning, though this view does not account for other patterns in the location-specific PC literature (Crump & Milliken, 2009, 2017, but see Bugg et al., 2020; Hutcheon & Spieler, 2017; see also Weidler & Bugg, 2016).[11]

In summary, the present set of experiments found minimal evidence for perceptual filtering, and thus the post-perceptual accounts outlined above (or other similar accounts) offer plausible mechanisms that could underlie location-specific PC effects. However, we are reluctant to fully rule out a perceptual CIF mechanism because it is possible that the present approach (intermixed visual search task), modelled after that used to assess CIF in proactive list-wide control paradigms, might have underestimated the extent to which reactive control is supported by CIF as Wendt et al. (2012) anticipated, a possibility we consider momentarily.

Location-specific effects are disrupted by intervening tasks

Turning away from the search task itself, an intriguing and largely persistent finding that emerged in the present investigation was observed in performance of the flanker task in the critical blocks. That is, it was difficult to establish a reactive location-specific PC effect in the flanker task when the search task was interspersed. As a reminder, in Experiment 2 the location-specific PC effect was only marginally reliable in the error rate data, and much smaller than in comparable prior research (i.e., two locations, with a two-choice flanker task; e.g., Corballis & Gratton, 2003; King, Korb, & Egner, 2012); further, the Bayesian analyses indicated that the data were more likely under the null. Thus, in Experiment 3 and the later experiments we began the experiment with a blocked list-wide + location-specific training manipulation that has increased the magnitude of reactive PC effects previously (e.g., Lehle & Hübner, 2008). In these experiments we indeed established robust PC (list + location) effects in this blocked phase. However, in the critical blocks that included the search task, we found no evidence for the location-specific PC effect in either Experiment 3 or 4. When we returned to a two-choice flanker task in Experiment 5, we did find a reliable (albeit relatively small) location-specific effect. Collectively these findings suggest that interspersing a secondary task (the search task) with a conflict task (the flanker task) may produce weak, or even non-existent, location-specific effects.

The difficulty in establishing a location-specific PC effect may be related to the fact that a sizable proportion of trials (1/3 of the trials in each location per block) during the critical blocks signalled no conflict because they were search trials. Considering again the episodic retrieval account, reactive location-specific control is believed to result from learning about the relationship between location and conflict based on an accumulation of experience (i.e., instances) over trials (e.g., Crump & Milliken, 2009). If search trials are treated as "no conflict" (i.e., all compatible) trials during this learning process, then the presence of the search trials would weaken the PC of the high conflict "MI" location to a 50% compatible location (18 incompatible flanker trials + 6 compatible flanker trials + 12 "compatible" search trials) potentially reducing the location-specific PC effect. Such a pattern of learning seems surprising, however, given that the search task was so distinct from the flanker task (i.e., different stimuli, responses, task sets). As such, one could have reasonably expected that conflict experiences associated with the flanker task would accumulate as they typically do when no search task is present, and experiences with the search task would be represented separately and not influence the location-conflict associations. Indeed, it appears that this happens when the search task is interspersed with flanker trials when list-wide PC is manipulated (Wendt et al., 2012; current Experiment 1).

Another possibility then as to why the location-specific PC effects were weak during the critical blocks pertains to the need to regularly switch (e.g., Monsell, 2003) between the flanker and search tasks.[12] In Experiments 2–5 participants not only had to retrieve the appropriate control setting based on the location in which a stimulus appeared in the critical blocks but additionally they had to discriminate whether it was a search or a flanker stimulus to activate the appropriate stimulus-response translation rules. It is possible that the need to determine both the control setting *and* the to-be-performed task interfered with location-specific adjustments in control. Notably, this explanation could also account for why the list-wide PC effect was robust to the inclusion of the search task. In the case of the list-wide PC manipulation including in Experiment 1 where stimuli appeared in two locations (see also Wendt et al., 2012), there was a single relevant control setting per block. Thus, while participants did have to discriminate whether a given stimulus was a search or a flanker stimulus during critical blocks, they did not have to activate a location-specific control setting. Rather, the same control setting was applied to (and known at least implicitly in advance of) all stimuli.

Future directions

Much remains to be learned about the mechanisms supporting reactive location-specific control, and this set of experiments highlights several new avenues for future research. First, it may be possible that reactive control *can* in some circumstances employ CIF, and the paradigm we chose (with a goal being to investigate if location-specific reactive control employs a mechanism previously documented to support proactive control) was not optimal to assess it. In future research a flanker stimulus could be presented briefly (to presumably retrieve the episodic record and instate the appropriate attentional setting; e.g., Crump & Milliken, 2009) and then change to a probe stimulus (a search task or something similar) prior to necessitating a response. This would allow one to ascertain whether reactive control employs a stronger perceptual filter on the flanking locations in the MI context as assessed by a probe, particularly when the initial eliciting stimulus is a flanker stimulus (see Gottschalk & Fischer, 2017, for evidence that the stimulus must be identified as relevant [e.g., identified as letters if the flanker task involves letters] for a location to trigger retrieval of the control setting).

Of course, this does not address the concern that the need to task switch may have contributed to the non-existent or weak CSPC effects, and accordingly the weak evidence for CIF. One approach to circumvent this problem would be to harness prior methods from the negative priming literature (see e.g., Frings et al., 2015) to assess if filtering is taking place. Negative priming is often assessed in paradigms where a distractor on trial *n*-1 becomes a target on trial n, with the logic that the degree to which responses are slowed on trial n indexes potential suppression or inhibition (or reinstatement of the episodic context of ignoring) of that feature on trial n-1. Thus, applying this logic to the current research question one could potentially address if there is CIF without a task switch. Flanker trials would remain as in the present experiment. However, occasionally there would be intermixed single letter "probe" trials in the MC and MI locations that sometimes shared the *identity* of the flankers on trial n-1. If CIF is occurring, one should expect slower RTs on flanker-identity-repeat probe trials in the MI than MC location. Importantly, this eliminates the need for any task switch—participants indicate the "central" letter identity on every trial (just sometimes on the probe trials it is the only letter).

Finally, we have discussed that it remains an open question whether reactive control effects exist at a perceptual or post-perceptual stage. Some prior research has begun to address this question by applying evidence accumulation models (e.g., Ulrich et al., 2015) to reactive control data sets (e.g., King, Donkin, et al., 2012). These models examine whether location-specific PC effects can be attributed to factors such as the rate of accumulation of information about the stimulus (i.e., a pre-perceptual mechanism) or how much evidence is required to make a decision (i.e., post-perceptual mechanism). A fruitful future direction could be to apply similar models to datasets that involve an interspersed search task or diagnostic items.[13] Patterns observed for such items cannot be attributed to learning biases (e.g., Schmidt & LeMercier, 2019) since the same set of items appears in high and low conflict contexts. Therefore, an understanding of the mechanisms involved in producing behavioural shifts in these items may be crucial to further understanding reactive control.

Conclusion

Given the theoretical importance of understanding the mechanisms supporting various forms of cognitive control, we examined if reactive control was supported by a CIF mechanism previously documented to support proactive control. Using a list-wide PC manipulation, we first reproduced Wendt et al.'s (2012) finding of CIF when proactive control is engaged. Then in four additional experiments, we examined whether reactive location-specific control employs a CIF mechanism and the evidence was minimally supportive. Furthermore, we discovered that interspersing a task that does not itself signal conflict (the search task) disrupts location-specific PC effects. This set of findings highlights important future research directions, including using novel approaches to assess whether CIF supports reactive location-specific control and understanding why location-specific PC effects are less robust to interspersed tasks than list-wide PC effects.

Declaration of conflicting interests

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Notes

1. We note that Wendt et al. (2012) contrasted this perceptual account with two post-perceptual accounts (e.g., having their effect on response or decision processes) that could produce list-wide PC effects, the focus in their study. We return to post-perceptual accounts in the General Discussion.
2. In response to a reviewer's request, we ran a power analysis based on the effects in Experiment 1 (MC target position benefit [MC flanker RT−MC target RT] vs. MI target position benefit [MI flanker RT−MI target RT]) using a paired-samples t test. The effect size on this test came out to ~ d=0.4, which means that to obtain power of .80 we needed

an N of 44 to detect that difference—right around our actual sample size in the present and subsequent experiments. However, this analysis should be interpreted cautiously given the controversies regarding post hoc power analyses.

3. Two participants did not complete the warmup block before the second PC bias blocks began. No data patterns changed based on the inclusion/exclusion of those participants.
4. In response to a request that arose during the review process, we also analysed error rate in a second way. Specifically, we analysed error rate for trials that fell within the bounds established by the participant-based *SD* trim (i.e., trials with RTs above 200 ms and +/- 2.5 *SD* from the participant's mean); no data patterns changed when this trim was applied with the exception of one previously marginally significant pattern being rendered non-significant (see Results section of Experiment 5).
5. Note that Wendt et al. (2012) analysed their comparable search task data with three levels in the position variable (left, right, centre). We had no a priori reason to expect a difference between the left and right location, so we simplified the analysis as currently reported.
6. Given this somewhat unexpected result we also examined flanker performance in the first quarter and last quarter (3 blocks each) of the critical intermixed condition to see if there was evidence for the effect early after it was established in the blocked manipulation or evidence for re-establishment of the effect after more practice. There was no significant location-specific PC effect in any case.
7. Like Experiment 2, we checked the location-specific PC effect when looking within the first and last quarter of the flanker task, and still did not find a significant location-specific PC effect in either case.
8. We again examined the location-specific PC effect in the first three and last three blocks of the critical intermixed condition; it was not significant in either case.
9. This is the only analysis that changed when we analysed the error rate data after applying the bounds established by the participant-based *SD* RT trims (see more details in Footnote 4 of Experiment 1). The patterns were similar but in the trimmed analyses this interaction failed to reach even marginal significance.
10. For curious readers, we note that we also ran an additional experiment in which reactive control was elicited in an *item specific* manner (left/right arrows were MC biased and up/down arrows were MI biased, but all flanker task stimuli were presented in the same location, cf. Bugg et al., 2011). In a similar paradigm to that of the current experiment, we discovered a robust item-specific PC effect in the flanker task, $F(1, 38)=7.66$, $p=.009$ when the search trials were intermixed $\eta^2p=.17$, but no evidence of CIF in the search task, $F(1, 38)=0.002$, $p=.966$, $\eta^2p=.00$, $BF_{\text{inclusion}}=.097$.
11. Moreover, as Wendt et al. (2012) noted, if the list-wide PC effect in their two-choice flanker task was attributable to contingency learning, they should not have observed CIF (i.e., learning about distractors predicting responses should not spill over into influencing the search task), an argument that can be extended to the present Experiment 1. A contingency-learning account also cannot explain another key extant pattern demonstrating proactive control in the flanker task, the transfer of the PC effect to diagnostic items (novel items that were 50% congruent across lists; Bugg & Gonthier, 2020).
12. We thank an anonymous reviewer for raising this possibility.
13. Essentially, seeking evidence for CIF using a search task can also be conceptualised as seeking "far" transfer of reactive control processes—from biased stimuli in one task to unbiased stimuli in a completely different task (with unique stimuli and response mappings). Seeking evidence using diagnostic items (e.g., Crump & Milliken, 2009) may be considered "nearer transfer." We also note that King, Donkin, et al. (2012) did analyse data from a task in which each stimulus was a unique set of faces, so their results come from a paradigm that cannot be attributed to contingency learning (see also Spinelli et al., 2019 for support of that approach).

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